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Economic thought and numerical  
observations : studies in 'political  
arithmetic'

Anthony M. Endres  
University of Wollongong

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ECONOMIC THOUGHT AND NUMERICAL OBSERVATIONS:  
STUDIES IN 'POLITICAL ARITHMETIC'

A thesis submitted in fulfilment of the  
requirements for the degree of

DOCTOR OF PHILOSOPHY

from

THE UNIVERSITY OF WOLLONGONG

by

Anthony M. Endres, MSocSc

Economics Department

1982

This is to certify that this thesis has  
not been submitted  
for a degree to this or any other university,  
or any other institution.

Anthony Endres

ABSTRACT

This thesis investigates the epistemological status of 'raw' numerical observations, and to a lesser extent, more formal statistical constructs in economic thought. The definition of 'economic thought' used throughout this work follows Joseph Schumpeter's broad conception in that it includes two interrelated aspects: formal economic analysis and more everyday, commonsense thought. A central question is the issue of justifying numerical observations as valid knowledge. After a brief study of the knowledge and evidential status of numerical observations in the history of political economy mostly prior to the advent of Keynesian macroeconomics there follows a literary survey of the role and status of numerical observations in the writings of Menger, Hayek, Mises and some other 'new' Austrian economists. Some common tendencies in their methodological doctrines, as they relate to numerical observations, are apparent among the older Austrians and these appear to have descended to 'new' Austrians. A selected set of summary points are generated from the survey of Austrian perspectives and these are used to give direction to ensuing literary studies. These studies include: (i) A methodological analysis of the knowledge status and limits of numerical observations in economic analysis. The approach throughout this investigation is philosophical. Morgenstern's concept of the 'accuracy of economic observations' proves useful in this respect, but this study goes beyond the boundaries within which Morgenstern discussed the notion. This study delves into Machlup's critique of 'operationalism' in economics with special

reference to the role of numerical observations in 'testing' economic theories. A central conclusion is that the increasing availability of numerical observations for use by economists in economic analysis - to design formal statistical constructs and 'test' economic theories - cannot remove uncertainty involved in interpreting or conferring meaning on the data. Uncertainty in this context arises from philosophical problems with numerical observations highlighted by Austrian themes elaborated in this thesis. These themes include mistrust, on a philosophical level, of aggregation in economic analysis; emphasis on the subjective appreciation of unique micro-level particulars; emphasis on the historical nature of numerical observations and acceptance of a fundamental difference between objects of thought and their 'counterparts' in the realm of sense perception. (ii) A study of the design, use and interpretation of numerical observations in the writings of three early political arithmeticians - Graunt, Petty and Davenant. An 'Austrian' theory of knowledge, gleaned for the most part from earlier chapters is used to develop the concept of a numerical indicator. An indicator is not a mute statistic. It is defined as a value-loaded, proximate numerical counterpart of something (a 'thought') other than the numerical observations it arranges. This indicator concept is then used to study the role of numerical 'facts' in the work of the political arithmeticians. (iii) A third study takes one of the many other suggestions arising from the survey of Austrian perspectives and analyses the historical process of operationalising unemployment in Australasia over specific time periods with reference to

official statistical sources and methods. It moves away from the context of the formal analytic aspects of Schumpeterian economic thought and the role of numerical observations therein. It elaborates on the more everyday aspects of economic thought which provided the rationale for validating different ways of operationalising unemployment through history. Connections between counting rules and formal economic theories of unemployment are made in the Australian study but these connections seem rather strained. The New Zealand study details various influences moulding operational concepts and sheds light on the iterative, interactive official design and interpretation process involved in creating and altering these concepts over time.



ACKNOWLEDGEMENTS

This thesis has a long history. My interest in methodology, the history of economic thought and Austrian economics was originally cultivated during my undergraduate training in the mid-1970s by (the late) Dr. Bill Reindler at the University of Waikato, Hamilton, New Zealand. In retrospect, his encouragement was invaluable in an intellectual environment which otherwise gave so little status to these much neglected areas of research. In the Australian academic environment my immersion in these areas of research received more encouragement. Constructive suggestions for orientating this thesis along the lines of an Austrian theory of knowledge framework were received from Professor R. Snape at the 10th Australian Conference of Economists. Professor Snape's generous comments as a discussant of a paper on my thesis themes and structure were a valuable psychological boost at a time when I was coming to view the thesis proposal as far too broadly conceived. Also, Professor P. Groenewegen's interest and encouragement at the first Australian History of Economic Thought Conference, and subsequently, deserve special mention. He turned my fascination with seventeenth century history of thought literature into a serious research programme - only a small portion of which comes within the scope of this thesis. Thanks above all to Professor K. Blakey - I was fortunate to have the services of his tolerant, open mind in the reading of a series of chapter drafts. Thanks are also due to the economic historians in the Department of Economic History and Research

School of Social Sciences, A.N.U., for inviting me to present a seminar on Australian unemployment statistics. Their comments helped sharpen my presentation on this subject. Thanks to the archivists at the New Zealand Archives, and the many librarians at the Alexander Turnbull Library (Wellington) and National Library of Australia (Canberra) who assisted me during 1979-82. Thanks finally to Anne-Maree Stewart - her competence on the word processor enabled this work to be continuously re-drafted at short-notice.

TABLE OF CONTENTS

	Page
Abstract	ii
Acknowledgements	v
Table of contents	vii
List of tables	ix
List of appendices	x
Journal abbreviations	xi
List of additional abbreviations for the New Zealand study	306
 PART I <u>INTRODUCTION</u>	
1.1     Aims and statement of the problem	2
1.2     Outline, orientation and some connections to previous research	2
1.3     Limitations	6
 PART II <u>HISTORICAL BACKGROUND</u>	
2.1     Numerical observations in the history of political economy	9
 PART III(A) <u>SUBJECTIVIST PERSPECTIVES ON THE LIMITS OF OUR NUMERICAL KNOWLEDGE</u>	
3.1     Menger	38
3.2     Hayek	42
3.3     Mises <u>et.al.</u>	55
3.4     Shackle's epistemics	75
Epilogue	90
 PART III(B) <u>FURTHER PERSPECTIVES ON THE LIMITS OF OUR NUMERICAL KNOWLEDGE</u>	
3.5     Morgenstern and beyond	93
3.6     Designing numerical constructs: fiat measurement and the role of valuation	111
3.7 <u>Operationalism in Economics: Toward a Reformulation</u>	122
3.7(A)   Bridgman's operationalism in physics	122
3.7(B)   A first critique of operationalism	127
3.7(C)   Machlup on operationalism in economics	143
3.7(D)   Concluding reflections on operationalism	169

PART IV STUDIES IN THE HISTORY OF POLITICAL ARITHMETIC

4.1	Preamble: converting numerical observations to indicators	180
4.2	Political arithmetic: intellectual background	189
4.3	<u>Political arithmetic: three literary expositions</u>	195
4.3(A)	Graunt's 'shop-arithmetic'	195
4.3(B)	Petty's 'number, weight or measure'	210
4.3(C)	Davenant on 'the use of political arithmetic'	225
4.4	Conclusion: from statistics to indicators	242

PART V OPERATIONALISING UNEMPLOYMENT IN AUSTRALASIA: TWO HISTORICAL STUDIES

5.1	Preamble to the case histories	257
(A)	<u>Concepts in Australian Unemployment Statistics to 1940</u>	
5.2(A)	Introductory note to the Australian study	265
5.2(B)	Counting the unemployed in the census	266
5.3	Trade union unemployment statistics 1891-1940: a reassessment	277
5.4	Statistics of State labour exchanges: a N.S.W. case-study	288
5.5	Epilogue (1)	301
(B)	<u>New Zealand Unemployment Statistics: A Study in Changing Statistical Design and Interpretation to c.1960</u>	
5.6	Counting the unemployed in New Zealand before 1900	307
5.7	The evolution of unemployment statistics in New Zealand 1900-1940	321
5.8	New operational concepts to c.1960	371
5.9	Epilogue (2)	394

PART VI AFTERWORD 401Bibliographies:

Bibliography for Parts I, II, III and IV	406
Bibliography for Part V	432

LIST OF TABLES

Table		Page
3.1	Measuring the Outcome of the Balance of Payments	103
3.2	Rate of Growth of Real Australian Non-Farm GDP	107
4.1	Davenant's "Scheme of the Income and Expence of the Several Families of England for the Year 1688"	229
4.2	Davenant's "Scheme of the Naval Trade of England for 1688"	235
5.1	NSW Labour Exchange Statistics to June 1899	291
5.2	NSW Labour Exchange Statistics: 1919	296
5.3	Statistics of the Christchurch Immigration Office: 1884	309
5.4	Statistics of the Public Works Department: 1887	311
5.5	Men Assisted by the Bureau of Industries 1891-1912	315
5.6	Gazetted Union Schedule for Unemployment Statistics: 12/9/1925	327
5.7	Quarterly Return Schedule U-1 Sent to Unions 1925-1930	328
5.8	"Unemployment As Estimated by Industrial Unions": <u>MAS</u> Table Headings	330
5.9	Weekly Statistics of Vacancies Filled and Applications Outstanding (ILO: 1921)	337
5.10	Activities of Labour Department's Employment Bureaux 1921-1922	338
5.11(A)	Applicants for Relief on Registers: 8th June 1935	351
5.11(B)	Numbers of Applicants not Placed, and Numbers in Receipt of Relief: 8th June 1935	352
5.12	"Registered Unemployed Males" - Summary for 8th June 1935	354
5.13	N.Z. Placement Service: Enrolment Returns	358-9
5.14	"Numbers of Unemployed" in <u>AJHR</u> 1939	363
5.15	Unemployment Statistics: <u>NZSR</u> 1939	364
5.16	"Number of Applications For Unemployment Benefits" 31/3/43-31/3/44	368
5.17	Persons "Enrolled for Employment": March 1942	372
5.18	"Disengaged Persons Enrolled for Employment" 30/9/1945-31/3/1946	380
5.19	"Disengaged Persons Remaining Enrolled for Employment" 1954-1955	389
5.20	Unemployment at Census Dates 1945-1961	389
5.21	Department of Labour Statistics: "Disengaged Persons" 1949-1959	393

LIST OF APPENDICES

Appendix		Page
V(A).1	Explicit Unemployment Concepts in Australian Censuses 1891-1939	304
V(A).2	Return of Registered Unemployed from State Labour Exchanges to Commonwealth Government 1905	305
V(B).1	Review of Unemployment - Degree of Employability: NZ Department of Labour 1937	398-9
V(B).2	NZ Census Rules for Counting the Unemployed to 1961	400

JOURNAL ABBREVIATIONS

<u>AAPSS</u>	<u>Annals of the American Academy of Political and Social Science</u>
<u>AEHR</u>	<u>Australian Economic History Review</u>
<u>AEJ</u>	<u>Atlantic Economic Journal</u>
<u>AEP</u>	<u>Australian Economic Papers</u>
<u>AER</u>	<u>American Economic Review</u>
<u>AJES</u>	<u>American Journal of Economics and Sociology</u>
<u>AJS</u>	<u>Australian Journal of Statistics</u>
<u>BNLQR</u>	<u>Banca Nazionale Del Lavoro Quarterly Review</u>
<u>CJE</u>	<u>Canadian Journal of Economics</u>
<u>Ecom.</u>	<u>Econometrica</u>
<u>Econ.</u>	<u>Economica</u>
<u>EHR</u>	<u>Economic History Review</u>
<u>EI</u>	<u>Economic Inquiry</u>
<u>EJ</u>	<u>Economic Journal</u>
<u>ER</u>	<u>Economic Record</u>
<u>HOPE</u>	<u>History of Political Economy</u>
<u>IJSE</u>	<u>International Journal of Social Economics</u>
<u>ILR</u>	<u>International Labour Review</u>
<u>JEI</u>	<u>Journal of Economic Issues</u>
<u>JEL</u>	<u>Journal of Economic Literature</u>
<u>JHI</u>	<u>Journal of the History of Ideas</u>
<u>JLE</u>	<u>Journal of Law and Economics</u>
<u>JPE</u>	<u>Journal of Political Economy</u>
<u>JPKE</u>	<u>Journal of Post-Keynesian Economics</u>
<u>JRSS</u>	<u>Journal of the Royal Statistical Society</u>

<u>NZEP</u>	<u>New Zealand Economic Papers</u>
<u>NWBQR</u>	<u>National Westminster Bank Quarterly Review</u>
<u>OEP</u>	<u>Oxford Economic Papers</u>
<u>QJE</u>	<u>Quarterly Journal of Economics</u>
<u>RES</u>	<u>Review of Economics and Statistics</u>
<u>RIW</u>	<u>Review of Income and Wealth</u>
<u>RSE</u>	<u>Review of Social Economy</u>
<u>SEJ</u>	<u>Southern Economic Journal</u>
<u>SJPE</u>	<u>Scottish Journal of Political Economy</u>
<u>ZN</u>	<u>Zeitschrift für Nationalökonomie</u>



## I. INTRODUCTION

### 1.1 Aims and Statement of the Problem

Expressed at a very high level of generality, this work aims to investigate the epistemological status of numerical observations (or data) in the interconnected areas of (Schumpeterian) economic thought and economic analysis. Its purpose is to develop a theory of the character and nature of knowledge generated from numerical observations in these areas. More specifically, the task here is to consider some of the limitations of our numerical knowledge emanating from the design and interpretation of numerical observations. As well, this work also includes an attempt to develop an appreciation of some of the consequences for knowledge of the availability of numerical data for use by economists. These broad aims imply, of course, that the content of the following studies will not exhaust the possibilities of the title.

### 1.2 Outline, Orientation and Some Connections to Previous Research

There are possibly many ways to contribute to a discussion and analysis of the problem and questions raised in 1.1 above. The problem had its suggestive origins in my reading of Austrian economics and was epitomised by Hayek's explicit concern for "the limitations of our numerical knowledge" in his 1974 Nobel Prize Memorial Lecture: "The Pretence of Knowledge".<sup>1</sup> As a result, the theoretical framework used to orientate this study in Part III(A) will be provided from a literary study of

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1 Hayek, F.A. 'The Pretence of Knowledge', in idem., Full Employment At Any Price? London, IEA Occasional Paper No.45, 1975, pp.30-41.

perspectives contributed by writers of Austrian School persuasion with special reference to their comments on numerical data. Preceding this, Part II provides an overview of the role and status of numerical observations in the history of economic thought mostly prior to the rise of Keynesian macroeconomics. This serves as a preamble to epistemological questions raised by writers of Austrian persuasion about the knowledge-status of numerical data. Many Austrian economists following on from Menger's pioneering methodological work during the 1880s have made genuine attempts to address questions of epistemology. As it relates to the aims of this work, some of these economists have been specifically concerned with the nature and limits of our numerical knowledge. Part III as a whole, then, involves a critical analysis of numerical observations as the supposed 'facts', drawing together some ideas from the writings of Menger, Mises, Hayek, Machlup, Morgenstern and others.

This work has two substantive links with previous research in economics. A central theme in the following studies can be traced to the writings of Machlup and Morgenstern detailed in Part III(B). In his 'Operationalism and Pure Theory in Economics' (1966) Machlup denied that there could be identity between an abstract concept and the set of practical operations in 'empirical' work that is purported to 'define' that concept.<sup>2</sup> In the narrower context of this work, Machlup asserted that there is always some 'distance' between a 'pure' concept and the

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2 Machlup, F. 'Operationalism and Pure Theory in Economics', in Krupp, S.R., ed., The Structure of Economic Science: Essays in Methodology, New Jersey, Prentice Hall, 1966, pp.53-67.

numerical data that 'defines' it. Machlup's expression of the inherent 'distance' between concept and operation or between concept and numerical data, is another way of identifying that realm within which limits on our numerical knowledge may lie. It is an expression which gives some direction for the following reflections and investigations into the problem set-out in 1.1. In his On the Accuracy of Economic Observations first published in 1950 and revised in 1963, Morgenstern approached the borderline of issues that are studied in this work. He found that 'accuracy' was "inseparably tied to the use" to which numerical observations are put. For Morgenstern, there was always a theory (concept, purpose) to which a statistic had to refer. An analysis of the process of meaningfully combining numerical data with its conceptual referent(s) would involve what Morgenstern understood to be "deep-lying problems which would require a more profound methodological study to unravel".<sup>3</sup> This is not to say, that the following work accomplishes what Morgenstern had envisaged as a "profound methodological study" of these questions. I cannot establish here a claim so grand, so pretentious. The overlapping theme in the issues raised by Machlup and Morgenstern is a key motivation for this work but not a proven conclusion. It indicates only a direction in which to proceed. I do not suppose that Machlup, Morgenstern and other writers of Austrian persuasion raise indisputable issues. I only attempt to reflect on the significance of their propositions if valid.

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3 Morgenstern, O., 2nd edition, Princeton, Princeton University Press, 1963, pp.3-4.

Part IV is an attempt to develop a theoretical notion of a numerical indicator out of the preceding Austrian studies. It is contended that an expanded definition of an indicator would serve to 'capture' the 'distance' between numerical observations designed operationally in the 'empirical' domain and the concepts that this data is used to refer to. Part IV is also an elaboration of one of a number of summary points raised from the study of Austrian and subjectivist perspectives in Part III (A). The bulk of Part IV is a series of case studies of three early English political arithmeticians: Graunt, Petty and Davenant. These studies are orientated in such a way as to illustrate the idea of numerical indicators. They consider the various factors involved in converting 'raw' numerical data into indicators.

Part V is devoted to one possible way of assessing the limitations on our numerical knowledge. It consists in narrative case histories of the design and use of series of numerical observations. It considers the official design and interpretation of series of unemployment statistics with special reference to Australian and New Zealand methods and sources. The Australian and New Zealand studies differ in orientation but they both take their points of departure from broad summary points derived from the subjectivist framework constructed in Part III (A). The Australian study reflects a rationalistic Austrian approach to the study of series of numerical data by explaining the stream of concepts or thought patterns underlying statistical design through time. The 'gulf' between statistics designed in the operational domain and more formal economic theories of unemployment propounded from time to time, is also

assessed. The New Zealand study elaborates on the many diverse phenomena influencing the design and interpretation of unemployment data through history. Both case histories detail in different ways the limitations, from time to time, of our numerical knowledge of unemployment. They also illustrate that the design and use of numerical observations, at least of unemployment, is an open-ended, iterative process.

Overall, in outline, this work involves predominantly literary studies in 'political arithmetic' on three levels. Apart from the historical sketch in Part II, Part III is a study in the methodology of economic analysis; Part IV is a study in the history of thought; and Part V is a study in economic history.

### 1.3 Limitations

From my reflections on this work, five weaknesses are suggested. First, limitations are necessarily a function of the assumptions made and the theoretical framework which directs the central propositions of a study. The limitations of this work are therefore partly an outcome of my choice of Austrian perspectives to provide a common starting point for each section. Some of the limitations of various Austrian views are mentioned as the work progresses. Second, while one component of this work is concerned with an analysis of the design and interpretation of numerical observations it makes incidental rather than direct or systematic reference to the problems of econometrics - and epistemological problems involved in the application of econometric techniques to numerical data. Also, as a result, more technical, as opposed philosophical,

issues surrounding aggregation in economic analysis are neglected.<sup>4</sup> This is also a defect of most, but not all, Austrian contributions to the subject. Third, it may be asked, 'why study seventeenth century political arithmetic and not more recent work on the design, use and interpretation of numerical data?'. My response is that, apart from entertaining a keen interest in the history of economic thought, the work of early political arithmeticians pioneered the active design, application and interpretation of numerical data for particular purposes. Their work provides as good a basis for insight on the notion of an indicator and on some ideas raised by the Austrians as current scholarly and interpretive work on such issues. Fourth, the historical studies in Part IV are selective in that they are limited in scope (i) to the problem of operationalising unemployment and (ii) to Australasian sources over specific time periods. Conclusions which may hold for unemployment in Australasia may not be applicable to other economic phenomena. Fifth, while this work is concerned with epistemology, no 'separate' theory of knowledge is provided directly from more recent philosophy or the sociology of knowledge to help develop an all-embracing framework for the case study sections. Instead a theory of knowledge is drawn from some common threads in Austrian (and other subjectivist) writings - with all the drawbacks this might suggest. This turns my list of major limitations full circle.

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4 A pioneering contribution in this technical field was made by Green, H.A.J. Aggregation in Economic Analysis, Princeton, Princeton University Press, 1964; and later, idem., 'Aggregation in Macroeconomics', in Harcourt, G.C. ed., The Microeconomic Foundations of Macroeconomics, IEA, Macmillan, 1977, pp.179-94.

## II. HISTORICAL BACKGROUND



## 2.1 Numerical Observations in the History of Political Economy

It is perhaps trivially true that 'statistics', in the sense of numerical observations, have played pivotal roles in the development and implementation of public policies. Indeed, the history of the organisation of official statistics is closely associated with a desire to "set out the milestones for the guidance of the administrator and legislator".<sup>1</sup> It is only in comparatively recent times that formal numerical constructions of aggregates such as production, income, employment, prices, labour force and so on, have been purposively and systematically designed for disparate economic research and policy uses. The etymology and history of statistics has taken the form of a long series of protracted episodes.<sup>2</sup> A brief sketch should suffice for the purposes of this work.

The term 'statistics' was originally applied to comparative descriptions of States or of territory (Staatenkunde), best represented by the seventeenth and eighteenth century German science of cameralistics (Kameralwissenschaft). The notion of a comparison of States can, of course, be traced back to Aristotle's Politeiai.<sup>3</sup> The early German cameralists of whom

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1 Koren, J. 'Introduction', in idem., ed., History of Statistics, New York, Macmillan, 1918, p.1.

2 For detailed, but by no means identical versions of the etymology and history see John, V., 'The Term "Statistics"', JRSS, Vol.46(4), 1882, pp.656-679; Yule, G.U., 'The Introduction of the Words "Statistics", "Statistical" into the English Language', ibid., Vol.68(2), 1905, pp.391-396, and Westergaard, H. Contributions to the History of Statistics, London, P.S. King, 1932.

3 See Westergaard, ibid., p.4.

Seckendorff, Conring and Becher were among the most outstanding, were chiefly jurists. They thought about the State in terms of the interests of German territorial Princes and their officials - interests which were largely focussed on estate and resource management and the care of State property. Many of their 'statistics' were non-numerical, viz., descriptions of the condition of nations in terms of climate, geographical setting, industrial organisation, military and financial resources, legal institutions, religious customs and so forth. According to Albion Small, cameralism "was a theory of managing natural resources and human capacities so that they would be most lucrative for the prince in whose interest the management was conducted".<sup>4</sup> Later cameralists meant by statistics the collection of both numerical and non-numerical 'facts' concerning States. Although, in an early study of the relationship between political economy and statistics, J.N. Keynes noted that even with later cameralists such as Achenwall, "[v]erbal description took the first place, and figures were

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4 Small, A.W. The Cameralists: The Pioneers of German Social Polity, New York, Burt Franklin, 1909, p.591. C.f. Cannan, E. A Review of Economic Theory, London, P.S. King, 1930, p.13. Cannan maintains that the name cameralist comes from the idea of a King's chamber - the treasury or counting house of the old rhyme: "The King was in his counting house, counting out his money, The Queen was in the parlour eating bread and honey". In a much broader sense, Small identifies cameralism with any "systematized governmental procedure, the application of which was made in the administrative bureaus" of the ruler. (op.cit., p.18).

used merely as accessory thereto".<sup>5</sup> By the early 1800s however, particularly in Britain, it became more and more acceptable to regard statistical work as having strong numerical connotations. Despite this tendency it was not apparently until the early twentieth century that statistics came to mean solely numbers and methods of analysing numbers.<sup>6</sup>

All definitional issues aside, if 'statistics' is concerned with the design and use of numerical observations, then the use of statistics for the purposes of government is evident, sporadically, since classical antiquity.<sup>7</sup> There was evidence, for instance, of reasoning with figures in ancient Greece;<sup>8</sup> of censuses in Rome from 435 BC for the purposes of public administration and finance;<sup>9</sup> for administrative purposes both

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5 Keynes, J.N. Scope and Method of Political Economy [1890], Fourth edition 1917, N.York, A.M.K. Reprints of Economic Classics, 1963, p.329.

6 On this point refer, e.g., to Cullen, M. The Statistical Movement In Early Victorian Britain, New York, Harvester Press, 1975, pp.11-12.

7 For a thorough survey of the early history, see Meitzen, A. 'History, Theory and Technique of Statistics', Supplement to the AAPSS, March 1891, Translated by R.P. Falkner.

8 Ibid., pp.16-17.

9 See, e.g., Boak, A.E., and Sinnigen, W.G. A History of Rome to AD.565, Fifth Edition, New York, Macmillan, 1965, pp.76, 285 and 364-5. An official quinquennial census was instituted in 435 BC. in order to register voters. A later revised Census associated with Augustus, was run in conjunction with a geographical survey for the ultimate purpose of levying land taxes.

in biblical times,<sup>10</sup> and in medieval Europe.<sup>11</sup> Exceedingly summarily, from the medieval period, collection and use of numerical information for policy advocacy and government administration (as in both seventeenth century English political arithmetic and the statistical surveys in France by Colbert and Vauban) was intertwined with Western magic and the evolution of experimental science.<sup>12</sup>

The origins of mystical numerology have been traced to Pythagorean philosophy which contained the fundamental idea that only through number can man grasp the nature of the universe. Bell maintains that "numerology was born the hour Pythagoras discovered the law of musical intervals". For Pythagoras, "[a]ll things are fittingly ordered according to the nature of numbers; number is the eternal essence; God is number; number is God".<sup>13</sup> Mystical numbers were supposed to carry hidden meanings or concepts and were used as symbols to convey both numerical values and mystical concepts. The success of mystical

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10 Old Testament, Pentateuch, Book IV: Numbers 1-5. For a full commentary on the design and use of numbers in the Bible see, Davis, J.J. Biblical Numerology, Michigan, Baker Books, 1971, especially pp.49-91.

11 See, Hallam, H.E. 'Some Thirteenth Century Censuses', EHR, Vol.10, 1957-58, pp.340-361; Coleman, O. 'What Figures? Some Thoughts on the Use of Information By Medieval Governments', in Coleman, D.C., and John, A.H. eds., Trade, Government and Economy in Pre-Industrial England, London, Weidenfeld & Nicolson, 1976, pp.96-112 and Meitzen op.cit., pp.15-22.

12 See, e.g., Smith, A.G.R. Science and Society in the Sixteenth and Seventeenth Centuries, London, Thames and Hudson, 1972, p.182f.

13 Quoted from Bell, E.T. Numerology, New York, Century Co., 1933, pgs 81 and 84.

numbers was dependent largely on the imagination of the interpreter. An important part of the deterministic Pythagorean legacy was the suggestion that a universal order, harmony and proportion was recognisable in 'nature' or 'reality'. This order was translatable simultaneously into numbers, sounds, shapes and matter, so that all the world is number.<sup>14</sup> The decline of Western magic roughly corresponded with the rise of empiricism and experimental science as well as theories of probability. In the history of science literature connections are made between principles of magic and 'scientific' doctrines of observable and repeatable experimental results and empirical associations recorded in numerical form.<sup>15</sup> Twentieth century empiricism "is logically identical to magic, but our greater powers of observation, measurement and manipulation often mean that our empiricism is more effective".<sup>16</sup> The rationale of science since at least the seventeenth century has been the apprehension of nature in such terms as to make its control possible. It was necessary that phenomena be construed in such a fashion that the uniquely heterogeneous may be reduced to quantitatively homogeneous and hence predictable, lawlike developments. For if phenomena were regarded in terms of their

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14 See, Redgrove, H. Magic and Mysticism, New York, Universal Books, 1971, p.18 & ff.

15 See, Thorndike, L. History of Magic and Experimental Science, Vol.1, New York, Columbia University Press, 1923, pp.200-13, 370-3, and 504-22; Seligman, B. The Mirror of Magic, New York, Pantheon Books, 1948, p.482, and Dantzig, T. Number: the Language of Science, London, Macmillan, 1959.

16 Willer, D., and Willer, J. Systematic Empiricism: Critique of A Pseudoscience, Englewood Cliffs, N.Jersey, Prentice-Hall, 1973, p.17.

unique aspects each observed event or case would be everlastingly new, nonquantifiable, unpredictable and uncontrollable. Concentration on recording repeatable elements or patterns of resemblances meant that each observed case was an index of an underlying, hidden causal law or regularity conformable to previous experience. In short, the use of numerical constructions in record keeping helped reduce phenomena of science to manageable, if not controllable proportions.<sup>17</sup> Statistical analyses of collections of social data drawing on contributions to the mathematical theory of probability became popular especially after 1850. Adolphe Quételet, for example, was a vigorous exponent of these analyses. For Quételet the uniqueness and heterogeneity of individual acts and phenomena disappeared when the "human species [was] considered en masse".<sup>18</sup> Nowadays, of course, numerical observations are combined with statistical techniques based on probability theory and the law of large numbers and are used to help detect 'law-like' regularities in the subject-matter of the social sciences. They provide the supposedly precise, neutral language through which these regularities are expressed.<sup>19</sup>

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17 C.f., Thorndike, L. The Place of Magic in the Intellectual History of Europe, New York, AMS Press, 1967. In this work Thorndike shows how the knowledge, skill and power of man in uncovering hidden "bonds of sympathy between different things" was a key principle of Western magic through which man attempted to "coerce" and control nature (p.32).

18 Quételet quoted in Hankins, F.H. Adolphe Quételet As Statistician, New York, Columbia University Press, 1908, p.87.

19 See, Irvine, J., et.al., eds. 'Introduction' to Demystifying Social Statistics, London, Pluto Press, 1979, pp.1-8.

The collection and use of formal numerical constructs on matters relating to political economy as a 'separate' field of inquiry coincides, approximately, with the early history of 'scientific' statistics or political arithmetic beginning with John Graunt, William Petty and Gregory King in the seventeenth century.<sup>20</sup> The first full-scale discussion of the connections between statistics and political economy by J.N. Keynes in his Scope and Method of Political Economy [1890] defines statistics as "the employment of numerical data" and he traces this activity back to Petty's early work on national income accounts.<sup>21</sup> Keynes' discussion is limited in at least four respects. (1) First, he does not discuss Cantillon's work which attempted to make Petty's 'par' and equation between land and labour numerically operative. Schumpeter, for example, alleges that "Cantillon's econometric zeal derived its direction from Petty" but he goes on to lament the loss of a supplement to Cantillon's Essai [1755] which contained much raw price data and numerical observations on workmen's budgets.<sup>22</sup> (2) Second,

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20 Karl Pearson's lectures on the history of statistics during the 1920s at University College London start with John Graunt. For a list of his lecture headings and topics refer to M.G. Kendall, Studies in the History of Statistics and Probability Vol.1, London, Griffin & Co., 1970, pp.479-81. This compares with Meitzen, op.cit., and Westergaard, op.cit., who both trace the history of statistics back to the Greeks.

21 Keynes, Scope, op.cit., p.330.

22 Schumpeter, J.A. History of Economic Analysis, London, Allen and Unwin, 1954, p.218. For comments on the lost statistical supplement c.f., Higgs, H. 'Life and Work of Richard Cantillon', in Higgs' edition of Cantillon's Essai, London, Macmillan, 1931, pp.382-3.

Keynes omits any mention of a statistical element in Adam Smith's Wealth [1776]. In Smith's first book, and especially the chapter on rent, there is frequent use of intertemporal and international comparisons of corn, rice, gold and silver prices from which Smith draws theoretical conclusions. Smith devotes some space to what he calls "observations ... made upon the price of corn", in particular. He makes mention of Gregory King's work and tabulates English wheat prices for each year 1202-1750.<sup>23</sup> Alfred Marshall saw much merit in the work of Smith and his contemporaries for their active use of available numerical (and also non-numerical) 'facts':

Hume, Smith, Arthur Young, and others had been led by their own instinctive genius and the example of Montesquieu occasionally to compare social and economic facts of different ages and different countries, and to draw lessons from their comparison.<sup>24</sup>

Why does Keynes fail to mention statistical elements in the work of at least the eighteenth century English political economists? His silence on this matter may have been justified insofar as eighteenth century scholars viewed political arithmetic as a classification of form rather than one of substance. In this respect, Hume complained c.1752 that "all calculations concerning the balance of trade are founded on very uncertain facts and suppositions. The custom house books are ... an insufficient ground of reasoning; nor is the rate of exchange

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23 Smith, A. An Inquiry Into the Nature and Causes of the Wealth of Nations [1776], E. Cannan, ed., New York, Modern Library, 1937, p.191 et.passim, and pp.251-258.

24 Marshall, A. Principles of Economics [1890], 8th Edition, London, Macmillan, 1922, p.762.



much better".<sup>25</sup> Throughout the eighteenth century there were some advances in the study of political arithmetic as far as vital statistics were concerned.<sup>26</sup> There were also some national income estimates that borrowed heavily from Petty and King's work.<sup>27</sup> It is also clear that Sir James Steuart uses some of the results of Graunt, Petty and King as descriptive aids in his Principles of Political Oeconomy [1767].<sup>28</sup> However, it was not long before Hume's view was reinforced by Adam Smith's announcement: "I have no great faith in political arithmetic".<sup>29</sup> To Smith it probably seemed that the lead which had been worked on by the devotees of political arithmetic, who

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25 Hume, D. 'Of the Balance of Trade' in idem, Essays, Literary Moral and Political, London, Ward-Lock & Co., 1890, p.185.

26 See some of the papers held by the Royal Society on population matters from the death of Petty (1687) through to the publication of Smith's Wealth (1776), in Johnson, E.A., Predecessors of Adam Smith: The Growth of British Economic Thought, New York, 1937, Appendix A, pp.394-401. Further, the translation of V. John's work, 'The Term "Statistics"', op.cit., reveals that the works of English political arithmeticians were widely read on the continent. Many lessons from English political arithmetic were subsequently incorporated in the largely demographic works of S'Gravesande, Kersseboom and Sussmilch in the eighteenth century. But these writers did not label their studies 'political arithmetic'. See John, p.665ff.

27 Studenski, P. The Income of Nations, New York, New York University Press, 1958, p.40f.

28 Skinner, A.S., ed., Vol.I, Chicago, Chicago University Press, 1966, pp.51-3. Steuart used an article on political arithmetic either from the 1738 or 1751 edition of Chambers' 'Cyclopedia'. Steuart's "carefully detached empiricism" (Skinner, 'Introduction', p.ix), goes some way toward explaining his disposition to use the numerical results of the political arithmeticians for descriptive purposes.

29 Smith, Wealth, op.cit., p.501.

were also notably contemporaries of Newton, yielded comparatively slight if not precarious results. (3) Third, Keynes does not discuss the work of the Physiocrats, who, in the eighteenth century develop a more abstract mode of numerical analysis different in orientation from the main body of political arithmetic (although, not arguably different from Petty's desire to posit an abstract par between land and labour). Quesnay's famous Tableau Economique [1758] is, in modern jargon, the first input-output analysis establishing numerical relationships between aggregates of production and consumption, investment and capital replacement. The early political arithmeticians following Graunt and Petty, designed numerical constructs out of unidimensional numerical records (tax returns, customs and administrative returns). The Physiocrats on the other hand, use numerical constructions to represent components in a model of 'the economy' and, in the last analysis, the numbers stood for certain, hypothetical interrelations in 'the economy'. These two distinct orientations to numerical construction, as Schumpeter writes, have in common "the spirit of numerical analysis". This is evidently a reason why Schumpeter, in the History of Economic Analysis, combines an account of political arithmetic with Physiocracy under the rubric "The Econometricians and Turgot".<sup>30</sup> Though originally quite distinct, these two forms of quantification carried out largely by the political arithmeticians and by the Physiocrats respectively, tended to

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30 Schumpeter, History, op.cit., pp.209-249, especially pp.209-23 and p.241.

merge at a much later stage in the development of economic thought when unidimensional numerical records were more actively used to help check the strength or magnitude of hypothetical quantitative interrelations in 'the economy'. This merger was not discernible until the 1880's: "[t]he quantitatively empirical did not tend to become merged with the quantitatively abstract until economists with strong empirical interests had attained a high degree of technical proficiency (e.g. H.L. Moore, I. Fisher)".<sup>31</sup> The growth of numerical estimation of economic relationships did not come so much as a response to the assault on formal theory by the Historical Schools or Institutionalists.<sup>32</sup> It was a slow development, contributed to by the Physiocrats, by earlier developments in experimental natural science and probability theory, and above all by the demonstrated successes of pioneers such as Juglar, Mitchell, Spiethoff, Jevons, Moore, Fisher and others. (4) Fourth, Keynes does not consider the use of numerical observations to 'test' whether theories were true or false. Hence, according to Blaug, Keynes "provides not so much as a single example of any economic controversy that was ever resolved by a statistical test". Nineteenth century classical economists in Blaug's recent characterisation, were "verificationists". They did not use numerical data to test the truth or falsity of economic theories.

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31 Spengler, J.J. 'Quantification in Economics: Its History', in Lerner, D., ed., Quantity and Quality, Illinois, Free Press, 1961, p.176.

32 For instance there is no evidence of these influences on the development of Moore's work. See Stigler, G.J. 'Henry L. Moore and Statistical Economics', Ecom., Vol.XXX, January, 1962, pp.1-21.

Some data was used to discover the strength of minor 'disturbing causes' (cetera) in the work of, for example, Malthus and Cairnes. If these 'disturbing causes' accounted for discrepancies between facts and theory the theory was then said to have been wrongly applied - although the theory itself was still true.<sup>33</sup> Cairnes, for instance, observes that an economic theory "can neither be established nor refuted by an appeal to the records ... - that is to say, by statistical or documentary evidence bearing on the course of industrial or commercial affairs".<sup>34</sup> Data was used by Classical economists to define boundaries of application of their theories but the question of using numerical data to help falsify a theory was never contemplated.

Discussion of J.N. Keynes' treatment, in 1891, of the connections between statistics and political economy has caused this historical sketch to run too far ahead in terms of chronological sequence. For, in the meantime, in eighteenth and early nineteenth century Britain, the contributions of political arithmeticians-cum-demographers such as Halley, Young and Malthus were gradually being overshadowed and broadened by the statistical movement in early Victorian Britain. This movement was given much impetus by the establishment of the Royal

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33 Blaug, M. The Methodology of Economics: Or How Economists Explain, Cambridge, CUP, 1980, p.86. See also pp.55-85.

34 Cairnes, J.E. The Character and Logical Method of Political Economy, London, 1857, p.99. C.f., the second edition [1875], London reprint 1888, where this statement is deleted. In the 2nd edition, Cairnes admits only that statistics can help in "discovery of the minor influences affecting economic phenomena" (p.97, my emphasis).

Statistical Society in 1834.<sup>35</sup> Political economists (including Richard Jones, Nassau Senior and later Jevons, Edgeworth, Newmarch and Giffen) played a key role in the early work of the Statistical Society as attested in records of the early meetings.<sup>36</sup> Also, the establishment of regular International Statistical Congresses from 1853 was important for promotion of economic statistics and led to improvements in their reliability and completeness.<sup>37</sup> Of prime interest in this work is the evolution of thought connecting statistics and political economy. McCulloch discussed the question briefly in his Outlines [1825]. He distinguished between political economists and statisticians:

The object of the statistician is to describe the condition of a particular country at a particular period; while the object of the political economist is to discover the causes which have brought it into that condition, and the means by which its wealth and riches may be indefinitely increased. He is to the statistician what the physical astronomer is to the mere observer.<sup>38</sup>

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35 Cullen, The Statistical Movement, op.cit., gives a thorough account of developments along these lines.

36 Royal Statistical Society, Annals of the Royal Statistical Society 1834-1934, London, Royal Stat. Soc., 1934. Malthus played a key role in the formation of the Society a few months before his death (see p.20). See also O'Brien, D.P., The Classical Economists, Oxford, Clarendon Press, 1975, who notes that "the Society ... helped to provide [a] ... means of welding the Classical economists into a scientific community" (p.13).

37 See Westergaard, Contributions to the History of Statistics, op.cit., pp.172-90.

38 McCulloch, J.R. Outlines of Political Economy [1825], McVickar, J., ed., New York, AMK Reprints of Economic Classics, 1966, pp.49-50.

In his work on 'statistics' McCulloch did not confine himself to that which is exclusively couched in numerical form. McCulloch rejected the idea that "everything in statistics may be estimated in figures".<sup>39</sup> For McCulloch, statisticians must be concerned with verbal description of the state of things in general. This is evident, for instance, in the title of his treatise published in 1846, which, while purporting to be partly 'statistical', contained hardly any numerical information and did not incorporate any formal statistical techniques. The work was titled: A Descriptive And Statistical Account Of The British Empire Exhibiting Its Extent, Physical Capacities, Population, Industry And Civil and Religious Institutions.<sup>40</sup> A few years before the appearance of this work, the first volume of Tooke's History of Prices appeared and this emphasised numerical information per se and did not make clear reference to formal principles of political economy.<sup>41</sup> Not until 1843 is their an

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39 McCulloch [1842], quoted in Cullen, Statistical Movement, op.cit., p.11. On McCulloch the 'Statistician', see also O'Brien, D.P., J.R. McCulloch: A Study in Classical Economics, London, Allen and Unwin, 1970, pp.84-96.

40 McCulloch, J.R., Fourth edition, 2 Volumes, London, Longman, 1854. C.f., McCulloch's contemporary, the Scottish statistician-cum-political economist Sir John Sinclair, author of the 3 Volume Statistical Account of Scotland [1785-90]. Sinclair's ultimate aim was "to lay a foundation for a work I contemplated [but never wrote], 'The Code of Political Economy', on the sure basis of statistical [numerical and non-numerical] researches", The Correspondence of Sir John Sinclair, London, Colburn and Bentley, 1831, Vol.1, p.294, my bracketed inserts.

41 Tooke, W., History of Prices Vol.I [1838] (some later volumes with W. Newmarch), Introd. by Gregory, T.E., London, P.S. King, 1928. Gregory notes the influence of some principles of political economy on Tooke's monetary 'theory' (e.g., Ricardian elements, p.37). He also maintains that theoretical treatment in Vol.5 and 6 [1857] "reach[es] a very high level of accomplishment" (p.114). C.f., Schumpeter, History, op.cit., p.521, on Tooke's "deficient command of economic theory".

article directly relating political economy and statistics in the Journal of the Royal Statistical Society. Statistics were said to "present nothing but a dull and barren show of figures"; they were "the materials of ... political economy", and only when figures are united with principles do they afford a "test" of political economy.<sup>42</sup> Similarly, in one section of a paper on the meaning of the term 'statistics' in the same journal during 1865, Guy asked: "Is there a science of Statistics; and, if so, what are its Nature and Objects, and what is its Relation to Political Economy and Social Science?" Quoting Sedgwick, Guy considered science as a form of measurement or calculation, and statistical science as a form of calculation "in figures of arithmetic". For the discipline of political economy formal statistical techniques based on probability theories were not widely used at this time in comparison to their extensive use in actuarial science. Even so, Guy maintained that statistics were the outcome of "a numerical method", which could be used as "the raw material of political economy". Statistics were "not heaps of facts without reference to their use or application"; they were numerical constructions designed for application by political economists and other social scientists.<sup>43</sup>

In the Character and Logical Method of Political Economy, Cairnes championed the role of deduction and downplayed the

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42 Lawson, T.C. 'On the Connexion Between Statistics and Political Economy', JRSS, Vol.6, 1843, p.322.

43 Guy, W.A. 'On the Original and Acquired Meaning of the Term "Statistics"', JRSS, Vol.28, December 1865, pp.487, 488-9 and 492-3.

possibility of using numerical observations to induce empirical laws. Cairnes believed that economic laws had to rest on more paramount a priori foundations than that which numerical records could reveal on their own.<sup>44</sup> J.N. Keynes complained some years later in his work on methodology that Cairnes had "summarily dismissed" the issue of the relationship between statistics and political economy. Keynes proposed to redress the balance and he argued that statistics function to suggest empirical laws, and can be used to supplement deductive reasoning by checking its results. In addition, Keynes asserted that "statistics play a still more important part in the application of economic science to the elucidation and interpretation of particular concrete phenomena".<sup>45</sup> Henry Moore pointed out that although the pure mathematical economists Cournot, Jevons, Edgeworth, and Pareto had devoted parts of their work to the elaboration of the deductive phase of economics, each of them had conceived of "an inductive statistical complement of the pure science without whose developments the a priori instrument must lack concrete

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44 Cairnes, Character and Logical Method, op.cit., 1875 edition, pp.34-39. This corresponded with J.S. Mill's view. Refer Mill, 'On the Definition of Political Economy and on the Method of Investigation Proper to It', in Collected Works of John Stuart Mill, Vol.IV, London, RKP, 1967, p.329, passim.

45 Keynes, op.cit., pp.333-4. C.f., Jevons: "The deductive science of Economics must be verified and rendered useful by the purely empirical science of statistics. Theory must be invested with the reality of life and fact", Theory of Political Economy [1871] 4th edn., London, Macmillan, 1911, p.22.



effectiveness".<sup>46</sup> This is supported by Jevons' comment in his Theory of Political Economy:

the very abundance of our [numerical] data is perplexing. There is not a clerk nor book-keeper in the country who is not engaged in recording numerical facts for the economist. The private account books, the great ledgers of merchants and bankers and public offices, the share lists, price lists, bank returns, intelligence, custom-house and other Governmental returns, are all full of ... numerical data ... Thousands of folio volumes of statistical, parliamentary or other publications await the labour of the investigator.<sup>47</sup>

These views were expressed by political economists on the continent prior to 1900 (although recent scholarship suggests that some key continental economists were reluctant to embrace the numerical data available to them).<sup>48</sup> Engel's law of consumption, an empirical generalisation in 1857 drawing partly on Le Play's studies in Belgium and on Ducpetiaux's numerical data, stands out as an exception.<sup>49</sup> By the early 1890s, most

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46 Moore, H.L. 'The Statistical Complement of Pure Economics', QJE, Vol.23 (1), 1908, p.2, my emphasis.

47 Jevons, W.S., Theory of Political Economy, op.cit., pp.10-11.

48 See, e.g., Blanqui, J.A., History of Political Economy in Europe, London, Bell and Sons, 1880, p.540f. As early as 1867, Marx was reluctant to embrace the statistics of Germany and continental Europe because they were "wretchedly compiled". Capital Vol.1 [1867], Moscow, Progress Publishers, 1954, Preface to the 1st German edition, p.20. C.f., the relatively subordinate role of numerical observations in the economics of Say, Cournot and Walras. On this point refer to Ménard, C. 'Three Forms of Resistance to Statistics', HOPE, Vol.12 (4), Winter, 1980, pp.524-541.

49 See Stigler, G.J. 'The Early History of Empirical Studies of Consumer Behaviour', in idem., Essays in the History of Economics, Chicago, Chicago University Press, 1965, pp.203-206.

continental economists, and especially German historical economists, (of course, barring the Austrians), would have accepted Cossa's summary judgement that "statistics, if managed aright, certainly do a great service to political economy by providing in empirical data a far more solid basis of argument than hypothetical cases". Although, to this was added a caveat that statistics "can never satisfactorily clear away the doubts attaching to economic principles arrived at by a priori reasoning".<sup>50</sup>

It was preeminently the administration of the affairs of statesmen (perhaps partly based on principles established by political economists) which was to enhance the growth of economic statistics and their use in the study of political economy. This is left in little doubt by Marshall in his Principles [1891]:

The rapid growth of collective interests and the increasing tendency toward collective action in economic affairs, make it every day more important that we should know what quantitative measures of public interests are most needed and what statistics are required for them, and that we set ourselves to obtain these statistics.<sup>51</sup>

Marshall thought that this achievement in quantitative analysis "stands over for the twentieth century", given that "the nineteenth century has in great measure achieved qualitative analysis in economics".<sup>52</sup> Increasing levels of government

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50 Cossa, L., An Introduction to the Study of Political Economy, London, Macmillan, 1893, Translated by Louis Dyer, pp.25-26.

51 Marshall, Principles, op.cit., p.492. See also his 'The Social Possibilities of Economic Chivalry', EJ, Vol.17(1), 1907, p.8.

52 Marshall, 'The Old Generation of Economists and the New', QJE, Vol.11, January 1897, p.123, his emphasis.

intervention in economic affairs often necessitated the collection of more numerical data. In surveying the progress of statistics up to 1914 in the United States, North found that the

days of laissez faire have gone forever. There is no longer any field of human activity into which it is not now accepted as both the right and duty of the State to intervene, by investigation and by remedial action. This reversal of the long prevalent theory of the governmental function has enormously widened the field of statistics ... [and] has increased their use. [S]tatistics have become the foundation of modern government, both in the administrative and legislative branches.<sup>53</sup>

Spengler's wideranging survey of the history of quantification in economics from the sixteenth century to 1920 reinforces this view. Before 1830, economics remained largely verbal in composition. By the closing decades of the nineteenth century, however, "economists were ready to adopt newer methods" and this is suggested by the rapid rate with which formal statistical techniques were put to work in economics in particular, for the construction of index numbers; for business cycle mapping and for the statistical determination of demand and supply curves (the latter being a development of the 1920s associated with Moore, Schultz and Working). Spengler noted that "the role of economics in public affairs" and the "economic role assigned to the State", among other factors such as data availability, accounted for a strengthening of bonds between economics and statistics.<sup>54</sup> In addition to Spengler's comments, it may be added that the launching of a new academic journal: Review of

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53 North, S.N.D. 'Seventy Five Years of Progress in Statistics', in Koren, History of Statistics, op.cit., 1918, p.21.

54 Spengler, 'Quantification', op.cit., pp.174-76.

Economics and Statistics in 1919 would have helped cement these bonds. Although, its aim had nothing directly to do with government policy. Its first preface stated that the journal was "to devote itself to the scientific criticism and interpretation of existing data ... which ordinarily serves as the basis for judgement concerning fundamental business conditions [and therefore] ... contribute to the general progress of economic science".<sup>55</sup> The only other survey, apart from Spengler's, of the growth of economic statistics and its connections to political economy for the period up to 1930 is contained in Hutchison's Review of Economic Doctrines 1870-1929. Generally, he related the closer connections between statistics and political economy to "the role of the State in economic life" which was "considerably transformed" over the period under review. For example, the statistics on poverty collected by Booth and Rowntree before 1900 "profoundly and immediately affected social policies, and the assumptions on which economic policies were based".<sup>56</sup>

The design and use of numerical observations even for the period 1870-1930 helped give some semblance of proportion to the problems faced by public policymakers. This tendency was more marked following greater emphasis on macro-structures and systematisation of macroeconomic theory and national income

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55 Bullock, C.I. 'Preface', RES, Vol.1 (1), 1919.

56 Hutchison, T.W., A Review of Economic Doctrines 1870-1929, 2nd edn., London, Macmillan, 1962, p.431 and p.427. Hutchison did not make detailed reference to the predilection of German historical economists to use numerical data mostly as descriptive aids (such as Schmoller in his Grundriss published in 1900). For details of the sympathetic attitude of these German economists towards statistical material see e.g., Keynes Scope, op.cit., p.317 passim and Gide, C., et.al., A History of Economic Doctrines, 2nd edn., Boston, D.C. Heath, 1948, p.389, 398, 408 and passim.

accounting after Keynes. Prior to 1930, it was generally regarded as "quite impracticable" for investigators to try to assemble figures for aggregate employment and income as a basis for theoretical analysis; this had to wait seemingly until Keynes' macro-theory of income determination.<sup>57</sup> Looked at more specifically though, it is evident for the early 1930s that the estimates of Clark, Kuznets and others were "in varying degrees motivated by the desire to quantify those macroeconomic variables to which the pre-General Theory theories of the business cycle had already attached crucial significance".<sup>58</sup> Keynes' work had its greatest influence on the development of national income and outlay accounts. His macroeconomic analysis was built around national income and outlay and attempts to test and apply his theory gave an impetus to the estimation of these aggregates. By assigning importance to both final outlays and income, as well as to government expenditures and receipts as strategic policy variables, he encouraged the institutionalisation of national income accounts.<sup>59</sup> Keynes had explicitly denounced "the frightful inadequacy of most of the statistics". On the other hand, he was highly sceptical of early econometric work which sought to apply statistical methods to economic

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57 Hutchison, Review, op.cit., p.428.

58 Patinkin, D. 'Keynes and Econometrics', Ecom., Vol.44, Nov. 1976, p.1107, my emphasis.

59 See Kendrick, J.W. 'The Historical Development of National Income Accounts', HOPE, Vol.2 (2), Fall 1970, p.305.

observations, as his debate with Tinbergen attests.<sup>60</sup>

Even before Keynes' influence, a reading of the history of the connections between statistics and political economy, suggests that numerical data increasingly helped define problems for economists and public policymakers; that statistics may have occasionally delimited problem understanding and restricted 'solutions' to some subset of all the possible 'solutions' that were actually possible. As Schumpeter realised: "[W]e need statistics not only for explaining things but also in order to know precisely what there is to explain".<sup>61</sup> In 1915, A.L. Bowley, a leading advocate of quantification and a contributor along with Flux and Stamp to the design of some isolated national income estimates, considered that measurement functioned to define problems for political economists and reformers:

On the one side measurement should result in accurate and comprehensible description that makes possible the visualisation of complex phenomena; on the other it is necessary to the practical reformer that he may know the magnitude of the problem before him; and make his plans on an adequate scale.<sup>62</sup>

The production and use of many statistics can evidently be as much a part of the process by which economic and social reality

60 See Keynes on 'Professor Tinbergen's Method' [1939], in The Collected Writings of J.M. Keynes, Royal Econ. Society, Macmillan, 1973, Vol.XIV, p.316ff. C.f., Patinkin, 'Keynes and Econometrics', op.cit., pp.1094-96 and Phelps, M.G. 'Laments, Ancient and Modern: Keynes on Mathematical and Econometric Methodology', JPKE, Vol.2 (4), 1980, pp.482-93.

61 Schumpeter, History, op.cit., p.14, my emphasis.

62 Bowley, A.L., The Nature and Purpose of the Measurement of Social Phenomena, London, P.S. King, 1915, p.225, my emphasis.

is created as it is a description of that reality. Facts do not often seem 'given' in nature for expression in numerical form; and as Joan Robinson noted in retrospect with regard to national income and product estimates, when the facts are expressed in numerical terms public "policy is affected by propaganda for the standard of life as it appears in the figures".<sup>63</sup> Again, even prior to 1930, economic thought in the Schumpeterian sense - "the sum total of all the opinions and desires concerning economic subjects ... [and] public policy ... that at any given time and place, float in the public mind" was profoundly affected by the all-round growth of economic statistics.<sup>64</sup> Economic statistics affected the results of scientific endeavour and everyday, commonsense aspects of economic thought; they influenced "the background of economists' work[;] ... public opinion on economic problems and had pervasive and imponderable effects on social self-consciousness and 'sophistication'".<sup>65</sup> By 1933, the editor of the first volume of Econometrica (the official journal of the Econometric Society founded in 1930), was able to proclaim that "[s]tatistical information is currently accumulating at an unprecedented rate".<sup>66</sup> Moreover,

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63 Robinson, J., Economic Philosophy, London, C.A. Watts, 1962, p.123-24.

64 Schumpeter, History, op.cit., p.38. Schumpeter called "the scientific aspects of economic thought" economic analysis. In some places he referred to analysis as "tooled knowledge" which is the "result of scientific endeavour". He distinguished economic analysis from economic thought by considering analysis as "the sum total" of all the technical, mental apparatus used by economists which is "not in use among the general public". Schumpeter understood that, through time, scientific and everyday, commonsense aspects of economic thought were not independent of one another. Ibid., see pgs. 7, 12n 2, 16, 21 and 52.

65 Hutchison, A Review, op.cit., p.427.

66 Frisch, R. 'Introduction', Ecom., Vol.1 (1), 1933, p.2.

by 1956 in the wake of the Keynesian macroeconomic 'revolution'

Frank Knight was able to report:

The growing accumulations of numerical information covering a wide variety of economic facts, coupled with the advance of statistical technique has been working a notable change in the character and content of economics as a whole; without nullifying any of its established principles it has given more knowledge of the actual degree of stability or variability in the relationships in real life. Interpretation of records covering averages and aggregates and their movements can add much to what can be inferred from commonsense principles or observation of particular cases. This does not mean, however, that economics will be or can be purely statistics, a new kind of "political arithmetic".<sup>67</sup>

What then did the growth in numerical observations mean, over and above its encouragement of econometric techniques? What were some of its consequences, for example, what was the character of the 'knowledge' it proffered? How may these knowledge consequences be assessed? Some economists have asked similar questions but have mostly refrained from suggesting any lines along which answers may be sought. Kenneth Boulding, in a similar vein to Hutchison, reflected on the rapid growth in economic statistics, but this time after 1930. He noted that the "development of ... economic indices is ... of great importance in assessing the effect of messages on the economic image". (Boulding's 'economic image' is more or less synonymous with Schumpeter's 'economic thought'). Boulding goes on to

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67 Knight, F., On the History and Method of Economics, Chicago, University of Chicago Press, 1956, p.26. For a synopsis of the main advances made in collecting and collating economic statistics especially in the post-Second World War period, see Ruggles, R. 'Economic Data', in D.L. Sills, ed., International Encyclopedia of the Social Sciences, Vol.4, New York, Free Press, 1968, pp.365-69.



point out that "the consequences of this rapid spread of [numerical] information have never been adequately assessed ...".<sup>68</sup> In the period from (say) 1930 to the present few social scientists have paused to reflect on the consequences of the tremendous growth in measures of economic control such as national income accounts, input-output tables and many other forms of numerical data which "might be creating new problems ... almost as, or even more, profound and unmanageable than those it was solving".<sup>69</sup> Any possibility for this kind of reflection was overwhelmed by the growing confidence among early twentieth century social scientists in social 'advancement' and scientific 'progress' through quantification. A reading of the history of quantification in political economy and economics, suggests that statistics, while usually lagging behind qualitative or conceptual work, were instrumental in sharpening the definition of concepts and policy problems. In short, quantification subtly transformed knowledge. From this summary glance at the history of statistics and its connections to political economy, it appears necessary to dwell further upon the subsequent character of this knowledge. It is also pertinent to consider some of the consequences of what Stigler, in an article on 'The Economist and the State', called "a growing insistence upon quantification" in economics. He considered the trend toward quantification as "a scientific revolution of the first magnitude" - not unlike Hutchison who has lately (and tentatively) labelled it as a "quantitative

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68 Boulding, K.E., The Image: Knowledge in Life and Society, Michigan, University Press, 1956, p.89.

69 Hutchison, A Review, op.cit., p.431, my emphasis.

revolution" in economics.<sup>70</sup>

Knowledge, as it pertains to images of the economic process, may well have been profoundly transformed through the growing practice after 1930, of designing and using numerical observations of diverse economic phenomena. This practice, as reflected in Knight's comment quoted in the foregoing paragraph, has been undoubtedly supported by the application of formal statistical techniques in economics. One way the rapid growth in numerical observations used in matters of economic thought and analysis might be understood and appraised is in terms of the presuppositions involved in their design and use. That is, how is numerical data, when designed and used, justified as valid knowledge? This is a question in epistemology, and epistemology, as Boulding argued, "has received rather scant attention at the hands of economists".<sup>71</sup> Following Machlup, epistemology may be defined (for the purposes of this work), as "the science of the nature and limits of knowledge".<sup>72</sup> This work is concerned with questions about the nature and limits of knowledge derived from the design and interpretation of numerical data with reference to its employment in matters of economic thought and analysis. In this context epistemology is

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70 Stigler, G.J. 'The Economist and the State', AER, Vol.55 (1), March 1965, p.17; Hutchison, T.W., On Revolutions and Progress in Economic Knowledge, Cambridge, CUP, 1978; p.239 and p.291.

71 Boulding, K.E. 'The Economics of Knowledge and the Knowledge of Economics', AER, Vol.56 (2), 1966, p.1.

72 Machlup, F., Methodology of Economics and Other Social Sciences, New York, Academic Press, 1978, p.59.

critical, in the sense that it questions the explicit and usually implicit presuppositions about statistical (numerical) knowledge. Thus, from the outset, numerical data in all its forms is derived from many different operations and must be regarded as problematic. Epistemology requires scepticism of certainties, or in this context, of 'given' numerical data. Indeed, Schumpeter advised economists to view numerical data as problematic because

it is impossible to understand statistical figures without understanding how they have been compiled. It is equally impossible to extract information from them or to understand the information that specialists extract for the rest of us without understanding the methods by which this is done - and the epistemological backgrounds of these methods.<sup>73</sup>

Section III constitutes the beginning of an analysis which concentrates on numerical observations and more formal statistical aggregates designed therefrom, which does not accept as given one element of what is, for Schumpeter, most problematic. A school of economists broadly labelled 'the Austrians' have been uncompromising advocates of what is largely a rationalistic stance on the status of numerical observations in economic thought and analysis. Generally stated, their position treats numerical observations not as accepted 'givens', but as subjects for debate and analysis. For these writers, numerical 'facts' and theory are complexly welded together and things become much less simple when the epistemological

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73 Schumpeter, History, op.cit., 1954, p.14. Similar advice is given by Devons, E. 'The Language of Economic Statistics', [1954] in idem., Essays in Economics, London, Allen and Unwin, 1961, pp.105-21.

background to the design and interpretation of numerical observations is filled in. These observations are not easily divorced from their use, since their meaning is inextricably bound up with the theory or explanatory system that they are attached to, and within which they are interpreted. When a particular series of observations is affirmed as valuable or useful the Austrians desire to inquire further into what this means and why the data is useful. They challenge the argument that numerical observations are useful because they might be the closest thing we have to the 'facts'. It is to the Austrians that this discussion now turns in recognition that numerical observations are not necessarily 'objectively' grounded and therefore not impervious to debate.

III(A) SUBJECTIVIST PERSPECTIVES ON THE LIMITS  
OF OUR NUMERICAL KNOWLEDGE

3.1 MENGER

Menger's subjective approach ... [carried] the conviction that economic theory must be based on individual decisions. Menger must have condemned the greater part of macroeconomics; ... He expressly permitted aggregation only as a makeshift device. Such an attitude in these days is extremist; but ... [w]e are too much tempted to be the slaves of our macroeconomic aggregates. It is good to be brought up with a shock, against the flimsiness of the foundations on which they rest. One of the strands of the Austrian tradition, truly Menger's, though descending to Mises and Hayek and Morgenstern [...], is the proclamation of that flimsiness; ...<sup>1</sup>

On the question of the character of knowledge generated from series of numerical observations and aggregative numerical constructs, some Austrian economists have confronted the issues directly. In his Untersuchungen [1883], Carl Menger (1840-1921), the founder of the Austrian School, divided economic science (Wirtschaftswissenschaft) - the study of "the field of economy" - into three groups:

- (i) historical ("realistic-empirical") sciences,
- (ii) theoretical ("exact") economics, and
- (iii) practical sciences of national economy (economic

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<sup>1</sup> Hicks, J.R. and Weber, W., eds., Carl Menger and the Austrian School of Economics, London, Oxford University Press, 1973, p.232.

policy and the science of finance).<sup>2</sup>

Menger maintained that numerical records from realistic-empirical research could not be used to test or check exact - theoretical research. This, for Menger, was akin to using measurements of real objects to test the universal, general principles of geometry. Menger placed 'statistics' - by which he appeared to mean any numerical material on "the state of societies" - under the rubric of historical, (realistic-empirical) science. Statistics, Menger wrote, "has the task of offering a description of all factors of social life (even the ones latent at a given moment) from which the movement of society results".<sup>3</sup> He pointed out that statistics can respond to its task "only under the presupposition of collective consideration of ... economic phenomena [;] ... it necessarily leads to the collectivist view of economic phenomena - to the ... statistics of 'national economy'". When building up the "collective image of human life", statistics, of necessity, were arranged and compiled to "make us aware of the immense number of individual phenomena of human life from the point of view of a collective outlook".<sup>4</sup>

Statistics, in the above sense, had important limitations.

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2 Menger, C., Untersuchungen über die Methode der Socialwissenschaften und der Politischen Ökonomie insbesondere [1883] (Problems of Economics and Sociology), Translated by Schneider, L., Urbana, University of Illinois Press, 1963, pp.38-9. For further detail see his Appendix IV, 'The Terminology and the Classification of the Economic Sciences', pp.206-213.

3 Ibid., pp.69-72 and 38-39 n.7. (All emphases hereafter are the authors' unless otherwise stated).

4 Ibid., p.209 n.140.

First, because "there is not a phenomenon of the real world which does not offer us the spectacle of constant change" and since "all real things are located in the stream of time", numerical constructs can only depict a small portion of changes in the measured phenomena extended in time. Further, when considering the development through time of "empirical forms", Menger contended that the 'same' economic phenomenon such as money "assumes such different forms in the course of the development of civilisation". Thus, its numerical representation as an aggregate at a definite moment might not be a valid one at the next moment. When it is perceived, for example, how

with some of the most important civilized nations money first appeared in the form of domestic animals, later of base and noble metals in uncoined state, then as coins, only to pass over finally into still more involved forms (combinations of money and money tokens!), it would be difficult to fail to see here the striking development of the empirical form of money. In both cases it is the same economic phenomenon which assumes such different forms in the course of the development of civilization. In the first case it is the equalization of want and plenty; in the second, the means of barter. But what a difference of empirical forms which we, after all, have characterized here only in their most striking phases! And we meet such developments of empirical forms in the realms of social phenomena not only by way of exception; on the contrary, they form the rule.<sup>5</sup>

The content of concepts such as money alter over time partly because acting individuals and individuals in groups, confer different subjective meanings and uses on its everyday empirical

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5 Ibid., pp.109-10 and pp.103-4, my emphasis.



form. Furthermore and as a consequence, the validity of intertemporal comparisons of functional coefficients of interrelationship between the numerical representations of two or more groups of economic phenomena expressed in terms of general, abstract concepts such as: 'money', 'prices', 'consumption', 'income', 'investment', 'employment' and so on, may be open to question. Since the empirical content of a concept such as employment is changing and changeable over time, depending on the way it is perceived in practice by individuals, any numerical 'relationship' discovered for a point in time between, say, traditional aggregates such as employment and investment, could only have a limited range of applicability. On the other hand, the abstract concepts (perhaps originally 'thought-up' from a few instances) have universal applicability.

A second limitation is that, while statistics may be able, within strict limits and qualifications, to pick up international and interlocal divergence among "simultaneous phenomena belonging to the same empirical form" numerical constructs obscure unique individual "local" differences of [so called] homogeneous economic phenomena". In other words, the "'same' concrete phenomenon of economy is not infrequently a different one in the single phases of its development"; for example: single 'unemployed' persons residing in the same place or 'apples' in a bowl at different stages of ripening. Menger emphasised that "real human phenomena are not strictly typical". Thus the comparability of numerical observations of the 'same' phenomenon at the same time (let alone at different times) in the same place (let alone at different places) is not beyond question. Aggregate notions of employment or output imply that there is some given objective entity which can be quantified

across individuals for the purpose of analysis and comparison. For Menger, quantification in this sense raised insuperable conceptual problems.<sup>6</sup> These difficulties and limitations, among others, were to be more fully explored by some later generation Austrian economists. In summary, Menger's scepticism toward all forms of counting and quantifying and the resulting insurmountable difficulties he thought were involved in using statistics to empirically test exact, abstract theory, are the outcome of his methodological individualism and the subjectivism embedded in his philosophic perspective.<sup>7</sup>

### 3.2 HAYEK

In his Nobel Prize Memorial Lecture (1974), Friedrich Hayek considered that one of the failings of economists to guide public policy more successfully was connected with their propensity to misapply the quantifying procedures of the physical sciences.<sup>8</sup> Imitation of the so-called measurement

6 Ibid., p. 110, 104 and 214. See also Menger's scepticism of any concept of national income as a basis for welfare measurement in his Grundsätze [1871], translated to Principles of Economics, New York, Free Press, 1950, Chapter 2, p.90f.

7 On Menger's philosophic outlook and its derivation from Aristotle, see Kauder, E. A History of Marginal Utility Theory, Princeton, Princeton University Press, 1965. Kauder refers to this outlook as "Austrian Aristotileism" (p.xx); see also p.104f. For some recent re-assessments of Menger's methodology and philosophy of social science see the papers in the special issue of AEJ, 6(3), 1978. See also Hutchison, T.W. 'Carl Menger on Philosophy and Method' in idem., The Politics and Philosophy of Economics, Oxford, Blackwell, 1981, pp.176-202. Hutchison concludes that Menger's work in this respect was an unusual mixture of "individualism" and "philosophical essentialism" underwritten by a "highly antinominalist" perspective (p.177 and 179).

8 See Hayek, F.A. 'The Pretence of Knowledge', in idem., Full Employment at Any Price?, London, IEA Occasional Paper No.45, 1975, pp.30-41. The following two works were used as background references to this discussion on Hayek: Barry, N.P. Hayek's Social and Economic Philosophy, London, Macmillan, 1979 and Machlup, F., ed., Essays on Hayek, London, RKP, 1977.

practices of the physical sciences has been carried to the point where it has produced a "'scientistic' attitude" amongst economists.<sup>9</sup> As a consequence economists have generally displayed "an almost exclusive concentration on quantitatively measurable surface phenomena [which] has produced a policy that has made matters worse". Hayek's contribution to the discussion of what he called "the inherent limitations of our numerical knowledge" is developed within the framework of his more general task which emphasises man's fallibilism and is interspersed throughout his voluminous writings.<sup>10</sup>

For Hayek, the study of economy and perforce society is the study of phenomena formed by subjective ideas:

in the social sciences it is necessary to draw a distinction between those ideas which are constitutive of the phenomena we want to explain and the ideas which either we ourselves or the very people whose actions we have to explain may have formed about these phenomena and which are not the cause of, but theories about, the social structures.<sup>11</sup>

Economy and society were not systems of objectifiable quantities or entities but inherently systems of subjective relationships between the knower and the known. The essential socio-economic functional relations cannot be measured and expressed in terms

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9 On this point see also Yeager, L.B. 'Measurement and Scientific Method in Economics', AJES, 16(4), July 1957, pp.337-346. Yeager noted that the Cowles Commission for Research in Economics originally adopted Lord Kelvin's slogan 'Science is Measurement' as its official guiding principle.

10 Hayek, 'The Pretence', op.cit., pgs.35 and 36, my emphasis. C.f., Hutchison, The Politics and Philosophy of Economics, op.cit., who remarks that: "In view of the modern pretensions of ... quantitative economists it has been specially desirable, if unpopular, to insist in recent years on these limitations" (p.218).

11 Hayek, F.A., The Counter-Revolution of Science, Glencoe, Illinois, Free Press, 1955, p.36.

of numerical constants. The actions and behaviour of economic agents depend on innumerable factors and for Hayek all these factors are intangible - for which no quantitative expression can be devised. Hayek wrote The Sensory Order to show, among other things, that there could not be a unified system of knowledge expressed in quantitative magnitudes, for example, a systematic description of the economic activities of a group of people by stable functional relations. This was because the human mind "must remain forever [in] a realm of its own, which we can know only through directly experiencing it, but which we shall never be able to fully explain or to 'reduce' to something else", for instance, to measurable statistical magnitudes.<sup>12</sup> His theory of knowledge expresses the idea that we come to know the external world as a series of orders or systems of general classifications which have been constructed by a process of the mind itself. The sensory order constructed by the mind is a unitary order the properties of which are known as a series of relations. Our knowledge of the external world is of an order of events subjectively known.

According to Hayek, statistics or numerical constructs in the social and policy-related sciences will not take direct account of the circumstances of time and place and the uniqueness of the particular. As well, coefficients of interdependency expressed in numerical form are "impotent" to deal with what he called "pattern complexity" at the micro-

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12 Hayek, F.A., The Sensory Order: An Inquiry into the Foundations of Theoretical Psychology, London, RKP, 1952, p.194. See also his development of a similar theme in 'The Primacy of the Abstract' in idem., New Studies in Philosophy, Politics, Economics and the History of Ideas, London, RKP, 1978, pp.34-49.

level, where phenomena are unique, interactive, and subjectively known.

Statistics, ... deals with the problem of large numbers essentially by eliminating complexity in the sense of a multiplicity of relations between individual elements and deliberately treating the individual elements which it counts as if they were not systematically connected. It avoids the problem of complexity by substituting for the information on the individual elements information on the frequency with which their different properties occur in classes of such elements, and it deliberately disregards the fact that the relative position of the elements in a structure may matter. In other words, it proceeds on the assumption that information on the numerical frequencies of the different elements of a collective is enough to explain the phenomena and that no information is required on the manner in which the elements are related. The statistical method is therefore of use only where we either deliberately ignore or are ignorant of the relations between the individual elements with different attributes, i.e., where we ignore or are ignorant of any structure into which they are organized. Statistics thereby enables us to regain simplicity and makes the task manageable by substituting a single attribute for the unascertainable individual attributes in the collective. It is for this reason irrelevant to the solution of problems in which it is precisely the relations between individual elements with different attributes which matter.<sup>13</sup>

Statistics might be used to solve problems by helping to determine functional interrelationships between groups of phenomena. Hayek was sceptical of the use of statistics in this

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13 Hayek, F.A. 'The Theory of Complex Phenomena', in Bunge, M., ed., The Critical Approach to Science and Philosophy, London, Macmillan, 1964, p.336. For an earlier statement of this argument see Hayek, The Counter-Revolution, op.cit., pp.62-3.

fashion since it often resulted in a vain search for numerical constants, or stable numerical values of the magnitude of mutual interrelationships. Such procedures, from Hayek's perspective, are dangerous if they presume that the established numerical relationships are anything more than transitory 'facts' of specific historical situations. Moreover, these procedures can lead to the erroneous belief that information on individual attributes and on the interconnection of individual elements with different attributes is superfluous. In turn they imply that the attribute chosen for quantifying is the most appropriate; that the level of aggregation does not overly obscure relations between individual elements and that all the individual attributes of phenomena (both potentially quantifiable and non-quantifiable) can be known, or were known, by a single brain. Overall, the practice of designing and using numerical observations can be an illustration of what Hayek, in a much broader context, referred to as "the synoptic delusion".<sup>14</sup>

The synoptic delusion can be elaborated with reference to Hayek's discussion of the use of numerical information at the level of economic and social planning. In his Individualism and the Economic Order he asked: "How can the combination of fragments of knowledge existing in different minds bring about results which, if they were to be brought about deliberately,

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14 Hayek, F.A., Law, Legislation, and Liberty VOL.I, Rules and Order, London, RKP, 1973, p.14. This delusion was elaborated in the context of an anti-Cartesian polemic expressing the impossibility of certain knowledge.

would require a knowledge on the part of the directing mind which no single mind can possess?". He appraised the potential of mass numerical observations and formal statistical constructs for use in central planning. That is, he considered whether statistical data such as that designed on the basis of the law of large numbers or in the light of a supposed mutual compensation of random changes, could give the directing mind necessary and sufficient information to plan for all the different, individual minds. A 'directing mind' may consider he has the necessary numerical observations (e.g., time series data) to take decisions, but there is no epistemological basis for supposing that sheer application of statistical technique to analyse these observations before decisions are taken in itself provides 'hard' quantitative evidence of future outcomes. A judgement, perhaps along probabilistic lines, would have to be made to the effect that individual variations away from past 'norms' identifiable in the data will (or will not) cancel out over large numbers of future instances. Gaps in the 'directing mind's' knowledge generated from a time series trend line projection, even if it is probabilistic, will remain precisely because past 'norms' relating to variation in human behaviour will not necessarily persist into the future. In any case, Hayek chose to emphasise the point that individuals in particular circumstances "possess unique information of which beneficial use can be made". This knowledge "cannot be conveyed to any central authority in statistical form ... Central planning cannot take direct account of these circumstances of time and place" so that decisions depending on the latter must

be "left to the 'man on the spot'", who is familiar with these circumstances.<sup>15</sup> Hayek attached special importance to knowledge of time and place. Constant small changes in economy and society are overlooked when there is any preoccupation with numerical aggregates in various forms of indicative state planning. Decisions at the micro-level cannot be solved by communicating all particular circumstances of time and place to a central planning authority which after processing and aggregating all this information, issues its orders. Much relevant information is lost in the aggregation process. For Hayek, the only numerical observations that matter are market prices, especially those relevant prices which relate to the "limited individual fields of vision" to which decisions of particular economic agents refer. Relevant prices and, of course, their subjective valuation by individuals, helps individuals co-ordinate their own plans. Utilising an implicit doctrine of an Invisible Hand, Hayek embarked on a lengthy explanation as to how prices can also automatically help to co-ordinate the separate plans of different people. Moreover, processes in economic life continuously alter the basic constellations of relevant numerical knowledge; this fluidity is the essence of the economic problem and explains for Hayek, why efficient central planning is an impossibility. On the

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15 Hayek, F.A., Individualism and the Economic Order, London, RKP, 1948, quotes from pgs. 54, 80, 83 (my emphasis), and p.79 respectively. Similar arguments can be found in Hayek's critique of central planning in his older work: Collectivist Economic Planning, London, Macmillan, 1935.



other hand a system of markets is adaptable to the fluidity of economic life. It is open-ended. It allows for continuous changes in social goals as individuals experiment and innovate in their private spheres of action. A system of markets thus maximises the utilisation of dispersed subjective knowledge and not GNP or any other measurable unit.<sup>16</sup>

So, contrary to widely held belief, statistics consist not of the knowledge of particular 'facts'; the 'facts' or data of the social sciences are the attitudes, values and opinions of social participants. In the case of "pattern complexity" the powers of quantification were limited by the practical impossibility of ascertaining all the particular interrelations that would allow macro-level theories the potential and power to predict specific, single events. Hayek's trade cycle theory in contradistinction, suggested an explanation for macro-phenomena through the use of conceptual devices which, in effect, attempted to reduce explanations of cycles to micro-phenomena.<sup>17</sup> All of Hayek's work implies that we could not check economic theorems in the even more complex "macro" world to the exclusion of micro-level particulars because the effects of numerous influences are hopelessly intermingled. Hayek maintained that when phenomena depend on human perception of many micro-level particulars, all the circumstances that will determine the outcome of human actions "will hardly ever be fully known or

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16 Hayek, The Political Order of a Free Society, London, RKP, 1979, pp.66-70.

17 Hayek, Monetary Theory and the Trade Cycle [1935], New York, A.M. Kelley Reprints, 1966, e.g., pp.166-7.

measurable". All that economists should strive for is an understanding of the general character of the pattern of human interactions since the factors which will determine particular manifestations of economic activities will be forever elusive to quantification and prediction.<sup>18</sup> Predictions in the social sciences are not of discrete events but of classes or patterns of events. For instance, we may know, in principle, and be able to explain what general effect a change in demand will have on prices (e.g., direction) but we cannot detail what actual price changes will occur (e.g., how far prices will change in a particular direction). The complex phenomena of the social sciences with their greater number of variables and difficulties of observation have to be explained by theories of much less predictive power in comparison to theories in physical sciences. Access to the requisite data and construction of any subsequent measurement (e.g., of direction and magnitude) is limited and measurement may gloss over important aspects of the economic process whilst highlighting mere surface manifestations. Hayek gave the example of a correlation between traditional numerical constructs, of 'aggregate demand' and 'total employment', which

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18 Hayek, 'Theory of Complex Phenomena', op.cit., pp.28-30. This admonition as to 'what economist should do' is somewhat peripheral to Hayek's main argument. More generally, what economists should do, primarily, is contribute to a mapping out of the broad constitutional order consistent with free markets and limited government (no matter how nebulous that task might be). See, e.g., Hayek, F.A. The Road to Serfdom, London, RKP, 1944; idem., The Constitution of Liberty, London, RKP, 1960, and idem., Law, Legislation and Liberty, VOL.I, op.cit., VOL.II, The Mirage of Social Justice and VOL.III, The Political Order, op.cit., 1973, 1976, and 1979 respectively.

even though only adduced as approximate, could be uncritically accepted as the only 'causal' connection that counted for theoretical or public policy purposes, especially when numerical data on other relationships was lacking. Worse still, in regression analyses the issue of 'association' versus 'causal' connection and problems in determining the direction(s) of 'causation' are usually glossed over in practice. In any case,

neither aggregates nor averages do act upon one another, and it will never be possible to establish necessary connections of cause and effect between them as we can between individual phenomena, individual prices etc. I would even go so far as to assert that, from the very nature of economic theory, averages can never form a link in its reasoning.<sup>19</sup>

Hayek developed similar, though more guarded assertions in his study of trade cycle theories two years earlier. In that study he asserted that numerical observations and averages could not confirm a theory. Numerical data could only be used "as corroboration" to judge the merits of a given theory by showing in what respects the theory failed to explain all the observed phenomena. It provided no proof of correctness, or confirmation of a theory - it merely identified new or unexplained problems. Statistical analyses of correlation, extrapolation, and covariation among trade cycle phenomena may help establish "empirical relations". However, these relations

continue to present a problem ... until the necessity for their interconnections can be demonstrated independently of any statistical evidence. The concepts on which such an explanation is based will be quite different

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19 Hayek, Prices and Production, 2nd revised edition, London, RKP, 1935, pp.4-5.

from those by which statistical inter-connections are demonstrated; they can be reached independently ... [T]he best that statistical investigation can do is to show that there still remains an unexplained residue of processes.<sup>20</sup>

In the case of time series data such as trade statistics, figures on employment and price changes etc., they are deemed useful only as information about these phenomena at a particular place and time. Generalisations from numerical records of unique historical events cannot produce valid generalisations in any positive sense. In a rather negative way these series are simply historical information which can "provide us with the data to which our theoretical generalisations can be applied".<sup>21</sup>

In a polemic on Keynes and on the evolution of Keynesian economics Hayek deplored the ascendancy of macroeconomics following the General Theory, and its subsequent influence on public policy. Keynes "was much given to thinking in aggregates" and this had encouraged his followers to concentrate on

statistically measurable ... averages and aggregates ... in the belief that there exist relatively simple and constant

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20 Hayek, Monetary Theory, op.cit., p.33, my emphasis. See also pp.31, 35.

21 Hayek, Counter-Revolution, op.cit., p.63. On the question of falsification in economic science, which he recognises in his more recent writings, Hayek considers that theories are falsifiable in principle. Falsification is a matter of degree and many theories are difficult to falsify in practice, even with ample numerical data; vide, e.g., 'Theory of Complex Phenomena', op.cit., p.29.

functional relations between ... demand, investment, output and that explicitly established values of these presumed 'constants' would enable ... valid prediction.<sup>22</sup>

Apart from difficulties in establishing causal connections between aggregate numerical data there is, for Hayek, no necessary causal connection between macro-totals and micro-decisions. Hence his mistrust of mass numerical observations and constructs and his emphasis on market process analysis - especially on the co-ordinative and informational effects of unique individual numerical observations thrown up by the price system at the microeconomic level. Functional relations between macro-totals which all macroeconomists tend to treat as at least quasi-constant, must be founded on the underlying microeconomic structure, especially on the relations between different prices which is often disregarded in macroeconomics. These relations change very rapidly because of the fluidity of the micro-structure in economic life. Overall, Hayek concluded that public policy recommendations based on the interpretation of a handful of numerical variables "is likely to do much harm" especially if it goes beyond attempts to describe and understand the "general character of events" into the realm of predicting and manipulating macro-phenomena that would ultimately lead to attempts to control "particular individual events".<sup>23</sup> No matter how many variables might be quantified,

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22 Hayek, Tiger by the Tail: The Keynesian Legacy of Inflation, London, IEA, 1972, pp.101-2.

23 Hayek, 'The Pretence ...', op.cit., p.31, 32.

and regardless of the sheer volume of available numerical data, Hayek vehemently denies in The Counter-Revolution of Science that this "scientism" (the slavish imitation and sometimes misapplication of natural science procedures of coding, classifying and measuring) would be adequate for an analysis of the subject-matter of economics. Scientism carried with it the seeds of an oppressive collectivism. Any requirement to replace the classification of economic and social events which our individual senses and minds proffer, by a form of classification more appropriate to quantification does not always do justice to the subject-matter of economics as a social science. Social science has to employ the Weberian method of Verstehen. This is a method of empathic understanding not compatible with quantification. Our understanding as social scientists of the actions of economic agents "is made possible by the fact that we have a mind like theirs, and that from the mental categories we have in common with them we can reconstruct the social complexes which are our concern". Scientism in the social sciences concentrates, instead, on that which is measurable, i.e., can be expressed in numerical form. It thereby neglects individual variation and uniqueness as well as first-person knowledge which can be picked up by Verstehen. It also leads to quantitative estimates which uncritically aggregate and hypostatise economic and social phenomena.<sup>24</sup>

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24 See Hayek, 'Scientism and the Study of Society', in the Counter-Revolution, op.cit., pp.13-79.

In summary, there are three interrelated threads to Hayek's argument about the limitations of knowledge obtainable from numerical data in the social sciences. In increasing order of importance: (1) the data is historical; (2) it cannot capture the subjective nature of our knowledge of economic and social affairs and (3) the inherent complexity of those affairs makes it impossible for the scientist-observer to reproduce their elements in manageable, quantitative form.

### 3.3 MISES ET.AL.

Other writers of subjectivist, Austrian school persuasion have stressed, inter alia, the historical nature of all numerical data and have explored the consequences and implications emanating therefrom. Ludwig von Mises' discussion of the "epistemological problems" relating to the status of numerical data can be gleaned from his treatise on economics, Human Action, first published in 1949.<sup>25</sup> He defined economics as praxeology: the study of human action, where individual action is consciously directed toward chosen, subjectively valued goals. The "infinite multiplicity and variety" of all human actions with all their accidental, special and particular characteristics and consequences is the subject matter of history. Economic statistics, which capture some of this variety, are history.<sup>26</sup> From these starting propositions Mises

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25 Mises, L., Human Action: A Treatise on Economics [1949]. All references in what follows are from the 3rd Revised Edition, Chicago, H.Regner Co., 1966.

26 Ibid., p.30 and 30n.1.

argued that numerical or statistical representation of historical experiences and events are "open to various interpretations" and are in fact "interpreted in different ways". Since ceteris does not remain paribus for very long, or alternatively since ceteris paribus clauses cannot usually be fully corroborated, sets of numerical observations of (say) "a change in one element only", tell us what happened in a nonrepeatable historical case. Consider an example:

If a statistician determines that a rise of 10 per cent in the supply of potatoes in Atlantis at a definite time was followed by a fall of 8 per cent in the price, he does not establish anything about what happened or may happen with a change in the supply of potatoes in another [place] ... or at another time. He has not "measured" the "elasticity of demand" of potatoes. He has established a unique and individual historical fact. No intelligent man can doubt that the behaviour of men with regard to potatoes and every other commodity is variable. Different individuals value the same things in a different way, and valuations change with the same individuals with changing conditions.<sup>27</sup>

Armed with an extreme methodological individualism and a thoroughgoing subjectivism Mises arrived at almost nihilistic conclusions: "Those economists who want to substitute 'quantitative economics' for what they call 'qualitative economics' are utterly mistaken ... The impracticability of measurement is not due to the lack of technical methods for the establishment of a measure. It is due to the absence of constant relations."<sup>28</sup> For Mises, a process of measurement

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27 Ibid., pp.55-56.

28 Ibid., p.55.



"consists in the establishment of the numerical relation of an object with regard to another object, viz., the unit of measurement".<sup>29</sup> Owing to the variable relationship between unit of measurement and object in the social sciences, systematic measurement in Mises' schema was impracticable. Therefore, the Austrian version of "[e]conomics is not, as ... positivists repeat again and again, backward because it is not 'quantitative'. It is not quantitative and does not measure because there are no constants."<sup>30</sup> In Mises schema it cannot be held that empirical 'laws' can be derived a posteriori from the study of historical data irrespective of whether such 'laws' are only deemed valid with respect to time, place, for certain periods, countries, nationalities etc. Mises conceded the validity of statistical studies of population movements and other social phenomena which show relative stability. The slow changeability of certain numerical ratios and constructs such as death rates and population growth rates was evidence of some stability. However, these constructs have only a limited range of autonomy and application. The only conclusion from the observations is that ratios (e.g., the number of yearly deaths divided by the number of the total population) change more slowly than the absolute numbers (of the deaths themselves). But nowhere in history is there evidence of several decades or even centuries in which every one of these ratios did not undergo considerable change in one direction or

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29 Ibid., p.209.

30 Ibid., p.56.

another.<sup>31</sup> Numerical observations might be legitimately used to develop statistical predictions (say) of tendency for the near future. Within the space of time, though, the historical conditions upon which these observations depended would change so that all predictions rooted in post hoc statistical observations may become obsolete and in any case would have to be highly provisional. This applies especially to all doctrines of a social, economic, financial and political kind which might be based on the supposedly 'exact' foundation of statistics.<sup>32</sup>

At the level of designing numerical observations, McKenzie noted in a recent paper contrasting neoclassical (Chicagoan) and Austrian economics, that neoclassical economic science of the Chicago School variant presupposed the possibility of objective measurement. For Chicago School theorists, "[t]he values people place on apples cannot be observed directly; but the apples themselves can ... be segmented from other objects, like oranges, and can be counted".<sup>33</sup> Contrariwise, the Austrians argue that counting the number of apples which people buy is without justification. Summing up the number of oranges and

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31 Ibid., passim. See also Mises, Theory and History, London, Jonathan Cape, 1958, p.11 and p.89f.

32 See Mises' critique of the uses of statistics for prediction in his Probability, Statistics and Truth [1928], reprinted London, Allen and Unwin, 1961, and Epistemological Problems of Economics, Princeton, Von Nostrand Co., 1960, pp.7-13 & 116-118. See also Spadaro, L. 'Averages and Aggregates in Economics', in Sennholz, M. ed., On Freedom and Free Enterprise: Essays in Honor of Ludwig von Mises, Princeton, Van Nostrand Co., 1956, pp.151f.

33 McKenzie, R.B. 'The Neoclassicalists vs. the Austrians: A Partial Reconciliation of Competing World Views', SEJ, 47(1), 1980, p.3.

apples they buy is also unjustified. Economic transactions and exchanges could not be quantified autonomously of or external to, the economic activity itself. Subjective individual valuations are the sole measuring rod because the circumstances of, say, in this case the consumption of apples can vary radically from person to person to the extent that a group of people consuming apples may not in fact be consuming the 'same' commodity. This Austrian perspective has far-reaching implications. If an applied, policy-oriented economics became synonymous with trying to change the behaviour or specific numerical values of some statistical construct, it may inadvertently leave out the question of how these statistics were at all related to the qualitative choices made by acting individuals as if the latter were unworthy of appreciation. Hence subjectivism and individual causation would be superseded by functional relations among objectified aggregates which have few, if any, real world referents in the actions of economising individuals. This is effectively to conjure the purposive acts of economic agents behind the variables, out of existence.

Kirzner has also criticised what might be termed the Marshall-Pigou variant of neoclassical economics which groped "toward the 'quantification' and the endowment with 'objective measurability' of the numberless subjectively felt urges and drives" which provide the impetus (say) for consumption activities. To presume, as Marshall did, that individuals' motives exerted a definite effect on money prices and that the resultant prices could be a means of measuring such motives, was to obfuscate the fact that "[p]rices are not measured in terms

of money; they are simply amounts of money given in exchange for goods; ... money ... is commonly used as a quid pro quo for goods."<sup>34</sup> Similarly, Mises on this point: "[t]he money equivalents as used in acting and economic calculation are money prices, i.e., exchange ratios between money and other goods and services. The prices are not measured in terms of money; they consist in money. Prices are either prices of the past or expected prices of the future".<sup>35</sup> Even if Marshallian economics can steer away from this problem it soon encounters another. Violent fluctuations in the purchasing power of money can render the measurement programme of the Marshallian approach untenable. According to the Austrians, money is not a fixed yardstick of the 'value' or objective quantity that inheres in things, nor is it a measure of the respective changes in this 'value'. With recognition that the purchasing power of money varied, economists and statisticians have embarked on an ambitious exercise of designing measures for changes in the purchasing power of money - an exercise that Mises et.al. regard as futile for the following reasons:

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34 Kirzner, I.M., The Economic Point of View [1960], reprinted Kansas City, Sheed and Ward, 1976, pp.99-100. According to Marshall: "The problems which are grouped together as economic because they related specially to man's conduct under the influence of motives that are measurable by a money price, are found to make a fairly homogeneous group" (quoted by Kirzner, p.100). Kirzner argued that any such homogeneity was "merely a happy accident" (p.100). Pigou's well-known attempt to define "economic welfare" in terms of anything which came within the ambit of "the measuring rod of money" was an extension of Marshall's approach. See Pigou, A.C., The Economics of Welfare [1920], 4th edition, London, Macmillan, 1932, p.32ff.

35 Mises, Human Action, op.cit., p.217.

- (i) All price records are subject to indeterminable errors and omissions and these errors if ever calculable, are not often published with price indices.
- (ii) Attempts to determine changes in the purchasing power of money rested on the "arbitrary character of procedures used for the compilation of averages". For example, statisticians must attach to various commodities in a consumption basket some coefficient of importance and such a weighting scheme must be based on an arbitrary rule of thumb. The same conclusion holds for the method which is used to compile averages (harmonic, arithmetical, geometrical) of the collected and adjusted data. Different choices in both weighting schema and averaging method will give different results none of which can be recognised as necessarily unique or unassailable.<sup>36</sup>
- (iii) Statistical constructs of price changes presupposed that the consumer and his or her imaginary basket of goods are constant elements and that the content of the basket does not vary from moment to moment. The Austrians contend, in contrast, that human valuations are always changing. This leads to changes in demand and production. This also entails that prices are inevitably changing and changeable. The interpretation of index number calculations very often assumed that human beings' "wants and valuations are rigid". However, only "if people were to value the same thing always in the same way, could we consider price

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<sup>36</sup> Ibid., pgs 220, and 221.

changes as expressive of changes in the power of money to buy things".<sup>37</sup>

- (iv) Index number calculations must also obscure the fact that the quality of commodities produced and consumed changes continuously.<sup>38</sup> In part this might be due to the evolution of new kinds of goods or to the technological features of commodities which constantly heralded new kinds of goods whilst older ones disappeared. Furthermore, according to Mises,

[i]t is a mistake to identify wheat with wheat, not to speak of shoes, hats and other manufactures ... An idiomatic expression asserts that two peas are alike; but buyers and sellers distinguish various qualities and grades ... A comparison of prices paid at different places or at different dates for commodities which ... statistics calls by the same name, is useless if it is not certain that their qualities - but for the place difference - are perfectly the same.<sup>39</sup>

- (v) The use of index numbers to construct fixed - quantity weights for a base period, weighted each commodity price by the quantity of that commodity sold in the base period. These weighted quantities were said to represent a 'typical' market basket proportion of goods bought over this particular period. Rothbard found difficulties with this method because, firstly, there is really no average buyer or consumer. There are only individual consumers.

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37 Ibid.

38 Quality in Mises work meant: "all those properties to which the buyers and would-be-buyers pay heed" (p.221).

39 Ibid., pp.220-221, my emphasis.

Each economic transaction is the result of different individual value scales and each had different effects on the various commodities traded. Secondly, "there is no 'average housewife' buying some given proportion of a totality of goods. Goods are not bought in their totality against money ...".<sup>40</sup>

All these "insurmountable obstacles" led Mises to the conclusion that any attempt to set up an eternal quantitative yardstick independent of human value judgements and toward which economic policy and analysis should be oriented and subservient, would be hopelessly in vain. The Austrians realise that measures of the purchasing power of money have actually become policy yardsticks, in the sense that they are used, for instance, to check the performance of macro-stabilisation policy. They appear contented with an iconoclastic position, neatly epitomised in Mises' statement that "the pretentious solemnity which statisticians and statistical bureaux display in computing indexes of purchasing power and cost of living is misplaced" since in

the actual world of change there are no fixed points, dimensions or relations which could serve as a standard. The monetary unit's purchasing power never changes evenly with regard to all things vendible and purchasable. The notion of stability and stabilization are empty if they do not refer to a state of rigidity and its preservation.

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40 Rothbard, M.N., Man, Economy and State: A Treatise on Economic Principles Vol.II, Los Angeles, Nash Publishing Corporation, 1962, pp.737-744. C.f., earlier Austrian doubts about index numbers in Weiser, F. Social Economics, [1914] London, Allen & Unwin, translated by A. Hinrichs, 1927, pp.290-92.

It was cases such as designing numerical constructs for macro-stabilisation policy in particular that "in the field of ... economics no sense can be given to the notion of measurement".<sup>41</sup>

On the question of 'testing' hypotheses with the use of numerical observations and constructs the Austrians generally consider that conventional hypothesis testing usually leads to ambiguous, debatable, indeterminate results requiring much faith if used as a guide to decisions and public policy formulation. In the first place, empirical 'testing' in economic analysis is made difficult by the inability of the economist to carry out controlled experimentation. To reduce the complexity of the subject matter into manageable proportions economists resort to a form of mental experimentation (Gedankenexperiment) in which certain variables are held constant by invoking heroic, sometimes implicit, ceteris paribus clauses. Whereas in the laboratory conditions of the natural sciences the investigator is capable of creating and replicating an artificially closed environment by physically holding some variables constant, in economics the closure is more predominantly a mental one using ceteris paribus.<sup>42</sup> In economics, 'testing' takes place from the outset of problem formulation and hypothesis framing, largely in

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41 Mises, Human Action, op.cit., pgs. 219-220 and 222.

42 I am assuming here that a distinction can be made between predominantly physical and predominantly mental operations, but I do not maintain that such operations are distinguishable in a sharp, clear-cut manner in practice. For a similar view, see Bridgman, P.W. 'Operational Analysis', in idem., Reflections of a Physicist, New York, Philosophical Library, 1955, pp.14-15.



the economist's imagination.<sup>43</sup> In most cases not all conceivable significant (or relevant) variables (some of which are included in the ceteris paribus clause) are identifiable, let alone in principle measurable. If they can be identified they are sometimes omitted usually because they are not measurable. If they are measurable, problems of spurious correlation can arise. If, as is likely, the measurable factors in the 'experiment' are interdependent, questions about direction(s) of 'causation' arise. Finally, ceteris may not remain paribus; the relevant variables other than the fluctuation in those factors of which particular account is taken in the 'experiment' may not be uniform and homogeneous over time. This implies that any 'experimental' results obtained in the present will not necessarily persist into the future. Overall, Austrian writers apparently disagree with Hutchison's conclusion that the validity of ceteris paribus assumptions in economics can be conclusively "decided only by

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43 For Mises the Gedankenexperiment has an entirely different meaning from a real, physical experiment. Mental experiments "make no reference to experience" and anything other than a Gedankenexperiment is inconceivable even when numerical data is used, Epistemological Problems, op.cit., p.12. C.f., Wisman, J.D., 'The Naturalistic Turn of Orthodox Economics: A Study of Methodological Misunderstanding', RSE, 36(4), December 1978, pp.263-285.

statistical investigation".<sup>44</sup>

In the second place, consider testing the hypothesis that: a substantial increase in the quantity of money is accompanied by a substantial increase in prices.<sup>45</sup> Now, the abstract variables in this statement must be given an operational definition so that empirical counterparts can be chosen and 'observed'. If the operational definition prescribes  $M_1$  as the counterpart of 'money' and the CPI as the counterpart of 'prices', 'testing' the hypothesis might then proceed. Before the test even begins, the Austrians would raise questions about the epistemological status of operational rules relating the abstract concepts to their 'observable' empirical counterparts or operational definitions. If the rules "are to be considered a priori", then (from a positivist viewpoint) we are merely talking about how we use words" and no correspondence between

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44 Hutchison, T.W. The Significance and Basic Postulates of Economic Theory, London, Macmillan, 1938, p.44, my emphasis. For an Austrian view on experimentation problems in economics see, e.g., Ropke, W., 'The Place of Economics Among the Sciences', in Sennholz, On Freedom and Free Enterprise, op.cit., pp.122ff; C.f., Stewart, I.M.T., Reasoning and Method in Economics, London, McGraw Hill, 1979, p.106, who denies that statistical work can somehow 'stand-in' for controlled experiments in economics. Econometricians have made some progress in dealing with issues raised here, especially with the identification problem (direction of causation); omitted variables bias; and the use of dummy variables to counter the problem of immeasurable, qualitative factors. On the achievements of econometrics, see, e.g., Hendry, D: 'Econometrics - Alchemy or Science?' Econ. 47, November 1980, pp.387-406.

45 The following example is drawn from Rizzo, M.J., 'Praxeology and Econometrics: A Critique of Positivist Economics', in Spadaro, L.M., ed., New Directions in Austrian Economics, Kansas City, Sheed Andrews and McMeel Inc., 1978, pp.48-50.

hypothesis and empirical 'reality' is established.<sup>46</sup> Rizzo proposed that links between say  $M_1$  and 'money' might be found by setting up falsifiable hypotheses. However, he found that the operational rules for counting  $M_1$  made no predictive statements so they were not amenable to falsification. Operational counting rules do not say X results in an increase in Y, all they say is that 'observable' variable X is an adequate empirical reference for its abstract theoretical counterpart Y but the criteria of adequacy are unclear. (This is one reason why Blaug noted that "economists ... cannot agree on definite empirical criteria for falsifying a hypothesis".<sup>47</sup>) Further, it is not possible to rewrite the referential rules of correspondence between numerical observations and abstract concepts in a way that makes them refutable, i.e., if the criteria applying the abstraction 'money' are applied, then  $M_1$  will be found to be an adequate (or inadequate) empirical counterpart. This requires that the investigator knows the criteria of adequacy prior to 'testing' which was to confirm these criteria in the first place. As Rizzo concluded, 'testing' the rules of reference or correspondence "is impossible unless we already know the criteria of applying theoretical terms. If we already know these, then testing is unnecessary. But, from a positivist perspective, it is

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46 Ibid., p.49.

47 Blaug, M., Economic Theory in Retrospect, 3rd edition, Cambridge, Cambridge University Press, 1978, p.7.

impossible to have knowledge ... which is given a priori".<sup>48</sup>

This typical rationalistic Austrian argument sets up an extreme, some would say, straw-man 'positivism' - and there appear to be many different brands of 'positivism'.<sup>49</sup> Rizzo explicitly addressed Friedman's positivism,<sup>50</sup> but recent literature has exposed fundamental inconsistencies in Friedman's version of the methodology of positive economics. For instance, Hollis and Nell maintained that Friedman frequently dropped his putative positivist guard. On the one hand, for Friedman, theory is only a filing system of tautologies and on the other hand a theory is the way we perceive the 'facts' but "if he [Friedman] sticks to Positivism, facts are given and concepts are optional".<sup>51</sup>

Caldwell lately went so far as to demonstrate that Friedman was a "dramatic instance" of an economist who did not draw on positivist philosophy of science in formulating his ideas.<sup>52</sup>

These caveats granted, the Austrian critique highlights dangers emanating from any so-called positive, 'scientific', quantitative procedure that is eventually used for policy

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48 Rizzo, 'A Critique', op.cit., p.49, my emphasis.

49 On "positivism" see Kaplan, A., 'Positivism', in Sills, D.L., ed., International Encyclopedia of the Social Sciences, Vol.12, Macmillan, Free Press, 1968, pp.389-394.

50 Rizzo, 'A Critique', op.cit., p.48. For the full Friedman version see Friedman, M., Essays in Positive Economics, Chicago, Chicago University Press, 1953, pp.3-43.

51 Hollis, M. and Nell, E.J., 'Two Economists', in Hahn, F.H. and Hollis, Philosophy and Economic Theory, Oxford, Oxford University Press, 1979, pp.51-52.

52 Caldwell, B., 'Positivist Philosophy of Science and the Methodology of Economics', JEL, 14(1), March 1980, p.62. C.f., in defense of Friedman, Boland, L.A., 'A Critique of Friedman's Critics', JEL, 17(2), June 1979, pp.503-22.

legitimation on the grounds that it can produce unambiguous results. The critique is especially forceful in identifying the role of underlying purposes in procedures which entail the choice of operational, numerical counterparts that help present a 'favourite' hypothesis in the best light (e.g., using a specific measure of money supply to best predict GDP). So overall, even if ceteris paribus clauses are corroborated, the use of statistical constructs in a testing process, and the results derived thereafter, leaves scope for intervening, unknown variables viz., the purposes, interests and mind of the investigator. This was why - after a study of methods used to test hypotheses in economics - Katouzian was moved to conclude on a sceptical note:

There can be no doubt that improvements in the art of 'political arithmetic' - including economic and social statistics, and econometrics - have been beneficial to 'pure economics', not only by making empirical testing possible, but also by demanding more precision in the formulation of economic hypotheses. But that is as far as it goes. The imperfections and inadequacies of the known techniques - while they are being applied with ever more enthusiasm and a surprising degree of confidence - leave large areas of empirical investigation open to disputes on the 'correct' interpretation of results.<sup>53</sup>

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53 Katouzian, M.A., 'Scientific Method and Positive Economics', SJPE, 21(3), November 1974, p.282, my emphasis. Katouzian (a non-Austrian) listed the choice of base-period when using time-series observations; the question of correlation versus 'causation' and problems involved in ascertaining direction of 'causation' that can all blur interpretation of results. (It may be added that Katouzian's study was encouraged and advised by Karl Popper). C.f., corresponding Austrian scepticism of empirical testing using numerical data registered by Lachmann, L.M., Capital, Expectations and the Market Process, Kansas City, Sheed Andrews and McMeel Inc., 1977, pp.176-7; and Littlechild, S.C., The Fallacy of the Mixed Economy, London, IEA, 1978, pp.22-26.

With the introduction of time into their analyses of the design and application of numerical data, the Austrian contributions have some overlap with Shackle's epistemics to be discussed presently in Section 3.4. Consistent with their subjectivist preconceptions, the Austrians understand that time present and time future, like other commodities in present and future, mean different things to different people. All human actions are directed by subjectively established purposes that differ between people. Units of time cannot be added together for the same reason that apples cannot be counted because evaluations of time differ interpersonally as well as intrapersonally. This means that the Austrians eschew consideration of an aggregate measure of such things as capital stock - since present value calculations presume a constancy in the standard by which time in the future is valued.<sup>54</sup> Aggregate production functions are also rejected since, generally, their use implies quantitative functional determination within a closed system of variables and separates movements of aggregate quantities from what is happening in heterogeneous individual markets and enterprises. Further, there can be no question of any quantitative determination of technological progress and change in a model of economic growth. Technology, as a

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54 See Kirzner, I.M., 'The Theory of Capital', in Dolan, E.G., ed., The Foundations of Modern Austrian Economics, Kansas Institute for Humane Studies, Sheed and Ward Inc., 1976, pp.138-43; and for a critique of conventional measures of capital and income flows see Lachmann, L.M., 'Toward a Critique of Macroeconomics', in Dolan, ed., ibid., pp.153-5. Lachmann argued that capital measurement was confounded by "our inability to measure ... the subjective nature of expectations concerning future income streams" (p.153).

concomitant of production is a process - a series of "mental acts that turn experience into a new awareness, and then into new application. Certainly, many errors are made in the process". Moreover according to Lachmann, "acts of the mind do not fit easily into the formal apparatus of a body of thought the main purpose of which is to produce a closed system within which it is possible to assign numerical values to a large number of magnitudes".<sup>55</sup>

What, then, of the use of numerical data and functional quantitative constructs apart from their possible employment as descriptive historical tools? Mises saw the use of numerical constructs as "a method of thinking" and persuading from the point of view of the purposes and interests of the individual. Mises wrote cynically that, in the case of index numbers "nobody acquiesces in an index number if he does not expect a personal advantage from its acknowledgement by public opinion. The establishment of index numbers does not settle disputes; it merely shifts them into a field in which the clash of antagonistic opinions and interests is irreconcilable".<sup>56</sup> Statistics of all kinds such as those that refer to the prices ruling in a particular market are past prices; they "describe ... the state of affairs at an arbitrarily chosen instant while life and action go on and do not stop"; they are helpful only as anticipatory tools for calculating expected prices and can be actively used to establish the results of past action. As

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55 Lachmann, Capital, Expectations ..., op.cit., p.121 and p.156.

56 Mises, Human Action, op.cit., p.223, my emphasis.

action is always ongoing, that is, always directed toward influencing the future state of affairs, the use of numerical data can be oriented to the future as well. However, the impressive precision and numerical exactitude of some numerical constructs cannot remove doubts because the future is uncertain. The development of numerical constructs of past events in order to help orientate ongoing human action toward the future is "not a means of knowing future conditions with certainty and it does not deprive action of its subjective and speculative character. But this can be considered a deficiency only by those who do not come to recognise ... that life is not rigid, that all things are perpetually fluctuating, and that all men have no certain knowledge about the future".<sup>57</sup> The spurious precision of economic numerology does not remove this kind of uncertainty and numerical data cannot be used to predict 'for certain'. This perspective leaves much room for 'surprise'.<sup>58</sup> It is in this context that measurement has little or no meaning for Austrian economists (with all the consequences this might have for the possibility of accurate and consistent quantitative prediction in the social sciences). As Knight commented: "If we accept

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57 Ibid., pp.214-217.

58 There is overlap here with Shackle's concept of "surprise" in economic decisionmaking. In Shackle's work: "surprise is that dislocation and subversion of received thoughts, which springs from an actual experience outside of what has been judged fully possible [e.g., from the manipulation of statistics], or else an experience of a character which has never been imagined and thus never assumed as either possible or impossible: a counter-expected or else an unexpected event". Shackle, G.L.S., Epistemics and Economics: A Critique of Economic Doctrines, Cambridge, Cambridge University Press, 1972, p.422, my bracketed insert. Refer also to idem., 'The Logic of Surprise', Econ., 22, 1953, pp.112-117.



the aphorism, 'science is measurement', as a definition of science, which is its only intelligible meaning, then there is no such thing as 'economic' science".<sup>59</sup> To round off on the subject of measurement and prediction, Lionnel Robbins lately reinforced the subjectivist, Austrian perspective: "quantitative prediction in economics is apt to be hazardous; much more hazardous than predicting the weather".<sup>60</sup>

A study of the design and use of numerical data with reference to human activity and to public policy cannot banish from its purview reference to subjective mental processes. Human beings are required to design and use numerical constructs and in the process confer meaning on them. It requires human beings to ascribe substance, if not existence, to statistical aggregates and take the disconcerting consequences of all the pitfalls of hypostatization that may result.<sup>61</sup> The formal argument is the same whether the numerical data is of a time-series form or cross-sectional, since the latter only reduces the level of aggregation. Debate surrounding the methodology of

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59 Knight, F.H., 'The Nature of Economic Science in Some Recent Discussion', AER, 24, June 1934, p.236. See also idem., 'Quantification: The Quest for Precision', in Wirth, L., ed., Eleven Twenty-Six: A Decade of Social Science Research, Chicago, Chicago University Press [1940], reprinted New York, Arno Press, 1974, especially p.169, and Gonce, R.A. 'Frank H. Knight on Social Control and the Scope and Method of Economics', SEJ, 38, 1972, pp.548-49.

60 Robbins, L., 'Economics and Political Economy', AER, 71(2), May 1981, p.3. See also idem., An Essay on the Nature and Significance of Economic Science, London, Macmillan, 2nd edn., 1935, p.106f.

61 On the dangers of hypostatizing collective concepts and constructs see Mises, L., The Ultimate Foundations of Economic Science [1962], Kansas Institute for Humane Studies, Sheed Andrews and McMeel Inc., 1978, p.78ff.

Austrian economics recently highlighted the as yet unsolved problem as to whether macro-level numerical constructs and correlations could be 'reduced' to their micro-level 'equivalents'. This question was originally posed by Nozick.<sup>62</sup> So far Austrian reply to Nozick's question has been in the negative. In defending the Austrian position it may be argued that all economic relationships must involve first and foremost, consideration of human purposes, implicitly or explicitly. Whereas Austrian microeconomics is able to trace phenomena back to the causal agents (individual decisionmakers or social participants),

macroeconomics includes only artificial constructs which, apart from the individual choices upon which they are indirectly based, have no causal explanatory power on their own. There are, to be sure, statistical correlations between various of the macroeconomic aggregates. But cut off from the purposes of human actors, the only causal agents in economics, they are powerless to form part of a causal genetic chain.<sup>63</sup>

In conclusion, one may not go so far as Menger and his epigones and argue that statistical constructs of past occurrences are of only incidental value for helping individuals

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62 Nozick, R. 'On Austrian Methodology', Synthese, 36(2), 1977, pp.353-92. See especially pp.361-3. Nozick's argument abstracts from the question of whether or not any macro-constructs have actually been 'reduced' to their atomistic micro-'equivalents' in economics. His case appears to rest on an analogy from physical science where macro-biological constructs are reducible to the micro-principles of chemistry.

63 Block, W. 'On Robert Nozick's "On Austrian Methodology"', Inquiry, 23(4), December 1980, p.407, my emphasis.

form expectations of future occurrences; that statistical correlations and functions are merely artifacts that "do not describe anything else than what happened at a definite instant of time in a definite geographical area as the outcome of the actions of a definite number of people",<sup>64</sup> or that the construction of some numerical aggregates and global measures belong to the "Dark Ages" of economics.<sup>65</sup> Perhaps it is sufficient to insist, after considering Austrian perspectives up to this point, that the meaning of numerical data and constructs must be constantly checked against the underlying human plans and purposes they are supposed to, and used to represent in summary form. We cannot, to paraphrase Hicks, escape the duty of justifying numerical constructs by pretending that they have been justified already.<sup>66</sup>

### 3.4 SHACKLE'S EPISTEMICS

The trumpet-call issued in 1930, and variously repeated since, to make economics quantitative, has released a flood of ... statistical effort. Yet it can be scarcely claimed that all is now well ... [S]ome of the most widely used economic statistics are not merely grossly inaccurate but basically meaningless.<sup>67</sup>

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64 Mises, Ultimate Foundations, op.cit., p.63.

65 Morgenstern, O., 'Thirteen Critical Points in Contemporary Economic Theory', JEL, 10(4), 1972, p.1185.

66 Hicks, J.R., 'The Scope and Status of Welfare Economics', OEP, 27(3), 1975, p.319. (Hicks was referring to measures of national product).

67 Shackle, G.L.S., 'Review of Morgenstern, On the Accuracy of Economic Observations', Kyklos, 17, 1964, p.513.

In Epistemics and Economics Shackle raised questions, not altogether central to his main theme, about economists' concentration on the behaviour of numerical aggregates and constructs as if they represented certain, given components of the macro-economy since "the subject matter [of economic analysis] consists of thoughts and not of objects, which makes [their] assimilation to mechanical science dangerous. Thoughts are transformed invisibly and radically with baffling swiftness, stultifying ... careful additions and subtractions of quantities which become obsolete before they are printed".<sup>68</sup> Shackle presented an interpretation of the character of numerical knowledge which reinforces the perspective being developed here: the universe of numerical knowledge generated from 'hard' numerical data, is an indeterminate, imprecise, problematic sphere. Economists have the means for quantifying many of the objects of their discourse. The art of quantification entails simplification because it relegates "form, structure and detail

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68 Shackle, Epistemics, op.cit., p.66. Of course, Shackle cannot be strictly labelled an economist of the Austrian School. A slightly different, though corresponding perspective of conventional economic methodology is offered by Nicholas Georgescu-Roegen. In his view, as a result of the quantitative and mathematical revolutions "economics now breathes mechanics throughout", 'Methods in Economic Science', JEI, 13(2), 1979, p.321. Another point of correspondence with Shackle and the Austrians is his view that "there is absolutely no reason for economics to treat as fiction the very springs of economic action - wants, beliefs, expectations, institutional attitudes, etc. For these elements are known to us by immediate acquaintance, that is, more intimately than any of the economic 'observables' - prices, sales, production and so forth". The Entropy Law and the Economic Process, Cambridge Mass., Harvard University Press, 1971, p.336. See also his study of 'Concepts, Numbers, and Quality', Chapter 2, pp.17-45 of Analytic Economics: Issues and Problems, Cambridge Mass., Harvard Univ. Press, 1966.

in favour of addable scalar quantity". Furthermore, it involves devising means to give "essentially and incurably imprecise notions an air of exactness and rigour" and results in expost "scalar measures of assemblages which exhibit limitless diversity". Shackle argued that attempts to scalarise the subject matter of economics and economic policy imposed a deterministic precision which from a micro-level standpoint obscured the vast richness of intricate interlocal and inter-temporal detail. Thus, for Shackle the use of mass numerical observations was consistent with the "economist's instinct to sweep everything into a few conceptual hold-alls".<sup>69</sup> This undoubtedly increased analytical scope but at the same time reduced magnification. Numerical data is created on the assumption of logical equivalence classes which reduce incommensurables to common terms in the counting and measuring process. For instance, index numbers can be utilised to help provide an unequivocal measure of changes in real income which is made up of many incommensurable, heterogeneous goods and services. The problem of measuring the degree and direction of changes in real income and stating the results in terms of a single scalar is made difficult not only because of the effects of changes in the structure of relative prices. It is made more difficult if it is admitted that goods and services may be perceived differently and possess diverse qualities among different people even though their market prices may be the same. A whole host of individual plans, expectations, decisions

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69 Epistemics, op.cit., pgs. 39, 9, unnumbered Preface and p.47 respectively.

valuations and psychological factors contribute to move goods and services about; to combine them so as to produce other commodities and to confer attributes on these commodities so that they can become objects of consumption and so forth. A global measure will not adequately capture this diversity which is especially evident at the micro-level. In this way, summary measures increase scope but at the same time reduce magnification. This represents another expression of the tension between two levels of explanation - the micro/macro dualism in economic thought and analysis. For example, in no case will there exist a common scale which can be unambiguously used to ascertain the magnitude of all the possible diverse elements in the category: 'output of consumer goods' before they are aggregated or averaged-out. Index numbers will help aggregation and first and second period market price indices can express the change in real output. In the process, it is arguable that some information loss will occur inasmuch as a quantity can obscure qualitative differences and submerge detail. The use of market valuation means, for instance, that aggregates such as real income, consumption, investment and savings (or even their partially disaggregated components) appear as single variables of homogeneous composition. This is a convenient simplification and depending on the purpose it may be quite justified, but as with all simplifications it carries with it hidden dangers. That is, a measure will indicate logical equivalence in terms of quantity but not always equivalence in quality. Further, if for example we are dealing with annual measures, these will not always

indicate logical equivalence over time (the annual period) since some variation may be possible at other temporal micro-intervals (daily or monthly and so on). Moreover, numerical data based on a sample presupposes that the domain for counting is constant and does not change over the process of sampling. In the phenomena of the social sciences, however, the domain is subject to change while sampling takes place.

Despite their difficulties, Shackle admitted that the development and application of economic statistics has been "a great simplifying revelation"; a "very great and remarkable achievement" which has led to "insights of a peculiar sort, without which the economic world would appear a mere chaos of proliferating and unintelligible detail, reasonable and orderly only in the small ... and otherwise lacking any sense of architecture". It was Quesnay's Tableau Economique that offered economists the possibility of measurement, arithmetic or adding-up which could be applied to the orderly and regularly interacting components of 'the economy'. Quesnay was the first thorough analyst of numerical relationships between aggregates of production, consumption, investment and capital replacement. Here, according to Shackle, was the means to construct a new language of economics.<sup>70</sup> Measurement bore a stamp of the physical sciences which were then strongly under the influence of Newton. At this time, modes of reasoning in physical science were centered around a conception that the

really important world outside was hard,  
cold, colourless, silent and dead; a  
world of quantity; a world of mathematically

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70 Ibid., pp.48-9 and p.363.

computable motions in mechanical regularity. The world of qualities immediately perceived by man became just a curious and quite minor effect of that infinite machine beyond.<sup>71</sup>

The physical world could be described by building up systems of scientific laws. Thenceforth it could be seen and understood as a vast, steadily working machine. It was reasonable to suppose that a similar set of principles could be discovered for the world of commerce and economy. Shackle pointed out that since Quesnay's time "scalar quantification, mechanicism, description of the structure and life of economic society in terms of stable mathematical functions in a closed system ... has seemed easy and tempting". In addition, scalarisation is a tool that economists have come to uncritically idealize and from which they have uncritically generalised. "Aggregation", wrote Shackle, can lead to "the reifying of 'income', 'investment', 'consumption', ... 'gross national product', 'the capital-to-output ratio' and a dozen or so other vast looming phantoms" that are often "treated as objects of worship".<sup>72</sup>

Now, measurement of components of 'the economy', that economic 'machine beyond', must always link number to concept or number to perceived components of 'the machine' in a post-hoc fashion. This has crucial implications for the limitations of

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71 Burt, E.A., The Metaphysical Foundations of Modern Physical Science: A Historical and Critical Essay, London, RKP, 1932, p.237, my emphasis.

72 Shackle, Epistemics, op.cit., p.361 and pp.362-3. Also on Quesnay, see Shackle, 'Evolution of Thoughts in Economics', BNLQR, No.132, March 1980, pp.14-15.



knowledge derived from numerical data and its application in an exante sense. Since economics "is about thoughts, it is therefore a branch or application of epistemics, the theory of thoughts". It deals not with the external world of things - of physical components - but with thoughts and deeds: "the world of the conscious mind, in its eternal station on the edge of the void of time". Shackle's introduction of the thinking person extended in time rendered quantification "a superficial facility" which "has tried for precision, certainty and reach of prediction whose basis is not there".<sup>73</sup> All numerical observations are built up from the past in a world of uncertainty where the future is inherently unknowable. Perception of the connections between these observations and the conceptual referent(s) they are used to stand-in for cannot be stable; the connections must be volatile and uncertain. Shackle's epistemics, with its stress on human decision, expectation and action shaping an unpredictable reality, revealed numerical data as objects created by a human subject or group of subjects - and not as measures of things autonomous to the process of human inquiry. From this perspective, numerical data was not the quantitative 'equivalent' of economic phenomena as realities independent of the observer(s). The observing subject designing, using and interpreting numerical observations is not conceivably detached from the observed, measured object. Shackle also leaves things open for the flow of time where expectations are formed in a process involving

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73 Ibid., Preface and pp.361-2.

imagination and mystery. In part, in Shackle's schema, the "future is unknowable but not unimaginable. Future knowledge cannot be had now, but it can cast its shadow ahead". Imagined future actions and hence decisions, are not fully determined by past events.<sup>74</sup> Imagination transcends 'given' numerical data of the past. As it plays a role in 'casting a shadow ahead' this must also entail the emergence of a varied pattern of possibilities and interpretations which can be conferred on numerical observations and constructs (even of the 'same' thing) through time. The economist's numerical data, wrote Shackle, are the result of an "art of heroic simplification; the art of the Gordian knot, carefully tied up in advance, however, by the man who is going to cut it". All this led Shackle to contend that numerical aggregates, index numbers, ratios and the like, by no means "tell of direct contacts with reality [;] ... in fact they are its denatured remnants. It is not for nothing that statisticians speak of their kitchen".<sup>75</sup> Ineluctably, the construction of statistics was, for Shackle, an exercise in imprecision. All numerical constructs are theory impregnated from the outset by the observer-cum-decisionmaker and imagination has to play a role in their application to 'cast shadows ahead'. Since insight into reality is insight from the

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74 Lachmann, L., 'From Mises to Shackle: An Essay on Austrian Economics and the Kaleidic Society', JEL, 14(1), March 1976, p.54. This theme is pursued in Shackle, Imagination and the Nature of Choice, Edinburgh, Edinburgh University Press, 1979.

75 Shackle, Epistemics, op.cit., p.10, my emphasis, and p.362.

viewpoint of conscious, acting human beings participating in reality, it is not possible to neatly separate the observer from the observed. 'Externally observed data' may be measurable but the knowledge of human beings is not. External changes denoted by numerical data do not emerge independently of the way in which these changes are perceived, of the way in which these changes affect interpretations and of the way in which these changes are discovered at all.<sup>76</sup>

Action, decision and choice informed by (say) series of 'hard' numerical data designed in the past will not render outcomes determinate. The very design and use of 'hard' numerical data is a process affected by Shackle's "act of creative imagination". This process is dependent on the filter of the human mind.<sup>77</sup> Choices among courses of action whose outcomes are indeterminate may be aided by the use of probabilistic and stochastic methods in formal economic analysis. These methods have undoubtedly motivated the tendency toward the collection and manipulation of numerical data. However according to Shackle, any search for what the future will be is futile in view of its probabilistic nature. If the future is unknowable how much does the probability calculus contribute to our insights into economic reality? For Shackle, probability frequency ratios designed from sets of numerical observations can affect expectations: "statistical frequencies

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76 These points draw heavily on Kirzner, I.M., 'The "Austrian" Perspective', The Public Interest, Special Issue, 1980, p.122.

77 Shackle, Uncertainty in Economics and Other Reflections, Cambridge, Cambridge University Press, 1955, p.68.

are ... amongst the materials out of which ... judgements arise".<sup>78</sup> The use of statistical probability may help recurring decisions, yet most decisions will still be open to unquantifiable subjective belief because they have to be made concerning novel, unrepeated ventures in a world of uncertainty. Of course, this will vary with the individual and the circumstances:

[P]robability theory [has shown] how to derive knowledge concerning aggregates of future events from aggregates of past events, but this technique is irrelevant to personally unique and crucial occasions of decisions by an individual. Frequency ratios ... have nothing to do with uncertainty. The individual can choose only between imagined and not realised experiences, for when experience is actually proceeding it is too late to choose something else. Thus decision must, it seems, be guided ... by the thrust of imagined and anticipated enjoyments and distresses.

For Shackle there is a wide gap between 'uncertainty' which can be reduced to a calculable relation by means of numerical observations and uncertainty that apparently does not offer any basis whatever for a rigorous calculus of probability. In summary Shackle considers that a significant element of immeasurable indeterminacy is an integral part of economic

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78 Shackle, Decision Order and Time in Human Affairs, Cambridge, Cambridge University Press, 1969, p.72. C.f., also Carter, C.F., 'On Degrees Shackle: or the Making of Business Decisions', in idem., et.al., eds., Uncertainty and Expectations in Economics: Essays in Honour of G.L.S. Shackle, Oxford, Blackwell, 1972, p.30ff.

processes.<sup>79</sup>

Shackle's world view has been described as a "bold challenge to neoclassical orthodoxy", the latter being characterised by a determinism borrowed from the Newtonian perspective in physical science with its world of supposedly given, stable data much of which can be expressed in terms of numerical constants.<sup>80</sup> It is at the level of world outlook that Shackle's work has striking affinity with writers in the mainstream Austrian tradition. As well, Shackle's perspective of the inherent limitations and uncertainties connected with knowledge which is an outcome of the process of designing and interpreting numerical data could be accommodated under the appellation of a "Heisenberg perspective".<sup>81</sup> Margenau showed

79 Shackle, et.al., eds., Uncertainty and Business Decisions: A Symposium on the Logic, Philosophy and Psychology of Business Decisionmaking Under Uncertainty, Liverpool, Liverpool University Press, 2nd edition, 1957, p.105, my emphasis. On Shackle's subjective conception of probability see Niehans, J., 'Reflections on Shackle, Probability and Our Uncertainty', Metroeconomica, 11, 1959, pp.76-78. C.f., Hicks, J. Causality in Economics, Oxford, Blackwell, 1979, especially pp.103-107 on "probability and judgement". For the as yet unsolved problems Shackle-type chance and indeterminacy has introduced for general equilibrium theorists concerned with economic adjustment processes and their means, so far, of dealing with it, see Fusfeld, D.R. 'The Conceptual Framework of Modern Economics', JEI, 14(1), March 1980, pp.21-23.

80 See Lachmann, 'From Mises to Shackle', op.cit., p.54. For a defence of neoclassical economics against the challenge of Shackle and the Austrians, see Boland, L. and Newman, G. 'On the Role of Knowledge in Economic Theory', AEP, 18(1), June 1979, pp.71-80.

81 For some other applications of Heisenberg to economic analysis see Ward, B. What's Wrong with Economics?, New York, Basic Books, 1972, pp.167-9; Wisman, J.D. 'Values and Modes of Rationality in Economic Science', IJSE, 7(3), 1980, pp.137-48; and for Austrian comments on some implications of Heisenberg, see Spadaro, 'Averages and Aggregates', op.cit., p.146; and Machlup, F. 'Economic Facts and Theory', in idem., The Political Economy of Monopoly, Baltimore, John Hopkins Press, 1952, pp.465-66.

that, in the field of quantum mechanics, Heisenbergian "uncertainty arises from a fundamental shift in the meaning of physical reality" as the observer interacts with the observed in the process of measurement. Margenau considered measurement to be something more than observation in that measurement culminated in the assignment of numerals to phenomena to produce numerical constructs. In microphysics Heisenberg's principle of uncertainty implied that the influence of the observer on the position and velocity of particles made it impossible to ascertain their position and velocity simultaneously. One of Heisenberg's examples started from the problem of determining the position of an electron particle. Its position could be ascertained with precision by using light of the shortest known wavelength, namely, gamma rays. Short wavelength entails that frequency is large so that its momentum is correspondingly great. Use of short-wave gamma rays for accurate measurement of position is therefore likened to a swift projectile that, when striking an electron, adds to the latter's momentum. This crucially alters the electron's original momentum to an unpredictable degree. An accurate position measure carried out by the scientist then disturbs the particle's momentum. Margenau comments rather boldly that "perfectly good measurements ... in the social sciences" may "spoil" or even "annihilate" a system akin to the process of measurement in physics.<sup>82</sup> A Heisenbergian notion might be loosely applied to a

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82 Margenau, H. The Nature of Physical Reality: A Philosophy of Modern Physics, [1950], Woodbridge Connecticut, Ox Bow Press, 1977, pp.370-71 and p.387, my emphasis. The example from Heisenberg's work is drawn from pp.366-68.

case where an econometrician may discover a provisionally applicable quantitative relationship derived from past numerical data between, say, forward foreign currency exchanges and given developments in domestic monetary policy. Once this new knowledge is released in the journals or media it may then be rendered erroneous; people would incorporate this 'discovery' in their plans and alter their financial dispositions according to this new system of knowledge. This could disturb or destroy the econometrician's original predictions. Or, it may initially lead to some reinforcement of these predictions, but in turn, call forth a counteractive policy response. Weisskopf recently developed some of the philosophical and epistemological implications for economics of this principle of uncertainty (a principle which now forms part of modern theory in physics but which has much broader epistemological significance). Generally, Heisenberg's ideas have their roots in twentieth century thought. Weisskopf hastily points out that Heisenberg's findings cannot be used as an indication of a direct influence on twentieth century social science and economics but merely characterize a pattern of thought. When applied to social disciplines this pattern of thought has special significance. It admits to an ineradicable source of indeterminacy in social science, when man is demoted "to a participant who cannot extricate himself from the reality he wants to analyse. This new world view exposes the helplessness and uncertainty which is inherent in the human situation and which was repressed and

denied in Newtonian thought".<sup>83</sup> Roughly expressed, a Heisenberg perspective of economic analysis generally admits reaction of the subject-matter to the process of inquiry itself. The observing subject interacts with observed phenomena in such a way as to lead to changes in the meaning of economic reality. This bears tangentially on a theme of these studies in 'political arithmetic'. It points out the need to appraise 'disturbances' through time to meaning or thought patterns concerning the phenomena subjected to a count or a measure - 'disturbances' evident in specific processes of designing, using and interpreting sets of numerical observations. It is exceedingly suggestive then, to see in Heisenberg's principle an application to the present problem - the problem of discovering and assessing some of the limits on knowledge obtainable from numerical data. Heisenberg provides an insight with very wide implications. That is, the object of knowledge - the phenomena we attempt to quantify - is not to be separated from, or considered independently of, the observer and the instrument (operation) the observer uses to acquire knowledge by compiling numerical data. This allows room for changes in the meaning of numerical data over time, or in other words, changes in thought patterns connecting or referencing the data to aspects of

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83 Weisskopf, W.A., 'The Method is the Ideology: From a Newtonian to a Heisenbergian Paradigm in Economics', JEI, 13(4), December 1979, pp.869-84. See also Fischer, C.C., 'A Comment on "The Method is the Ideology"', JEI, 15(1), March 1981, pp.193-195; Weisskopf, 'Reply to Professor Fischer', ibid., pp.196-197 and Tool, M.R., 'Observations on the Fischer Comment', ibid., pp.197-199.



'reality'. This is the essence of the situation presented by Heisenberg's principle. It is, of course, not a question of any formal and rigorous application of the principle but only of something qualitative and suggestive.

## Epilogue

In the present context, and drawing on the perspectives surveyed here from Menger to Shackle, five tentative conclusions are suggested. They will act as broad guideposts in what follows and are selectively drawn up in the light of Hayek's comment, previously quoted, expressing concern for the limitations of numerical knowledge. With following chapters in mind they also draw together, in by no means any exhaustive way, some of the ideas surveyed in this section.

- (i) Numerical observations are historical data. They cannot be used to logically entail or predict with certainty a particular, future occurrence.
- (ii) Numerical observations, however designed, can obscure micro-level uniqueness.
- (iii) Numerical observations, when interpreted, 'refer' to unobservable variables (concepts) - 'references' which will depend on the context (time, place and interpreter). Taken together, (i), (ii) and (iii) leave a gulf of uncertainty when 'referring' number to concept.
- (iv) The compilation of numerical observations and their interpretation as stand-ins (or 'references') for abstract concepts is a process rather than a one-time event. Various time-place influences may act upon design and interpretation. These influences help transform concepts (thoughts, interpretations) about the content of quantified phenomena over time. In turn, new concepts help transform counting/measuring rules.

- (v) These conclusions suggest that some limitations on numerical knowledge might be assessed through history in terms of what may be understood as an open-ended, iterative process of designing and interpreting numerical observations.

These conclusions require further exploration. Section III(B) concentrates on extending and elaborating further (mostly Austrian) views on questions of operational definitions in economic analysis - with special reference to numerical data and with indirect reference to conclusions (i), (ii) and (iii). Section IV will further investigate conclusion (iii) - with special reference to the concept of an 'indicator' in conjunction with a study of the early history of political arithmetic. Section V will apply conclusions (iv) and (v) by way of two narrative case histories.

III(B) FURTHER PERSPECTIVES ON THE LIMITS  
OF OUR NUMERICAL KNOWLEDGE

### 3.5 MORGENSTERN AND BEYOND

... the way we describe the economy today is extremely primitive in spite of the outpouring of statistics. We will eventually experience painful surprises as those physics suffered when quantum mechanics arrived and showed that there is ... an uncertainty principle, much to the distress of classical physicists. (Morgenstern).<sup>1</sup>

When any questions are raised concerning the nature and limits of our numerical knowledge, issues surrounding notions of the accuracy of numerical observations cannot escape consideration. Accuracy in this sense was first explored systematically in economics by Oskar Morgenstern. Morgenstern recognised that in the physical sciences, scientists have been concerned with accuracy of measurements and observations from the earliest beginnings. The careful work of observation performed for millenia in astronomy, reaching its peak in Tycho Brahe who tried to correct imperfections in his measuring instruments, is often cited in this respect.<sup>2</sup> This work in astronomy had to precede Newton whose theory may have been inconceivable without Brahe as predecessor.<sup>3</sup> In the situation of the social sciences such as economics there is no comparable tradition of exploring measurement errors and evaluating their

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1 Morgenstern, O., 'Descriptive, Predictive and Normative Theory', Kyklos, 15, 1972, p.704.

2 Morgenstern, O., On the Accuracy of Economic Observations, 2nd edn., Princeton, Princeton Univ. Press, 1963, Preface and p.45.

3 See Koopmans, T.C., 'Measurement Without Theory', [1947], reprinted in Beckmann, M. et.al., Scientific Papers of T.C. Koopmans, N.Y., Springer, 1970, especially pp.112-113.

impact on the development of economic analysis. As well, in economics, the development of numerical observations of components of 'the economy' may be more complicated since economies are subject to tremendous and rapid changes while for the purposes of Newton and his predecessors the sky could be considered static.

Morgenstern considered that there is hardly sufficient discussion and awareness of measurement accuracy in economics -

a consequence, in part, of the fact that contemporary economics is primarily deterministic. The fact that we are living in a stochastic universe has barely penetrated, least to the standard textbooks where the idea of uncertainty is almost never mentioned. So it is not surprising, albeit regrettable, that the inevitable measurement errors ... are never systematically incorporated in theory, in computation or interpretation.<sup>4</sup>

It is the role of uncertainty in the process of interpreting numerical data with reference to economic analysis, which is one of the focal points of this section. Many economic decisions including those involving economic policy questions, often have the characteristic that numerical and non-numerical data are combined with some theory in the act of decision. The central concern of this discussion is with one facet of this act of decision regarded by Morgenstern as obiter dicta. Morgenstern confined his study of accuracy, for the most part, to questions of accurate compilation of numerical data before its active use, application and interpretation by a human subject or group of

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4 Morgenstern, op.cit., 1972, p.702, my emphasis.

human subjects in a decisionmaking process. Despite the fact that he found the notion of accuracy to be "inseparably tied to the use of which [data] is put", Morgenstern did not fully develop a theory of accuracy or reliability of numerical data for specific uses and purposes. Morgenstern made it a rule in his book to concern himself chiefly with errors in primary data in the sense of 'raw' numerical observations. He vowed to "stay away as far as possible from ... uses of data, and further conceptual issues" even while it becomes clear to him as the study progresses, that this is not always possible.<sup>5</sup> It was his conviction that any decisionmaker using numerical data must consider those factors which lead to errors or inaccuracies in terms of counting definitions. He largely restricted his analysis to these factors rather than "with the deep-lying problems of combining quantitative and non-quantitative evidence as it enters economic analysis [because] such a discussion would require a more profound methodological study than is attempted here".<sup>6</sup> It is proposed in this section to move toward an analysis of some of these more deep-lying problems.

The value of numerical data depends not only on its immediately recognisable, sometimes calculable, level of accuracy but also upon the way the data is used in conjunction with qualitative information and qualitative judgements. This is expressed in Morgenstern's broad sketch of the concept of

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5 Morgenstern, op.cit., 1963, p.3 and p.283. For the difficulties Morgenstern encountered in trying to keep to this rule, see Ch.15, pp.283-301.

6 Ibid., p.4.

accuracy:

The very notion of accuracy and the acceptability of a measurement, observation, description, count - whatever the concrete case might be - is inseparably tied to the use to which it is put. In other words, there is always a theory or model, however roughly formulated it may be, a purpose or use to which the statistic has to refer, in order to talk meaningfully about accuracy. In this manner the topic soon stops being primitive; on the contrary, very deep-lying problems are encountered, some of which have only recently been recognized.<sup>7</sup>

One problem of accuracy, heretofore neglected in the literature, will be addressed here. It relates to an element of uncertainty that can arise when a qualitative openness or residual meaning is discernible between operational rules for compiling economic statistics (sampling, coding, aggregating, estimating, etc.), and the concept (Morgenstern's "purpose") that the statistics are used to represent or refer to. The relationship between a series of numerical observations operationally designed by convention and abstract, sometimes multidimensional, multivariate concepts, will depend on a presumed but by no means certain, or precise correspondence. The rules that specify what observable properties of phenomena to quantify seldom exhaust the whole meaning of a concept of interest. This source of indeterminacy is especially evident whenever numerical data is actively used or applied in a particular context for specific purposes when it may have been originally compiled in other contexts for other purposes. The remaining part of 3.5 constitutes an introductory vignette of some far-reaching

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7 Ibid., my emphasis.



implications of this kind of uncertainty involved in the process of designing and applying numerical data. Subsequent parts of III(B) will begin a more detailed study of the uncertain correspondence between practical procedures of data compilation and abstract concepts with specific reference to Machlup's methodological contributions and with reference to the measurement of some economic concepts.

### Implications

(1) The uncertain 'gap' between high-level abstract concepts in theoretical economics and their numerical 'counterparts' (if any) in more empirical work, has an implication for the potential contribution that quantification per se could make to the development of 'pure' formal economic theory and to economic policy. Two founders of quite different measurement traditions in economics may be consulted on this question. Alfred Marshall, the leader of the Cambridge economics measurement tradition marked by the works of Pigou, Clark, Meade and Stone (the last two building on Keynes' theory), strove to emphasise in his Principles the need to collect statistics relevant to "public interests".<sup>8</sup> Marshall warned, however, that quantification "must never be separated for an instant" from qualitative analysis and that statistical work "must be kept subordinate" to the formation of concepts.<sup>9</sup> While he thought

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8 Marshall, A., Principles of Economics, 8th edn., London, Macmillan, 1920, p.492.

9 In a letter to A.L. Bowley in 1906, in Pigou, A.C., Memorials of Alfred Marshall [1925], N.Y., AMK Reprints of Economic Classics, 1966, pp.428-9.

numerical data may be "very misleading when applied to new problems", he added that "many of the worst fallacies involved in the misapplication of statistics are definite and can be exposed" in the process of application.<sup>10</sup> He counselled caution, for instance, in collecting and interpreting labour statistics. It was advisable to collect and apply these statistics slowly so as to allow time to resolve conceptual problems, or as he wrote, to allow "disputed points of principle to be settled". Settlement in the realm of principle could not take place solely in a technical, specialist environment; statistics for use in public policy decisions had to be "tried in open court" and needed to be accessible to public scrutiny. Marshall suggested publishing labour statistics of all kinds in local newspapers. He saw the need, ideally, to integrate the design and application of statistics with community-wide perception and understanding of their meaning and significance.<sup>11</sup> In another measurement tradition, Wesley Mitchell, influenced by Veblen's institutionalism and its emphasis on economic change, set out to use statistics in order to account for the business cycle. The National Bureau of Economic Research was set up under Mitchell's influence in 1920 and has been concerned with business cycle measurement to the present day. In spite of his immersion in the numerical data which he used to relate to business cycles, Mitchell saw "slight prospect" that quantitative work would ever be able to solve the

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10 Marshall, Principles, op.cit.

11 In Pigou, Memorials, op.cit., p.372.

problems that qualitative discussion of the economic process was continuously suggesting.<sup>12</sup>

(2) Fundamental problems for economists in a policy-advice context are posed by the indeterminate correspondence between numerical constructs and their higher-level, conceptual referents. As Coddington argued in a discussion on economists and public policy: "the statistics by means of which the state of the economy is assessed are ... uncertain to a degree which is itself uncertain".<sup>13</sup> Coddington attributed this uncertainty not simply to sources of unreliability in terms of recording error in statistical design, but more crucially to "the fuzziness of economic concepts" which the numerical data is used to stand for. With reference to experience in Great Britain he noted how, in an unemployment survey during 1967,

a drop in unemployment of 468 persons was recorded and widely reported and commented upon, although at the same time, it could seriously be argued that the unemployment figures underestimated the situation by something of the order of a million people because of various forms of 'disguised unemployment'. Of course, once the figures are 'seasonally adjusted' they no longer refer to a number of actual persons anyway. As another example, the 'uncooking' or 'recooking' of balance of payments statistics in the summer of 1969 [resulted in a] ... reworking of the figures [which] managed all but to abolish a recorded deficit of some £500 million per year.<sup>14</sup>

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12 Mitchell, W.C., 'Quantitative Analysis in Economic Theory' [1925], in idem., The Backward Art of Spending Money and Other Essays, N.Y., AMK Reprints of Economic Classics, 1937, p.25. The possible influence of Veblen's methodology on Mitchell's conclusion should not pass without mention here. Veblen's analysis of the economic process was embedded in a system which "embraced all aspects of the cultural environment irrespective of their capacity for quantification"; Coats, A.W. 'The Influence of Veblen's Methodology', JPE, 62(6), 1954, p.536.

13 Coddington, A., 'Economists and Policy', NWBQR, February 1973, p.67.

14 Ibid., p.60 and pp.61-2.

In both the examples cited in this quotation, numerical data was allegedly used as proxies for 'unemployment' and 'balance of payments' - uses which Coddington seriously reprovved. Did the figures really show or measure what they purported to or did they border on the ambiguous? What did they mean? To what extent did their meaning overlap with the concepts they were used to represent? Differing answers to these questions imply divergent judgements and interpretations. This is to say that the correspondence between measure and concept is uncertain or not fully known. The stand-in role of numerical data for a concept may require a great deal of faith on the part of the user - the more so when concepts are unclear. As Morgenstern pointed out, the degree of uncertainty in this particular sense, is a function of the "coarse structure" of theories and concepts that the quantified variables are supposed to correspond with.<sup>15</sup> Lack of clarity in underlying abstract concepts can render any great precision in the use of numerical constructs (e.g., to three or more decimal places) unwarranted and inappropriate. A threshold can be reached where the pursuit of ever-increasing numerical accuracy may lead to trade-offs resulting in reduced relevance once the precision of corresponding concepts is considered.<sup>16</sup> (The processes by which the statistics are compiled may also be so imperfect as to make excessive numerical precision quite spurious). Marshall was well aware of this

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15 Morgenstern, On the Accuracy, op.cit., 1963, p.95.

16 On this point see, Coddington, A., 'Positive Economics', CJE, 5(1), 1972, pp.11-12.

issue as early as 1901 as evidenced by his remarks in a debate with the economic statistician, A.L. Bowley: "Surely the thing to do is to build the basis of our economic structure soundly and not to put a varnish of mathematical accuracy to many places of decimals on results the premises of which are not established within twenty or fifty per cent".<sup>17</sup> The formal argument is the same today despite the widespread availability of computer facilities and outpouring of official statistics. The precision of quantities does not always correspond to an equally precise and refined background of concepts within which the numbers are used to stand-in as a foreground. This implies uncertainty as to what is actually being measured.

(3) Correspondence between concepts and their empirically well-delineated, sometimes sampled 'counterparts' is likely to remain uncertain and proximate because of time and place variations as well as policy action or inaction. These variations leave room for personal judgement and can require constant reclarification of concepts because they give concepts - hence existing numerical data - different meanings. Instructive on this issue is, first of all, literature on the history of economic thought. Tucker's study of the concept of profits on capital in British thought detailed how the very content of the term profit changed frequently. These changes were often reflected in changing social and economic institutions. One of his comments is worth repeating here:

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17 In Pigou, Memorials, op.cit., p.423.

A particular discussion may appear at first sight to persist more or less continuously over a long period of time; whereas in fact beneath the mere verbal similarities there may have emerged new meanings which alter the whole nature of the point in question.<sup>18</sup>

Other contributions have identified particular historically unique features of the problems which changes in economic history set for economists to solve that are of overriding importance in bringing about 'revolutions' and 'progress' in economic knowledge. Hutchison claimed that "historical changes in the social-economic environment, or in institutions, change the content of central concepts such as 'money', 'markets' and 'unemployment'". He then used these changes to explain the decline and fall of English classical political economy and the Jevonian revolution as well as both the genesis and decline of the Keynesian revolution.<sup>19</sup> The design and use of numerical data in a world where the content of concepts is always changing can lead to uncertainties of 'fit' between measure(s) and concept(s).

An example might be useful at this point. Norton has shown in a policy-advice context the difficulties involved in measuring "the outcome of the balance of payments" (concept) by way of two series of observations: net monetary movements and

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18 Tucker, G.L.S., Progress and Profits in British Economic Thought 1650-1850, Cambridge, Univ. Press, 1960, p.74.

19 Hutchison, T.W., On Revolutions and Progress in Economic Knowledge, Cambridge, Univ. Press, 1978, p.300.

exchange rates over a specified period of time.<sup>20</sup> Consider the following table Norton compiled from official statistics:

TABLE 3.1

Measuring the Outcome of the Balance of Payments

Please see print copy for image



Notes: n.a. not available.

a After 1970-71, this series excludes changes in the net foreign currency assets of the Australian trading banks.

Source: Norton, ibid., p.111, Table 5.

During the 1960's net monetary movements may have sufficed as a measure of balance of payments outcomes since exchange rates were inflexible. In the 1970's, though, exchange rates moved frequently. This means that the balance of payments must now be assessed on the basis of numerical observations of both price and quantity. If the exchange rate and net monetary movement are used in this respect they are sometimes consistent with each other in showing 'outcome' but weights might have to be applied

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20 Norton, W.E., 'Information Problems and Economic Advising', AJS, 22(2), 1980, p.110.

to each series to determine the direction of the outcome. At other times, as shown by the annual data in Table 3.1 for 1971-72 and 1973-74 the two series point in contrary directions. Weighting problems are more complex in these cases. This uncertainty as to what the data is showing is compounded if the observations are taken (say) on a quarterly basis where additional complications as to direction might be found. More indeterminacies in interpretation arise ad infinitum if other series are introduced to fill out the balance of payments picture. For example, variations in official overseas borrowings might be needed to give an interpretation, in turn, of the outcomes of the net monetary movement or the exchange rate. More subtle difficulties with weighting can arise if a whole cluster of numerical observations are used as proxies for a concept that is regarded as a vector - (and not merely a scalar) -requiring measurement of both magnitude and direction (e.g., welfare, poverty, etc.). In these cases, magnitude is one aspect of the process of quantification as is the direction that the concept's corresponding quantities announce. In any case, the point here is that time variations and perhaps policy inaction as well as changes in international trade patterns and institutions have led to a situation where exchange rates are changing. In the 1960's the net monetary movement could be used as a guide, more or less on its own, to the outcome of the balance of payments. Now, at least for some policy-advisers, net monetary movements must share the stage with the exchange rate.<sup>21</sup> The extent of difficulties in interpretation partly

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21 For other examples see Norton, ibid., pp.110-12.



results from the absence of a fully-developed theory relating the numerical information to their conceptual referents under changing conditions.

(4) Another implication of the theme being outlined here concerns the use of numerical data in conjunction with econometric techniques. The use of statistics to facilitate further measurement with these techniques can yield indefinite, ambiguous results if the investigator uses different numerical constructs that purport to measure the 'same' thing. To cite one instance, in a time series analysis Taubman used three different official sets of 'savings' estimates to see if they yielded similar results when incorporated in regressions. As it turned out, the character of the savings function judging by the size and significance of the coefficients depended crucially on the choice of the savings series. He found no way of determining which series was 'best' in any scientific sense.<sup>22</sup> Variables in econometric models may well have several numerical counterparts each deficient in some way, deviating from the theoretical terms in the equations for which the numbers can only be an approximate analogue. An econometrician cannot get exactly what he wants so that selections, adjustments and personal judgements are usually the rule rather than the exception. The availability of numerical data may not be a good reason for regarding econometric work as virtuous because it can

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22 Taubman, P., 'Personal Saving: A Time Series Analysis of Three Measures of the Same Conceptual Series', RES, 50, 1968, pp.125-29.

still yield ambiguous results. Furthermore, the application of econometric methods can presuppose that variables are measured accurately and are without error in a technical (counting rule) sense. 'Disturbances' usually enter into the equations and not into the quantified variables. To be fair, it is often noted that statistical measures are faulty or unreliable. In a radically different sense, it is not often understood that some of the familiar numerical or empirical 'counterparts' (proxies) used are vague inasmuch as they carry different meanings in different contexts. For instance, investment is a concept which may relate to expenditure designed to increase money demand; or to the output of certain types of goods, or to all changes increasing resource productivity.<sup>23</sup>

(5) If in the beginning numerical data is unreliable, faulty or subject to radical revisions over time, the problems already sketched here could seem to pale into insignificance. With reference to public policy decisions, one of the key determinants of the value of numerical data for policy guidance is the length of the lag due to administrative processing between data collection, compilation and eventual data publication. More significantly perhaps are recent findings that the extent and pattern of official revision of major series such as Real Non-Farm Gross Domestic Product in Australia was such that they have "severe implications for the informing of policy decisions and their repercussions for economic activity

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23 See Streissler, E., Pitfalls in Econometric Forecasting, London, IEA, 1970, p.19, passim and Stewart, Reasoning and Method in Economics, op.cit., p.184f.

as well as calculation on productivity and employment possibilities".<sup>24</sup> Hogan's results, in summary form, are shown in Table 3.2.

Table 3.2

Rate of Growth of Real Non-Farm Gross Domestic Product  
in Australia (%)

	Initial Estimates (1)	Estimates Provided 1 Year Later (2)	Estimates Provided 2 Years Later (3)	Latest Available Estimates (4)
1969-70	6.85	6.85a	6.99	7.19
1970-71	4.41	4.53	4.46	4.83
1971-72	2.87	2.89	3.71	3.95
1972-73	5.22	5.58	6.42	6.29
1973-74	5.46	5.69	5.95	5.94
1974-75	-2.62	-1.17	-0.15	0.14c
1975-76	0.82	1.96	2.43b	2.41b
1976-77	3.53	4.23b	-	3.85b
1977-78	1.80b	-	-	1.86b

Note: a Constant price estimates were published initially for June quarter 1971 on a 1966-67 base. Hence the estimates for 1970-71 are listed as the same in columns 1 and 2.

b Based upon the revised 1974-75 constant price basis.

c Constant 1974-75 price series appears to show a rise of 1.04 per cent; linking problems between the old and new series suggest a need for caution when interpreting that calculation.

Source: Hogan, *ibid.*, p.385, Table 1.

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24 Hogan, W.P., 'Quicksands of Policymaking', *AEP*, 18(2), 1979, pp.385-6.

The 'initial estimates' are annual series disclosed after the June quarter estimates are published. These initial measures do not succeed in measuring the real annual economic gains. From Table 3.2 a general upward revision of estimates for the seventies is evident.<sup>25</sup> Apart from realising that the foundations of quantification for economic aggregates such as GDP are based on shifting sands, Hogan commented that these revisions, among others,

point to worries about the basis for entering judgements on measures appropriate to economic circumstances. The application of economic policies has to be taken in the context of the time as it is then perceived. What is clear is the uncertainty as to what has taken place in the economy during immediate past quarters and the even greater uncertainty as to what the position is at the time critical policy measures are being explored and then determined. All these doubts bear on monetary and wages issues as much as fiscal matters ... [G]overnment policies [may have been] subject to continuous review [but] the basis of interpretation is so flimsy.<sup>26</sup>

While having important consequences, the kind of 'uncertainty' and error associated with the official publication and revision of numerical data has one point in common with error associated with faulty collation and collection procedures. Both have an affect on data quality and in many cases some quantitative estimates of the patterns of variation in data revision and error can be worked-out. This means that this kind of 'uncertainty' and error is potentially calculable. Data

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25 On these revisions, see also Gruen, F.H., 'The Statistician's Revision of Gross Non-Farm Product', *ibid.*, pp.384-96.

26 Hogan, *op.cit.*, 1979, p.389.

revisions result from a lack of fit between numerical data and counting rule over time. In contrast, data error results from a lack of fit between numerical data and counting rule at one point in time, or perhaps more precisely, over the period of sampling and initial data compilation.

The kind of 'uncertainty' and imprecision associated with numerical observations that is central to this work is of a completely different order to those discussed in the last paragraph. It is not calculable. It is not an observation error but rather an 'error' associated with the use or application of data. It cannot be expressed in numerical form. It arises from an indeterminate correspondence between numerical data and abstract concept that a statistic is used to refer to. The idea that uncertainty is not reducible to quantification corresponds with Frank Knight's distinction between risk and uncertainty:

Uncertainty must be taken in a sense radically distinct from the familiar notion of risk ... It will appear that a measurable uncertainty, or "risk" proper, ... is so far different from an unmeasurable one that it is not in effect an uncertainty at all. We shall restrict the term "uncertainty" to cases of the non-quantitative type.<sup>27</sup>

This kind of uncertainty can be ascribed to the divergence between a statistic and its active use for a purpose - as a proxy for a concept. It forms the whole basis of the present task which is to follow through the methodological consequences

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27 Knight, F.H., Risk, Uncertainty and Profit [1921], Reprinted Chicago, Chicago, University Press, 1971, pp.19-20, my emphasis.

of that different order of uncertainty inherent in the process of designing and using numerical information not susceptible to measurement and hence to elimination. In other words, problems with data quality and compilation are reducible but uncertainty in this sense may be ineradicable regardless of the level of quantitative precision; no matter how much numerical data is collected and irrespective of the improvements that might be made in data quality and econometric techniques. A well-organised official statistical service will not eliminate the inherent imprecisions involved in interpreting numerical data. This data is part of what might be called the 'empirical' domain. When numerical data becomes an 'empirical' finding (implying interpretation or application in a particular context) it appears that as such it can never be either certainly true nor certainly untrue. This comes about "because of two classes of causes only one of which ... [is measurable] in terms of probabilities". The other component may be termed uncertainty.<sup>28</sup> My main concern in the expositions in following sections of this study is to explore and evaluate the significance of the particular kind of uncertainty, already outlined, which is involved in the process of designing and interpreting numerical data in the context of economic analysis.

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28 Stewart, Reasoning and Method in Economics, op.cit., p.62; my emphasis. Stewart adds: "given that empirical findings can never be either certainly true nor certainly untrue, what can they be? In trying to sort this question out ... we enter an area that has not been all that well explored ... in economics".

### 3.6 DESIGNING NUMERICAL CONSTRUCTS: FIAT MEASUREMENT AND THE ROLE OF VALUATION

Formally viewed, measurement is the assignment of numbers to the properties of objects or events in such a way that a one-to-one correspondence is maintained between the relations among the properties measured and the characteristics of the numbers assigned. Most forms of measurement, interpreted in the broadest sense of the term, involve the assignment of numbers to phenomena according to rules of scaling. This is the sense in which measurement is used in the following discussion.<sup>29</sup> Forms of measurement may be differentiated by the type of scale (nominal, ordinal, cardinal, ratio, etc.), and by the type of link between the scale and the concept measured. In most of the social sciences including economics, measurement by fiat or measurement by conventional definition is predominantly employed.<sup>30</sup> It is a form of indirect measurement, i.e.,

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29 The basic references for the following discussion on fiat measurement are, in order of importance, Torgerson, W.S., Theory and Methods of Scaling, N.Y., Wiley, 1958; Stevens, S.S., 'On the Theory of Scales of Measurement', Science, 103(2684), 1946, pp.677-80; Nagel, E., 'Measurement', in Danto, A. and Morgenbesser, S., eds., Philosophy of Science, N.Y., World Publishing Co., 1960, pp.121-40; Gorden, R.L., Unidimensional Scaling of Social Variables: Concepts and Procedures, London, Macmillan, 1977; and Guénou, R., The Reign of Quantity and the Signs of the Times, London, Luzac and Co., 1953.

30 Another form of measurement sometimes called direct (fundamental) measurement is limited to measuring weight and length. It is based exclusively on principles relating different amounts of a single property to each other without involving other properties. In scaling length for instance, the relationship between different lengths can be shown to be approximate to a cardinal scale by superimposition and combination of objects of various lengths. See, Stevens, op.cit., 1946, p.680 and Nagel, op.cit., 1960, p.136.

measurement by fiat ... ordinarily ... depends on presumed relationships between observations and the concept of interest. Included in this category are indices ... of the social and behavioural sciences. This sort of measurement is likely to occur whenever we have a ... concept that on a priori grounds seems to be important but which we do not know how to measure directly.<sup>31</sup>

Since many economic concepts are complex and multidimensional, for example, income, capital, employment and so on, it is necessary to specify rules or criteria for quantifying these phenomena. These rules are based on presumed but by no means certain relationships to the respective concept of interest. Consider as an example, A.C. Pigou's concept of interest: "economic welfare".<sup>32</sup> Pigou limited his thought to the measurement of economic welfare by defining it in pecuniary terms. This recourse, while it may have been pragmatically the best course open to him, alludes to a fundamental problem of fiat measurement - its accompanying arbitrariness. Pigou developed a theory of "national dividend" so as to help provide a definition for measurement. His monistic, pecuniary

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31 Torgerson, Theory, op.cit., p.22, my emphasis. See also Marschak, J., 'Probability in the Social Sciences', in Lazarsfeld, P.F., ed., Mathematical Thinking in the Social Sciences, Glencoe Ill., Free Press, 1954, especially pp.198-9. Marschak points out that numerical averages and frequency ratios are fiat measures inasmuch as they are not immediate data of experience but are an indirectly apprehended "summary" of the data of perception based on "a priori information".

32 Pigou, A.C., The Economics of Welfare, 4th edn., London, Macmillan, 1932.



formulation was justified theoretically on the assumption that 'economic' factors were properly defined as being concerned with those goods and services that come within the purview of "the measuring rod of money". This was a convenient definition because it defined a quantity that was in practice measurable but it was not necessarily the only possible definition. Pigou undoubtedly recognised this when he commented that his counting definition drew "arbitrary lines in nature".<sup>33</sup> In theory the national dividend was an objective monetary counterpart of the subjective "state of consciousness" called economic welfare. Economic welfare was "a balance of satisfactions" (utility) derived from the use of national dividend over the disutility involved in producing it. Assuming a "stable general culture", which implied that any microeconomic effects of alterations in market conditions, industries, taxes, or subsidies would have a negligible overall impact on "other aspects of welfare" the national dividend would have a reasonably close correspondence to economic welfare. If the impact was not negligible then the effects on other aspects of welfare could feed back and change states of consciousness about economic welfare. The ceteris paribus clause, viz., the stability of culture, ensured that changes in the money measure labelled national dividend adequately represented changes in economic welfare. With frozen states of consciousness, or stable culture, it could then be deduced that for any given change in the national dividend social perception of that change would always remain roughly the

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33 Ibid., p.31.

same. If not, the reliability of the fiat measure as a counterpart of economic welfare could be impugned. This means that Pigou assumed that correspondence between national dividend and economic welfare was stable over time. In practice, to adopt a wider counting definition of national dividend as a measure of economic welfare would have been "tantamount to abandoning dependence on the measuring rod of money" and no other feasible counting unit appeared available at the time.<sup>34</sup>

Apart from quantification on the basis of a formal economic theory, measurement by fiat also commonly involves the collection of raw numerical data by administrative fiat and by data collection rules laid down by official statistical agencies. For instance, a count of those people unemployed is a product of applying practical counting and coding rules to a population so as to identify and classify the unemployed. By starting with a fairly open idea - 'unemployed' - we find that it is defined and closed by a choice of counting rule made by the agency that produces the statistics. The rules might vary according to the administrative functions of the counting agency, according to practical data gathering constraints (financial resources, etc.) and will depend on prevailing theory about the condition called unemployment.

In economics, numerical data, interpreted in a broad manner, are measures. Counting always begins with a nominal classification involving a judgement of class inclusion or exclusion, for example, employed/unemployed; measurable in

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<sup>34</sup> Ibid., p.21 and p.33.

money terms/not measurable in money terms; male/female and so on. Thenceforth, scaling is applied in order to obtain some kind of summary measure. Scaling, including nominal scaling, cannot be universally correct or Wertfrei since weighting schemes are dependent on the point of view from which they are chosen. This is to say that all weighting schemes used in economic measurement are valuations. This can be elaborated by reconsidering Pigou's counting definition for the national dividend. His application of the money measuring rod assumed logical equivalence relationships inasmuch as each additional (say) one dollar of national dividend was equal to every other at any point in time. In other words, every additional observation or sample of that value was equally representative of one dollar's worth of dividend. All elements that made up national dividend and added (say) one dollar's worth of money value could be assumed identical for counting purposes. Money provided a homological system of weights for aggregation. This implies that for national dividend measurement an equivalence class would be construed by treating all personal incomes of (say) one dollar as identical for counting purposes - no matter whose dollar it was. The choice of counting unit, or what amounts to the same thing in this case, the choice of market valuation, makes everyone's valuations or preferences congruent.

Creation of nominal scales in which phenomena are classed as logically equivalent or homogeneous is a value-loaded exercise. Phenomena are reduced to their modes of resemblances or like characteristics. For example, people labelled unemployed can be separated from those employed. This reduction

or classing makes counting meaningful. The determination of homogeneity is more of an art than a 'science' because an observer who discerns relatively unvarying modes of resemblance or degrees of likeness is not observing a physical 'fact' outside of himself. Homogeneity is a value judgement and is relative to the scope of observation.<sup>35</sup> In other words 'quantity' is not inherent in the things quantified but is an outcome of the choice of operational rules associated with quantification. A brief digression into philosophy of science may clarify this. According to Popper in his Logic of Scientific Discovery, numerical constructs, among other things, must rely on "the doctrine of the primacy of repetitions". This doctrine posits that repeated instances furnish "a kind of justification" for the acceptance of things such as specific counting rules based originally on nominal scales. At the very least, says Popper, repetitions may "arouse expectations and beliefs" in the observer about the 'facts'.<sup>36</sup> In criticising this doctrine Popper argued that any repetitions an observer might discern are only approximate. By "saying that a repetition is approximate" he means "that the repetition B of an event A is not identical with A, or indistinguishable from A, but only more or less similar to A".<sup>37</sup> Repetition is rooted in

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35 See Emmer, R.E., Economic Analysis and Scientific Philosophy, London, Unwin, 1967, p.187ff.

36 Popper, K.R., The Logic of Scientific Discovery, 3rd edn., London, Hutchison, 1972, p.420. Popper also links the idea of repetition with concepts of probability.

37 Ibid.

similarity which is inherently relative to time, place and mind of the observer. Consider the following diagrams:

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Source: Popper, ibid., Appendix \*X, p.421.

From these diagrams there seem to be many possible kinds of similarity. Some diagrams are similar and may be classed and counted with respect to shading or its absence, others with respect to shape, size, etc. Popper concluded that "similarity and with it repetition always presuppose the adoption of a

point of view ... interest or an expectation [which are] logically prior to, as well as temporally (or causally, or psychologically) prior to repetition". This means that 'observables' can be counted in a particular way if only the appropriate point of view is adopted as to relevant repetitions.<sup>38</sup> It also shows how naive it is to look upon numerical data designed from 'observations' as something ultimate or given. In another contribution Popper contended that "all observation involves interpretation in the light of our theoretical knowledge, ... [and] that pure observational knowledge, unadulterated by theory, would, if at all possible, be utterly barren and futile".<sup>39</sup>

That the choice of a counting rule involves interpretation and valuation can be illustrated in many other ways as well. Consider, as just one instance, an index number problem relating to real income measurement. Suppose, after prices and quantities have changed, that the application of first and second period price indices as weights has measured the resulting change in income over a particular period. Assume also that the resulting measure is unequivocal insofar as it satisfied the test:  $\sum p_1 q_2 > \sum p_1 q_1$  and  $\sum p_2 q_2 > \sum p_2 q_1$ . Now, the

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38 Ibid., p.420 and p.422. Popper declined to define the term 'observation': "it should be introduced as an undefined term which becomes sufficiently precise in use" (p.103). Hereafter 'observation' will be used to mean any activity that involves visually attending to aspects or features of a phenomenon. For various uses of the term see Achinstein, P., Concepts of Science: A Philosophical Introduction, Baltimore, John Hopkins Press, 1968, pp.168-78.

39 Popper, K.R., Conjectures and Refutations, 3rd edn., London, RKP, 1969, p.23.

altered total will provide no information about the changing and changeable, structural and functional aspects of total real income. For example, suppose it was asked: 'why has income changed?'. In part, an answer to this question will need to draw on a theory such as one which relates changes in real income to other phenomena - perhaps distribution. At the societal level or the level of general economic equilibrium, the size of real income and distribution are interrelated and are determined simultaneously. Income distribution can affect prices and vice-versa. Therefore, implicit in the structure of relative prices as used in this sort of index number calculation is the changing and changeable distribution of income. Use of the measure calculated in terms of market prices without qualification, is tantamount to accepting the distribution of income that helped in the design of the calculation. This gives rise to an awkward question: real income has unambiguously increased according to the counting rule but to whose benefit or detriment? To measure real income in terms of prevailing prices (especially without qualifications about, say, compensation payments to losers) is to accept an implicit value judgement to the effect that the subjective valuations by the beneficiaries of a change should have primacy over the subjective valuations by the losers.<sup>40</sup> Further, to be 'universally' valid the yardstick or unit of aggregation must be independent of income distribution otherwise it implies tacit interpersonal comparisons,

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40 For a summary treatment of this issue see, e.g., Nath, S.K., A Perspective of Welfare Economics, London, Macmillan, 1973, pp.57-69.

or 'normative' premises. In the Political Element in the Development of Economic Theory Myrdal stated trenchantly that:

A social unit of value which measures what is relevant from any conceivable set of political valuations ... is impossible in principle, not just because of statistical difficulties.

He hastened to add that:

Definitions of the unit of measurement are obscure when they are given at all: e.g., 'valuation by the free market', 'social valuation' ... [And with regard to the question of index numbers:] There is no such thing as an ideal index which is theoretically correct, independent of the point of view from which the weights are determined.<sup>41</sup>

So much, it seems, for impartial trial by fiat measurement. As Myrdal was patently aware, the objectivity of economic 'science' is challenged if the idea that numerical data are theory dependent or value-loaded is taken too far.

In addition, statistical techniques that proceed to manipulate numerical data designed by fiat do not guarantee the objectivity of a researcher's conclusions. Statistical techniques do not obviate the necessity for someone to decide on a priori significance levels, or on confidence intervals when using methods of estimation. Atkins linked the a priori element with uncertainty:

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41 Myrdal, G., The Political Element in the Development of Economic Theory, London, RKP, 1953, p.132, my emphasis. See also, e.g., Dobb, M., Welfare Economics and the Economics of Socialism, Cambridge, CUP, 1969, Ch's 3 and 6.



By its very nature statistical inference is uncertain inference, concerned with probability and not certainty. Instead of recognising this uncertainty however, an arbitrary significance level is chosen [by a social scientist] to make a rigid cut-off point beyond which data are regarded as inconsistent with the null hypothesis.<sup>42</sup>

Popper also noted how scientists are often inclined to "turn to probability as an 'Ersatz' for certainty".<sup>43</sup> In the assessment of evidence probability theory can be employed to help 'test' a hypothesis. The problem is that numerical data, based on the observation of particular cases, are never absolutely consistent with the hypotheses couched in more universal theoretical terms. The probability calculation in itself may be objective but there is no objective decision rule which governs how it is to be used in determining the 'outcome' or 'results' of a 'testing' process. As well, the statistician's Type I and Type II errors may be calculated. The decision rules actually employed will be arbitrary or based on convention but this does not mean that they carry a warrant that is rooted 'nature'. The problem of assessing and deciding on the numerical evidence, that is, which error the decisionmaker should expose himself to, cannot be separated from the differential, uncertain consequences which might ensue if the hypothesis is accepted or rejected. The

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42 Atkins, L., 'The Significance of "Significance Tests"', in Irvine, J., et.al., eds., Demystifying Social Statistics, London, Pluto Press, 1979, p.99. See also Gordon, S., 'Social Science and Value Judgements', CJE, 10(4), 1977, especially p.536ff.

43 Popper, Conjectures, op.cit., p.58.

magnitude of the Type I and Type II errors may be known but the consequences of a decision may not be fully known - instead, they may be intangible and uncertain. Frank Knight found, in particular, that business and policy decisions

deal with situations which are far too unique generally speaking, for any sort of statistical tabulation to have any value for guidance. The conception of an objectively measurable probability or chance is simply inapplicable ... [I]t appears to be meaningless and fatally misleading to speak of the probability, in an objective sense, that a judgement or decision is correct.<sup>44</sup>

Furthermore, in considering future consequences it has to be understood that any high degree of confidence that numerical data, representing frequencies found in the past, will hold as regularities in the future is still based on an a priori judgement of indeterminateness. The numerical observations do not act as evidence for conclusions or decisions based on a proposition of regularity; they do not give any grounds for reasoning to a proposition of regularity.

### 3.7 OPERATIONALISM IN ECONOMICS: TOWARD A REFORMULATION

#### 3.7(A) Bridgman's Operationalism in Physics

Measurement by fiat involves establishing pragmatic operational criteria for scaling actual observations with respect to abstract, sometimes multidimensional, concepts. (e.g., Pigou's 'economic welfare'). Fiat measurement procedures close-off these high-level abstract concepts in terms of a

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44 Knight, Risk, Uncertainty, op.cit., p.231.

specific counting definition (e.g., Pigou's 'national dividend'). The rules of counting and aggregation could be arbitrary or ad hoc and this, for Torgerson, was problematic:

The major difficulty with measurement by fiat is the tremendous number of ways in which such defined scales can be constructed ... Although ... investigation may establish that many lead to virtually the same result and hence may be considered to be equivalent operational definitions of the same concept many will also lead to quite different results in which case they are operational definitions of different concepts.<sup>45</sup>

The operational definitions that are used for particular purposes often become accepted by convention but convention rarely provides certain, unambiguous links between counting definition and the underlying concept of interest when the numerical data is actively used and interpreted. Thus, the whole notion of an operational definition deserves further discussion.

The term 'operationalism' was the outgrowth of a special form of 'positivism' or 'empiricism' first articulated by the experimental physicist P.W. Bridgman. It is stated originally in The Logic of Modern Physics (1927) and revised in his later contributions.<sup>46</sup> Bridgman's point of departure is an explicit statement admitting the influence of Einstein on his own thinking about useful as opposed nonsense concepts in physics.

45 Torgerson, Theory, op.cit., pp.24-5.

46 The following discussion draws on three of Bridgman's contributions, The Logic of Modern Physics, London, Macmillan, 1927; idem., The Nature of Physical Theory, N.Y., Dover Publications, 1936; and idem., Reflections of a Physicist, N.Y., Philosophical Library, 1955.

Einstein's theory of relativity contained an implicit proposal for developing a new type of concept - that based on physical operations which an observer performs in order to verify the existence of an object and in some cases, to measure its magnitude. Bridgman begins a polemic against the 'purely' abstract science of physics which is "as far removed from reality as the abstract geometry of the mathematicians, built on postulates". Instead, concepts of physics had to have an operational character. For example, Bridgman proposes an operational concept for 'length':

The concept of length is ... fixed when the operations by which length is measured are fixed: that is, the concept of length involves as much as and nothing more than the set of operations by which length is determined. In general, we mean by any concept nothing more than a set of operations; the concept is synonymous with the corresponding set of operations ... If we have more than one set of operations we have more than one concept, and strictly there should be a separate name to correspond to each set of operations.<sup>47</sup>

Bridgman demands that the set of operations 'equivalent' to any concept be a unique set, otherwise uncertainties may creep in when the measure is used. To safeguard what he understands as the objectivity of science, all operations are required to be founded on intersubjective agreement in the sense that different observers or measurers must be able to perform the "same" operation with "reasonable agreement" in their results.<sup>48</sup>

Bridgman distinguishes several kinds of operation that may be

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47 Bridgman, op.cit., 1927, p.5, my emphasis, and p.10.

48 Bridgman, op.cit., 1936, p.10, and 1955, p. 6 and p.22.

invoked in specifying the meaning of scientific terms. The principal ones are (1) instrumental (alternatively, experimental or physical) operations - as in the measurement of length these consist in the use of various devices of observation and measurement, and (2) mental operations, verbal operations and "paper-and-pencil" operations - including, among other things, techniques of mathematical and logical inference as well as experiments conducted in the scientist's imagination. There are many mental constructs and models in physics which are made "to enable us to deal with physical situations which we cannot directly experience through our senses but which we have contact indirectly and by inference". Bridgman admits that such constructs usually involve an element of imagination. While mental constructs are "useful and unavoidable", he warns of their great dangers if they do not ultimately make reference to some physical or instrumental operation.<sup>49</sup>

Operationalism may also include directions and rules for 'finding' an object. For example,

no one ever directly experienced an atom, and its existence is entirely inferential. The atom was invented to explain constant combining weights in chemistry. For a long time there was no other experimental evidence of its existence, and it remained a pure invention, without physical reality, useful in discussing a certain group of phenomena. It is one of the most fascinating things in physics to trace the accumulation of independent new physical information all pointing to the atom, until now we are as convinced of its physical reality as of our hands and feet.<sup>50</sup>

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49 Bridgman, op.cit., 1927, p. 53 and pp.55-60.

50 Bridgman, The Logic, ibid., p.59.

In practice, most physicists employ a combination of physical and mental operations (the latter not defined by 'purely' physical operations). Most of these mental operations are confined to mathematics and rules of logic, or probability concepts which Bridgman terms "intermediate constructs". They are at one remove from the ultimate aims of theory that (a) it engender conceivable and practically possible instrumental operations and that (b) it be "expressible in terms of operations applicable in the concrete physical situation". Operationalism becomes, for Bridgman, a criterion of meaning. The constructs of physics are "good" if "there is a unique correspondence between it and the physical data in terms of which it is defined". Further, if "a special question has meaning, it must be possible to find operations by which an answer may be given to it". Unless the scientist knows the operations, then the meaning of a scientific term cannot be understood - the term is in fact meaningless.<sup>51</sup>

So convinced of the usefulness of the operational technique for distinguishing science from metaphysics or sense from nonsense, Bridgman is sure that "no less important results" are to be obtained by its application to "social questions". For, "we nearly always have preconceptions as to what our social concepts are good for and the character of the underlying operations, and we use the concepts subject to those preconceptions whereas analysis will often disclose that the operations which build up

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51 Bridgman, op.cit., 1955, p.16, and 1927, pgs. 59, 28 and 30.

the concept are such that we have no right to our expectations".<sup>52</sup>

### 3.7(B) A First Critique of Operationalism

In a study of the main tenets of operationalism Hempel concludes that "the central ideas of operational analysis ... are so vague that they constitute not a theory concerning the nature of scientific concepts but rather a program for the development of such a theory".<sup>53</sup> Indeed, it is this sort of programme Fritz Machlup embarks upon in making use of Bridgman's ideas and applying them, critically, to economics. His first elaborate analysis of operationalism is contained in his paper on 'Operational Concepts and Mental Constructs in Model and Theory Formation' (1960). The following is a synopsis of Machlup's critical appraisal of Bridgman's contribution. It may be remarked that Machlup only cites Bridgman (1927).<sup>54</sup> The following synopsis is inclusive of insights gained from Bridgman's later contributions as well as Carnap's "partial interpretation" approach to operationalism developed in the

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52 Bridgman, Reflections, op.cit., p.22. C.f., also idem., The Way Things Are, Cambridge Mass., Harvard University Press, 1959, p.251 passim.

53 Hempel, C.G., 'Operationalism, Observation and Theoretical Terms', in Danto, A. and Morgenbesser, C., Philosophy of Science, N.Y., World Publishing Co., 1960, p.103.

54 Machlup, F., 'Operational Concepts and Mental Constructs in Model and Theory Formation', Giornale degli Economisti, New Series, XIX, Sept.-Oct., 1960, pp.555-564. See also Machlup, 'Operationalism and Pure Theory in Economics', in Krupp, S.R., ed., The Structure of Economic Science: Essays in Methodology, New Jersey, Prentice Hall, 1966, pp.53-67.

philosophy of science literature but not explicitly discussed by Machlup. Lastly, the following points are not in any order of importance for this work.

Firstly, Bridgman called for the exclusive use of operationally defined concepts in all the sciences. Instead of stating a definition of a concept in terms of its properties, it had to be defined in terms of actual operations. What a person meant by a term is to be found by observing what is done with it, not by what is said about it. Machlup perceives an "unguarded aside" in Bridgman's division of operations into several types; that is, physical operations and mental operations. This division left openings wide enough to include general thoughts and imagination among "operations" so that, for Machlup, operationalism interpreted in this way, had little substantive content. Having considered this, Machlup sympathetically interprets "mental operations" as those which are supposed to apply only to concepts of pure mathematics and not to concepts in the more empirical sciences such as economics. This interpretation is consistent with two of Bridgman's later works of which Machlup is apparently unaware. Thus for Bridgman in 1936:

We have ... suggested that we may ... construct our concepts in other ways, as is so often done in mathematics, and then experiment with the structure we may erect in terms of such concepts to see whether the concepts are useful [.; these] ... operations are mental operations, and have no necessary physical validity. The use of such concepts may be very suggestive and stimulating.

By 1955, Bridgman was willing to permit mathematical concepts such as probability and the tools of symbolic logic, as "inter-



mediate constructs" enabling the eventual development of operations applicable in "the concrete physical situation".<sup>55</sup> In any case, Machlup finds that for economics, Bridgman's instrumental operations mean all sorts of operations that entail recording of numerical data and documenting qualitative data. In both cases the means of arriving at the recorded data had to be fully specified. Bridgman's 'operations' in the context of economics would refer, for the most part to (1) original observations that are recorded (apparently subsuming definitions and statements about what is to be observed); (2) the manner of recording and (3) what is actually done with the data recorded.<sup>56</sup> It is crucial for the purposes of this work to note, in anticipating one conclusion in the following sections, that movement from (1) through (3) involves increasingly greater admixtures of theory or mental constructs. 'Operation' (3) may be said to eventually merge more fully into the realm of mental reflection.

Synonymity between concept and operation is the second point which Machlup finds debatable. Synonymity seems to make sense for such things as distance and weight. More often, fiat measures based on nominal scales or classificatory groupings, (for example, genus or species) are employed where there is no question of identity or synonymity. The operations (or rules) applied (say) to identify a particular plant as a member of a species cannot become synonymous with the particular plant in

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55 Bridgman, op.cit., 1936, p.11, my emphasis, and 1955, p.16.

56 Machlup, 'Operationalism', op.cit., p.57 n.4.

question. Or, the operations by which economists identify and measure unemployment of human resources are surely not 'identical' with a particular unemployed person. Each unemployed person may differ at the micro-level in unique ways (e.g., by the specific circumstances which brought that person into a state of unemployment) but have sufficient resemblances to be covered by the economists' measure. Bridgman was seriously concerned with ontological issues despite his strong empiricist bent. Machlup seizes on this as an admission of a possible divergence between concept and operation. Bridgman ascribed physical reality to mental constructs such as the 'atom' since it was a "good construct" with close connections to operational bases of inference than some other concepts such as 'calorific fluid' or 'electric field'. (This is admittedly a vague requirement needing more definiteness as to what constitutes sufficient "closeness"). Machlup sees an inconsistency in the purported synonymy between concept and measure as a result of all this: "if the concept is nothing more than the set of operations, the concept cannot possibly at the same time be an object with an independent existence apart from the operations".<sup>57</sup>

Bridgman's proposition that theory formation should make use of predominantly operational concepts is sympathetically and

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57 Machlup, 'Operational Concepts', op.cit., p.561. This might be construed as a 'straw-man' argument given Bridgman's view (based exclusively on examples from physics) that in the final analysis synonymy is a matter of convention. Further, "operations ... are conventions. The number of such possible conventions is infinite"; Bridgman, The Way Things Are, op.cit., 1959, p.47. See also p.44f.

loosely reinterpreted by Machlup. For economics, this proposition should be taken to imply that operational concepts should have primacy and that non-operational concepts be admitted only when no operational substitutes can be found.

A detailed analysis is required of what Machlup calls a "somewhat highhanded" implication of operationalism: that a term could be declared meaningless if it is not expressed in terms of operations. Unfortunately, Bridgman's operationalism overlooks situations where a theoretical system made up of several propositions and terms may contain some directly verifiable propositions and terms, some which need not be directly verifiable, and some which are not composed of, or directly identified by, operational concepts at all. The latter, according to Machlup, are perfectly meaningful (more on this point in 3.7(C) anon). A somewhat intermediate position is represented by Carnap in the philosophy of science literature. Carnap argues that theoretical terms, in particular, are and should be, partially interpreted into an "observation language". Those terms and propositions not directly verifiable had to at least be partially operational (interpreted) in order to be admitted as meaningful.<sup>58</sup> Bridgman's view that, in most cases,

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58 The following discussion draws on Carnap, R., 'Methodological Character of Theoretical Concepts', in H. Feigl, et.al., eds., Minnesota Studies in the Philosophy of Science, Vol.I, Minneapolis, 1956, pp.38-76; idem., 'Intellectual Biography' and 'The Philosopher Replies', in Schilpp, P.A., ed., The Philosophy of Rudolf Carnap, Illinois: La Salle, Open Court, 1963, pp.3-81 and 859-999 respectively; Carnap, 'Theories as Partially Interpreted Formal Systems', in Brody, B., ed., Readings in the Philosophy of Science, N.J., Englewood Cliffs, Prentice Hall, 1970, pp.190-99 and Carnap, 'Testability and Meaning', in Grandy, R.E., Theories and Observations in Science, N.J., Englewood Cliffs, Prentice Hall, 1973, pp.27-46.

concepts are useless unless they can be fully measured or defined operationally is reinterpreted by Carnap along the lines that operational definitions rarely exhaust the whole meaning of abstract concepts even in the physical sciences. Most theoretical systems postulate the existence of some 'latent' hypothetical properties to be used in the explanation of phenomena at the observable level. Carnap's version runs as follows. The vocabulary of a theory is composed of different classes of terms; abstract-theoretical and non-theoretical. Theoretical terms must be connected by the theory to non-theoretical terms (referring to observable data, items and events) if they are to retain meaning or scientific legitimacy and if scientific theory is to be separated from mere metaphysical speculation. Carnap finds that limits must be placed on the introduction of theoretical terms that are not at least partly connected to observed data or in the narrower context of this study, limits might be placed on theoretical terms not at least indirectly measurable by fiat. Carnap concludes that two kinds of concepts are required: (1) those defined in 'purely' observational language and (2) those concepts that refer to unobservable 'latent' hypothetical properties the existence of which is postulated in order to obtain better explanations and predictions at the observable level (e.g., of Bridgman's "atom" or Pigou's "economic welfare"). The concepts designating latent properties must be related to observed data but this relation was more complex than originally presented by Bridgman - who enunciated the view that scientific concepts had to be more or less completely defined by specific sets of measurement operations. Propositions

connecting the language of legitimate theoretical terms and observation language are called "semantical rules of correspondence". These rules are defined as a mixed sentence in which at least one theoretical term and one non-theoretical term occur.<sup>59</sup> These correspondence rules give the meanings of the theoretical terms which figure in them. They may sometimes constitute a partial definition of a theoretical term and/or they may be hypotheses suggested by a theory which connects observable phenomena to unobservable phenomena by at least some indirectly verifiable empirical relations.

Consider, for instance, Pigou's economic welfare which was an unobservable 'subjective state of consciousness' similar to the more or less intuitive concept of 'warmth' in science. Economic welfare was partially defined in terms of a fiat counting definition based on the market value of goods and services (national dividend) (c.f., warmth is replaced by the more precise concept of temperature in physical science). Carnap's requirements are that the new concept be more precise than the old one, that it be more helpful, simple and bear some similarity to the more abstract concept, although "close similarity is not required and considerable differences are permitted".<sup>60</sup> Pigou's concept of economic welfare was connected to national dividend - the money value of goods and services which could be observed and recorded in the market-place. Pigou also developed an elaborate theory stating the conditions, or

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59 Carnap, 'Replies', op.cit., 1963, p.959.

60 Carnap, R., Logical Foundations of Probability, 2nd edn., Chicago, Univ. Press, 1962, p.7.

indirectly verifiable ceteris paribus rules, under which the measure labelled national dividend was linked to the theoretical term economic welfare, (see 3.6 above). In order to maintain the scientific legitimacy of such constructs it is necessary, according to Carnap, that they possess at least some operational referents. Whatever the precise nature of the rules of correspondence, and they are likely to be far more subtle than the example cited here, the importance of Carnap's contribution for this work is that it generates the thesis that: observable properties of objects or events and so forth, seldom exhaust the whole meaning of a concept. In his words, "[t]he interpretation of theoretical terms is always incomplete" because terms have an open-ended character.<sup>61</sup> He asserts, in summary, that to interpret, discuss or reason with the observation terms of a calculus is automatically to only partially interpret the latent, mental constructs that they refer to. Conversely, his requirement is that the mental constructs or theoretical terms must have some observable referents which need not be strict counterparts.

Concepts denoting latent, hypothetical or mentally constructed properties of (say) events are used for designing explanatory hypotheses and theories in close association with observation (operational) terms. Rather than expatiating on this question at a philosophical level, consider as one illustration, W.C. Mitchell's explanation of the event he calls by the abstraction "the business cycle". Mitchell attempts to

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61 Carnap, 'Biography', op.cit., 1963, p.80. See also p.78.

develop an explanation of the cycle without much assistance from any existing formal theory. The oft-quoted measurement without theory debate emerged from Mitchell's measurement programme though Mitchell was not a major contributor to the methodological discussion.<sup>62</sup> Mitchell starts from the position that the cycles which characterise observable economic activity consist of a complex, patterned combination of quasi-mechanical phenomena but the cycle itself was not directly observable. The theoretical term 'business cycle' referred, in turn, at a lower level of theory, to unobservable latent properties in business experience that could be connected to observable phenomena. The term is translated by Mitchell into numerous dimensions through a measurement operation. For measurement and classification of dimensions of the cycle he relies on a collection of an array of statistical series designated by an observation language, for example: physical production data; price indices; bond yields; call money rates; bankruptcies and many others. The subsequent measurement effort, according to Mitchell, is a "descriptive analysis of the cumulative changes by which one set of business conditions transforms itself into another set".<sup>63</sup>

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62 See Koopmans, T.C., 'Measurement Without Theory' [1947] in Beckmann, M. et.al., eds., Scientific Papers of T.C. Koopmans, N.Y., Springer Verlag, 1970, pp.112-131 and Koopmans, 'Reply' [1949] in ibid., pp.148-56. C.f., Vining, R., 'Methodological Issues in Quantitative Economics' [1949] in Beckmann, ibid., pp.132-48 and Vining, 'Rejoinder' [1949], ibid., pp.156-62.

63 Mitchell, W.C., Business Cycles and Their Causes [1913], Berkeley, Univ. of California Press, reprint, 1972, p.ix. See also idem., Business Cycles: The Problem and Its Setting, N.Y., NBER, 1927; Mitchell and Burns, A.F., Measuring Business Cycles, Studies in Business Cycles No.2, N.Y., NBER, 1946 and a posthumous work, Mitchell, W.C., What Happens During Business Cycles: A Progress Report, N.Y., NBER, 1951.

He divides the cycle into phases and finds that each postulated phase, as mapped out by trends in the associated statistical series, had a characteristic development pattern related to trends in preceding phases. In one sense Mitchell employs an a posteriori method of investigation but without a controlled experiment. In the physical sciences a controlled experiment is usually indispensable for a posteriori derivation of propositions whenever (as in this case), the complex 'effect' (business cycle) is produced by several interlinked 'causes'. In another sense, applying Carnap's terminology to Mitchell's work, it may be said that Mitchell uses statistics designed on the basis of everyday observation or business language. He combines this use with concepts denoting latent cyclical properties such as cyclical "behaviour", "contractions", "troughs", "turning points" and so forth. As a consequence of Mitchell's predilection for sifting numerical data outside the context of a fully worked-out, formal model, Koopmans alleges that Mitchell would have benefitted from a fuller utilization of "the concepts and hypotheses of economic theory ... as part of the process of observation and measurement". He accuses Mitchell of "seeing, counting and measuring cycles" before incorporating the tools of formal economic theory. 'Pure' theory, according to Koopmans, is needed in order to model the cycle; statistical series were just a supplement used to check the predictive content of the hypotheses deduced from the formal model. Furthermore, without resort to explicit, formal theory "the movement of economic variables [appear] ... as if they were eruptions of a mysterious volcano whose boiling cauldron can never be penetrated". To use another analogy, it seems to



Koopmans that Mitchell employs a method akin to studying the mechanism of a clock by analysing a record of its tickings. It appeared as if Mitchell's explanation of the business cycle was supposed to emerge spontaneously from the statistical series or be automatically inscribed on a tabula rasa. Though, whether Mitchell actually measures without 'theory' depends on the interpretation given to 'theory'. Koopmans prefers the explicit, tentative modelling procedure with its attendant apparatus of endogenous variables, exogenous variables and structural equations. His theory is to be used as a formal analytical tool and would include reference to underlying motives of rational economic agents. Koopmans complains that not a single demand or supply curve or other equation expressing the behaviour of men or technical laws of production is explicitly employed by Mitchell.<sup>64</sup>

Mitchell intimates to J.M. Clark his sardonic view of the modelling procedure later recommended by Koopmans:

Men have always deluded themselves ... with strictly logical accounts of the world ..., they have always fabricated theories for their own spiritual comfort and practical guidance which ran far beyond the realm of fact without straining the powers of belief. Each system [may have been] tolerably self-consistent - as if that were the test of 'truth'!<sup>65</sup>

In a manner akin to Carnap's recommendations, Mitchell restricts his discourse to concepts that were at least partially

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64 Koopmans, 'Measurement Without Theory', op.cit., pgs. 114, 123, and 129-30.

65 Letter published in Clark, J.M., Preface to Social Economics [1936], N.Y., AMK Reprints of Economic Classics, 1967, pp.412-3.

interpreted by corresponding observation terms, in this case, those associated with series of numerical data. In a definition setting out his basic concept of "business cycles" Mitchell notes some of the latent properties of cycles (as listed above).<sup>66</sup> He later emphasises that this general definition which broke down the abstract "cycle" into lower-level theoretical terms such as "troughs" and "turning points", "would serve its purpose ... only ... [if used] as a guide to observation".<sup>67</sup> Mitchell's perspective of scientific concepts is founded on a partial interpretation approach. It gives primacy to operational concepts and requires that theoretical terms have links to observable conditions. Mitchell draws back from any position which may be labelled as 'pure' operationalism. In the final analysis, he finds that theoretical terms could not be fully operationalised à la Bridgman, since "there are no simon-pure economic facts".<sup>68</sup>

Mitchell uses theoretical terms but his 'theory' cannot be easily reconciled with that used by Koopmans. Koopmans wanted to deduce observed regularities from general principles. The contrast between Mitchell's 'theory' and Koopmans' 'theory' is sharpened by Schumpeter. Schumpeter differentiates between theory as an explanatory hypothesis (Mitchell-type) and theory

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66 Mitchell and Burns, Measuring Business Cycles, op.cit., p.3.

67 Mitchell, What Happens During Business Cycles, op.cit., p.7, my emphasis.

68 Mitchell, W.C., 'Facts and Values in Economics', Journal of Philosophy, 41, 1944, p.219.

as a formal conceptual tool (Koopmans-type).<sup>69</sup> Mitchell relies on the former function of theory. He is concerned to describe regularities in the co-existence and co-variation of observed phenomena which he thinks would be acceptable as explanations. With regard to explanatory theory Schumpeter evidently recognises that it is something less than strict operationalism. Explanatory theory allows for intuition and surplus meanings which are the residual of, as well as the starting point for, measurement. Thus, the operations of measurement do not exhaust a concept; a concept cannot be exhaustively defined once-and-for-all in terms of actual quantitative operations. Schumpeter says that we

must first identify the phenomenon to be investigated and in doing so must inevitably introduce elements that will exert some guiding influence upon our factual research; in other words there is no such thing as factual investigation or, ... 'measurement' without any 'theory' at all.

In his History of Economic Analysis, Schumpeter crystallizes all this by developing the notion of 'vision': "the preanalytic

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69 Schumpeter, J., 'Wesley Clair Mitchell (1874-1948), QJE, 64(1), 1950, pp.143-4, my emphasis. For an earlier version of Schumpeter's appreciation of Mitchell's work see his review essay of Mitchell, op.cit., 1927 in the QJE, 45(1), 1930, pp.150-72. In this review Schumpeter refrains from the use of terms such as "explanatory hypothesis" and formal "conceptual tool". But he does note that "Professor Mitchell does not take kindly to what savors of theoretical construction ... [H]e seems to overstate the importance of the experimental, and to understate the importance of the theoretical side ... In places he forgets or denies that there is such a thing as theoretic proof or disproof of a proposition, and seems to consider 'theories' as so many suggestions of which one is really as good as any other before being put to the decisive judgement of statistics" (pp.152-3).

cognitive act that supplies the raw material for the analytic effort".<sup>70</sup> The measurement procedure includes material provided by the researcher's vision of things and is shaped by it. In other words, Mitchell's vision provides him with preconceptions and prior judgements about the character of the business cycle. The process of data selection and numerical association that follows involves explanatory hypotheses. Rather than being set-out at the start, phases of the cycle are explained out of the masses of accumulated numerical data. Mitchell hopes that his "preliminary notion of what to look for" would be revised in the process and that the method of explanatory hypotheses would eventually give a thorough account of business cycle fluctuations.<sup>71</sup> Koopmans on the other hand maintains that an econometric model based on a generalised theory of economic interrelationships would be more efficient at defining and limiting needs for numerical data and would produce more cogent results.

Mitchell's work was subsequently criticised because it appeared to be too descriptive - this being the consequence of a lack of theory in the conceptual sense to illuminate cause and effect relationships. At the most, Mitchell's effort may have only helped identify leading and lagging elements of cycles; he suggested with explanatory hypotheses 'what' was occurring but neglected the 'why' question. The validity conditions (institutional, political, social, technological) of the

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70 Schumpeter, 'Mitchell', op.cit., p.144 and History of Economic Analysis, London, Unwin, 1954, p.41.

71 Mitchell, What Happens During Business Cycles, op.cit., p.6.

numerical relationships were not stated. Whether or not the chosen series were 'good' signals depended on the researcher's judgement and an almost infinite variety could have been chosen. Even though some resemblances in patterns of co-variation and co-existence could be traced in the chosen statistics, each phase of a cycle was found to be unique. The sequence and pattern of movement in the numerical data had a tendency to alter continuously. This means that the structure of the cyclical movement, as mapped out by the chosen array of statistics, changed often, so that it was well-nigh impossible to test the economic significance or relative weights of the chosen statistics in the cyclical process. It is now generally accepted that Mitchell did not succeed at creating a complete explanatory system. Generally, his results had low predictive value. Hansen claims that Mitchell failed to explain the causes of cyclical change: "[t]he driving forces back of the cycle movement, Mitchell was never able to disclose".<sup>72</sup> Hirsch argues that Mitchell gave up his search for a comprehensive explanatory theory of the business cycle in the later years of his life.<sup>73</sup>

It is not purported here that Carnap's framework can be legitimately transferred to economics and used to assess, say,

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72 Hansen, A.H., 'W.C. Mitchell: Social Scientist' [1949], reprinted in Burns, A.F., ed., W.C. Mitchell: The Economic Scientist, N.Y., NBER, 1952, p.304. See also p.308.

73 See, Hirsch, A., 'The A Posteriori Method and the Creation of New Theory: W.C. Mitchell a Case Study', HOPE, 8(2), 1976, pp.195-206. It would be somewhat pretentious, however, to assert that there are no exceptions to this view. Friedman, for instance, contends that Mitchell was first and foremost an economic theorist; see Friedman, M., 'W.C. Mitchell: The Economic Theorist', in Burns, op.cit., 1952, pp.237-83.

Mitchell's work on business cycles. Although, on the application of his ideas to economics it may be remarked that in 1936 Carnap states rather baldly that "[i]n our century other branches of science such as ... economics, have begun to apply the method of [partially interpreted, legitimate] theoretical concepts to some extent".<sup>74</sup> Mitchell definitely eschewed many standard economic concepts from formal microeconomic and macroeconomic theory which he thought unhelpful. He also tried to develop explanatory theories using theoretical terms primarily linked to observation terms which roughly approximated Carnap's requirements. Carnap's 'legitimate' theoretical terms could be said to serve as criteria of cognitive significance for directing observations and arranging observational results. Carnap's framework attempted to restrict scientific discourse to an empiricist language whose very structure excluded the formation of 'non meaningful', 'unhelpful', highly abstract concepts. It was a framework that aimed to reduce the uncertain correspondence between measure and concept or between observable quantities and abstract qualities. The attempt proved unworkable because the framework "was either too strict and thus ruled out as meaningless theoretical terms which seem essential for scientific practice", (Koopmans' complaint about Mitchell's neglect of formal economic principles might be related at this point), or else "too loose and demonstrably let in any term whatsoever".<sup>75</sup> Carnap's work reached this 'looser' position some 20 years after his original formulation of the difference between theoretical and observation languages. It is a position

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74 Carnap, in Schilpp, Philosophy of Carnap, op.cit., p.80.

75 Grandy, R.E., 'Introduction', in idem., ed., Theories and Observations in Science, N.J., Englewood Cliffs, Prentice-Hall, 1973, p.7.

which Carnap admitted is incompatible with the

empiricist criterion of meaningfulness ...  
[S]ince the connection between a theoretical term t and observation terms, which is the basis of the interpretation, becomes weaker as the chain from observation terms through rules of correspondence and postulates to the term t becomes longer, it seemed plausible to think that in this context we must speak of a degree of significance.<sup>76</sup>

Carnap settled for a weaker, indeterminate criterion that theoretical terms be to some degree confirmed or disconfirmed by evidence. He retreated from Bridgman's quest for the strict operational certainty of scientific terms. Confirmation is not the clear-cut all-or-none matter that strict operationalists might have preferred. Confirmation is always only a matter of degree and about differences of degree there can be disagreements. Carnap was prepared, in the last analysis, to allow for a far greater distance between non-theoretical terms relating to observed data and their conceptual referents than Bridgman was willing to concede.

### 3.7(C) Machlup On Operationalism in Economics

Machlup's statement of the significance of operational definitions in economics, and for economic measurement in particular, allows at least as much distance as Carnap between numerical data (and other observational data) and concepts of interest. Machlup avers that indirect, partial measurement is a feature of economics and in this sense he goes beyond the edge of certainty. His position has some affinities with Carl Menger's philosophy of social science and will be evaluated using examples of the design and application of numerical

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<sup>76</sup> Carnap in Schilpp, ed., Philosophy of Carnap, op.cit., p.80; vide also Carnap, 'Testability and Meaning', op.cit.

information in formal economic analysis.<sup>77</sup>

First, according to Machlup, all operational concepts in economics such as GDP, consumption, government expenditure, and so on are originally based on 'raw' data of observation, that is, from income tax returns, census data, data which is the outcome of administrative processes and so on. The numerical constructs such as GDP are fiat measures impregnated with theory from the outset. There can be no fiat measurement without any theory at all if by theory in this context is meant some proposition as to what observations should be made; some way of counting and some underlying purpose for arranging and using the resulting data. Revealing Bridgman's influence, Machlup calls theory in all these senses "mental constructions". Second, revealing Menger's influence, Machlup considers that the above

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77 The following discussion draws on Machlup, 'Operational Concepts', op.cit., pp.573-80; idem., 'Operationalism', op.cit., pp.56-58 and idem., 'Ideal Types, Reality and Construction', in Machlup, Methodology of Economics and Other Social Sciences, N.Y., Academic Press, 1978, pp.223-65. In the latter article Machlup shows the influence of the founder of the Austrian School - Carl Menger. While Machlup distinguishes between operational concepts and pure constructs, Menger differentiates the "realistic-empirical" and "strict or exact" approach to economics yielding "real types" and "strict (ideal) types" respectively. The realistic approach involves operational concepts. The exact approach involves pure mental constructs. (See, especially ibid., pp.230-33 and Section 3.1 above). For Menger, the pure, exact (or universal) mental constructs may possess some kind of minimal empirical content - and insofar as this is the case these constructs are not strictly 'pure' or a priori. More important, testing these 'pure' constructs by realistic - empirical research is, for Menger, a "methodological absurdity" - Menger, Problems of Economics, op.cit., p.69. As Hutchison comments: "It is not clear how they can be tested at all", The Politics and Philosophy of Economics, op.cit., p.181. On this point see also Winch, D.M. 'Marginalism and the Boundaries of Economic Science', in R.D.C. Black, ed., The Marginal Revolution in Economics, Durham, Duke University Press, 1973, pp.63-65.



functions of theory can be distinguished from mental construction at a higher level of abstraction - in the realm of pure constructs or ideal types. Design of 'pure', "fictitious" mental constructs is not carried out in a vacuum, i.e., independent of experience. However, Machlup considers that they are not only different in degree but also different in kind from theory formed and used in combination with propositions about the real objects of sense experience. 'Pure' constructs are used as links in hypothetico-deductive systems. They are not used as links in propositions that make a posteriori generalisations about concrete phenomena of experience, but may have some indirect reference to observable phenomena. Every operational concept must have a 'pure' construct as a higher-level theoretical referent but the reverse may not hold. For instance, a 'pure' construct such as 'economic man' will not have any direct referents in the domain of experience or a counterpart subject to immediate sensory observation, but it may nevertheless be a necessary heuristic link in a meaningful investigation of economic changes and interrelationships.<sup>78</sup>

'Pure' neologisms may get their start from occasions when, say, numerical data fail to fit the consequences suggested by an accepted theoretical system. The construct "money illusion", for instance, was apparently developed after a reduction in real wages observed via increased consumer prices led to observed results with respect to labour supply (a supply that had allegedly remained unchanged) which contradicted deductions from a theoretical system in which labour supply was an increasing function of real wage rates. In this case the 'pure' construct

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78 Machlup, F., 'The Problem of Verification in Economics', SEJ, 22, July 1955, pp.13-14.

was developed without direct reference to observables, since "no one could reasonably claim to have any direct experience of illusion suffered by other minds".<sup>79</sup>

Throughout his discussion of operationalism, Machlup alludes to a continuous interplay or interactive feedback process between 'raw' numerical data/fiat measures and mental constructs in the operational domain, and a similar interactive process between operational concepts and 'pure' constructs. He adds a rather equivocal comment to the effect that the "theoretical usefulness of operational concepts and empirical usefulness of pure constructs are powerful constraints in the formation of the two types of concepts".<sup>80</sup> As it turns out, to anticipate the interpretation of Machlup's position which follows, these are differential constraints in economic analysis - the constraint of theoretical usefulness should work to a far greater degree on formation of operational concepts than the reverse.

Consider the 'pure' construct the 'supply of an industry'. Supply is a 'pure' construct. A variety of operational referents for this construct might be found by statistical inquiry. Assume that supply is defined as quantity supplied by a particular group of firms producing a homogeneous product such as cloth and that the firms measure supply by weight. Wholesalers on the other hand may purchase the cloth by weight and sell by square metre since consumers use the cloth for purposes for which only a square measure is significant. The recorded quantity of cloth sold and bought at the same time

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79 Machlup, 'Operational Concepts', op.cit., p.579.

80 Machlup, 'Operationalism', op.cit., p.57.

increases in terms of tonnes and decreases in terms of square metres. Thus far, the supply of an industry is by no means unambiguous. At least two different quantities may ensue from this statistical inquiry. The possibilities for measurement are not at all exhausted by one counting operation. With non-homogeneous commodities there are many possible combinations of product shape, size and quality designed by different, if not the same, firms. Quantity supplied can then be measured by a variety of operations. Choices have to be made among a variety of possible measures none of which is necessarily a 'better' fit of the pure construct - 'supply', which is not measurable. Choice of a measure will depend on context and purpose and more mental constructs might have to be used in making the choice. Another example from microeconomics is the 'pure' construct: 'price'. It may have many operational referents in part depending on the degree of homogeneity of the commodity in question.<sup>81</sup> These may be designed by asking sellers what price they charge or what their net returns are; by looking at price tags; by looking at invoices filed in business offices or recorded entries in ledger accounts; by looking at price lists published in catalogues of a specific trade or in newspaper advertisements or by asking buyers what they paid. As well, wholesale prices may be obtained and, depending on the significance of geographical position for the market in question, 'delivered' prices may need to be tabulated. Prices

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81 Although, according to Machlup, the degree of homogeneity may not matter. He gives the example of the "relatively homogeneous" commodity of foreign exchange which has a variety of operational counterparts. See 'Operational Concepts', op.cit., pp.575-6.

quoted may also differ from prices paid because of means and timing of payment, bargaining abilities, liquidity positions of parties and so on. "The diligent researcher", writes Machlup, "may end up with dozens of different [numerical] series ...; he will realize that they all contain so many 'impurities' that none can be said to be a very good analogue of the pure construct".<sup>82</sup> Again, selections and manipulations (corrections, seasonal adjustments, etc.) of 'raw' data will entail a movement away from direct findings of observation rendering resulting figures less operational. Logically prior to these judgements are additional subjective interpretations of the meaning of 'price' on the part of original record-keepers. Working further behind all this are subjective interpretations conferred on 'price' by economic agents involved in a transaction. If Mr. A is observed paying a certain price for a commodity from Mr. B's store, the observer cannot always know 'for certain' whether he has bought the commodity at a mutually agreed discount or at a cut-price to compensate for a perceived structural fault in the commodity. Perhaps A and B do not agree on the meaning of the resulting price - one regarding the transaction as a bargain and/or the other as a substantial gain from what otherwise might have ensued. The subjective meaning of 'price' for economic agents and the interpretation of record-keepers and record-analysts may all diverge. The analyst cannot, in any case, obtain numerical data except usually through reports of others about acting economic agents engaged in the activities under

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82 Ibid., p.575.

investigation. As a corollary, the investigator "cannot build useful constructs and theories in [complete] disregard of constructs and theories formed and communicated by men of the type he observes".<sup>83</sup>

In a note on 'Facts, Theories and Measurements' Machlup draws on contributions from Weber and Schutz to the science of human action. He argues that social science is concerned with human actions or with certain aspects of the results of human actions. As a consequence, numerical data and constructs may have real existence "only in the minds of the actors and of other parties concerned [with using and interpreting them]. Thus there is a serious difficulty in establishing what it really is that economists ... can 'observe'".<sup>84</sup> Also, this means that attaching numerical values to observed outcomes of human actions presupposes theory, supposition and 'pure' constructs although this theory may have to be tentative, implicit and changeable. Lower level theory used to compile numerical data may be altered as more observations are analysed and as it interacts with 'purer' theory of a higher order at a higher level of abstraction. An example might be a restructuring

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83 Machlup, 'If Matter Could Talk' [1969], reprinted in Bitros, G., ed., Selected Economic Writings of Fritz Machlup, N.Y., N.Y. Univ. Press, 1976, p.17.

84 Machlup, The Political Economy of Monopoly, Baltimore, John Hopkins Press, 1952, p.450. Although, in a more recent paper Machlup says he has come to the view that "in observing phenomena in the real world ... [what is] 'real' is best equated with 'in black and white'", e.g., what is written on price tags, etc. See idem., 'Ideal Types, Reality and Construction', in idem., Methodology, op.cit., p.258.

and supplementation of numerical data collected originally for GNP, for the purposes of designing other fiat measures. That is, operational definitions have been updated in line with new higher-level, 'pure' theories of what constitutes economic welfare. For instance, Nordhaus and Tobin design a "measure of economic welfare"(MEW) which supplements and reorientates numerical data originally used to calculate GNP, as does Samuelson with his "measure of net economic welfare".<sup>85</sup> Moss also explores the possibility of developing socio-economic accounting data for the purpose of constructing a set of more general "welfare outcome" measures.<sup>86</sup> In part, new fiat measures such as these are refashioned in response to:

- (1) critical assessment of existing numerical data and operational concepts which do not appear to fulfill the role for which they were previously cast;
- (2) demonstrably changing human valuations and conceptions of economic welfare; and
- (3) changing definitions of the goals, objectives and content of economic activity. Operational concepts or numerical constructs will not necessarily have a timelessly valid

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85 Nordhaus, W.D. and Tobin, J., 'Is Growth Obsolete?' in Moss, M., ed., The Measurement of Economic and Social Performance, N.Y., Columbia Univ. Press, 1973, pp.509-32, and Samuelson, P.A., 'From GNP to NEW' [1973], reprinted in Nagatani, H. et.al., eds., Collected Scientific Papers of P.A. Samuelson Vol.IV, Cambridge, MIT Press, 1977, pp.725-727.

86 See Moss, M., 'Social Challenges to Economic Accounting and Economic Challenges to Social Accounting', RIW, 26(1), March 1980, pp.1-17.

correspondence with their higher-level, 'pure ' conceptual referents. As Cicourel argues in his work on social measurement:

measurement by fiat is not a substitute for examining and re-examining the structure of our theories so that our observations ... and measures of the properties of social objects and events have a literal correspondence with what we believe to be the structure of social activity.<sup>87</sup>

This generates the responses: which theories and whose theories? The respective proponents of alternative operational concepts for economic welfare perhaps perceived an inadequate correspondence between deduction from some 'accepted' model (connecting (say) GNP to economic welfare) and empirical observations. This set-off a search for new 'pure' concepts of economic welfare and culminated in new operational concepts. Machlup insists that investigators in economic analysis have "similar minds" to those economic agents whose actions, and the outcome of whose 'actions, constitute the subject matter economics. So a 'good' choice of operational concepts to stand-in for 'pure' constructs, such as MEW for economic welfare, would depend in part, on introspection:

The invention and design of new constructs is affected by analogical reasoning based on various kinds of experience ... [I]t is a very special type of experience that affects invention and design of a new construct in the social sciences: each construct must pass the test of empathic understanding or imagined introspection.<sup>88</sup>

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87 Cicourel, A.V., Method and Measurement in Sociology, London, Free Press, 1964, p.28.

88 Machlup, 'Operational Concepts', op.cit., pp.579-80.

For Machlup, Max Weber's concept of Verstehen (or roughly, understanding or introspection) was indispensable not only for explaining intersubjective agreement among economic analysts but also agreement between those analysts and everyday economic agents. It was also important in the process of verifying correspondence rules between operational concepts and 'pure' mental constructs. Verification, in this context, is a loose criterion of confirmability: "that there seems to be considerable 'support' or 'evidence' for the proposition in view of a marked ... consistency between that proposition and statements about particular observations".<sup>89</sup> Machlup gives no clue, though, as to just what degree of support or specific kind of evidence is required. What is a legitimate use of an operational concept must presumably be weighed-up on a case-by-case basis and is conditional on intersubjective agreement among economic analysts. In any case, tests of correspondence rules evidently require some movement into the social context of science - a realm of intersubjective meanings which are surely based on mental reflection.

If verification is examined in more detail, Machlup shows that 'empirical testing' presupposes that investigators possess and use operational counterparts as proxies for 'pure' constructs. The 'pure' constructs are part of an analytic model which generates hypotheses and in turn these suggest operational proxies. Machlup finds that, in economics, 'testing' of hypotheses is inherently imprecise. In a reply

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89. Machlup, 'Problem of Verification', op.cit., p.12. See also pp.16-17.



article on the notions of "ultra-empiricism" suggested by Hutchison, Machlup advances what might be called an 'impossibility' thesis concerning convergence of social science 'testing' procedures with those of physical science. In economics, for example, quantitative precision has its limits; "verification will [therefore] ... not be the rigorous kind of thing it is in the physical sciences with its numerical constants".<sup>90</sup> His thesis can be further elaborated in the following guise:

- (i) In economics the requirement that all 'pure' constructs have operational counterparts is not feasible. For example, the ideal type of mechanical maximiser (economic man) is useful for neoclassical microeconomics because of its heuristic value in ordering otherwise chaotic actions of economic agents.<sup>91</sup>
- (ii) Even the use of operational concepts in the process of verification involves many imprecise mental operations and judgements which produce debatable results.

Now, Latsis recently labelled Machlup's methodology 'conventionalist' in the sense that Machlup accepts as meaningful and helpful constructs (including propositions) that are conventions (resolutions, procedural rules, postulates) with

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90 Machlup, ibid., p.21. See also his 'Rejoinder to a Reluctant Ultra-Empiricist', SEJ, 22, 1956, pp.483-93.

91 Machlup makes a strong case for the use of constructs such as economic man and champions the notions of utility and profit maximisation as central principles of 'pure' economics in his essay, 'The Universal Bogey', in Peston, M. and Corry, B., eds., Essays in Honour of Lord Robbins, London, Nicolson, 1972, pp.99-117.

regard to analytical procedure.<sup>92</sup> Machlup concurs with that label.<sup>93</sup> Consider, for the purposes of illustration, Machlup's following statement. Its context is the presupposition that postulates of neoclassical economics are to be applied as procedural rules to solve a particular problem. For the 'pure' constructs,

a rigid verification requirement would be out of place. Usually the judgement of the analyst will suffice even if he cannot support it with more than the most circumstantial evidence or mere "impressions". Suppose he deals with a simple cost-price-output problem in a large industry, how will the analyst determine what "type of case" it is with regard to "market position?" Lacking the relevant information, he may first try to work with a model of perfect [competition] - although he knows well that this cannot fit the real situation - and will note whether his deduced results will be far off the mark. He may find the results reasonably close to the observed data and may leave it at that. For to work with a more "realistic" assumption may call for so many additional assumptions for which no relevant information is available that it is preferable and unobjectionable to continue with a hypothesis contrary to fact.<sup>94</sup>

Here and in all his other work, Machlup favours a system of indirect verification which is focussed on conclusions and consequences and not the fictitious, 'pure' constructs contained

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92 Latsis, S.J., 'A Research Programme in Economics', in *idem.*, ed., Method and Appraisal in Economics, Cambridge, Univ. Press, 1976, pp.1-42, and especially pp.9-14.

93 See Machlup, Methodology, *op.cit.*, p.460.

94 Machlup, 'Problem of Verification', *op.cit.*, p.15.

in the neoclassical economist's analytical apparatus.<sup>95</sup> Thus, suppose, along with Machlup, that a 'pure' construct: 'firms prefer more profit to less profit under equally risky conditions', (maximisation of returns) is combined with a group of other 'pure' constructs about economic institutions and conditions and about a certain kind of market structure. Suppose also that a major external change affects these firms such as the imposition of an import quota on foreign companies producing a product in common with these domestic firms. A hypothesis may have been suggested by the analytical model constituted by the above group of premises which stated that, say, a quota would increase the excess of domestic over foreign prices of that product on the home market.<sup>96</sup> The hypothesis is operational to the extent that it stipulates something about observable events. Machlup maintains that if these consequences "are found to be in relatively good correspondence with observed events" then the maximisation of returns assumption would be

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95 For example, see his 'Theories of the Firm: Marginalist, Behavioural, Managerial', AER, 57(1), 1967, p.2, passim. C.f., the great debate in economic methodology initiated by Friedman's Essays in Positive Economics, Chicago, Chicago University Press, 1953, pp.3-43, on the 'irrelevance of assumptions thesis'. For a recent contribution to this debate see Boland, L.A., 'On the Futility of Criticizing the Neoclassical Maximisation Hypothesis', AER, 71(5), December 1981, pp.1031-36; and for a survey of relevant literature see Blaug, Methodology, op.cit., pp.104-11.

96 Machlup, 'Rejoinder', op.cit., p.484. On Machlup's point that hypotheses give rise to operational suggestions that are not directly entailed by 'pure' constructs, see Rivett, K., '"Suggest" or "Entail"?: The Demarcation and Confirmation of Economic Hypotheses', AEP, 9(15), December 1970, p.140 n.24. C.f., Stewart, Reasoning and Method, op.cit., p.43, for different meanings of the term 'hypothesis'.

"indirectly verified". Data selection and interpretation problems, among others, effectively rule out direct verification. The first problem relates to the domain of applicability of the model constituted by 'pure' constructs and more operational hypotheses. Some agreement has to be reached on what constitutes product homogeneity in this case, i.e., the product these firms have in common. An answer to this question should help sort out what prices to observe. Secondly, Machlup warns that, on the basis of the law of large numbers, only prices resulting from the interaction of a "large number" of firms should be considered and not the prices of particular firms. Thirdly, Machlup's example, even though it was not fully elaborated, posits an implicit ceteris paribus clause. The clause implies that other factors not specified in the premises had a negligible effect on the excess of domestic over foreign prices of the commodity in question. If all these other factors were not somehow checked out, some doubt might be thrown on the value of any actual price data used to 'test' the hypothesis. Machlup refers to a "relatively good correspondence" between the less operational concept (hypothesis) generated from 'pure' constructs and the more operational concept (observed numerical data) which indirectly verifies the original, pure construct - maximisation of returns. Unless 'testing' was an exclusively personal process then some intersubjective agreement among investigators would be required to define relevant observed data. Further, a degree of correspondence not regarded by investigators as "relatively good" (perhaps in relation to other hypotheses generated from other combinations of 'pure'

constructs belonging to some other analytical model), may suggest that maximisation of returns was incorrectly applied to the context in question. It may also suggest that the hypothesis was incorrectly specified (ceteris was not paribus). Anyhow, such negative results would not necessarily result in rejection of the analytical model.<sup>97</sup> Hypothesis testing using operational concepts will be inconclusive because in principle operational concepts are more or less theory laden and in practice, a particular operation may not be easily replicable independently of the variation of a large number of factors (some not fully identifiable) involved in any concrete problem. Machlup therefore grants that any tests will be more nearly of the character of illustrations than of verifications of the kind in physical science with controlled experiments and automatic replication.<sup>98</sup> Interpreting Machlup's argument so far, it is not difficult to concur with Katouzian that a rule of 'empirical'

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97 If a criterion of falsifiability was employed here instead of Machlup's loose criterion of verifiability, the theoretical model may still survive. The 'sophisticated' view of falsificationism apparently held by Popper for scientific practice in the physical sciences, requires a "large number" of replicable tests resulting in negative evidence in order to conclusively falsify a theory. See Blaug, Methodology, op.cit., pp.23-27 and p.266. Unfortunately, even the application of Popper's criterion to economics overlooks the problem, in practice, of repeating or replicating a set of operations independently of the variation in the large number of factors which are considered to affect hypothesised results. On this point see Losee, J., A Historical Introduction to the Philosophy of Science, London, OUP, 1973, p.183. On some profound doubts (1) about the applicability of Popper's criterion to economics see Caldwell, J., 'Review of Blaug's Methodology', SEJ, 42(1), 1981, pp.242-45 and (2) about the likelihood of making Popper's criterion 'work' smoothly in economics (since it requires substantial intersubjective agreement among economists), see Endres, A.M. 'Why Bother With Methodology?', NZEP, 16, 1982 (forthcoming).

98 Machlup, 'Problem of Verification', op.cit., p.19.

verification, even if strictly adhered to as providing conclusive tests, would "be prescriptive as opposed descriptive ... [T]he rule of empirical verification is not a positive but a normative category".<sup>99</sup>

All this may not mean, according to Machlup, that 'pure' concepts are inviolate. He grants that they could be rejected together with the whole theoretical model of which they form a part. For example, maximisation of returns may be rejected with the whole of neoclassical microeconomic theory in favour of another model. It remains to be seen, however, just what conceivable circumstances or results would undermine the apparatus (neoclassical theory) since, (say) the observed numerical data can be designed and looked at in varying ways. For instance, Bauer and Walters (the latter a specialist econometrician) have reflected on the "inconclusive nature of most empirical work in economics" and they found that the "data emerging from economic processes are susceptible to widely differing interpretations".<sup>100</sup> Numerical observations which are often used as the economist's 'hard' observed data require both selection and interpretation. These activities may be coloured by the observer's interests, disciplinary perspectives, prevailing theories and anticipated results. The notions of disciplinary perspectives, matrices and 'paradigms' have now been extensively discussed in historiography of science by Kuhn,

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99 Katouzian, H., Ideology and Method in Economics, London, Macmillan, 1980, p.52.

100 Bauer, P.T. and Walters, A.A., 'The State of Economics', JLE, 18(1), April 1975, pp.13-14.

Lakatos and many others.<sup>101</sup> Machlup takes processes of data selection for granted although, in a number of places, he explicitly recognises the importance of "[v]alues of the economist as analyst which influence him in the choice of his terminology and in the acceptance of available statistical data".<sup>102</sup> He addresses data interpretation rather controversially:

When there is an apparent conflict between observations and the theory they are supposed to test, the observations can usually be disqualified as of uncertain reliability; and where this will not do the conflict can usually be reconciled by means of auxiliary hypotheses.<sup>103</sup>

All this could amount to evasive tactics and immunising stratagems in defense of 'pure' constructs central to say, neoclassical price theory. Constituent auxiliary hypotheses may be formulated in a way that preserves the theory.<sup>104</sup> Latsis

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101 Standard references for this extensive literature are: Kuhn, T.S. The Structure of Scientific Revolutions, Chicago, Univ. Press, 1970 edition; Lakatos, I. and Musgrave, A., eds., Criticism and the Growth of Knowledge, Cambridge, Univ. Press, 1970 and Latsis, Method and Appraisal, op.cit.

102 Machlup, 'Positive and Normative Economics', in Heilbroner, R., ed., Economic Means and Social Ends: Essays in Political Economics, Englewood Cliffs, Prentice-Hall, 1969, p.114.

103 Machlup, The Economics of Seller's Competition, Baltimore, John Hopkins, 1952, p.73.

104 See, e.g.,: (1) Krupp, S.R., 'Types of Controversy in Economics', in idem., ed., The Structure of Economic Science, Englewood Cliffs, Prentice-Hall, 1966, pp.39-52. Krupp observes that the "degree of confirmation of an entire theory is highly intertwined with value judgements which reflect ... the selection of its constituent hypotheses" (p.51). A similar conclusion could be arrived at about degrees of falsifiability if Popper's criterion was used in economics. (2) Rosenberg, A., Microeconomic Laws: A Philosophical Analysis, Pittsburgh, Univ. of Pittsburgh Press, 1976. Rosenberg discusses the "insulating tendencies of neoclassical micro-theory", in Ch.7, pp.139-52.

also finds that "Machlup's interpretation of the neoclassical theory of the firm generates built-in defence mechanisms enabling reconciliation of almost any recalcitrant evidence with the theory".<sup>105</sup> The significance of these assertions for the present work is that mere availability of 'hard' numerical evidence may not give determinate solutions.

It may be reiterated at this point that Machlup understands corresponding Bridgman-type operations in economics to consist of the actual recording of observations including statements about what to record; the means of recording, counting or measuring, and what is actually done, in practice, with recorded data. Theories about what to look for, how to record, and about purposes for using the data were involved in each operation (refer to 3.7(A) above). Machlup's interpretation breaks down the more general phrase 'empirical work' by identifying actual principles and practices involved. Samuelson and Gordon provide somewhat narrower interpretations of operationalism in the context of economics. Samuelson develops "operationally

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105 Latsis, Method and Appraisal, op.cit., p.11. Examples of defence strategies used by Machlup can be found in Latsis' paper. Reference might also be made to Machlup's comment in defense of marginalist theories of the firm against behavioural and managerial theories. The latter "were addressed ... to different applications, namely, to explanations of the conduct of individual firms that are not under the pressure of effective competition ... In contradistinction, the general theory of marginalist reactions of firms to events or changes applies essentially to industries with effective competition - and hence with profits insufficient to pursue objectives other than survival through maximisation of net income" (Machlup, Methodology, op.cit., pp.372-73). Latsis' comment on this kind of strategy is that all Machlup can "tell us ... [is] the perfectly competitive [marginalist] model is only applicable where it can be applied" (op.cit., p.11).



meaningful theorems" by which he means "simply a hypothesis about empirical data which could conceivably be refuted if only under ideal conditions".<sup>106</sup> Gordon uses the phrase "operational proposition" to denote a statement that "implies ... an operation that could in principle be performed, the result of which tests the proposition".<sup>107</sup> Now firstly, in comparison with Samuelson, in no place does Machlup rule out hypotheses about observable data from his interpretation of operations, but he does distinguish between the practices of designing and recording data and hypotheses of differing degrees of generality about that data. In the present work, Machlup's distinction alludes to the likely additional layer of theory involved in hypothesising about data compared with actual data design and recording. Secondly, Gordon's operations that could "in principle" be performed would be distinguishable from, and much wider than, those that are performed in practice on a set of unique, concrete cases in a specific time and place.<sup>108</sup> Again, differential layers of theory are likely to be involved at each of these levels.

Consider the example where Machlup claims that 'equilibrium' like 'economic man' is not an operational concept in economics. Equilibrium is merely a tool for "mental

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106 Samuelson, P.A., The Foundations of Economic Analysis, Cambridge Mass., Harvard University Press, 1947, p.4.

107 Gordon, D.F., 'Operational Propositions in Economic Theory' [1955], reprinted in Breit, W. et.al., eds., Readings in Microeconomics, New York, Holt-Rinehart, 1968, p.48.

108 On this point, refer to Machlup, 'Professor Samuelson on Theory and Realism', AER, 54(3), 1964, p.733.

reflection" - it is a "useful fiction", a pure construct at many removes from "the particular data of observation". In other words, as a thought object it should not be mistaken for an object of sense perception. He goes so far as to argue (with examples from the economic analysis of balance of payments equilibria), that attempts to develop direct operational 'counterparts' of the 'pure' construct had not been successful.<sup>109</sup> This contrasts with Finger's contention that any conceivable concept of equilibrium is 'operational' if it is couched in terms of a testable prediction. Finger proposes a predictive test for equilibrium exante:

the real world counterparts of the endogenous variables of a model will remain constant so long as real world counterparts of the exogenous variables remain constant. Thus the concept of equilibrium is reflected in a statement such as 'If A, B, and C are constant, then X and Y are constant' and it seems obvious that such statements are as operational as statements of the 'If A increases X will decrease' type.<sup>110</sup>

Per contra, Machlup claims that expost, "I cannot recognise an equilibrium ... no matter how hard I look". For Machlup, equilibrium, in theory, is "a constellation of selected, inter-related variables so adjusted to one another that no inherent tendency to change prevails in the model which they constitute". But Machlup's operational concepts cannot predict once-and-for-all to directly test the 'pure' concept: equilibrium. Finger implies that a series of operations is synonymous with or equivalent to the concept: equilibrium. He implies an identity

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109 Machlup, 'Equilibrium and Disequilibrium: Misplaced Concreteness and Disguised Politics', EJ, 57(269), 1958, p.11.

110 Finger, J.M. 'Is Equilibrium and Operational Concept?', EJ, 81(323), 1971, p.610, my emphasis.

by using the words "reflected in". Finger also misinterprets Machlup's position on the meaning of operational concepts. Finger's "real world counterparts" are Machlup's operational concepts, but equilibrium is not operational. This is more than just a semantic issue or a confusion of exante and expost. Equilibrium is not an operational concept because there is always an immeasurable gulf requiring something like a mental 'leap' between statistical figures (representing what Machlup calls an "operational subset") and the equilibrium construct. The philosophical difficulty, ignored by Finger, is one of justifying connections between particulars (in the operational subset) and the universal thought category they exemplify. Thus, Machlup: "I cannot recognise the 'facts' of a 'real situation' - as an equilibrium ... in the sense discussed, that is, as a position where 'everything could go on as it is' and where 'the situation calls for no further adjustments' to anything that has happened". In addition, any model of equilibrium necessarily abstracts from the 'real' world which "has infinitely more variables ... and their interrelations are neither known, nor, I fear, knowable". In other words, it is not possible to know and specify all the conditions present when operations are carried out. As well, interrelations among variables may change unpredictably through time. A selected number of variables and selected combinations between variables may be chosen for a model of equilibrium, "depending on the analytical or didactic habits, skills and purposes of the economist".<sup>111</sup> Selection and purpose, it may be recalled, are

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<sup>111</sup> Machlup, 'Equilibrium', op.cit., p.12, my emphasis. C.f., Samuelson, "Dr. Machlup and I might continue to argue about ... variants of the view that what is a fact is a very subjective thing ... [U]ndoubtedly there are deep-lying problems here that need further study", 'Theory and Realism: A Reply', AER, 54(3), 1964, pp.738-39.

part of the Machlup-process of operationalism in economics. Operational concepts cannot make conclusive, testable, predictive statements about their abstract referents in the manner understood by Finger. Alternatively stated, paraphrasing Machlup - a rigid verification requirement for the 'pure' construct would be out of place because of the many layers of theory involved at each step in the testing process. (Hence Finger's complaint that Machlup had "not addressed himself to the question of whether or not economic theory can be tested" which seems to imply that there is only one theory involved, and that theory can be directly tested).<sup>112</sup> Numerical data (based on theory) can be used for a purpose (based on more theory), to stand-in for higher level operational concepts generated as predictive hypotheses from 'pure' constructs in an analytical model. The use of numerical data as variables constituting an equilibrium model (e.g., for the ultimate purpose of a policy recommendation) is another step involving the addition of more layers of theory to the data - and is not the operationalisation of equilibrium. Machlup sums up his argument in a subjectivist and relativist manner:

To characterise a concrete situation "observed" in reality as one of "equilibrium" is to commit the fallacy of misplaced concreteness. At best, the observer may mean to assert that in his opinion the observed and duly identified situation corresponds to a model in his mind in which a set of selected variables determine a certain outcome, and that he finds no inherent cause of change - that is, that he believes only an outside disturbance, not in evidence at the moment, would produce a change in these variables. This, of course, is a personal judgment,

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112 Finger, 'Equilibrium', op.cit., p.611.

meaningful only if the variables are fully enumerated and the assumptions about their interrelations are clearly stated. As matters stand, any concrete economic situation may correspond at the same time to an equilibrium of one model and a disequilibrium of another.<sup>113</sup>

Again it may be reiterated that Machlup denies a major role for operational concepts such as numerical data and hypotheses related to observable data, in directly discrediting 'pure' constructs. In short, for Machlup all 'testing' in economics using numerical data is roundabout, inconclusive and illustrative.

In macroeconomics, use of theoretical models to refer to ex ante numerical values of macro-terms presents another difficulty. In comparison, all numerical constructs are designed by operations carried out in an historical, ex post fashion. 'Pure' constructs in a macro-economic model - investment, savings, consumption, employment and so on - are usually measured by aggregate, ex post numerical magnitudes. Formal macro-models that postulate functional relations between these variables may be difficult to interpret even with the availability of numerical data. Measurability of ex ante functional relations is not possible - only past functional relations can be established. In the interim, new series of

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113 Machlup, 'Equilibrium', op.cit. It should be noted that the 'fallacy of misplaced concreteness' was originally propounded by the philosopher A.N. Whitehead: "it consists in neglecting the degree of abstraction involved when an actual entity is considered merely so far as it exemplifies certain categories of thought"; Whitehead, Process and Reality: An Essay in Cosmology, New York, Macmillan, 1929, p.11. See also, idem., Science and the Modern World [1927], Cambridge, CUP, reprint 1953, pp.64-66.

numerical data and changes in the conditions held constant in the model (the latter more especially influencing selection and manipulation of numerical data), can render past measures obsolete or irrelevant. The grounds on which an investigator dares assume that say the aggregate investment function calculated from data of past years will hold in future years, can be fundamentally unsound. Machlup concedes that "laws of large numbers" might make it appear more plausible to assume somewhat greater stability of some macro-relations. There are few other reasons he can adduce in support of the proposition that measurability is more decisive and reduces uncertainties for the analyst in macroeconomics than in microeconomics.<sup>114</sup>

Machlup's position on operationalism in economics may also be approached from a vantage point which turns on his general definition of the subject-matter of economics. Economics, as a social science, is concerned with "the result of human actions which can be interpreted only with reference to motives and purposes of the actors, that is, to the values entertained by them". Human actions involving the use of numerical data must involve reference to motives, values and purposes. The design of operational concepts is inseparably tied to their use. Uses involve purposes. The process of designing and using fiat measures in economic analysis, by economists, deals with mind-related, intention-related and value-related constructs. It is

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114 Machlup, Essays in Economic Semantics, N.Y., Norton, 1967, p.126. Machlup considers one other reason in support of this proposition, viz., that some macro-models could dispense with some variables and some functional relations between variables which would be important in a micro-model. However, this argument could also work in reverse. For his argument see ibid., pp.120-26 and Economics of Seller's Competition, op.cit., 1952, pp.418-9.

ipso facto associated with mental reflection and apprehension as to what the operational concepts (or measures) can be used for, or interpreted to 'show'. For individuals, numerical data might be used to inform present action oriented toward an uncertain future. In economic analysis operational concepts can be used to assist theory verification. In the process of reflection and interpretation both in everyday, commonsense economic thought and in formal economic analysis, more mental constructs are instilled consciously or unconsciously into what were, in the first instance, predominantly 'raw' operational concepts. Operational concepts ('raw' numerical data and higher-level fiat measures), cannot be adequately circumscribed as mute 'givens' which close the door to speculation and meaning when they are actually used for specific purposes. Conversely, mental constructs (including 'pure' constructs) in economic analysis cannot be adequately defined in terms of operational concepts or 'observables' or in terms of operations which involve recording of numerical data derived from observation. The manifold immeasurable, perhaps even metaphysical factors involved in the design and interpretation of operational concepts suggest an element of uncertainty. Machlup's conclusion that "we cannot be sure about the prevalence of non-quantifiable factors" in the subject-matter of the social sciences, still holds today despite the increasing availability of numerical data.<sup>115</sup>

Overall, Machlup reformulates Bridgman's operationalism almost beyond recognition. The ideas in his general methodo-

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<sup>115</sup> Machlup, 'Are the Social Sciences Really Inferior?', SEJ, 27(3), 1961, p.162 and p.170, my emphasis.

logical essays draw away from Bridgman's position and in their mature form have evolved to a point where economic analysis is considered pre-eminently as 'pure' theory. Operational concepts are relegated to a back-seat role whilst bold mental experiments are seen as "the only way of developing a general theoretical system fertile with useful inferences". Machlup proclaims that it is necessary "to go beyond the specific empirical evidence" and replace predominantly operational by 'pure' concepts. Machlup's comments on testing theory contend that only "occasional" verification of hypotheses suffices to ensure confidence in the whole theoretical system from which the hypotheses are derived. He recommends that

theory ought to have sufficient links with the protocol domain, with data of observation [operational concepts]. Links are 'sufficient' if they allow us to choose intelligently the theoretical arguments that apply to particular 'concrete' situations and, in addition, if they allow us to subject the theoretical system to occasional verification against empirical evidence.<sup>116</sup>

Machlup's case for the primacy of theoretical work, while seeking to make allowances for operational considerations, does not really succeed, explicitly, at harnessing these allowances to a process of designing and using numerical data (and other operational concepts) which takes place on a more operational -

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116 Machlup, 'Operationalism', op.cit., p.67.



more theoretical continuum.<sup>117</sup> He is subsequently reproved in the literature for seemingly supporting the "superiority of unrealistic theoretical analysis" in economics; and for "commending empirical work ... if it never really makes a difference" to his habit of "discounting all tests of economic theories that critics have devised".<sup>118</sup>

### 3.7(D) Concluding Reflections on Operationalism

The introduction of notions of operationalism to economics especially as a result of the combined effect of the growing availability of numerical observations and post-war developments in econometrics, was viewed by many as the coming of age of the science. According to Leontief during the late 1950's, "economics came to be considered ... a quantitative science par

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117 Machlup's "bipartite division" between 'pure' constructs and operational constructs was critically appraised by Rivett "'Suggest or Entail"?' op.cit., pp.140-41. C.f., also Melitz, J. 'Friedman and Machlup on Testing Economic Assumptions', JPE, 73(1), 1965, p.55 n.47. In their criticism these writers neglected Machlup's allusion to the varying layers of theory in all operational constructs. This is a much looser interpretation which has some support from (1) B.B. Seligman 'On Questions of Operationalism: A Review Article', AER, 57(1), 1967, who argues that "in a social science such as economics we may still need a measure of looseness not envisaged in strictly operationalist formulations" (p.159); and (2) Margenau, H. 'What Is a Theory?' in Krupp, Structure of Economic Science, op.cit., p.35, who refers to theoretical "bulges" on the protocol (or operational) plane.

118 On these views respectively see Rotwein, E., 'Empiricism and Economic Method: Several Views Considered', in Samuels, W.J., The Methodology of Economic Thought, New Brunswick, Transaction Books, 1980, p.123; and Blaug, Methodology, op.cit., p.115.

excellence".<sup>119</sup> At a time when this view was widely held, Papandreou wrote: "[a]s long as we ... demonstrate that operationally meaningful hypotheses could be formulated in economics we could argue that economics should take its place side by side with physics, chemistry, and other 'advanced sciences'". Papandreou added a caveat expressing uneasiness about Bridgman's strictures, and appeared to favour a more liberal 'partial interpretation' approach similar to Carnap's work (in 3.7(B) above).<sup>120</sup> That this uneasiness gave-way to the uncertainties of the 1960's and 1970's is indicative in Machlup's reassessment of tenets of operationalism in economics. Machlup's suspicion of notions of strict operationalism and operational 'counterparts' was contemporaneous with developments along similar lines in philosophy of science of which the following discussion will make a brief mention.

The idea that terms used to quantify economic phenomena can be expressed in a 'neutral' observation language have been put to rest in Section 3.6 where it was argued that fiat measurement, shot-through with valuation, predominates in economics. What is meant by 'observation' cannot be known logically prior to purposes and theories underlying the design and use of numerical observations. A theme of this work is that there is no necessary one-to-one correspondence between a concept and the operations that define a fiat measure which is

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119 Leontief, W., 'The Problem of Quantity and Quality in Economics', in Lerner, D., ed., Quantity and Quality, Glencoe, Free Press, 1961, p.118.

120 Papandreou, A.G., Economics as a Science, Chicago, Lippincott Co., 1958, p.7. See also pp.10-11.

used, in turn, as a stand-in for that concept. Some early versions of operationalism, as in Bridgman's work, usually asserted a one-to-one correspondence. Such assertions rest on the possibility of clearly distinguishing between terms which are largely operational (relating to publicly observable or countable things) and those terms that are theoretical/mental (relating to unobservables). Subsequent revisions and reformulations have increasingly blurred any such distinction.<sup>121</sup>

To illustrate, in a recent work in economics Hollis and Nell abandon any 'positivist' distinction between primarily operational/ measurement terms and primarily theoretical terms. They favour a more rationalist view of all numerical data as theory-laden and thus call into question the status of so-called 'brute atomic facts' in economics. One of their points of discussion important for this study is that which attempts to appraise "the relation of theoretical economic terms to the facts which are instances of them". The first problem they encounter is the question of specifying meaningful numerical referents, by convention or by pragmatic appeal, for theoretical terms. To ask if the retail price index is a 'good' measure of inflation, defined as say an upward movement in the general or average level of prices, depends on the merits of the case in question. That is, according to Hollis and Nell, criteria of

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121 Machlup's reformulation has been given emphasis in this study. C.f., contributions to philosophy of science literature on this issue: Putnam, H., 'What Theories Are Not', in Grandy, op.cit., 1973, pp.111-123; Losee, Historical Introduction, op.cit., pp.178-93 and Achinstein, Concepts of Science, op.cit., pp.121-201.

application (rules of data selection and interpretation) are required for using the retail price index and relating it to the general or average level of prices. A conventional or pragmatic approach would decide upon criteria of application according to 'what is usually done' or what is useful depending on the case, respectively. Hollis and Nell are not convinced that either approach helps in deciding what is or is not a 'right' or 'wrong' application of the numerical data, since these approaches (a) may not admit appraisal of theories used to compile the data; (b) may give the impression that criteria of application are independent of the relationship and role of higher-level theoretical terms (in the sense that 'inflation' may have to play a part in a theoretical system or explanation in conjunction with many other theoretical terms) and (c) the question of who is entitled to set criteria of application is left unanswered. For Hollis and Nell, notions of operationalism are not a way out. To know how, say, the retail price index is designed is not to know what the term retail price refers to or what the index can be used for. There is room left to query whether actual operations used to compile the index were appropriate as stand-ins for "retail-price". Further uncertainties may arise almost in an infinite regress if questions are posed about whether the official "basket of representative goods" used to direct data compilation is an adequate definition of "retail goods" and so on. Hollis and Nell are moved to conclude that there are no operational

economic concepts "that function as merely self-adhesive labels".<sup>122</sup>

The above example from economics literature, though perhaps overly rationalistic, illustrates why Karl Popper is inclined to call operational definitions "circular". Popper defends his powerful anti-inductivist view of science as an endless dialectical sequence of "conjectures and refutations" against the circularities of operationalism. One of Popper's illustrations may be used for clarification. Contrary to Popper's stance, operationalists seem to believe that experiments (operations, measures, etc.), are conclusive and not open-ended; that theoretical terms can be circumscribed by a set of operations. However, even simple, standard operations employed to test whether "sugar is soluble in water" would be circular. The operation should have to say:

X is soluble in water if and only if (a) when X is put into water then it (necessarily) disappears, and (b) when after the water evaporates a substance is (necessarily) recovered which again, is soluble in water.<sup>123</sup>

Popper shows that the whole notion of operationalism collapses since explication of the operations performed cannot be developed without any theoretical terms. This corresponds with Machlup's perspective of operationalism in economics. Furthermore, theoretical terms and their numerical referents

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122 Hollis, M. and Nell, E., Rational Economic Man: A Philosophical Critique of Neoclassical Economics, Cambridge, Univ. Press, 1975, p.96, 97-99, and 110.

123 Popper, Conjectures, op.cit., 1969, p.440. See also idem., Logic, op.cit., 1972, p.278.

cannot be considered in isolation from other terms and referents used in an explanation. As Popper points out, "operationalism ... must be replaced by 'theoreticism' ...: by the recognition of the fact that we are always operating within a complex framework of theories" and that we do not simply aim at correlating single operations with single theoretical terms or sentences, but at whole explanations.<sup>124</sup> Therefore, the meaning of numerical data is dependent on the role the quantities play in an explanatory system. It might then be argued that so-called operational concepts such as rules specified for measuring national income are theory 'laden' or 'burdened'. Accordingly, in order to know what the rules and resulting measure means, we must learn the theory that plays a role in the cognition of counting rules. In order to know what a measure of national income is, we must know the relevant theory that directs data compilation and interpretation. If understanding a numerical construct entails knowing a whole theoretical system and if its meaning is to be thought of as the role a measure plays in that system, then the 'measure of national income' will be determined in different ways according to complete theories about national income determination. Different economic theories have suggested basic operational concepts of national

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124 Popper, Conjectures, op.cit., 1969, p.63. On this point see also Hempel, C.G., 'Empiricist Criteria of Cognitive Significance: Problems and Changes', in Grandy, Theories and Observations, op.cit., 1973, pp.53-72. Hempel also argues that attention should not be focussed on isolated individual terms that play a crucial role, say, in the cognition of numerical counterparts. Rather, the whole set of terms of a theory and their relation to each other should be a central concern.

income and gave purpose and meaning to the resulting estimates. Several successive economic doctrines exercised a marked influence on various operational concepts and utilisation of respective national income estimates, i.e., the 'mercantilist' doctrine of the second half of the seventeenth and first half of the eighteenth centuries; physiocratic and Smithian doctrines of the second half of the eighteenth and first half of the nineteenth centuries; Marxian and Marshallian doctrines of the second half of the nineteenth and first half of the twentieth centuries, and the Keynesian doctrine of more recent vintage. The meaning of respective estimates cannot be considered in isolation from their theoretical referents.<sup>125</sup> To say all this is to reject an assumption of operationalism in physical science - namely that there are, and indeed must be, terms in a theory (operational terms) whose meaning can be given independently of the theory and can remain constant from theory to theory. As Feyerabend asserts, if this is not the case, and if (say) all forms of operation and measurement are theory laden, then it is difficult to see how any agreement between theories is possible. Moreover, theory-ladenness implies that two numerical constructs of, for example, national income derived from two different theories, and designating empirical referents for terms in those respective theories, would be irreconcilable. The two sets of numerical data would be irreconcilable since they were based on two incommensurable concepts. "Introducing a new theory", Feyerabend writes,

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125 For instance, see Studenski, P., The Income of Nations, Part I: History, N.Y., New York Univ. Press, 1958, pp.11-25.

"involves changes of outlook both with respect to the observable and with respect to the unobservable ... and corresponding changes in the meaning of even the most 'fundamental' terms of the language employed".<sup>126</sup> There was much debate in the philosophy of science literature on the theory-laden thesis. Feyerabend's thesis only bears tangentially on this study. Decisions to use specific counting operations and to select appropriate numerical counterparts for concepts will depend on systematic connections between terms in the context of a "paradigm" (to loosely employ Kuhn's term). For instance, to maintain continuity with an example developed in previous chapters, Pigou's application of the measuring rod of money to calculate national dividend as a counterpart of "economic welfare" was part of a complete theoretical system. It was part of a set of ideas anticipated in Marshall's welfare theory - one component of which was to make economics a science of wealth founded originally at the micro-level on "measurement of a person's motives" in terms of money.<sup>127</sup>

Two important qualifications are now in order. First, the same series of numerical data derived from what Machlup labels the 'protocol' (data recording) domain and derived from what I

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126 Feyerabend, P.K., 'Explanation, Reduction and Empiricism', in Feigl, H. et.al., eds., Minnesota Studies in the Philosophy of Science, Vol.III, Minneapolis, 1962, p.29, my emphasis. See also Feyerabend, 'On the Meaning of Scientific Terms', in Grandy, op.cit., 1973, pp.176-83. The debate that ensued in the philosophy of science literature as a consequence of Feyerabend's thesis is well summarised by Grandy in his 'Introduction', ibid., p.12ff.

127 Marshall, Principles, op.cit., pp.15 and 19. On this point refer also to Parsons, T., 'Wants and Activities in Marshall', QJE, 34, 1931, pp.106-7.



prefer to call the 'more operational' end of a continuum of operational activities, can be used for different purposes in the context of entirely different theories. That is, trade figures, income estimates, interest rates and so on, could take their place in explanations that are rooted in entirely different analytical traditions, habits, models, (or 'paradigms'). But their actual use in these different analytical models to help interpret an event, justify an argument, or to help design and test a hypothesis constitutes other Machlup-type operations. The analyst will usually have different ends and purposes in mind in each case and the 'raw' numerical data will often take on additional and different layers of meaning (become less operational) in each respective model. The 'raw' numerical data may, for instance, be filtered or adjusted in some way to suit the analyst's purpose. In any case, the step from 'raw' data recording and design to its use in particular explanatory frameworks involves increasing admixtures of theory or mental constructs. Second, my conjecture that numerical data is differentially laden with theory in the operational process of its design and interpretation does not mean that any method of designing and interpreting a set of numerical observations is as correct or as adequate as any other in a specific time-space context. The scientific status of a series of numerical observations in both the occasions of design and interpretation with respect to an event or a problem, is ensured if this process is always open to critical questioning. Openness to scrutiny and criticism are crucial elements in the Popperian view of science, and have the

potential to counter, though not always purge, inherent elements of error and uncertainty involved in designing and using numerical data.

This study of perspectives on the limits of our numerical knowledge has attempted to reflect on the epistemological uncertainty that is attached to theory-laden numerical data and formal fiat measures in economics. It has gone beyond questions of data quality and availability which Morgenstern became pre-occupied with. It challenges the notion that any decisions may be made as to whether or not observed numerical data, and other numerical constructs, apply in a given situation without recourse to theoretical notions and inferences. If theoretical suppositions are required, in the first place, to compile numerical observations, then the practice of isolating 'hard' numerical 'facts' in economics seems to lack solidity and certainty - and the more so when the numerical data is used with additional theory for a specific purpose. Quantification cannot yield determinate results; it founders on an uncertain correspondence between theory and operation. This is one reason why, according to Ward, economists are now finding that application of numerical data in 'testing' theories "has been disappointingly indecisive. We are entering an era when more complex and less precise criteria of verification than the positivist ones are going to play an increasing role in the actual practice of economics".<sup>128</sup> The meaning of quantified variables often remains ambiguous if not examined in the context

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128 Ward, What's Wrong with Economics?, op.cit, p.177. C.f., Mayer, T. 'Economics as a Hard Science: Realistic Goal or Wishful Thinking?', EI, 18, April 1980, pp.165-78.

of the specific theoretical systems in which they are designed and more especially, the explanations within which they are used and interpreted. In broader language, it is asserted here that quantity and quality cannot be considered in dialectical opposition. The 'dichotomy' between mutually exclusive quantity and quality is unhelpful; quantity and quality are more valuably understood as interactive - as part of a creative tension between reciprocal ways of knowing.<sup>129</sup> Of course, all this is not to deny that the deterministic, quantitative universe of early twentieth century science and social science has left a deep impress on contemporary economics.

A question that requires further unravelling is the purported interactive feedback process of designing and interpreting numerical data; that is, the continuous interplay on an 'operational continuum' between data recording and its use in conjunction with (i) different layers of theory and (ii) specific times and places. This is one of the questions addressed in following chapters.

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129 See Rapoport, A. Operational Philosophy, New York, Harper Row, 1953, p.158ff; Margenau, 'What is a Theory?', op.cit., p.36. Margenau refers to "a kind of epistemological feedback" between what he terms the "protocol domain" (of operations) and the domain of higher-level, abstract constructs. For a more general philosophical discussion see Lerner, D., 'On Quantity and Quality', in idem., ed., Quantity and Quality, op.cit., 1961. Refer, also, to a work that attempts to break new ground in economics along these lines: Katzner, D.W., 'On Not Quantifying the Non-Quantifiable', JPKE, 1(2), 1979, pp.113-28.

IV STUDIES IN THE HISTORY OF POLITICAL ARITHMETIC

#### 4.1 PREAMBLE: CONVERTING NUMERICAL OBSERVATIONS TO INDICATORS

Some of the Austrian ideas in Part III can be drawn together by reassessing and expanding on the concept of a 'numerical indicator'. The rationale for studies in the history of political arithmetic that follow might in the first place though, be traced to the now burgeoning interdisciplinary literature on the concepts of a numerical economic or social indicator. The 'social indicators movement' as it is now called, made its modern point of departure with Raymond Bauer's book: Social Indicators.<sup>1</sup> Social scientists in Australia and New Zealand have now started to contribute to the debate concerning, inter alia, appropriate methodologies for indicator design; the functions of indicators and theoretical issues addressed in this section viz., the differences between statistics and indicators. This last issue can be said to include debate about various factors involved in the process of designing numerical observations and converting them into

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1 Cambridge, Mass., MIT Press, 1966. On the development of a scientific 'movement' in this area see Gross, B.M. & Straussman, J.D., 'The Social Indicators Movement', Social Policy, 5(3), 1974, pp.43-54. (There is a cognate literature on indicators of monetary policy - that is, on using money market variables to measure "the direction in which monetary policy has changed in the recent past and/or the effect of monetary policy on employment, output and prices at some unspecified future dates"; see Brunner, K., ed., Targets and Indicators of Monetary Policy, San Francisco, Chandler Publishing Co., 1969, p.2 passim. However, this literature does not discuss the knowledge-status of numerical variables and is firmly within the positivist tradition since it considers numerical data as unquestionable 'givens'. In addition it hardly gives any attention to the concept of a numerical indicator).

numerical indicators.<sup>2</sup> The Australian Bureau of Statistics (A.B.S.) has set up a Social Survey and Indicators Section and now regularly publishes a compendium of what are labelled "social indicators".<sup>3</sup> Some international organisations such as OECD have been attempting to design indicators of social well-being with reference to a number of broad social concerns or objectives common to OECD countries, whereas UNESCO has started to organise a set of development indices.<sup>4</sup>

The 'social indicator' publications of the A.B.S. are "based on the use of social indicators in the planning and evaluation of government social programmes and in social reporting". These publications beg the question when it comes to explaining what constitutes a social indicator. The term 'indicator' is all too often used with laxity; many of the statistics previously published and collected in a routine

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- 2 Three surveys on these issues are worth mentioning: Cant, G. et.al., eds., Social Indicators for Development Planning in New Zealand, Wellington, NZ National Commission for UNESCO & NZ Social Development Council, 1979; Fanchette, S., 'Social Indicators: Problems of Methodology and Selection', in UNESCO Reports and Papers in the Social Sciences, No.30, Dept. of Social Sciences, UNESCO, 1974, and Owens, H., 'Social Indicators' in Scotton, R.B. and Ferber, H., Public Expenditure and Social Policy in Australia, Vol.II, Melbourne, Longman Cheshire, 1980, pp.261-311.
  - 3 A.B.S., Social Indicators, No.1, 1976, Ref. No.13.16, Canberra; No.2, 1978, Catalogue No.4101.0, Canberra; and No.3, 1980, Cat. No.4101.0, Canberra. C.f., New Zealand developments: NZ Dept. of Statistics, 1980 List of Social Indicators, Wellington, Dept. of Statistics, mimeographed.
  - 4 See, e.g., OECD, Measuring Social Well-Being: A Progress Report on the Development of Social Indicators. Paris, OECD, 1976; c.f., The 1980 OECD List of Social Indicators. Manpower and Social Affairs Committee, MAS (80) 17, 1st Revision, Paris, OECD, June 1980, and Iyer, R., 'Social Indicators - A Review of Work in UNESCO and Other International Organisations', in Cant, op.cit., Social Indicators, pp.144-61.

fashion are now being rebaptised social indicators. The A.B.S. legitimately finds fault with the theoretical literature for not producing an acceptable definition:

The relationship of social indicators to social statistics and to social reporting cannot be adequately explained without a clear understanding of what constitutes a social indicator. Unfortunately, social indicators literature is not sufficiently unified on a definition to enable one to be stated without further explanation.<sup>5</sup>

The term 'social indicator', for instance, has been defined in many different ways. Only two out of ten definitions found in the indicators literature by Horn barely approximate a definition to be advanced in forthcoming pages as an outgrowth of Part III. They are (1) "Indicators are quantitative variables that somehow reflect the human condition in a social setting" (Galtung) and (2) "A social indicator is defined as the operational definition ... of any of the concepts central to the generation of an information system descriptive of social concepts. These central concepts may be categorized as systems components, systems goals, social problems and policy goals" (Carlisle).<sup>6</sup>

From the standpoint of an economist working in this field, the role and status of numerical observations in Austrian

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5 A.B.S., op.cit., 1980, p.xvii.

6 Quoted in Horn, R., 'From Social Statistics to Social Indicators', AJS, 20(2), 1978, p.144. C.f., Horn's more up-to-date survey of definitions, none of which comes nearer to the perspective being developed here, 'Social Indicators: Meaning, Methods and Applications', IJSE, monograph issue, 7(8), 1980.

economics and the history of efforts to design, use and institutionalise economic statistics can be helpful when attempting to expand and clarify the processes involved in converting statistics to indicators. Controversies about the notion of a social indicator possess an inescapable quality of déjà vu given some understanding of the history of some early economic measurement programmes from 1890-1950.<sup>7</sup> Following Morgenstern's example and the study of Austrian approaches in Part III, it is contended here that the very practice of measuring, observing or counting is inseparably tied to the use to which resulting numerical data is put. When a set of numerical observations directed by a fiat counting rule, is interpreted for some purpose to refer to something other than itself or to refer to some other sense of reality, it becomes an indicator. Use of numerical observations in a particular context presupposes intentions, purposes, interests and value judgements on the part of the interpreter. This also necessitates that numerical observations must be 'filtered' through the human mind as they are designed and used. The numbers will be used to mean something. In other words, a numerical indicator will be more than a mute statistic. Two writers have developed a similar perspective on the notion of an indicator in cognate disciplines. In the relatively new field of the 'policy sciences' De Neufville characterises an indicator as all those factors involved in "procedures for collecting and combining data to stand in for a concept"; whereas, Bunge, a philosopher

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7 See Endres, A.M., 'Economics of Social Measurement Processes', IJSE, monograph issue, 9(1), 1982, especially pp.5-20.



of social science, notes that a numerical indicator is an observable variable assumed, rightly or wrongly, to point to or estimate some other usually unobservable variable. An indicator is then a symptom of something else.<sup>8</sup> In the language of Part III, an indicator is an observable, operational 'counterpart' of a higher level abstract concept. Further, in order for numerical data to actually become a 'counterpart', it has to be interpreted in a specific context. Once data is used for a purpose - reasoned with, interpreted, used to describe, prescribe or prognosticate - it is then standing-in for something else, usually a directly unobservable phenomenon. In the process of use, the boundaries between 'raw' numerical data and the sense of reality that it stands-in for become blurred. The data coalesces with this 'other sense of reality' to form an indicator. This description is perhaps a crude way of expressing the ultimate unity between 'quality' and 'quantity' or theory and numerical observations. It also shifts the discussion about numerical indicators into the realm of theories of knowledge. As Frank Knight commented, "[W]e cannot separate the discussion of reality from the discussion of the knowledge of reality". Further, for the purposes of this section, Knight's 'Austrian' theory of knowledge might be stated here. The context of interpreting numerical observations to stand in for a 'sense of reality' is both subjective and intersubjective. Knowledge of any 'sense of reality' is

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8 De Neufville, J.I., 'Validating Policy Indicators', Policy Sciences, 10, 1978, p.173. Bunge, M., 'What is a Quality of Life Indicator?', Social Indicators Research, 2(1), 1975, pp.65-80.

inseparable from (a) self-knowledge of the knower and (b) knowledge of other knowers and of their knowledge, or of their "minds". ... The essential point is that knowledge of ... reality presupposes "valid" inter-communication of mental content, in the sense of knowledge, opinion or suggestion among the members of a knowing group or intellectual community.<sup>9</sup>

Conversion of numerical data into indicators requires a great deal of faith as well as analytical skill on the part of the interpreter of the data. A considerable amount of faith is required when the conceptual referents are vague or the explanation relating numerical data to those referents is undeveloped or unconvincing to 'other knowers' in a particular group or community. Kaplan regarded any lack of correspondence between data and theoretical concepts in the context of social science as part of an early stage of inquiry. He has the following to say about this early stage as compared to a later more advanced stage of inquiry:

In every context of inquiry we begin with terms that are undefined - not indefinables, but terms for which that context does not provide a specification. As we proceed, empirical findings are taken up into our conceptual structure by way of new specifications of meaning, and former indications and references in turn become matters of empirical fact ... [A] process of "successive definition" can be understood so as to take account of the openness of meaning of scientific terms. For the closure that strict definition consists in is not a precondition of scientific inquiry but its culmination. To start with we do not know just what we mean by our terms, much as we do not know just what to think

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9 Knight, F.H., "'What is Truth" in Economics?', JPE, 48(1), February 1940, p.11 (my emphasis) and p.6 respectively.

about our subject-matter. We can, indeed, begin with precise meanings, as we choose; but so long as we are in ignorance, we cannot choose wisely. It is this ignorance that makes the closure premature.<sup>10</sup>

Conceivably for Kaplan, numerical indicators would play a role in exploratory stages in the development of new measures or series of numerical data. His method of successive definition could be applied in order to obtain operational counting definitions somehow consistent with observable phenomena and therefore eventually suitable for classifying these phenomena once-and-for-all. At the culmination of inquiry the "former indications and references" were supposed to "become matters of empirical fact". In contrast, and following Austrian contributions discussed earlier, it may be argued that, first, the 'empirical facts' are always debatable depending on the person and context. The 'facts' can be interpreted in different ways. Second, scientific inquiry in economics never culminates in complete certainty or, in other words, in the complete closure of scientific terms around a series of operational (counting) definitions. That is, inquiry does not close-off concepts to the point where there is no residual meaning that leaves the concept non-controversial (e.g., the concepts of income, output, unemployment and so forth, have no absolute boundaries). This is to argue, in turn, that indicators play a

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<sup>10</sup> Kaplan, A., The Conduct of Inquiry, Methodology for Behavioural Science, San Francisco, Chandler Publishing Company, 1964, p.77.

more general role in economic inquiry since operational counting or measuring rules seldom exhaust the whole meaning of theoretical concepts for all times and places. In short, the content of 'numerical knowledge' is rarely definitive - it is, and should be, open to doubt and criticism.

A general definition of an indicator can be provided from preceding discussion and explored in what follows. A numerical indicator, whether it be 'economic' or 'social' is a theory and value-loaded, always proximate numerical counterpart of something (a 'thought') other than the numerical observations that it arranges. This definition is designed to capture some of the questions raised in preceding discussion of Austrian views. It allows for subjectivism in the use of data and for the perspective that all numerical 'facts' are laden with values. The data is subject to interpretation or the 'filter' of the human mind (or minds) in a context (including social context) when they become indicators - with all the uncertainties and knowledge consequences this might involve. In addition, room should be allowed in the process of using numerical observations and transforming them into indicators for an aspect of futurity, time and expectation that is likely to be involved. Indicators are tied up with human valuation and purpose that, in turn, imply norms, goals and objectives. Numerical data, however, is historical. It can be used to represent the state of some unobservable variable in connection with a desired objective or norm (in relation to that variable), which it may be hoped to reach in the future. Recalling a perspective from Part III - only through imagination and purpose can past data be transcended and used to 'cast shadows ahead'.

The following section on political arithmetic seeks to lay bare implicit assumptions involved in interpreting numerical data to stand-in for theoretical concepts. It seeks to explore the different realms that might be involved in the notion of an indicator. Each part of the next section asks, with varying emphases: How was the data designed? What was it used to mean? How did the user know x stood-in for y? What factors were involved in validating this knowledge, i.e., why were the values of numerical data accepted as symptomatic of other unobservable variables? Were the factors that justified the use of data in a particular way at all subjective? Was use a matter of inter-subjective convention and/or hypothesis? Finally, were numbers used to provide a way of specifying a discrepancy between past and desired, future circumstances? If so, in what ways were numbers used to contribute to the formulation, establishment or legitimation of norms and goals with the future in mind? These questions can be used as a means of orientation for following literary case studies of the 'art of reasoning by figures' articulated by three seventeenth century English political arithmeticians.<sup>11</sup> The study concentrates on the theory and method of political arithmetic with a view toward elaborating a theory of numerical indicators. In accordance with this aim, the study will be unashamedly selective and does not purport to

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11 This project was originally inspired by Samuel Hollander's comment about unexplored subjects in the history of economic thought. One subject he listed was "the role of factual information, e.g., in Mercantilism, in Petty ...". Hollander, 'Teaching the History of Economic Thought: Report of a Symposium at Bristol 1973: Appendix', HOPE, 7(1), 1975, p.117.

provide an exhaustive exposition of the practice of political arithmetic and its subtle changes during the seventeenth and eighteenth centuries.

#### 4.2 POLITICAL ARITHMETIC: INTELLECTUAL BACKGROUND

"Political arithmetic was not an isolated innovation but part of a wide intellectual movement".<sup>12</sup>

The beginnings of most types of statistical work, particularly development of numerical information on economic and population matters, can be traced to the period which began with Francis Bacon's Novum Organum [1620] and ended with Newton's Principia [1687]. It was an age in which "the most tangible product [of social science] ... was the use of statistics, the quantitative study of social facts".<sup>13</sup> As an early chapter in the history of economic and social statistics, political arithmetic developed before formal laws and theories of probability had been formulated and integrated into a fully-fledged social science discipline of applied statistics.<sup>14</sup> A belief that reasoning with numbers made for an improvement in public policies and that measurement was symmetrical with understanding or explanation pervaded seventeenth century social science literature. The Royal Society, founded in 1662, adopted

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12 Clark, G.N., Science and Social Welfare in the Age of Newton, 2nd edition, London, Clarendon Press, 1949, p.132.

13 Ibid., p.120.

14 Refer, on this point, to Westergaard, Contributions to the History of Statistics, op.cit., 1932, Chapters 2 and 3; also Hacking, J., The Emergence of Probability, London, Cambridge University Press, 1975, pp.102-110.

Bacon's inductive empiricism as an official dogma. This was also a time when concerted efforts were being made to draw up a coherent picture of the world and what might be 'known' about it; to purposively place all phenomena in some order and to collate 'facts' from all parts of the world. Natural historians at the time were immersed in naming and classifying plants, animals and minerals just as anatomists were exploring the human body and systematically classifying its component parts while surveyors were placing on the map everything worth knowing (place names, distances, etc.). It was no coincidence, then, that the early pioneers of the numerical analysis of economic and social phenomena who saw the potential policy uses of numerical information, were at first surveyors, cartographers and/or anatomists.<sup>15</sup>

Three streams of thought may be identified as having a formative influence on political arithmetic in the seventeenth century and subsequently. They may be set out briefly as follows. Political arithmetic became prominent during the seventeenth century at a time when experimental science took some great leaps forward under the influence of Newton and Bacon. First, Newton's outlook on the universe, the fertile seeds of which had taken root much earlier in Hobbes' philosophy,

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15 On the significance of the occupations of various political arithmeticians, see Clark, Science and Social Welfare, op.cit., p.126; Letwin, W., The Origins of Scientific Economics, English Economic Thought 1660-1776, London, Methuen, 1963, p.116ff., Greenwood, M., 'Medical Statistics from Graunt to Farr' [1941], reprinted in Kendall, M.G. et.al., eds., Studies in the History of Statistics and Probability, London, Griffin, 1970, pp.91-2.

stressed the machine-like character of physical phenomena in which each observable component played a role that was rigidly predetermined by inexorable laws.<sup>16</sup> Newton's method conjoined inductive experimentalism with mathematics and proceeded by gradual ascent from particulars of observation to general physical laws and constants. Newton stressed the primacy of things portrayed to the senses and downplayed the value of a prioriism:

For Newton there was absolutely no a priori certainty such as Kepler, Galileo and preeminently Descartes believed in, that the world is through and through mathematical, still less that its secrets can be fully unlocked by the mathematical method already perfected.

This interpretation of Newton's methodology distinguishes Newton from Descartes by the former's "stronger empiricism".<sup>17</sup> In correspondence with Newton, political arithmeticians definitely shared a "non-Cartesian outlook".<sup>18</sup> Second, Bacon's well-known methodological tenet was that if scientists were to become good empiricists and experimentalists, they had to collect and arrange observed facts and put any knowledge obtained from them to practical use. Thus in praise of induction Bacon wrote: "it

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16 See Chalk, A.F., 'Natural Law and the Rise of Economic Individualism in England' [1955], reprinted in Rima, I., ed., Readings in the History of Economic Theory, New York, Holt Rinehart, 1971, especially pp.43-45.

17 Burtt, The Metaphysical Foundations of Modern Physical Science, op.cit., p.208, my emphasis. C.f., Hanson, N.R., 'Hypothesis Fingo', in Butts, R.E. et.al., eds., The Methodological Heritage of Newton, Oxford, Blackwell, 1970, p.32 et passim.

18 Mini, P.V., Philosophy and Economics, Gainesville, University of Florida Press, 1974, p.38.



is safer to begin and raise the sciences from those foundations which have relation to practice and let the active part be as the seal which prints and determines the contemplative counterpart".<sup>19</sup> Bacon went beyond simple induction via collection or enumeration of observed facts. He required the facts to be arranged in a way that would enable scientists to arrive at general laws which had varying degrees of generality and preferably of increasing generality as the results of experiments were observed.<sup>20</sup> The methodological canons of Newton and Bacon left their impress on writers who hoped to discover regularities in the economic and social world by applying the very same canons. Third, in addition to the new scientific spirit of experimentalism, the Reformation with its shortlived installation of a republic and other revolutionary changes in England's government during the seventeenth century would have had some influence on the orientation of political arithmetic. The nation's political climate was highly uncertain during the seventeenth century.<sup>21</sup> Political arithmetic was demonstrably under the intellectual influence of Thomas Hobbes. Generally, the Hobbesian outlook of the political arithmeticians

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19 Bacon, New Organun, [1620], New York, Bobbs-Merril Inc., 1960, p.123.

20 See Russell, B., History of Western Philosophy, London, Allen and Unwin, 1962, p.528.

21 On political turmoil at the time see e.g., Hill, C., Reformation to Revolution: A Social and Economic History of Britain 1530-1870, London, Weidenfeld & Nicolson, 1967, pp.97-152, and idem., The World Turned Upside Down: Radical Ideas During the English Revolution, London, Temple Smith, 1972. On the effects of turmoil on the practice of political arithmetic see Studenski, The Income of Nations, op.cit., p.27ff.

showed up in their awareness of different kinds of uncertainty facing the group (the 'State'). The seventeenth century mode of social scientific, numerical analysis of economic and social matters revolved around concerns about the nation state. In this period of nationalism dominated by 'mercantilism' the design and use of numerical observations was addressed to the needs of statesmen. It was underwritten by Hobbesian nominalist assumptions about the nature and sources of order in economy and society. Political arithmetic, according to Buck's important contribution of late, was "predicated on a view of society as inherently disordered"; it involved the use of quantification as a powerful means of reducing civil strife, alleviating social anxiety and of creating and legitimising order.<sup>22</sup> The following literary exegeses reinforce and expand on Buck's contention. It may be noted as a forewarning that some political arithmeticians, unlike later statisticians, did not presume that economic and social phenomena were susceptible to numerical analysis only insofar as they were ordered by causal laws of nature; they also regarded causal laws as discrete entities - human constructs made possible through "long Custom and general

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22 Buck, P., 'Seventeenth Century Political Arithmetic: Civil Strife and Vital Statistics', Isis, 68(1), 1977, pp.67-84.

Consent".<sup>23</sup> Above-all, political arithmeticians tried to use numbers as a means of consensus creation.

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23 Petty, W., Political Arithmetick, London [1690], reprinted in Hull, C.H., ed., The Economic Writings of Sir William Petty, London, Cambridge University Press, 1899, Vol.I, p.243. Hereafter Hull's edition will be abbreviated as PEW. Petty is not always consistent on this particular point. In one place he warns of the "vanity and fruitlessness of making Civil Positive Laws against the Laws of Nature", Treatise of Taxes, London, 1662, PEW, p.48. C.f., his views on civil laws which restricted the free exportation of money and bullion, Quantulumcunque Concerning Money, London, 1682, PEW, p.445.

#### 4.3 'Political Arithmetick': Three Literary Expositions

##### 4.3(A) Graunt's 'Shop Arithmetick'

The first general account of the theory and method of political arithmetic can be found in the writings of William Petty (1623-87) a contemporary of the pioneering social 'statistician' - John Graunt (1620-74). Petty explained his method in Political Arithmetick (written c. 1671 and published posthumously, 1690) as follows:

The Method I take to do this is not very usual; for instead of using only comparative and superlative Words, and intellectual Arguments, I have taken the course (as a Specimen of the Political Arithmetick I have long aimed at) to express myself in Terms of Number, Weight, or Measure; to use only Arguments of Sense, and to consider only such Causes, as have visible Foundations in Nature; leaving those that depend upon the mutable Minds, Opinions, Appetites, and Passions of particular Men, to the Consideration of others.<sup>1</sup>

Petty's specimens of political arithmetic reflected, firstly, an interest in applying the science of the 'Body Natural' to the 'Body Politick', an analogy he borrowed from Bacon.<sup>2</sup> They were exercises in applied social science. Petty thought the anatomy of a nation, in terms of its component parts, could be dissected and explored with the instruments of "number, weight or measure". As a corollary, "arguments of sense" could help build

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1 PEW, p.244. Petty had earlier used the term 'political arithmetic' in a letter to Anglesea 17/12/1672 (PEW, p.240n.) and in the Epistle Dedicatory to the Duke of Newcastle prefixed to his Discourses of Duplicate Proportion, 1674, PEW, pp.236-8.

2 Ibid., passim.

up conceptions of, and sometimes reinforce preconceptions about, regularities in economic and social phenomena. These arguments, could be reinforcing, in turn, to contemporary social customs - those less mutable patterns of human behaviour which "particular men" might wish to subvert. In other words, political arithmetic could also be used to help give rationale to contemporary social practices based on custom and social consensus. It aimed to "shew the great effect of Unity, industry and obedience, in order to the Common Safety".<sup>3</sup> Moreover, political arithmetic was important because it had the potential, given the political context at the time - which placed a high value on social and economic self-determination - to provide a form of quantitative knowledge free from the disturbing effects of idle controversy.

The first notable example of this method, at least in Anglo-centric literature, is John Graunt's "Shop-Arithmetick" on mortality in London.<sup>4</sup> Graunt's book was called Natural and Poltical Observations ... upon the Bills of Mortality ... with Reference to the Government, Religion, Trade, Growth, Ayre, Diseases, and the several Changes of the said City of London (1662). The Bills of Mortality were returns of the deaths which occurred in London. They were prepared by the Parish Clerk's Company, and the first published specimen goes back to 1592. From 1603 they were published uninterruptedly once a week, and

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3 Ibid., p.313.

4 Graunt used the epithet "shop" rather than "political" arithmetic. See PEW, p.323.

the number of parishes included in them was from time to time altered.<sup>5</sup> With the objective "not [to] engage [him]self in idle and useless speculation", Graunt set out to analyse the number of births and deaths obtained from weekly returns of parish registers. These registers were kept up to date chiefly because of the King and peoples' interest in, and fear of, periodic outbreaks of "some of the more formidable Diseases" especially the plague. He considered that his study of the Bills would help alleviate public anxiety and "preserve the Subject in Peace and Plenty".<sup>6</sup>

One first principle that forms the foundation for production of numerical indicators that can be gleaned from Graunt's early contribution to the art of political arithmetic is a critical appreciation of 'raw' numerical observations. Before Graunt manipulated 'raw' data to derive numerical ratios and constructs he showed a high degree of methodological skill in examining the reliability of this data. The observations themselves were a step below formal statistical constructs. Graunt realised that the validity of his numerical analysis was dependent on data accuracy in terms of counting rules specified for data compilation. Counting rules were rooted, in part, in some common social practices. He described the several steps taken to compile the Bills of Morality:

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5 For a thorough history of the Bills see PEW, pp.1xxx-xxxii; and for an introduction to the controversy over their authorship see p.lii passim of Hull's 'Introduction' and Greenwood, M., 'Graunt and Petty - A Re-Statement', JRSS, 96, 1933, pp.76-81.

6 PEW, p.334 and p.395.

When any one dies, then, either by tolling, or ringing of a Bell, or by bespeaking of a Grave of the Sexton, the same is known to the Searchers, corresponding with the said Sexton ... The Searchers hereupon (who are ancient Matrons, sworn to their Office) repair to the place where the dead Corps lies, and by view of the same, and by other enquiries, they examine by what Disease or Casualty the Corps died. Hereupon they make their Report to the Parish Clerk, and he, every Tuesday night, carried in an Accompt of all the Burials and Christnings happening that Week, to the Clerk of the Hall. On Wednesday the general Accompt is made up and printed, and on Thursday published and dispersed.<sup>7</sup>

The parish registers were by no means free of reporting error. Variable social habits and practices by region influenced returns and led more especially to some defects in "the Accompts of Christnings". The registers also contained definitional and coding inconsistencies attributable to the practices of parish searchers or observers. Graunt could not initially understand "to what purpose the distinction between Males and Females is inserted". Furthermore, he critically appraised the raw data before him and queried, for instance, definitions of "Aged"; "What the Searchers mean by Infants ... as whether children that cannot speak ... or children under two years old"; coding schemes which placed fatalities caused by "Cough" under the title of "Aged"; and differences between searchers who sometimes recorded that "one died of the Headach, though the Physicians were of the Opinion that the Disease was in the Stomach". Further, he found that some codes may be superfluous

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<sup>7</sup> Ibid., p.346.

if causes were interrelated whereas disaggregation by age was, for his purposes, a much higher priority:

I should not be satisfied whether the Infants died of Wind or of Teeth or of the Convulsion, & c. or were Choaked with Phlegm, or else of Teeth, Convulsion, and Scowring, apart or together, which they say do often cause one another; for I say it is somewhat [more important] to know how many die usually before they can speak, or how many live past any assigned numbers of years.<sup>8</sup>

Graunt seemed to be tacitly aware that all exercises in enumeration require, as a first approximation, some agreed classificatory schema which names whatever is to be counted and analysed (a nominal scale). In this respect Graunt's work is in the Baconian tradition - a contribution to natural history. As Graunt noted in his epistle dedicatory to Sir R. Moray:

Sir Francis Bacon reckons his Discourses of Life and Death to be Natural History; ... I humbly bold to think Natural History also, and consequently that I am obliged to cast in this small Mite into your great Treasury of that Kind.<sup>9</sup>

The ability of contemporary natural historians to come to inter-subjective agreement about the context and content of names was also an example and prerequisite for social analysts contemplating forms of quantification. It had its precedent in the writings of Hobbes - a contemporary of Graunt. In the Leviathan, Hobbes wrote:

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8 Ibid., p.362, 347, and pp.348-52.

9 Ibid., pp.322-3. C.f., Kargon, R., 'John Graunt, Francis Bacon and the Royal Society: The Reception of Statistics', Journal of the History of Medicine, 18, 1963, pp.337-48.



[W]ithout words there is no possibility of reckoning with numbers ... The Latines called ... accounting Ratiocinatio and that which we in bills or books of account call Items, they called Nomina, that is Names ... Reckoning [refers to] ... the Consequences of general names agreed upon ...

Hobbes' nominalism held that only particulars of sense experience were real. Universal names 'agreed upon' (or perhaps imposed by an authority) were predicable of many particulars. Universals were but observable likenesses among the particulars of sense experience.<sup>10</sup> In manipulating numerical observations to reach more general numerical constructs Graunt appreciated that if the original observations were coded inaccurately by searchers this would not unduly affect his purpose. Some variability in this regard seemed inevitable. The searchers, being a part of "the generality of the world" could be expected to distinguish between coding categories without too much "errour". For example, in the case of a man of seventy five years who died of a "Cough ... I esteem it little errour as to many of our purposes if this Person be in the Table of Casualties, reckoned among the Aged, and not placed under the Title of Coughs". Most of Graunt's purposes for using data on mortality and morbidity were, above all, connected to an intention to help alleviate social anxiety about epidemics: "whereas many persons live in great fear and apprehension of

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10 Hobbes, T., Leviathan [1651], London, Penguin edition, 1968, pp.104, 106, & 111. On Hobbesian nominalism vide, McNeilly, F.S., The Anatomy of Leviathan, London, Macmillan, 1968, pp.35-47. Nominalism "carried the germ of the operational idea" according to Rapoport, Operational Philosophy, op.cit., p.vii, my emphasis. For a sophisticated epistemological study of connections between nominalism, classification and natural history in the sixteenth and seventeenth centuries, see Foucault, M. The Order of Things: An Archeology of the Human Sciences, London, Tavistock Publications, 1970, pp.125-65.

some of the more formidable and notorious Diseases following; I shall only set down how many died of each [so that] ... persons may better understand the hazards they are in".<sup>11</sup>

From the inadequately compiled material on the "poor despised Bills" Graunt judiciously extracted a number of formal, numerical constructs including the excess of male over female births; the approximate numerical equality of the sexes; the high rate of mortality in earlier years of life and the excess of rural over urban death rates.<sup>12</sup> These constructs were based on some theory or starting assumptions and were one step below the notion of an indicator as outlined in Section 4.1. To illustrate: received opinion in Graunt's time held that the population ratio was around 3 females to 1 male. Graunt showed that an approximate 1 to 1 ratio obtained. The exact ratio (the formal numerical construct) he found was 16 males to 15 females. This ratio was not left to stand mute. Graunt used it as an indicator to legitimise "the Law ... against Fornications and Adulteries" and the laws which favoured monogamous marriage against polygamy. He thought that "if there were universal liberty, the Increase of Mankind would be but like Foxes at best". Order and stability in family and social life were central to his next conclusion that, the numerical ratio indicated a "Blessing to Mankind ... [;a] natural Bar to Polygamy: for in such a state Women could not live in that parity and equality of expense with their Husbandes, as now, and

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11 PEW, pp.348-9 and 350.

12 Ibid., p.369ff.

here they do". Here the numerical construct signalled an underlying natural law ("Bar") which in Graunt's view should be reinforced by social practice. In addition, Graunt used the construct to help shore-up prevailing preconceptions about the inappropriateness of "Licentiousness", adultery and similar 'anti-social' practices.<sup>13</sup>

Graunt then proceeded to construct another estimate, namely the population of an area without having to directly count the people concerned. He assumed that the number of child-bearing women in the community was about double the number of annual births "for as much as such Women ... have scarce more than one Child in two years". He then reasoned as follows:

The number of Births I found, by those years wherein the Registries were well kept, to have been somewhat less than the Burials. The Burials in these late years at a Medium are about 13,000 and consequently the Christenings not above 12,000. I therefore esteemed the number of Teeming-Women to be 24,000: then I imagined there might be twice as many Families, as of such Women; for there might be twice as many Women Aged between 16 and 76, as between 16 and 40, or between 20 and 44; and that there are about eight Persons in a Family, one with another, viz. the Man and his Wife, three Children and three Servants or Lodgers ...

This gave him 48,000 families and a population of 384,000. In this example Graunt advanced a set of assumptions (a theory) and used a simple statistical technique of averaging ("at a medium"; "one with another") to build a numerical construct but not an indicator.<sup>14</sup> Graunt tried to cross-validate the above population estimate with two further estimates based on different

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13 Ibid., pp.376-8.

14 Ibid., pp.384-5.

theories, and one estimate based on a population estimate furnished by a Census of London taken in 1631 for taxation purposes.<sup>15</sup> For the sake of brevity it may be added that another one of Graunt's outstanding statistical constructs was a life table.<sup>16</sup>

Most of Graunt's work (which he modestly considered as a "Pamphlet not two hours reading"<sup>17</sup>), involved construction of ratios, the differences between ratios, time-trends in ratios and identification of geographical and seasonal variations in births and deaths. He also had a grasp of the technique of association.<sup>18</sup> His most fundamental insight was an explicit recognition that he was not the producer of these numerical observations. This had implications for data inaccuracy and incompleteness from the standpoint of the purposes he had in mind for using this data. For the most part he arranged the data to form numerical constructs which were a second step toward the development of indicators formulated in a way that "Government may be made more certain and regular". The searchers' figures could be used informally to furnish a "Text

15 Ibid., pp.398-421. Briefly described, one construct was based on a sampling of three parishes and the other designed on the basis of an area of habitation in conjunction with a guess at the density of habitation.

16 Ibid., p.387. Refer for a comprehensive discussion, e.g., to Sutherland, I., 'John Graunt: A Tercentenary Tribute', JRSS, Series A, 126(4), 1963, pp.550-52.

17 PEW, p.321.

18 E.g., he was able to show that a prevailing view that "great Plagues come with Kings Reigns" (p.369) was a myth; that there was an inverse association between christenings and burials both in London and in the country - years with more burials having fewer christenings and vice-versa.

to talk upon in the next Company", but Graunt envisaged that the 'raw' data could be organised as a step toward "greater uses" in public policy-making.<sup>19</sup> For instance, his skill with arranging numerical data helped quell any concern on the part of the authorities and public policy-makers, namely, "the Sovereign and his chief Ministers", that periodic outbreaks of the plague might threaten, in the long term, the value of houses in London (with all the possible consequences this might have for commerce and sources of public revenue). In a reassuring tone, Graunt infers from his figures that:

The time wherein the City hath been Re-peopled after a great Plague; ... we affirm to be by the second year. For in 1627 the Christenings (which are our Standard in this Case) were 8408, which in 1624, next preceding the Plague-year 1625 (that had swept away above 54000 were but 8299; and the Christenings of 1626 (which were but 6701) mounted in one year to the said 8408 ... Now the Cause hereof, forasmuch as it cannot be a supply by Procreations; Ergo, it must be by new Affluxes to London out of the Country.

So, using these figures as indicators, "it followeth, that, let the Mortality be what it will, the City repairs its loss of Inhabitants within two years; which Observation lessens the Objection made against the value of Houses in London, as if they were liable to great prejudice through the loss of Inhabitants by the Plague".<sup>20</sup> Graunt also goes far beyond figures furnished in the Bills to hand out moral warnings. Social disintegration was likely if people allowed themselves to act in "the most

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19 Ibid., p.347, 396, 333.

20 Ibid., p.321, 367-8.

dangerous way that leads ... into misery". Graunt also warned that some natural "Bars" to activities that threatened social order and morality - bars "which keep some men within bounds" and away from damaging sexual "extravagancies" - were liable to be overlooked if the Bills were actually taken at face value. For the purpose of strengthening this particular case, he saw fit to cast doubt on the figures relating to French pox: "in the aforementioned 229250, we find not above 392 to have died of the Pox. Now, forasmuch as it is not good to let the World be lulled into a security and belief of Impunity by our Bills, which we intend shall not be only as Deaths heads to put men in mind of their Mortality but also as Mercurial Statues", or indicators, as to proper, socially integrating, harmonious conduct. These figures, unlike most other returns left Graunt highly incredulous: "I wonder ... why so few died of it [?]".

Upon inquiry he maintained that many who died of the pox were incorrectly returned by searchers as those who had succumbed to ulcers, sores, or consumption. Further, he found "that all mentioned to dye of the French Pox were returned by the Clerks of Saint Gile's and Saint Martin's in the Fields only" - so searchers in other parishes were likely to have been coding incorrectly. He was convinced that bribery and corruption were behind all this:

only hated persons, and such, whose very Noses were eaten off, were reported by the Searchers to have died of this too frequent Malady ... In the next place, it shall be examined, under what Name or Casualty such as die of these Diseases are brought in: I say, under the Consumption; forasmuch as all dying thereof dye so emaciated and lean (their Ulcers disappearing upon Death) that the Old-women Searchers, after the mist of a

Cup of Ale, and the bribe of a Two-groat fee, cannot tell whether this emaciation or leanness were from a Phthisis, or from an Hectick Fever, Atrophy, &c. or from an Infection of the Spermatick parts.<sup>21</sup>

In this case, as in others, Graunt reinterpreted the numerical data and used it directly as an indicator to suit underlying moral suppositions.

Graunt further outlined the purpose behind his pioneering effort to design numerical constructs in the final section of his Observations. He set himself a rhetorical question: "To what purpose [did all] this bustling and groping [with figures] tend?". In other words, why all this concern with quantity - to know "The number ...? How many ...? How much ...? What proportion ...?". Answering in a bold, repetitive way, he firstly extolled the virtues of that kind of knowledge derived from numerical observations - knowledge of "intrinsick value ... where of Trade and Government may be made more certain and regular". Numerical data assisted the sovereign and his ministers to govern in a way attuned to regularities discernible in social life and economic activity. "[I]nference from the numbers" was intrinsically useful inasmuch as it indicated where there were opportunities for trade and where "Trade might not be hoped for". Numerical information could therefore be useful because it helped "preserve the Subject ... in Plenty". It could show whether or not consumption patterns indicated by "population, religion, age and rank" of the people in a particular

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21 Ibid., pp.356-57.

region or nation were "unfit subjects of Trade".<sup>22</sup> On the subject of trade and commerce, Graunt draws out by no means obvious practical consequences from some of the numerical data before him. The numbers are used as proxies for higher level, more abstract concepts. Consider the following summarised sequence of Graunt's argument: (i) Few people (51 out of 229250 observations) starve in London. (ii) There are (by casual observation) a "vast number of Beggars swarming up and down" the city. (iii) The capacity of national wealth must therefore be sufficiently large to feed these beggars (a first abstract conceptual referent for the numerical data). (iv) Beggars should not be forced to do useful work lest their product be shoddy and the Dutch, who already subsidise idlers, will gain British trade (a second referent making a judgement and prognosis about future consequences, originally from past data). The significance of this last conclusion is that lost trade meant a reduction in national wealth, presupposing that "there is but a certain proportion of trade in the world". This behooved each nation to compete fiercely for as large a share as it could secure of that putative fixed proportion. A second strand of Graunt's argument rested on the importance he attached to the role of quantification as a tool for use in the more general "Art of Governing". This strand casts the numbers into indicators relating to the problematics of social order. Thus, in paraphrase, his argument (v): Roaming beggars create disorder and defile city thoroughfares. Poverty has social

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22 Ibid., pp.355 & 395-97.



dysfunctions. The numbers show that beggars must be "maintained by voluntary contributions". However, in order that beggars might "live regularly and not in ... Debauchery", the State should keep them - making sure, of course, that they were not to earn anything or get involved in production for trade.<sup>23</sup> Following Hobbesian political theory, Graunt held the view that no division of sovereignty was desirable and that extensive controversy and dissent impaired the sovereignty of government. So one overriding use for numerical data was to help "preserve the subject in Peace" and "even to balance Parties and Factions both in Church and State". Implicit in Graunt's concluding remarks is the presumption that in numerical data is the potential for formulating a hard science generating "clear knowledge" free from controversy. The data could be interpreted as observable symptoms of, or reference points for, conceptions of prevailing social ends: economic prosperity and political and social order. It also identified possible means of attaining and preserving those ends in the future.<sup>24</sup> Graunt's mode of argument with figures was relatively novel. His ideas were not unusual at the time and many of them had been advocated some thirty years earlier by others.<sup>25</sup> It was his way of using numerical observations as indicators that was radically different. As Kendall remarked, Graunt "thought as we think

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23 Ibid., pp.352-54.

24 Ibid., pp.396-97.

25 See, for ideas anticipating Graunt on commerce, Buck, P.W., The Politics of Mercantilism, New York, Ontagon Books, 1942, pp.28-29; and on social issues, Hacking, Emergence of Probability, op.cit., p.106.

today, with the most imperfect materials ... [He] reasoned with the ... data".<sup>26</sup>

To conclude, this perspective of Graunt's 'shop-arithmetic' finds itself at odds with Hull's assessment which makes out an unconvincing case for the proposition that, instead of using figures to support conclusions which he already had in mind, "Graunt uses his numerical data as a basis for conclusions, declining to go beyond them".<sup>27</sup> Per contra, Graunt's statistical work was impregnated with values, veiled assurances and warnings to statesmen and individuals. Second, Graunt exhibited a bland optimism that social regularities evident in past data will, and indeed in some cases should, continue into the future. Third, he was more than a mere technical demographer.<sup>28</sup> He fused many of his formal numerical constructs with higher level concepts - fusions not altogether self-evident if viewed independently of philosophic-intellectual and socio-historical influences acting on Graunt at the time. These higher level concepts were evaluative ideas and for their part are analysable as elements of meaningful human interpretations of numerical data. Nonetheless, the 'validity' or 'truth' of these interpretations cannot be deduced from the data as such.

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26 Kendall, M.G., 'Where Shall the History of Statistics Begin?', [1960], reprinted in idem, ed., Studies in the History of Statistics, op.cit., p.46.

27 Hull, PEW, p.lxxv, my emphasis.

28 Buck, 'Seventeenth Century Political Arithmetic', op.cit., p.72. C.f., 'Greenwood, Medical Statistics from Graunt to Farr', op.cit., pp.74-82. Greenwood concentrates solely on Graunt's formal statistical constructs and highlights achievements and mistakes in the calculations; see also Sutherland, 'John Graunt', op.cit.

4.3(B) Petty's 'Number, Weight or Measure'

Petty provided more examples of the method he originally articulated. He proceeded to use numerical constructs in such a way that they became fully-fledged numerical policy indicators. His work for the Royal Society emphasised the utility of a Baconian method for government: "God send me the use of things and notions, whose foundation are sense and the superstructure mathematical reasoning; for want of which props so many Governments do reel and stagger".<sup>29</sup> This admonition marks a central theme of political arithmetic. Petty constantly strove to obtain numerical observations as a necessary precondition for designing inferences to posit cause and future consequence, in order to contribute to public policymaking. In particular, Petty intended to use these observations in order to discount the "fears of many concerning the Welfare of England" with views toward specific proposals for public policy reform and toward reducing social conflict which often arose out of fears about 'the facts' - fears which, in Petty's mind, had no substance.<sup>30</sup> In 1671 Petty suggested that an office for a "Registrar General of the People" be established in order to furnish the King with "a true state of the nation at all times". He later contended that a "King's Accountant" be appointed with similar functions. These functions were mainly to collect numerical observations about population; consumption of common food items; church

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29 Marquis of Lansdowne, The Petty Papers: Some Unpublished Writings of Sir William Petty, London, Constable and Co., 1927, p.111. (Hereafter this work will be referred to as LPP).

30 PEW, Pol. Arith., p.241.

revenues, houses, prisoners, wages, occupations, school attendances, accounts of trade and so forth. Petty sketched research rules for "enquiring into the state of any country".<sup>31</sup> On questions of designing enumerations, most of the names Petty chose as items of account (such as "wealth") were drawn from the language of everyday economic thought. Petty disliked reading books: "it is doubtful whether Petty had any acquaintance worth mentioning with such economic writings as existed in his day".<sup>32</sup> He "was a man of affairs and not a scholar" and this, coupled with "an instinctive familiarity with his subject-matter" made for pragmatic itemising.<sup>33</sup>

In a work preceding Political Arithmetick, Petty stated his conviction that

Ignorance of the Number, Trade and Wealth of the people is often the reason why the said people were needlessly troubled ...; ... (by reason of not knowing the State of the people, viz. how many there were of each Taxable sort, and the want of sensible marques whereby to rate men ...) ...<sup>34</sup>

His suggestions for the kind of marking (coding) schemes that had to be instituted to organise and disaggregate numerical observations were based on implicit economic-theoretic pre-suppositions. He did not propose to capture 'raw' data by the stabs of an index finger. Preconceptions and intuition were

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31 See LPP, pp.172-177.

32 Hull, 'Introduction' to PEW, p.lx.

33 LPP, p.218. See also Strauss, E., Sir William Petty, London, Bodley Head, 1954, p.178.

34 PEW, Treatise of Taxes [1662], p.34.

involved in his recommendations for the observation and counting of diverse economic phenomena. On the side of public finance he saw that a simple enumeration of existing "Titles of Faculties and Callings" was no measure of actual ability to pay poll money and could not be used to estimate how much tax could be raised. Further, any such tax carried with it "vast inequalities: since it led to "men of unequal abilities all paying alike and those who have greatest charge of children paying most". More often than not, "by how much the poorer [people] are, by so much the harder they are taxed". Overall Petty came out in favour of a straightforward count of the taxable population coded according to "Estates or Riches" upon which more proportionate universal taxation could be based. Though, Petty understood that with some qualifications and for purely practical, administrative reasons, hearth taxes would be the easiest to levy: "it being easie to tell the number of Harths". So, on the grounds of simplicity he was willing to concede that hearth taxes were "the fittest to ground a certain Revenue upon".<sup>35</sup> He also favoured greater reliance on expenditure taxes. His recommendations on taxation were designed to encourage the sovereign to levy more regular taxation that did not produce so much uncertainty for trade and commerce; that was less burdensome, more equitable

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35 Ibid., p.94. The possible source of Petty's compromise here is noted by Chandaman, C.D., The English Public Revenue 1660-1688, Oxford, Clarendon Press, 1975: "It is a suggestive coincidence that Petty's celebrated 'Treatise ...' ... supplied a reasoned justification of Hearth money as a method of taxation [and] was written ... when the Commons were still groping for a means of supply, and was published in the same month as the Hearth Tax grant." (p.78).

and as a consequence, less socially divisive.<sup>36</sup> On the side of public expenditure regulation Petty alluded to categories for numerical observation and aggregation as they related to certain, specific occupational groups ("titles and offices") that must be devised so as to count (a) the number of government officials - since the State's support of "the superfluous, supernumerary, and antiquated" could be a reason for a sovereign's high tax demands; (b) the number of lawyers, physicians, clergymen, merchants and retailers because an excessive number of these "vain pretenders" in service-related occupations did not always contribute significantly to "Art and Industry" and added further to public charges. In categories (a) and (b) Petty assumed the desirability of some officially determined maximum number. A third category (c) must elicit idle and redundant labour so that the State could proceed to direct such labour into wealth-creating, sometimes State, employment. He added a proviso to the effect that any State employment - be it the construction of "a useless Pyramid upon Salisbury plain" or removal of "the Stones at Stonehenge to Towerhill" - be "without expense of Foreign Commodities". Any public monies spent on this kind of employment or "expended on Entertainments, magnificent Shews, triumphal Arches &c" would then flow into the hands of tradesmen and other useful people at home. Petty was definitely keen on converting numerical observations into social indicators so as to help efficiently

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36 Ibid., pp.62-64, p.91 & 93. Hobbes' economic views on expenditure taxes anticipated, if not influenced, Petty's ideas, vide, Levy, A., 'Economic Views of Thomas Hobbes', JHI, 15, 1954, p.593.

allocate scarce public funds. For example, he argued for the use of mortality and morbidity data as criteria for regulating the number of medical physicians and nurses. His intention was to regulate the charges of universities and specifically, medical schools, on public funds. Thus he tried to show "the learned college of that Faculty" a way of calculating "how many Physicians were requisite for the whole nation" and "how many students of that art to permit and encourage". Mortality and morbidity data, in Petty's schema, were legitimate symptoms of the need for more (or less) physicians.<sup>37</sup>

The next logical step in the counting process (the "account of the people") - an elaboration of formal operational counting rules for actual statistical surveys of, for instance, trade, housing, unemployment or morbidity - is not to be found in Petty's writings. He appeared to rely on some commonly shared meanings among the enumerators. It is not apparent that Petty envisaged any of the difficulties Graunt discovered in the disparate meanings that could be placed on coding terminology, alternatively nominal scales, which directed enumerators' observations with all the implications this might have had for data quality.<sup>38</sup> Petty made no distinction between expert 'scientific' operational definitions for observation and commonsense definitions, agreed upon and known to everyday social participants through actual experience. His categories were the categories of human action, of politics, not those of a

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37 Ibid., pp.23-24.

38 Ibid., p.26ff.

'detached' intellect.<sup>39</sup> Petty worked in a period before the advent of more sophisticated techniques of statistical enumeration and statistical inference based on formal theories of probability. These techniques treated mass numerical observations in a more mechanical manner. According to Westergaard's reading of the history of statistics, the political arithmeticians were able to narrow the distance between observer and numerical 'fact' and this had "the advantage of keeping the observations themselves more in view".<sup>40</sup> Keynes' more general comment that seventeenth century writers reasoned in close combination with their observations or "actual experiences" also upholds Westergaard's contention.<sup>41</sup> Petty furnished illustrations of his method largely in the absence of what he considered completely satisfactory numerical records. He made use of any figures that came to hand including haphazard returns from various tax offices (hearth, excise and customs data) and a scattering of birth and mortality data. He did not elaborate on the inadequacies of his data and in many cases simply imagined that if satisfactory numerical observations were available they could be used as indicators for the purpose of ultimately showing "the weight and importance of the English Crown". He aimed to "shew the way to a new kind of

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39 Refer to Appleby, J.O., Economic Thought and Ideology in Seventeenth Century England, Princeton, Princeton University Press, 1978, p.258.

40 Westergaard, Contributions to the History of Statistics, op. cit., p.272.

41 See, Keynes, J.N., The General Theory of Employment, Interest and Money, London, Macmillan, 1936, p.346ff.



Knowledge" derived from "ratiocination": the process of drawing general, meaningful policy conclusions from formal numerical constructs.<sup>42</sup> Despite the fragmentary observations made available by the government of the day Petty was able to provide suggestive, illustrative examples of his method. Petty's Treatise of Taxes and Contributions [1662] contained much of the rationale for his concern for "number, weight and measure". He maintained that "wealth" was a key consideration in any fiscal system. There were obstacles, however, that "aggravate the several sorts of Publick Charges" needed to finance public spending and one of these was "ignorance of the Number, Trade and Wealth of the people". Without knowledge of this sort the Prince may not be able to assess what taxes his people could bear. Quantitative observations of population; wages of labour in various occupational groups; the number of estates and their livestock and produce; those "who live with little labour"; seasonal variations in wealth and trade and the amount of money in circulation were all necessary for the statesmen so he could levy equitable taxes. A dearth of satisfactory numerical records often precipitated a "Prince's hardship towards the people", burdensome taxation, social oppression and social conflict. Petty argued that quantitative information could also help avoid wars with foreign powers. In his view, wars were often entered into on the grounds of mistaken inferences about the resources of England and use of a Prince's territory (in a geographical sense) as a measure of wealth. However, the true

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42 PEW, Pol. Arith., Lord Shelbourne's 'Dedication', p.239.

"greatness ... of a Prince lies in ... the number, art and industry of his people, well united and governed".<sup>43</sup> An overriding theme in Political Arithmetick was to show that a conception - "the power and wealth of England" - could be compared with that of other nations such as France and Holland using numerical data on population, housing, shipping, rents, interest rates and so on, from the respective countries. At least, Petty tried to persuade statesmen, even in the absence of sufficient data, that these measures were appropriate proxies for "power and wealth"; that they should become conventions for the purpose of comparison. However, he does not adumbrate a way of weighting this data in any sort of overall index.<sup>44</sup> In all, Petty was concerned to determine his country's resources and its capacity to survive both internal and external conflict in a quantitative way. His central intention, when converting numerical observations into statistics and then indicators, was "to quiet men's Minds".<sup>45</sup> Without this purpose, the statistics would not have been transformed into fully-fledged numerical indicators with meaningful consequences and implications which ran far beyond operations used for constructing the figures.

Petty first undertook to estimate England's wealth in Verbum Sapienti [1664]. Petty formulated a theory of wealth and this became evident as he built up the numerical construct. He presupposed that wealth was a stock of consumable goods and their

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43 Ibid., Treatise of Taxes, pp.21-2.

44 Ibid., Pol. Arith., pp.302-5.

45 Ibid., Verbum Sapienti [1664], p.117.

means of production. Production was the creation of all useful, tangible things and intangible services. Money was found to be the most convenient common denominator for all these constituent items. As Studenski perceived, Petty's theory

was a natural extension to the nation as a whole of the well-established custom in those countries of comparing wealth and power of individuals of substance or rivaling houses of nobility in terms of their yearly income. From the very beginning, this broader concept took on the specific meaning of the monetary value of the nation's annual production and consumption.<sup>46</sup>

Petty identified "the wealth of the Kingdom" with its counting definition - national outlay: "the Annual Expense of the People". Every man, woman and child consumed "Food, Housing, Cloathes and other necessities". Petty averaged out the expense per capita to 4½d. per diem or £6.13s. 4d. per annum. Estimating population at 6 millions (from Graunt's data and via hearth tax data),<sup>47</sup> he arrived at a total expense of £40 millions. His estimate in terms of per capita average times population apparently included an imputed money value to household production in addition to market transactions. On the income side he estimated that £15 millions came from property. He obtained this by estimating the annual income from each category of property (agricultural land, houses, cattle, shipping; coin, gold and silver; 'other' viz., "wares,

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46 Income of Nations, op.cit., p.11, my emphasis.

47 On how Petty might have obtained and cross-checked population data, see Hull, 'Introduction', PEW, p.lxvii.

merchandise, utensils of plate and furniture"). He decided that £8 millions came from land, £2.5 millions from house rentals and £4.5 millions from all other property. Petty then concluded that "the labour of the People" must give rise to the remaining £25 millions. He assumed that one half of the estimated population earned on average 7d. per head, (taken from a division of six wage earning classes earning from 2d. to 12d. per day for labour), and "abating the 52 Sundays and half as many other days for accidents, Holy days, sickness, recreation, & C". These factors thus considered the remaining £25 millions could be accounted for.<sup>48</sup> On the basis of fragmentary records coupled with heroic guesses, some crude averaging and preconceptions regarding the wealth-creating power of labour Petty became "the true originator of the concept of national income".<sup>49</sup> Theory was used to combine numerical information so as to construct this statistical estimate. On the surface 'theory' consisted of a simple technique of averaging with all its associated assumptions. Below the surface was a preconceived idea that labour produced a large share of wealth; or to use his favourite epigram: "Labour is the Father and active principle of Wealth, as Lands are the Mother".<sup>50</sup> This, Petty thought, was an underlying natural law that careful use of

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48 Ibid., Verb. Sap., pp.107-12.

49 Studenski, Income of Nations, op.cit., p.13; vide also Kendrick, 'Historical Development of National Income Accounts', op.cit., pp.284-87.

50 PEW, Treatise, p.68. This idea also appears in 'Graunt's' work: "Hands being the Father, as Lands are the Mother and Womb of Wealth", PEW, Observations, p.377.

available numerical observations was to reinforce. (When no concrete numerical data existed Petty extended his 'number, weight or measure' into a study of hypothetical, quantitatively abstract relations in the economy. This is where he made some pioneering contributions to the theory of political economy. He outlined a labour theory of value from which he attempted to explain numerical relationships between different economic phenomena: between the quantity of a product and the quantity of silver for which it would exchange; between wages and rent; between the price of land and the rate of interest and so on. He recognised and tried to deal with the problem of a universal measure of value. That is, he wanted to find a general measure by which the actions of the forces of nature and the labouring activity of man could be compared. This rested on the problem of what he called "the par ... between lands and labour so as to express the value of anything by either alone". Petty considered this to be one of the "most important considerations in political oeconomies". It must be emphasised, however, that all this work in areas of quantitatively abstract 'statistics' was not linked even indirectly, to any concrete enumerations).<sup>51</sup>

When concrete numerical data is not used it is possible to agree with Letwin, that writers like Petty were able to distance the technical aspects of economics from moral aspects. Petty was "willing to consider the economy as nothing more than an intricate mechanism, refraining for the while from asking

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51 PEW, Political Anatomy of Ireland [1672], p.181.

whether the mechanism worked for good or evil".<sup>52</sup>

Petty's immediate purpose when reasoning from actual enumerations was to advance judgements and generalisations based on his arithmetic. Statesmen were encouraged (1) to increase England's manpower since the productive power of labour was a key source of wealth<sup>53</sup> and (2) to levy a proportional 10% income tax on all property owners and employed people that would obtain an annual public revenue of £4 millions - which was sufficient for peacetime needs and in excess of the current amount being raised. At the level of numerical constructs his estimate of wealth also enabled calculation of the amount of "Money sufficient to drive the Trade of the Nation". Again, he started with a preconceived notion, this time with an analogy from anatomy: "money is but the Fat of the Body Politick, whereof too much doth as often hinder its Agility as too little makes it sick". His numerical construct of the necessary money to pay £40 millions per annum outlay was also dependent on assumptions about "revolutions and circulations" of the amount required to implement this annual expense. For poorer people who were paid once a week "then 40/52 parts of £1 millions of Money" would suffice. But speculating more generally, if "they had a mixed Circle between One week and 13, then add £10 millions to 40/52, the half of which will be  $5\frac{1}{2}$ , so as if we have £ $5\frac{1}{2}$  millions we have enough" (apparently rounding weekly

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52 Letwin, The Origins of Scientific Economics, op.cit., p.148.

53 Petty had anticipated this well before making any calculations and inferences from numerical data, e.g., Treatise [1662], PEW, p.34.

settlements of 40/52 up to £1m.).<sup>54</sup>

In addition to building up preconceptions of underlying economic laws, Petty employed political arithmetic to work-out causes and effects of general social habits and practices. To illustrate: he arranged numerical observations to form statistics and then turned them into social indicators which were based on undeveloped cause-effect relations. First, in comparison to London, where according to the records available two out of sixteen people died in hospital, a larger proportion of the Paris population died in hospital (two out of fifteen). This construct was then used to indicate that the institutions and practices of physicians and others in London's hospitals were "better and more desirable than those of Paris". Second, it may also indicate that "the Air of London is more wholesome". Third, fewer people chose to use hospitals in London than in Paris; that is to say, "the numbers of those at London who choose to lie sick in Hospitals rather than in their own Houses are to the like People in Paris as one to twenty". So this indicates a cause, viz., "Poverty or want of Means in the People of Paris than those of London".<sup>55</sup> In these instances, Petty designed and used statistics to help create rather coarse theories of association. In the hands of Petty statistics were transformed into analytic instruments. He used them to define the problems or issues at hand and in turn, to reinforce (possibly preconceived) hypotheses about those problems. The

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54 Ibid., Verb. Sapienti, pp.110-113.

55 Ibid., Five Essays in Political Arith. [1687], p.508.

resulting coarse theories may not have been formulated in the same way if numerical data was not available. Of course, this is not to say that theories about conditions in respective hospitals could not have been propounded without numerical information. Petty thought that numbers reduced controversy and gave a more solid, certain basis for argument - no matter, it seems, how spurious the statistics or presuppositions.

Petty's "specimens of political arithmetic" were more valuable for the method espoused rather than the estimates proffered. As Hull wrote after surveying Petty's contribution:

The data of statistics do not now, nor did they ever present themselves spontaneously for scientific elaboration. In order therefore that legal provision should be made, and that money should be forthcoming, for their ascertainment, it was first necessary that the value of ... statistical deductions ... be demonstrated by the intelligent use of those sparse materials which lay ready to the student's hand.<sup>56</sup>

Petty's method was part of an intellectual movement that persuaded future political arithmeticians to obtain access to, - and collect more - numerical data and to develop the art of reasoning with constructs designed therefrom. These constructs became policy indicators because they were used as interpretive instruments to be approved to statesmen. Petty saw the design of formal numerical constructs as an indispensable preliminary to practical and effective policy reform. The constructs, based on theory, were organised to demonstrate the desirability of reforms which he thought appropriate. Again, quoting Hull, when

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56 Ibid., 'Introduction', p.lxvi.



Petty "could not ascertain directly the number ... of some phenomenon in which he was interested, he reckoned out what he desired to know, upon the basis of what he already knew". To Hull's last phrase could be added 'or what he already considered self-evident 'truth'. Petty invariably "appears to be seeking figures that will support a conclusion which he has already reached".<sup>57</sup> Needless to say, the foundation of his indicators was by no means beyond dispute as the passage of time was to reveal. First, Political Arithmetick had a direct political orientation that "Charles II may if he will, make himself independent of the bribes of Louis XIV". At the time it was understood that this work had offended the King of France, and it remained unpublished as a consequence until after Petty's death.<sup>58</sup> Second, one of his followers, Charles Davenant, pointed out that the customs and hearth data which Petty utilised were incomplete. As well, Davenant alleged that "the great genius" often compromised his arithmetic in order to please the King. Petty "rather made his court than spoke his mind"; he often advanced a proposition "not quite right in itself, but very grateful to those who governed". As a result of Petty's patronage, "the King was ... lulled asleep by a flattering council". Having suggested that the power of England was so much more formidable than France, English officialdom was lulled into a "lethargy which ... since proved so fatal and expensive". Writing about the subsequent war with France,

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57 Ibid., p.lxxv, lxvii, my emphases.

58 See Ibid., p.lxi and Pol. Arith., 'Dedication', by Shelbourne, pp.239-40.

Davenant went so far as to suggest that blind faith in Petty's arithmetic (which was privately circulated on an extensive scale during the 1670s and 1680s)<sup>59</sup> eventually "proved pernicious to the interest of England".<sup>60</sup> Finally, while Petty tried many examples to illustrate the value of reasoning with numerical observations Letwin wrote that Petty "could never quite get over the daydream of this childhood social science" that proofs of favourite hypotheses

lie hidden in the facts impatient to reveal themselves to anyone who will trouble to collect enough facts and search out their deeper meanings. Petty should not on this account be regarded as an antique comic. The daydream has not yet completely faded from the minds of social scientists.<sup>61</sup>

#### 4.3(C) Davenant On 'The Use of Political Arithmetic'

Improvements in, and access to tax records in England, particularly the graduated poll tax, encouraged Charles Davenant (1656-1714) to cultivate an interest in political arithmetic. For a time, Davenant was a consultant administrator with access to a variety of official statistics. Davenant also had access to Gregory King's (1648-1712) estimates of population and

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59 On this see Ibid., pp.236-8.

60 Davenant, C., Discourses on the Public Revenues and on the Trade of England [1698], Vol.I of C. Whitworth (ed.), The Political and Commercial Works of that Celebrated Writer Charles D'Avenant, (London, 1771), Reprinted, London, Gregg Press, 1967, pp.127-130. (Hereafter Davenant, Works). Davenant's international comparisons of wealth (using Gregory King's estimates) repudiating some of Petty's contentions can be found in Davenant, Two Manuscripts [1693-6], Usher, D.P., ed., Baltimore, John Hopkins University Press, 1942, pp.82-95.

61 Letwin, Origins of Scientific Economics, op.cit., p.138.

national wealth. Davenant popularised King's work long before the latter had published his results.<sup>62</sup> King followed Petty's conceptualisation and method of calculating national wealth in his measures for 1688 through 1695 (with projections to 1698). He added a category to account for yearly increases or decreases (usually due to war) in national wealth or "stock". By differentiating between annual expense and income - a distinction notably prompted by the availability of production data as well as consumption figures - he was able to make an advance on Petty's calculations by categorising the difference between expense and income as a change in "stock".<sup>63</sup> Unlike Petty, King was "cautious in his conclusions. He allowed the figures to tell their own story, adding only a minimum of generalisations".<sup>64</sup> The following discussion of Davenant's theory concentrates on his conversion of King's data into indicators.<sup>65</sup>

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62 On Davenant's life and times see Waddell, D., 'Charles Davenant (1656-1714) - A Biographical Sketch', EHR, 11(2), 1958, pp.279-88.

63 King, Natural and Political Observations and Conclusions upon the State and Condition of England [c.1696], reprinted in Barnett, G.E., Two Tracts by Gregory King, Baltimore, John Hopkins Press, 1936, pp.127-8.

64 Studenski, Income of Nations, op.cit., p.30.

65 Davenant probably made use of King's data in some of the latter's piecemeal drafts of the Natural and Political Observations that he had access to. In following references to "King's" data it is often the case that the data is not strictly that which appears in King's final manuscript. As well, even in the final manuscript, "King felt very uncertain of his data" and it is likely that "Davenant knew of this uncertainty and [sometimes] revised the data according to some ideas of his own". See Evans, G.H., 'The Law of Demand - The Roles of Gregory King and Charles Davenant', QJE, 81, 1967, p.490.

For Davenant, political arithmetic was an art "not commonly practised" by his contemporaries. This art of "reasoning by figures upon things relating to government" was purportedly cultivated originally by Xenophon. Xenophon "was the first author that ever argued by Political Arithmetic".<sup>66</sup> Davenant employed this art of reasoning by figures to help persuade statesmen about the state or tendency of some key social, economic and political concerns. In the abstract, these concerns included concepts such as:

- (1) how to "preserve gains in the balance of trade";
- (2) how far to escalate a war in the light of a concern to ensure civil order and public safety at home;
- (3) how to compare the relative "strength of nations";
- (4) how to distribute "the burden of taxes" so as to ensure requisite public finance and at the same time preserve "public order".

In his reasoning Davenant approached an understanding of the 'law of large numbers'. He recommended "computing upon the whole" by which he meant that:

He who will pretend to compute, must draw his conclusions from many premises; he must not argue from single instances, but from a thorough view of many particulars; and that body of political arithmetic, which is to frame schemes reduceable to practice, must be composed of a great variety of members.<sup>67</sup>

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66 Davenant, 'Discourses on the Public Revenues and on the Trade of England' [1698], Part I, 'Of the Use of Political Arithmetic', Works, op.cit., Vol.I, p.128 and 'Introduction' to Xenophon, 'A Discourse Upon the Revenue of the State of Athens', ibid., p.307.

67 'Political Arithmetic', ibid., p.135 and 145.

The raw numerical material required to assist this "computing facility" were the customs ("the very pulse of the nation"), hearth money and excise records, as well as records relating to contemporary duties on marriages, births and deaths. He was satisfied that the "wonderful genius", Gregory King had corrected and arranged these returns to form numerical constructs.<sup>68</sup> Davenant set about using them as a "footing to fix [his] reasonings upon". Davenant's subsequent discourses on the figures were, he maintained, the result of "much Thought and Strict Inquiry" and provided material for sound decisions on the part of statesmen.<sup>69</sup>

In his "An Essay on the Probable Methods of Making a People Gainers in the Balance of Trade" [1699] Davenant employed King's population data to create indicators: especially to build up a conception of the legitimate meaning of 'gainer in the balance of trade'. A presumption underlying his discussion is that it was of prime importance for England to have an excess of commodity and non-commodity exports over imports. A country such as England, with few gold or silver mines, had to aim to increase its stock of precious metals by way of trade. "Gain" in this context entailed an increase in the stock of precious metals.<sup>70</sup> Davenant's evaluation of the figures was coloured from the outset by this particular pre-analytic 'vision' (in the Schumpeterian sense) of this most desired end for all economic activity. The economic process (and likewise all reasoning with figures) had to be managed judiciously so as to achieve this end.

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68 Ibid., pp.136-38.

69 Davenant, Two Manuscripts, Usher, ed., op.cit., p.82 and 94.

70 Davenant, Works, II, pp.165-382.

TABLE 4.1

Davenant's "Scheme of the Income and Expence of the Several  
Families of England, calculated for the Year 1688"

Please see print copy for image

Source: Davenant, Works, Vol.II, p.184.<sup>71</sup>

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<sup>71</sup> For a complete breakdown of King's methods of estimation see Studenski, Income of Nations, op.cit., p.32f; and on his means of estimating population see, e.g., Glass, D.V., 'Gregory King's Estimate of the Population of England and Wales, 1695', Population Studies, 3(4), 1950, pp.340-41.

Figures in Table 4.1, originally compiled from King's work, left open many ways for Davenant to examine which particular class was "a burden" and which "a profit" to the public. He intended to work out a way of making those who were considered burdensome somehow "useful to the whole" so that the "nation will be more and more a gainer in the Balance of Trade". The main thread of his argument and the circumstances behind it, were then set out:

And though war, and national debts and engagements might heretofore very rationally plead for excises upon home consumption, yet, now there is peace, it is the concern of every man that loves his country, to proceed warily in laying new ones, and to get off those which are already laid, as fast ever he can.<sup>72</sup>

Now, "if we look carefully into all the distinct ranks of men enumerated" in Table 4.1, Davenant said we infer that the majority of people chiefly depend for subsistence on a minority because about 500,000 families contribute to supporting around 850,000 families. He then uses these numbers to suggest that, firstly, new taxes on consumption goods "must of necessity retrench" the families and expenses of nobility and gentry. This, in turn, would have negative spin-off effects for the employment of "degrees below" gentry viz., merchants, tradesmen, artisans and others who, according to the data, contributed a high proportion to wealth creation. The figures were to become symptoms of latent, unobservable 'risks' associated with indirect taxes which had the potential to threaten wealth creation and employment. (Of course, this argument ignored the

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72 Davenant, Works, II, p.197.

uses to which indirect tax revenue might be put). Davenant felt he only needed to justify the use of King's data in this way by appealing to the balance of trade. Indirect taxes so his argument proceeded, usually fell on commodities like malt, coal, salt and leather. From casual empiricism he contended that these were important budget items for servants, workmen and artificers and if taxed, would necessitate a rise in their wages since their "income must bear some proportion with expense ... or they cannot live". Considering the numbers in these classes, rising wages would increase the price of domestic labour relative to foreign labour with many deleterious consequences for the balance of trade. The latter was invoked as a "chief object of the public care". Davenant believed he need not argue the point further. He had used the numbers to show that "excises either directly or indirectly fall upon the whole body of the people". The figures supported a chain of reasoning; they pointed to actions which should not be proceeded with now or in the future because they could threaten the balance of trade.<sup>73</sup> Davenant prefaced his next argument from Table 4.1 with the announcement that "the bodies of men are without doubt the most valuable treasure of a country". It was, moreover, "the number well employed" that made a country "powerful". Like Petty his presuppositions were that plentiful labour supply was a necessary though not a sufficient condition for invention and industry. As well, like Petty, Davenant was concerned to obtain and use numerical observations to ascertain the extent of employment and to identify those who were not well-employed.

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73 Ibid., pp.198-201.



Whereas Petty had few numerical observations, Davenant was able to reason from King's data. First, he reasoned that persons in lower ranks/ classes "being more in number" had the potential to "do more [to] contribute to an increase in the nation's wealth than [those of] higher rank". For Davenant the numbers in lower ranks referred to a potential to facilitate greater 'art' and thence wealth. 'Art' was a factor of production - a kind of technology which helped combine other factors and changed the relationship between input and output.<sup>74</sup> According to Johnson,

[t]o the extent that art constituted a factor of production, the nation was provided with an additional means of growing rich. Indeed, not only was the productivity of this technology an addition to national ... wealth, but the "art" itself was wealth, since it could be capitalized on the basis of its separate yield.<sup>75</sup>

To cite Davenant himself on this: "We shall ... say that maritime knowledge, improvement in all kinds of arts, and advances in military skill, as also wisdom, power and alliances, are to be put into the scale when we weigh the strength and value of a nation".<sup>76</sup> Davenant thus advanced well beyond King's population and income figures and adorned them with a theory of wealth creation. The figures show, as a whole, a potential for future wealth creation - this potential being legitimated by a theory linking training in arts and skills with wealth. Davenant

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74 Ibid., pp.201-3.

75 Johnson, Predecessors of Adam Smith, op.cit., p.274.

76 Davenant, 'Discourses on the Public Revenues', Works, I, p.381.

singled out cottagers, paupers, beggars and vagrants as a "burden to the public" since they were not endowed with useful arts. These lower ranks did not add something to the "nations general stock" and had to be supported by the savings of higher ranks. This last point is evident in the way King presented his data in Table 4.1. On the other hand there is quite a leap from what is immediately evident in the method of data presentation to arguments turning on what the numbers mean in terms of potential wealth creation and means of wealth creation. One of these means, for instance, "laws against voluntary idleness" was on no account obvious from the numerical data. In this and other instances Davenant used numerical data for support and reinforcement of his abstract preconception that labour was a "valuable treasure". The numbers were not used to 'test' his propositions. Instead they were used as a starting point, with much faith and personal judgement, in his "continuing hunt after truth".<sup>77</sup> When publishing Gregory King's figures in full and for the first time in 1786, Chalmers regarded King's work as that of an "unconcerned spectator". He noted Davenant's publication of some of King's work prior to 1700, but heavily discounted this work because Davenant "was no unconcerned spectator of those eventful times".<sup>78</sup> Despite this, beneath what may be considered Davenant's simplistic reasoning from King's figures was a capacity for analysis which might go unnoticed if charges of partiality are carried too far. For

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77 Works, II, pp.202-3 and I, p.193.

78 Chalmers, G., An Estimate of the Comparative Strength of Great Britain, London, John Stockdale, 1786, p.68; see also p.42.

example, argument from the numerical data against consumption taxes reveal an ability to discern linkages in the economic process. Davenant's evaluation of the data recognises a possible trade-off resulting from a policy action designed to augment public revenue but indirectly conflicting with the objective to gain by international trade. Analysis still functioned within the confines of his image or vision of the economic process.

Davenant's overriding desire was to use statistics, however bogus, to legitimise policy recommendations about improving the trade balance - purposes not obvious from the way King compiled his data. Davenant wanted to reinforce the view that all forms of public revenue raising were not beneficial to trade. He developed a prolix case against duties on imported salt which he found to be "so dangerous to the very being of trade". He saw that duties may push up labour costs. More important was "the more general prejudice" such a duty "may bring to navigation [which] is ... of much higher consequence". He reproduced some figures from King's calculations to assist his reasoning in this regard and never questioned their credibility.<sup>79</sup> (See Table 4.2).

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79 According to Westergaard, Contributions to the History of Statistics, op.cit., "Davenant is unable ... to find the limits within which the numbers are probably lying" (p.41). C.f., a critical evaluation of faults in trade statistics presented by Davenant in Davis, R., 'English Foreign Trade 1660-1700' [1954] reprinted in Minchinton, W.E., ed., The Growth of English Overseas Trade, London, Methuen, 1969, pp.85-90.

TABLE 4.2

Davenant's "Scheme of the Naval Trade of England for 1688"

Please see print copy for image

SOURCE: Works, Vol.II, p.270.

Davenant approvingly recited from this table that in the case of  
freight:

	1.
That our gain upon the balance by	
our own shipping, being ... ..	1,250,000
And our loss upon the balance by	
foreign shipping, being ... ..	<u>550,000</u>
The encrease of money, or adequate	
treasure, by the Balance of Trade	
in general, might be, anno 1688,	700,000

Davenant (and King) were interested in the balance of payments in the sense of a net balance of obligation payable in specie. Payment on account of shipping freight was therefore recognised as having the same significance as commodity trade.<sup>80</sup> On the surface this reproduction of King's figures seemed rather otiose. It led to an anticlimax since there was no way Davenant was able to show in numerical terms what the imposition of a salt tax would do to trade or the balance of payments. In his reproduction of figures and in his associated verbal interpretation it appears that Davenant more than hoped to highlight the relatively small £700,000 against all the other figures in King's table. At least the drift of his argument is that duties on salt could whittle away any gains from shipping freight - gains not easily 'visible' when viewing commodity trade only. He argued that meat was a principal shipping expense so that "the duty on salt makes victualling a very heavy burden". An air of expectation then entered into the argument. Davenant maintained that it would be only a matter of time before "foreigners, observing how dear freight is with us, will trade in their own ships". Moreover, English merchants would neglect to look after "those gains which England was want to

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80 This is consistent with Viner's understanding of the analytic capabilities of writers such as King and Davenant: "The mercantilists have sometimes been charged with failure to see that the international balance does not consist only of commodity exports and imports, and many suppose that the "invisible items" are a recent discovery. But most of the important writers of the seventeenth and eighteenth centuries took care to point out that allowance must be made for non-commodity items in explaining the net balance in bullion". Viner, J., Studies in the Theory of International Trade, New York, Harper Brothers, 1937, p.13.

make by freight" and instead be concerned with "their own temporary profit". So the "dangerous consequences of the Salt Duty" are shown by figures (albeit over ten years old) which identified an invisible trade gain at a time when no duty was imposed.<sup>81</sup> Davenant did not seem to recognise the significance of the historical nature of numerical data and the limits this may have had on his inferences. Given contemporary analytic understanding of forces at work in international trade, King's numerical construct (£700,000) became part of an explanation about the impact of duties on the totem-like "balance of trade" and beyond that, the "interests of the realm". Davenant's interpretation of the meaning of King's figures was justified by his appeal to what was almost a convention in a large portion of the pamphlet literature at this time viz., the beneficence of self interest and market processes in economic affairs. It was a belief that "there existed a naturally effective regulator which would enhance the nation's economic well-being better than man-made laws".<sup>82</sup> The numerical data played an important evidential role here. They were not used simply because of a new-found fashion or enthusiasm for bandying about commercial statistics. They were used as legitimators of presupposed cause-effect relations in economic life - and specifically in

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81 Davenant, Works, II, pp.273-75. The figures Davenant produced for the year 1688 were exclusive of the effects of any salt duty. According to Chandaman, English Public Revenue, op.cit., p.362, duty on salt ceased in 1682 (and was not reimposed until the late 1690s).

82 Appleby, Economic Thought and Ideology in Seventeenth Century England, op.cit., p.193. C.f., Grampp, W.D., Economic Liberalism: The Beginnings, New York, Random House, 1965, Chapter 2, 'The Mercantilists as Liberals', pp.48-97 and Pocock, J. 'To Market, to Market: Economic Thought in Early Modern England', Journal of Interdisciplinary History, 10(2), 1979, pp.303-09.

this case, in international trade and payments. Thus Davenant frequently expressed the following view:

Trade is in its nature free; finds its own channel, and best directeth its own course: and all Laws to give it rules and directions, and to limit and circumscribe it, may serve the particular ends of private men, but are seldom advantageous to the public.<sup>83</sup>

What status did the numerical observations have in all of this? Why did Davenant even see fit to use them? He interpreted the £700,000 in this particular case, to stand in for "a gain in the balance of trade" - the number was an observable effect of freer trade without a salt duty. That gain to the public which automatically ensued from free trade was, for Davenant, a predictable but unobservable phenomenon. As Appleby wrote, Davenant was not unlike many other economic analysts of the late seventeenth century who dogmatically asserted the supremacy of free trade; he was "laying the groundwork for a science of economics based on traditional laws" of self-interest and economic freedom and was also legitimating those practices at the same time.<sup>84</sup> Numerical data became observable dependent variables of these underlying economic 'causes'. Davenant displayed an ability to look beyond surface manifestations (numerical data) into first causes. Back of the numbers, then,

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83 Davenant, 'An Essay on the East-India Trade' [1697], Works, I, pp.98-9. C.f., also p.345f; and Vol.II, p.267, 284-5 passim.

84 Appleby, Economic Thought and Ideology, op.cit., p.193. See also pp.82-3. C.f., also Mini, Philosophy and Economics, op.cit., p.37.

lay venerable and traditional beliefs about 'causes' in economic affairs. Davenant gave the statistics meaning when he interpreted them within the framework of higher-level theoretical constructions. The statistics did not stand mute; they became indicators - a necessary part of his vocabulary of legitimation. In order to reason from past data to underlying regularities and to likely future consequences Davenant was dependent on indeterminate a priori judgements. These judgements did not suggest themselves from the numerical data at all - they were brought in from 'outside'. Davenant conferred on King's data meanings which talked-up and reinforced what he (and others) thought were fundamental economic 'truths' of the time.<sup>85</sup>

Reflecting an anti-Cartesian outlook, Davenant was sure that the extent of sheer faith in construction of indicators could be lessened by active participant observation. Numerical data was used as a first stepping stone in reasoning but the direction of each subsequent step was at least partly determined by Davenant's requirement that indicators point to schemes reduceable to practical economic policymaking. (Hence his preoccupation with matters of trade and public finance). Political arithmetic was of no use to statesmen if it was "a

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85 Davenant's personal interests in seeking a job along the customary lines of patronage cannot be separated from this 'talking up' process. Prior to 1699, Davenant was an unqualified harbinger of free trade - for personal reasons. On the role of personal interests in Davenant's work see Coleman, D.C., 'Politics and Economics in the Age of Anne', in idem et.al., eds., Trade, Government and Economy in Pre-Industrial England, London, Weidenfeld and Nicolson, 1976, p.190 and pp.200-201. C.f., also Waddell, D., 'Charles Davenant', op.cit., p.282 passim.



matter of mere speculation". In order to "form a sound judgement" when reasoning with figures, a practitioner of political arithmetic

must look into all the public revenues; ... understand something of their management; ... not be a stranger to the product ... of any country and place; ... and only from this general view he must frame any scheme that may be useful to the public ...<sup>86</sup>

Davenant repeatedly assured that his "Computations ... are not made at Random"; they are not "matters of mere fancy ... but the Result of long Observation".<sup>87</sup> Davenant's emphasis on narrowing the extent of speculation in using available numerical data by supplementing reasoning with participant observation contrasts with Heckscher's sweeping judgements that "'political arithmetic' [was] ... more often dependent upon arbitrary generalisations ... or without any factual basis whatever", and that "[t]here are no grounds whatsoever for supposing that the mercantilist writers [evidently including Davenant] constructed their system - with its frequent and marked theoretical orientation - out of any knowledge of reality however derived".<sup>88</sup> In order to use numerical constructs such as those designed by Gregory King, Davenant understood that meaningful interpretation was contingent on a political arithmetician remaining conversant with his subject-matter. Interpretation of

86 Davenant, Works, Vol.II, pp.145-46.

87 Davenant, Two Manuscripts, op.cit., p.93. Also Works II, p.146 passim.

88 Heckscher, E., Mercantilism Vol.II, Revised 2nd edn., London, Allen and Unwin, 1955, p.346 and 347.

numerical data was not to be carried to an Olympian level of abstraction. For Davenant, numerical observations had to be used in conjunction with a "general view" gained from participant observation, and even then, resulting inferences or indicators were only "probable conjectures" that "may be useful".<sup>89</sup>

In conclusion, this short study of Davenant's conversion of King's statistics into indicators illustrates the point that the meaning of numerical observations is a function of context. A study of numerical indicators is a study of the active interpretation of numerical data. Such a study cannot be carried out without at least partially discussing the socially constructed, intersubjective reality - the prevailing systems of ideas, concepts or unobservable variables - through which the mind of the user was organised and through which the data was interpreted.

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<sup>89</sup> Davenant, Works II, p.134.

#### 4.4 CONCLUSION: FROM STATISTICS TO INDICATORS

One superficial conclusion from the preceding literary studies of political arithmetic might be that the creation of numerical indicators involves three distinct steps:

Step (1): collection of 'raw' numerical data from the domain of sense experience according to operational counting rules;

Step (2): development of formal statistical constructs based on explicit theory; and

Step (3): valuation and meaning conferred on statistical constructs when they are applied in conjunction with reasoning and judgement to 'show something' or legitimise a public policy etc., in a specific time-place context.

It may seem that only at Step (3), according to this conclusion, that a statistic becomes an indicator. However, this conclusion is clearly an oversimplification. While it is possible to distinguish analytically between different steps, this neglects the interactive feedback process between the steps. Each step in the schema above was interrelated in the work of the political arithmeticians. Often valuations led to a particular way of manipulating and arranging numerical data before meaningful conclusions were drawn from them. In most of Graunt's examples and even more evidently in Petty's work, formal statistical constructs were preceded by 'favourite hypotheses' about what the data would or should show. Petty's suggestions for the collection of certain kinds of data were entirely of a piece with the ideas he had in mind for its use or application. In practice, then, the three steps outlined above cannot be neatly separated. Further, in the practice of

seventeenth century political arithmetic there is a continuous movement back and forth between the steps in transforming statistics into indicators. This interactive process is especially notable (as a short term phenomenon) between Steps (2) and (3) (since data collection procedures could only be altered over the long term). Graunt identified elements of Step (3) (intersubjective valuations/agreement) in the way searchers collected and coded data (at Step (1)). Judgements are especially clear in the way Gregory King presented his figures. For example, it was not difficult to infer from his method of presentation that particular social groups were a drain on the national income. This means that even the mode of statistical presentation somewhere in the range between Steps (1) and (2), involves elements of Step (3). From the outset at Step (1) a mental construction process is involved; choices are made; observable likenesses in the phenomena of sense experience are sifted and selected out and counted according to specific (mostly preconceived) categories. Theory is at work here. Error may creep in at this first stage so that the data will only be approximate in relation to counting rules. Step (1) merges into Step (2) since the former (a) influences what kind of statistical constructs can be designed at (2) and (b) determines what phenomena have to be left out of consideration in the extended quantifying process at (2). For example, in constructing a ratio, observation errors at (1) will affect and place limits on the applicability of results at (2). Movement further into the realm of statistical constructs involves more explicit theory (e.g., Graunt's method of estimating

population). Choices are made about what raw observations from Step (1) are relevant to the estimating problem in hand; weighting schemes are used as well as formal statistical techniques. The latter, in the case of the three writers surveyed in Section 4.3 was more or less restricted to simple averaging - though, Davenant alluded to a law of large numbers. More crucially, much analytical skill is used to design statistical constructs. That is, skill is required to select out some aspects of reality (as identified in the first instance by numerical observations) and then design formal constructs - such as Petty's "national wealth". The method of designing these constructs can also suggest one type of 'problem' formulation rather than others. This involves valuation, even if, as in Petty's constructs, such valuation with respect to (say) national wealth calculation was based on the social custom of counting yearly incomes in terms of money units. All this tends to merge or overlap with more active application of statistical constructs at Step (3). These constructs are used evaluatively and persuasively to give a sense of proportion to a 'problem' and to imply some 'causes' rather than others. The constructs are formalised at Step (2) and gradually transformed into fully-fledged indicators that refer to some unobservable 'causes' or sense of reality. The process of meaningfully referencing numerical data to unobservables is rooted in some taken-for-granted sense of reality constructed out of inter-subjective agreement among the members of a knowing social group in a specific context. The data is arranged and used to refer to another sense of reality arising at Step (3). Use or

application often involves legitimation and/or forecasting from past data to future desirable (or undesirable) consequences - consequences which are justified by appeal to underlying causal regularities. These regularities are brought in from 'outside' the numerical data and are social constructions. In no way, however, does the initial set of numerical observations entail conclusions and regularities derived from their arrangement and interpretation by Graunt, Petty or Davenant. Disregarding observation error in collecting data, the connections between number and concept (theoretical conclusions) are far less certain or determinate than they might have been if original numerical observations had logically entailed their results (e.g., Davenant's use of data to derive results against a salt tax). One key reason for this uncertainty was the sheer ignorance of these writers about underlying causes. As a corollary, their coarse theories of association rested on crude suppositions. (Justice towards the political arithmeticians requires some mention of the glaring defects in their theories precisely in order to explain, and thereby excuse part of what they said).

In the complete interactive process of design and interpretation of numerical observations, the data designed at Step (1) seem to have been the closest thing that Graunt, Petty and Davenant had to 'the facts' at a specific time and place. Retreat from the domain of collecting and collating numerical data resulted in a shift away from the 'hard facts'. Such a movement led to progressively less precision and more persuasion at each level. There was arguably more faith and fabrication at Step (3) than at Step (1) even while, of course, much faith and

fabrication was involved in the collection of 'raw' data. To some extent, Davenant (and even Petty) recognised that increasing fabrication could be offset by participant observer knowledge from the sensory domain. In the case of participant observation which proffers supplementary non-numerical data, then self-knowledge of the 'knower' which is highly subjective, would have to be relied upon to validate the meaning of a numerical indicator. Another way of understanding the use of numerical data is in a 'purely' subjectivist context which connects data interpretation and use more or less exclusively to the personal interests of the user. In contrast, much more emphasis in the expositions of political arithmetic in Section 4.3 was placed on the influence of social and intellectual context on the mind of the user. It may be granted, in addition, that important individualist, subjectivist elements seem to enter into the creation of indicators especially at Steps (2) and (3). Increasing judgement and intuition is apparent at these levels. Schumpeter once wrote that intuition

though prescientific, it is not preanalytic. It does not simply consist in perceiving facts by one or more of our senses. These facts must be recognised as having some meaning or relevance that justifies our interest in them and they must be recognised or related - so that we might separate them from others - which involves some analytic work by our fancy or commonsense.<sup>1</sup>

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1 Schumpeter, J.A., 'Science and Ideology', AER, 39(2), 1949, p.350.

Identification of intuition in designing and using numerical observations opens the way to a discussion of one important outcome of all processes that turn statistics into indicators. Fertile interaction between intuition and numerical data can help build up concepts and theories that have Schumpeterian analytic content. Numerical data helps from a knowledge of how various economic and social factors might be bound together. Interaction between the design and application of numerical data turns the original numerical observations into a kind of lens. This is employed as an interpretive instrument through which the user (of the data) and others could perceive 'the facts' and tie disparate elements of reality together. This lens or indicator is a way of marking-out the dimensions of a 'problem'; helps discern cause-effect relations and delimits 'problem' boundaries.

In Schumpeter's writings seventeenth century political arithmetic is placed under the heading of 'econometrics'.<sup>2</sup> He contends that political arithmetic "illustrates to perfection what econometrics is and what econometricians try to do". What they have in common is their "spirit of numerical analysis". With some minor changes it may be inferred that numerical analysis resembles economic analysis - that is, they both involve development of technical, intellectual or mental apparatus ("analysis") capable of illuminating 'reality'. This simply extends Schumpeter's definition of economic analysis to

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2 See e.g., Schumpeter, 'The Commonsense of Econometrics', Ecom., 1(1), 1933, pp.6-7.



the related field of numerical analysis. In Schumpeter's view historians of economic thought neglected the analytic content of political arithmetic and concentrated, instead, on reproving political arithmeticians' special pleading.<sup>3</sup> Now, political arithmetic was evidently more than mere description; it was "not [just a] conscious elaboration of some economic system".<sup>4</sup> The interpretation of numerical observations was intended to provide direction and act as a springboard for argument and analysis. Granted, the numerical analysis of political arithmetic took place within the straitjacket of a system of imagery or 'vision' of the economic process. Even so, some theory was refined out of those aspects of political arithmetic that utilised concrete enumerations. The generalisations of Graunt, Petty and Davenant from such enumerations were the joint products of figures and reasoning. Many of the political arithmeticians worked on practical problems of their time - problems with which they were often involved in their everyday working life. They usually had an eye to particular events or policy proposals. At the same time they hammered out concepts from the interplay of participant observation, numerical observations and applied mathematics. In modern language, concepts such as labour theory of value; velocity of circulation of money; public works as a remedy for unemployment; and national income accounts are all to be found in (of course) inchoate forms in Petty's work. Petty and his

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3 Schumpeter, History, op.cit., p.209 and pp.211-12. On his definition of "analysis" see p.3 and p.12.

4 Hull, 'Introduction', PEW, op.cit., p.lxi.

followers recognised the importance of these and many other phenomena and tried to analyse them. Their "arguments of sense" and arrangement of figures even if often hazardous and speculative, evolved novel concepts. Recognition of what bound phenomena together constituted a prescientific, analytic advance in the Schumpeterian sense. According to Schumpeter, particularly in Petty's work "we find theoretical considerations full of vigour and thoughtfulness at every step" in the employment of any available numerical observations.<sup>5</sup> The numbers may well have been predominantly employed to reinforce 'visions' though beneath all this is a definite analytic progression. Any attempt to interpret seventeenth century political arithmetic solely in terms of the incorporation or otherwise of numerical 'facts' faces the task of reconciling the account with the role played by the simultaneous evolution of a capacity for economic and numerical analysis.<sup>6</sup> Numerical observations seemed to play their part in augmenting this capacity because they provided what was thought to be a solid ground for extended reasoning. The political arithmeticians, in

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5 Schumpeter, Economic Doctrine and Method [1912], English translation, New York, Oxford University Press, 1954, p.30. On this point refer also to Rubin, I.L., A History of Economic Thought [1929], English translation, London, Inks Links, 1979. According to Rubin: "Petty's interest in statistics was not for their own sake, but because they afforded material for theoretical analysis" (p.69). See also Winch, D.M. The Emergence of Economics as a Science 1750-1870, Fontana Economic History of Europe, Vol.III, London, Collins, 1971, p.11 and 12.

6 The 'reconciliation' (if it is one) that is claimed here relies on my personal evaluation of analytic quality. According to Schumpeter "everyone of us must rely on his personal evaluation of analytic quality - the only kind of value judgements that are both permissible and unavoidable in a history of scientific economics - a matter in which often the only agreement attainable will be the agreement to differ"; History, op.cit., p.348.

using numerical data to construct statistics, simultaneously gained an appreciation of the shortcomings of their data, formulated some important concepts, suggested what their numbers referred to, and subsequently drew out policy conclusions which ran beyond or ahead of the numerical data available. Generalising somewhat, they were also aware that the indicators required a great deal of faith (in data quality; in connections between measure and concept), and that there was a role for judgement as well as analytic skill. Their indicators were corrigible hypotheses. As Petty noted aptly, they are "at worst ... sufficient as Suppositions to shew the way to that Knowledge I am at".<sup>7</sup> (Davenant also considered that his indicators were only 'probable conjectures'). Schumpeter had a keen appreciation of the tentative nature of the numerical indicators produced by the political arithmeticians. These indicators were akin to lenses, as already outlined, that had "to find out what the facts actually were". Initially, they were "untrustworthy indications and vague impressions". These lenses were in many first instances out of focus and then gradually refocussed as a consequence of the interaction between the growing use of numerical data and their application in conjunction with explicit theory. Thus Schumpeter appreciated that the art of political arithmetic initially employed "the kind of theory that ... retreats before advancing factual knowledge and must eventually be replaced by it ... [As well,] many of the considerations ['visions'] that were originally developed in

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7 Petty, Pol. Arith., PEW, op.cit., p.245.

order to form an idea of the facts served later on to interpret them".<sup>8</sup> In this view, then, an indicator is a way of focussing on 'the facts' since numerical observations are never fully available, or complete. The relationship between the numbers and their referent(s) is a corrigible hypothesis that must eventually be replaced as theories, but not necessarily fundamental 'visions', are refined in interaction with the data. In turn, theory is used again to interpret what the numbers 'show'. The lens, in other words, needs continuous refocussing as the context alters, and as new data becomes available.

Many of the central ideas of the three political arithmeticians surveyed in Section 4.3, (for instance, ideas about underlying 'causes'), evolved well before actual numerical data became available. These ideas were used in association with numerical data to help create a consensus about what the numbers 'showed'. The data helped legitimise these ideas. Legitimation is a process of explaining and justifying for a purpose. In Graunt's case, for example, numbers were used as legitimators of phenomena in order to help talk-up social agreement - not merely on the relevance of numbers to statecraft - but also on the threat of the Dutch to English trade and thence England's wealth, or on the dangers posed by certain social practices to social stability. In Petty we find that the use of data to stand-in for higher level, more abstract concepts (as signs of causes) depended on what Berger calls "socially objectivated knowledge". Such knowledge "consists of interpretive schemas,

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8 Schumpeter, History, op.cit., p.253.

moral maxims and collections of traditional wisdoms that the man in the street frequently shares with the theoretician".<sup>9</sup> The maxim about labour being the father of wealth (Petty) or a treasure (Davenant) is used to explain and justify the interpretation of numerical observations in particular ways. The maxims posit how things are and/or should be tied together causally. As an entity, objectified phenomena assume the quality of an object outside of, but usually at least partly related creatively to, mankind (rather than being 'pure' products of nature). The numerical data then becomes (1) a legitimator of aspects of a social and economic order based on these maxims, and (2) part of the entity component of objectification which is apparent in the reality afforded to arithmetic and quantitative conclusions. The resulting numerical indicators are human products that are available both to the user of the data (also producer of the indicator) and to others as elements of a sense of reality which is commonly shared, self-evident and 'known' in a particular context or community. As a process, objectification is therefore both intrapersonal and interpersonal. The maxims of the political arithmeticians are not usually pushed to the extreme of being facts of nature, though some of their statements favour this interpretation. A second order of legitimation distinguished by Berger and Luckmann, one just below a "total symbolic universe" is one that refers to realities at least some distance from those of everyday experience. At this level of legitimation

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9 Berger, P.L., The Social Reality of Religion, Harmondsworth, Penguin Books, 1967, p.30.

"explicit theories [are propounded] by which an institutional sector [e.g., 'economic' activity] is legitimated in terms of a differentiated body of knowledge".<sup>10</sup> Such legitimations contain fairly comprehensive frames of reference for a particular sphere of human affairs. Specific sectors are then explained and justified by means of specialised bodies of 'knowledge' which are comparatively more complex than 'knowledge' in everyday life. Correspondingly, something of a second order of legitimation is discernible in concepts such as 'balance of trade' and 'wealth of England' as they are explicitly theorised by Davenant and Petty respectively. These concepts functioned as frames of reference for their manipulation and interpretation of numerical data. The resulting indicators can therefore be said to refer to and legitimise, these 'other senses of reality', not directly observable or distinguishable in everyday experience. In the process of legitimation, that is, in the process of using and interpreting numerical data in a particular way these objectivated constructions of 'other senses of reality' are assumed to automatically establish more certainly in the application of numerical data. This is tantamount to assuming tacit agreement on the way data is selected and interpreted. A third order of legitimation outlined by Berger and Luckmann, functions to establish more agreement, if not seal the agreement. Symbolic universes constitute this level of legitimation. These "are bodies of theoretical tradition that integrate different provinces of meaning and encompass the

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10 Ibid.

institutional order in a symbolic totality ... [S]ymbolic processes are processes of signification that refer to realities other than those of 'everyday experience'. This is a most comprehensive level of legitimation once again removed from everyday life.<sup>11</sup> Again, an all-embracing frame of reference is established here. For instance, Graunt refers to the need to 'preserve the subject in peace and plenty' to help justify the application of his data. Petty constantly calls on a need to make sure that 'people are well-united and well-governed'. Davenant calls on a need to develop political arithmetic so as to 'frame schemes useful to the public' and to help 'preserve public order'. While these writers applied numerical data in connection with specific spheres of human activity (trade, population, taxation, etc.,) all these sectors are integrated in total, all-embracing necessities which literally constitute a universe because all human experience is conceived of as functioning within it. At this last level, all less than total legitimations are theoretically integrated in a Weltanschauung. Lastly, for the political arithmeticians numerical observations and arithmetic played one overriding role that can be distinguished in this whole process of legitimation. All the other senses of reality became real, or more correctly stayed real, because numbers and arithmetic were employed so as to constitute tangible, 'hard' physical evidence of their existence.

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11 Berger, P.L. and Luckmann, T., The Social Construction of Reality: A Treatise in the Sociology of Knowledge, New York, Doubleday, 1966, pp.87-89. See also Berger, Social Reality of Religion, op.cit., pp.40-41.

Occasionally the political arithmeticians referred to future events when reasoning from numerical data which, of necessity, only summarised past events. In using the data to reason to a proposition of regularity (or otherwise) and/or to desirable regularities potentially in their grasp as a future, these writers called on ideas from 'outside' the data. (Sometimes they blandly assumed that past regularities identified by the arrangement of the data would continue unchanged into the future, e.g., Graunt on the repopulation of London after a plague). Again, these ideas of regularity and desirable regularity were drawn from socially constructed legitimations that these writers were a party to at the time (e.g., Graunt interpreting the data to justify continued monogamous marriage or Davenant interpreting rather dated trade statistics to justify continued freer trade).

This study of the writings of seventeenth century political arithmeticians has enabled an elucidation of at least some of the many elements involved in the creation of numerical indicators. It suggests that the term numerical indicator is more appropriately defined in a general way as in section 4.1. In order to appreciate the manifold elements involved not just in designing statistics but also in their use, indicators need to be differentiated from a statistics. Indicators, as with statistics, are always theory and value - loaded at the level of data collection (which forms numerical observations) and data arrangement (which forms statistical constructs). On both these levels, and perhaps more so at the level of data arrangement, purposes for using the data as a fully-fledged indicator may



impinge on the practices involved. Beyond these levels though, statistics are finally transformed into indicators as they are actively used and applied in a specific time-place context. Indicators involve more than a simple arrangement of numerical observations because they are used to stand-in for other senses of reality which are not directly observable or measurable.

V. OPERATIONALISING UNEMPLOYMENT IN AUSTRALASIA:

TWO HISTORICAL STUDIES

## 5.1 PREAMBLE TO THE CASE HISTORIES

Two conclusions from Part III(A) are worth repeating at this stage:

'(iv) The compilation of numerical observations and their interpretation as stand-ins for abstract concepts is a process rather than a one-time event. Various influences may act on design and interpretation through time. These influences help transform concepts (or thoughts or interpretations) concerning the content of quantified phenomena over time. In turn, new concepts help transform counting/measuring rules.

(v) ... Some limitations on numerical knowledge might be assessed through history in terms of what can be understood as an open-ended, iterative process of designing and interpreting numerical observations.'

Conclusion (iv) was originally suggested, in embryonic form, by Menger's discourse on the changing and changeable content of what he referred to as 'the same economic phenomenon' through time. Further investigation in ensuing chapters has revealed that numerical data (or numerical observations or statistics) can be used to stand-in for more abstract conceptions of phenomena. It is the task of Part V to expand, with examples, on the influences impinging on the process of designing and interpreting numerical data to stand-in for the 'same' phenomenon through history. It is also proposed by conclusion (v) that this process is ongoing and open-ended and this will be elaborated in the following studies.

The following narrative case histories concerning the design and interpretation of numerical data on 'unemployment'

seek to apply conclusions (iv) and (v). Using description and comparison the following studies seek to grasp some of the limits on numerical knowledge of 'unemployment' by studying how the condition of being without work was quantified (and hence, defined and perceived) by official statisticians through history. It is proposed that changes in the nature and scope of the statistics might reflect changes in dominant perceptions, concerns, attitudes, thoughts and 'knowledge' about unemployment. It is not intended here to impose present-day concepts of unemployment on past practices in order to find the limits on numerical knowledge, even though this may be unavoidable, unconscious and inadvertent in places. The limits official statisticians placed on their counting rules can be appraised to find out what they thought passed for adequate (or inadequate) numerical counterparts of unemployment in a specific time-place context.<sup>1</sup> Appraisal will take the form of inter-temporal comparisons of counting rules as well as elucidation of influences acting on the formulation of counting rules. These influences may have delimited understanding of unemployment from time to time. Knowledge of unemployment can be transformed or constrained, as the case may be, by the process of designing

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1 The aim here is to glean "historical knowledge" according to Collingwood's ideal: "Historical knowledge is the re-enactment in the historian's mind of the thought whose history he is studying". R.G. Collingwood, quoted by Cairns, J.F., 'Some Problems in the Use of Theory in History', ER, 26, Dec. 1950, p.247 n.42. Two other references were used to background the methodology of economic history favoured here: Collingwood, R.G. The Idea of History, Oxford, Clarendon Press, 1946, and Tuma, E.H., Economic History and the Social Sciences: Problems of Methodology, Berkeley, University of California Press, 1971.

numerical data. This process may have had important practical consequences if actions hinged on the statistics, that is, if the data became a policy decision guidepost. In other words, the way a phenomenon such as 'unemployment' is counted or quantified can place boundaries on the way people think about its nature and extent. It is in these senses, therefore, that the phrase 'limits on numerical knowledge of unemployment' should be understood and applied in what follows.

Counting rules used to compile official statistics of unemployment can help define the meaning of unemployment in various contexts (times and places). In this sense operational concepts used to design the statistics can become a straitjacket within which thinking about unemployment takes place; the statistics may become a determinant of thought. Paradoxically, on the other hand, the statistics are a product of thought as well. Following the ideas of Popper and Austrian writers in Part III, it is clear that there can be no numerical observations without theory, however rudimentary. There can be no counting without any theory at all, if by theory is meant some underlying idea about what to count and some purpose for designing and using a fiat measure. Counting does not take place unless it is laden with some theory from the outset. Aspects of observed phenomena must be explicitly or implicitly identified as similar and countable. Statistics of unemployment are therefore a product of thought inasmuch as they require an observer (or a group of observers) to design and apply counting rules to a population. Rules might differ according to the theory an observer has in mind about the state of being unemployed. E.B. Schumpeter studied eighteenth century

English trade statistics at least partly along these lines. The statistics were considered as products of particular streams of thought. Schumpeter "seemed to be asking, "what did the ... [statisticians] have in mind when they had the trade data recorded in the particular forms that they did specify from time to time?"<sup>2</sup> In addition, perspectives favoured by statisticians will often reflect an underlying schema of dominant social values or 'economic thought' patterns which in the Schumpeterian sense "float" in the (by no means homogeneous) "public mind". "Economic thought", J.A. Schumpeter writes, partly consists of "the opinions on economic matters that prevail at any given time in any given society and belong to the province of economic history".<sup>3</sup> All this constitutes a reason for viewing the following as studies in 'political arithmetic'. Another reason follows mutatis mutandis from J.A. Schumpeter's definition of the study of 'political economy'. Economists' ideas, says Schumpeter, move with the times; significant changes occur in their views about practical policy questions and the "sum total

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2 Committee on Research in Economic History, quoted by T.S. Ashton, 'Introduction' to Schumpeter, E.B., English Overseas Trade Statistics 1697-1808, Oxford, Clarendon Press, 1960, p.vi, my emphasis. A similar case history approach can be found in De Neufville, J.I., Social Indicators and Public Policy, New York, North Holland, 1975, pp.70-119. De Neufville studies three series of American statistics in historical perspective. C.f., Layton, T.B., "Techniques for Evaluating Historical Statistics with Case Studies of Selected New Zealand Series", Unpublished Ph.D. Thesis, Victoria University of Wellington, New Zealand, 1980. Layton studies series of agricultural statistics largely to check their Morgenstern-type accuracy in terms of counting rules.

3 See Schumpeter's full definition of 'economic thought' quoted in section 2.1, note 64 (above) from his History, op.cit., p.38 and quoted here from ibid., p.52, my emphasis.

of these views ... together with the schema of social values that underlies these views" he calls "Political Economy".<sup>4</sup> Underlying streams of thought about unemployment (a phenomenon which often appears as a practical public policy problem) can be gleaned from counting rules and methods of statistical compilation used in the 'political arithmetic' of statisticians as contained in official governmental publications and archives. In this respect the following studies parallel broader studies in political economy. They also overlap with Garraty's intellectual history of unemployment except that changing concepts of unemployment are evaluated by way of studies in 'political arithmetic', that is, through the medium of official statistical methods and sources. Furthermore, whereas Garraty compiled a transnational history from classical antiquity to the mid-1970s, the following case histories are confined to Australia (to 1940) and New Zealand (to 1960).<sup>5</sup>

Unemployment is a rather fluid, kaleidoscopic phenomenon and its meaning will depend on time, place and viewpoint. Garraty's history reveals that "the apparently straightforward idea of unemployment is replete with ambiguities".<sup>6</sup> Statistics of unemployment at any one time will narrow down its meaning to a specific operational counting definition which is used to organise a set of numerical observations about it. These

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4 Ibid., p.1141.

5 Garraty, J.A., Unemployment in History: Economic Thought and Public Policy, New York, Harper Row, 1978. Shorter, less detailed versions of these case histories are forthcoming respectively, in Endres, A.M. and Cook, M. 'Concepts in Australian Unemployment Statistics to 1940', AEP (forthcoming) and Endres, A.M. "Designing Unemployment Statistics in New Zealand: A Case History of 'Political Arithmetic'", AEHR, 22(2), September 1982, pp.151-71.

6 Ibid., p.4.

definitions will usually include a detailed catalogue of administrative instructions enabling unambiguous classification of individual observations. The rules must be sufficiently precise in most official contexts so that all persons using the procedures will achieve the same results. This means that the concept of unemployment, while tending to begin as an open, amorphous idea, is narrowed down and gains precision in the specification of counting rules. Even so, operational counting rules may still change, and be subject to different influences through time.

The extent of any correspondence between statistics designed in the more operational domain and analytic, formal, abstract theories of unemployment prevalent at different times will also need assessment. Counting procedures will necessarily take place at a remove from the abstract, conceptual realm. That is, according to Machlup's argument discussed in Part III(B), the gulf between 'pure' theory and actual practice (operations) can be very wide. What sort of gulf existed in the measurement of unemployment between practical counting definitions designed by officials who produced the numerical observations and more formal, contemporary economic theories of unemployment? The first study of Australian series will address itself, inter alia, to this question. The Australian study, above all, will search for operational concepts in the statistics. In some cases a search for official purposes behind statistical design will also be warranted in order to find those additional layers of theory or interpretation coextensive with methods of compiling, presenting and publishing the statistics in particular forms. The New Zealand study will make use of



parliamentary debates and government archive material at various points in order to delve further into influences moulding statistical design and into the official interpretations underwriting statistical compilation and presentation. The New Zealand study will be given some direction by the following broad framework of questions:

1. What circumstances prompted the creation of unemployment statistics?
2. What explicit and implicit layers of theory underpinned the creation and form of presentation and coding of the statistics?
3. What influences operated on the choice and design of the statistics?
4. In what ways were the statistics used to define or reflect public policy problems (or non-problems)?
5. What factors such as reinterpretation of the concept and perceived inadequacies of the statistics brought about changes in statistical design?

The more detailed New Zealand study, above-all, will constitute an analysis of the continuous interplay between data recording and interpretation on a continuum of operational activities involving different layers of theory or informal, everyday patterns of economic thought about unemployment.

V(A) CONCEPTS IN AUSTRALIAN UNEMPLOYMENT  
STATISTICS TO 1940

5.2(A) Introductory Note to the Australian Study

The starting point for this study is the proposition that designing statistics to represent concepts like 'unemployment' is a process and not a single event. Unemployment has numerous dimensions and no absolute boundaries. Indeed "its conceptual limits are not definite boundaries, but wide battlefields over which economic and social philosophies are still fighting."<sup>1</sup> Some ambiguities relating to the term 'unemployment' may be removed if official operational definitions are accepted as they are expressed in the statistical specifications and data presentation methods used by various governmental counting agencies. Emphasis in this Australian study is directed to the changing operational counting rules and their underlying theoretical presuppositions.<sup>2</sup> It is contended in places that notions of unemployment reflected in the official data are statistical artifacts which have often been rationalised to serve the needs of dominant interests.<sup>3</sup> Also, very generally,

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1 Long, C.D., 'The Concept of Unemployment', QJE, 58, Nov. 1942, p.2.

2 For a discussion of the historical links between formal economic theory and labour force statistics in the United States, see Adams, A., 'Who's in the Labour Force: A Simple Counting Problem?', AER, 69(2), May 1979, pp.38-43. This article is a summary of some of the work in National Commission on Employment and Unemployment Statistics, Concepts and Data Needs: Counting the Labour Force, Vol.1, Washington, U.S. Government Printing Office, 1980. No comparable study of Australasian statistics is available along these lines. For a systematic study of statistics in Great Britain see Garside, W.R., The Measurement of Unemployment: Methods and Sources in Great Britain 1850-1979, Oxford, Blackwell, 1980.

3 On the influence of powerful groups see Moses, S., 'Labour Supply Concepts: The Political Economy of Conceptual Change', AAPSS, 418, March 1975, pp.26-44.

statistics with the label 'unemployed' have been published in Australia since 1891 and counting definitions have changed in response to alterations in economic and social conditions. No attempt is made here to determine the 'best' available estimate of unemployment at different times or to gauge what the level of unemployment might have been, if it had been measured in different ways. No attempt is made to calculate the quantitative significance of counting rule changes. This is a study of how the condition 'unemployment' was perceived and quantified by statisticians in their historical context both before and for the most part, after the term was first used in official statistical publications. Possible influences limiting statistical design: practical administrative constraints, the demands of users, and prevailing public employment policies will be noted. Also, the correspondence between Australian methods and statistical recommendations made from time to time by international organisations will be considered.

#### 5.2(B) Counting the Unemployed in the Census

Prior to 1891, some thirty-eight Censuses were taken in the various colonies, the greatest number being in N.S.W.<sup>4</sup> If the N.S.W. Censuses prior to 1891 are taken as a representative sample, some of the terms used for coding and classifying 'classes' and 'occupations' allude to undeveloped unemployment concepts. Before proceeding further, Garraty's perspective will be adopted here:

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4 N.S.W. (9 Censuses); South Australia (8); Queensland (6); Tasmania (6); Victoria (4); Western Australia (4); and (1) throughout Australia in 1881. See Commonwealth of Australia, Census of Population 1966, Vol.2, Part 1, Table 9, p.9.

Discussing the history of unemployment before the condition received that name presents problems. I do not use the word as precisely as it is employed by other modern experts because in historical perspective their "unemployment" is a special case. For my purposes unemployment means "the condition of being without some socially acceptable means of earning a living", and the unemployed are persons capable of labour in need of its rewards, but idle, without regard for their willingness to work or the suitability of what they can do to the needs of society. These definitions are ... too broad to describe modern conditions accurately but narrower ones would force the exclusion of material relevant to the history of the subject.<sup>5</sup>

The first Census taken in N.S.W. (1828) divides the population into occupational groups (alternatively construed as 'socially acceptable means of earning a living'). The only hint of an 'unemployed' group are persons who were not returned with an occupation in this Census. As Sainty and Johnson remarked: "persons working on road gangs, etc. are not given an occupation. It could be assumed that they were government labourers but that may not have been the case; no occupation is shown."<sup>6</sup> The "unspecified class" in the 1841 Census (N.S.W.) comprised some 56% of the total number of inhabitants in the State, and the "residue of the population" in the 1846 Census (N.S.W.) probably contained some persons who were not taking

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5 Garraty, J.A., Unemployment in History, op.cit., p.10, my emphasis.

6 Sainty, M.R. and Johnson, K.A. (eds), Census of New South Wales, November 1828, Sydney, Library of Australian History, 1980, p.23.

part in socially acceptable livelihoods. In this connection and according to the perspective adopted here, the "Alms people and paupers etc" class of occupations added to the 1846 Census classification could not be construed as unemployed at the time. Yet some children who did not 'earn their daily bread' in the "residue" could have been regarded as unacceptably idle (unemployed) in a social sense.<sup>7</sup> Not until the 1861 Census (N.S.W.) does a category for "persons receiving public support gratuitously" appear and this apparently subsumed the earlier "paupers" code. Some part of a group labelled "supported by the community (prisoners, insane, destitute, supported by charity or a burden to the State)" in the Colonial Censuses of 1881, may well have been capable of participating in the production process in order to earn a living independently but did not have the requisite means for doing so. In summary, from the classification of occupations in the 1861, 1871, and 1881 Censuses no count seems to have been made of any persons who might be considered strictly 'unemployed' even according to Garraty's broad conception unless something can be read into the "occupations not stated" class (1861) or the "otherwise unspecified" class (1881). It may be noted that in 1881 "daughters with no specified occupation" were included in the separate category of "women, children and domestic servants" and thus could not be considered unemployed using Garraty's definition as adopted here.<sup>8</sup>

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7 Statistician's Report, Census of New South Wales 1891, pgs. 82 and 87.

8 See Census of N.S.W. 1861, p.619 and Coghlan, T.A., The Seven Colonies of Australasia, Sydney, 1890, p.134.

A model householder's Census schedule was designed by Statisticians who attended the Conference of Australasian Statisticians (1890). The Conference sought "to decide upon more uniformity of system in the matter of taking a census". The schedule drawn up by the Conference differed little from that used in the 1881 N.S.W. Census of population except that employers, persons engaged on their own account, wage-earners, and unemployed, could be separately stated.<sup>9</sup> This means that the unemployed were first counted directly after the idea was conceived and placed in the Colonial Census questionnaires for 1891. Previously, Census investigators dealt with the phenomenon as an integral aspect of something else - poverty, destitution and the like.<sup>10</sup> The Census neologism - 'unemployment' - appeared to catch on automatically in the community and its meaning appeared well-understood. No explicit age limits were placed on the unemployed grade in the 1891 Census, (nor, e.g., limits on hours unemployed). Interpretation of the unemployed grade was left entirely to respondents. In presenting and tabulating the data, however, "those considered unemployed" by an official statistical criterion were only those people who stated their "former employments" so that any person 'unemployed' who did not state their previous occupation is not

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9 Statistician's Report, Census of N.S.W., 1891., pgs 5 and 6.

10 C.f., corresponding experience in Great Britain during the 1880s where "observers were at last learning to disentangle the problem of unemployment from that of poverty. It was the beginning of wisdom in social investigation", T.S. Ashton, 'The Relation of Economic History to Economic Theory', Econ., 13, 1946, p.86.

included in the official enumeration. Persons who had not had their first job were omitted as were those who considered that they had no single occupational role.<sup>11</sup> The Statistician's approach corresponded with the theory of 'gainful workers' evident in definitions used in a United States Census as early as 1870,<sup>12</sup> and in the Census of Australia in 1881 where the 'occupation' inquiry applied to the entire population. Unemployed gainful workers are those who have usually worked at, or previously proven their ability at, producing marketable goods and services. The gainful worker concept was implicit in the unemployed grade first operationalised in the 1891 Census, but it was not explicitly defined. This accords with the international experience of manpower concepts:

whereas in the disappearing order, occupation (Beruf, profession) was sharply defined - the doctor and the barber, the cabinetmaker and the silversmith - the "gainful occupation" concept of the emerging industrial society was not. Thus occupation, once a universal social category implying both a man's position in society and his "callings" became a pragmatic qualifier of manpower, indicative of the level of economic activity to which a member of the workforce belonged.<sup>13</sup>

The official enumeration in this Census included as unemployed in the data tabulation aborigines as well as those described in the Census volumes as "compulsorily unemployed",

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11 Census of N.S.W. 1891, op.cit., p.325 and p.305.

12 See Hauser, P.M., 'The Labour Force and Gainful Workers - Concept, Measurement and Comparability', American Journal of Sociology, 54(1), 1949, p.339.

13 Morton, J.E., On The Evolution of Manpower Statistics, Michigan, Institute for Employment Research, 1969, p.19.



viz., people who

"were incapacitated from following their ordinary occupations ... [and therefore] from earning a living. These by the very nature of their afflictions are precluded from active participation in the battle of life. They are compulsorily unemployed, in most cases permanently, and have to be cared for by others ... [In N.S.W.,] the unemployed compulsorily prevented from following their ordinary vocations may be estimated at about a third of their total number."<sup>14</sup>

The gainful worker concept assumed that persons played a more or less stable occupational role irrespective of their physical or mental health. The enumeration of unemployed did not include persons in some public institutions (gaols, asylums, reformatories).<sup>15</sup>

The 1901 Census of Australia gave unemployment a time reference: "A person was returned as unemployed if not at work for more than a week immediately prior to Census".<sup>16</sup> Those who did not state their usual occupation in 1901 were included in the officially unemployed total. The 1901 definition therefore excluded from its purview persons who were on temporary (less than a week's) lay-off; temporarily ill; or unable to work because of lockouts or strikes or bad-weather during the period of investigation. Long term sickness and old age (exclusive of persons in public institutions) did not disqualify people from being enumerated as unemployed and, as before, there were no

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14 Census of N.S.W. 1891, op.cit., p.306.

15 Ibid., p.314 & ff.

16 Census of N.S.W. 1901, p.766 n.1. See also deliberations on this definition in the 'Proceedings of the Conference of Australasian Statisticians', Parliament of N.S.W., Journals, Vol.62, Part 2, 1900, pp.975-982.

limits on age or hours out of work.<sup>17</sup> Overall, in the period up to Federation, there is negligible evidence in the underlying unemployment concepts of a concern to identify a willingness or ability to work and ipso facto this was tantamount to ignoring the influence of changing economic conditions on attitudes to work. The assumptions throughout seemed rooted in conceptions of 'usual' or 'normal' conditions.

The 1911 Commonwealth Census question 14(b) read: 'If out of work state period'. The refined statistical version of unemployment used to compile the returns excluded any persons who had been out of work for less than a week prior to the Census date and no longer than a year.<sup>18</sup> This change was motivated by the recognition that people out of work for more than a year were not considered unemployed "in the ordinary acceptation of the term". Rather, they were likely to have been permanently incapacitated. The Commonwealth Bureau of Census and Statistics (CBCS) subsequently noted a considerable fall in the age group of "70 and upwards" unemployed and inferred that "[t]his fall is, no doubt, due to the fact that many persons permanently unemployed, classed themselves as 'dependants'. Instead of defining persons "Engaged in Undefined Industrial Pursuits" out of the unemployed class, the CBCS sympathetically understood that "persons unable to obtain employment in their usual trade or occupation naturally are likely, when unemployed,

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17 Census of N.S.W. 1901, p.751.

18 Statistician's Report, Census of the Commonwealth of Australia 1911, p.391.

to state their occupation in an indefinite manner".<sup>19</sup> All persons with unspecified occupations were now accepted as unemployed.<sup>20</sup>

The onset of the First World War prompted the Commonwealth Government to call for a War Census, duly formalised in the War Census Act (1915).<sup>21</sup> A question on male unemployment was included but it seemed redundant since the results on occupations and related matters were enumerated and compiled with specific reference to "the number of men of military age who were engaged in occupations which might directly or indirectly be of special importance in connection with the war."<sup>22</sup>

The 1921 Census attempted to investigate the 'causes' of unemployment. Respondents were requested to state the number of working days out of work up to and including the day before Census date and whether such unemployment was due to scarcity of employment, or to illness, accident, strike, lockout, old age, or 'other' causes.<sup>23</sup> This Census, although using different

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19 CBCS, Labour Bulletin, Labour and Industrial Branch, No.2, April-June 1913, p.103, and p.100.

20 See *ibid.*, pp.101-2. It may be noted that results on unemployment were published exclusive of Aboriginals in this Census and thereafter.

21 The Acts of the Parliament of the Commonwealth of Australia, 1914-15, No.20, pp.117-122. The Act also contains the Census Schedule.

22 Knibbs, G.H., The Private Wealth of Australia and Its Growth as Ascertained by Various Methods Together with a Report on the War Census 1915, Melbourne, Govt. Printer, 1918, p.18, my emphasis.

23 Statistician's Report, Census of the Commonwealth of Australia 1921, Vol.II, p.236.

means, was successful in putting into effect the recommendations of the Joint Committee of the International Statistical Institute and International Unemployment Association which in 1912 had maintained that:

instead of asking the workman if he is without work or without an employer, the Census bulletin should formulate as the question, "Have you worked on the ...?, and with whom?". To all workpeople who reply negatively to the first question a supplementary form should be given asking them for details as to the causes of their unemployment. Drafted in this manner, confusion as between the different kinds of unemployment is avoided and it is possible to consider only those in which unemployment is due to lack of work.<sup>24</sup>

In the way the data was compiled and presented all or any of the 'causes' categories in the 1921 Census could have been considered as unemployment by users of the data because the unemployed were disaggregated by cause.<sup>25</sup> The data was left open to interpretation by users as to whether (say) old age constituted a 'cause' of unemployment or could be considered unemployment at all. The 'scarcity of work' cause was rather vague; it did not identify fundamental causes for 'scarcity', and it did not distinguish scarcity due to workers' 'voluntary' refusal to accept a current wage rate (in expectation of a higher wage later) from scarcity due to a sheer lack of work at that current wage rate. Although such a distinction was common

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24 Quoted in, ILO, Methods of Compiling Statistics of Unemployment, Studies and Reports, Series N, No.7, Geneva, 1925, pp.31-32.

25 Census 1921, op.cit., pp.239-240.

in formal economic theory at this time, it was clearly difficult to allow for in measurement.<sup>26</sup>

Whereas the 1911 Census placed restrictions on the duration out of work which should count as unemployment, the 1921 Census expanded the definition to include persons as unemployed who were out of work for under 10 days or more than 90 days prior to the day before the Census. The duration code was open-ended. At the short-duration end, the exploration into 'causes' such as illness or strikes necessitated more openness and flexibility in the duration code. At the long-duration end, there must have been an official 'taken-for-granted' reliance on some general social mores regarding the difference between being out of work and unemployed in comparison to being in a state of retirement and the like.

In 1933 a Census was taken and continued the 1921 inquiry into 'causes' of unemployment. Notably, in the 'causes' category an additional phrase appeared: 'other voluntarily unemployed' which was part of the code 'other causes'.<sup>27</sup> This implied that some of the causes listed were now officially deemed 'voluntary' but the data was presented in Census volumes in such a way that users could make their own decisions. This Census did not deal with the emerging phenomenon of public relief works at the time. No guidance was given to respondents

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26 See, Pigou, A.C., Unemployment, London, Williams and Norgate, 1913, pp.12-29 and Mills, F.C., Contemporary Theories of Unemployment and of Unemployment Relief, [1917], New York, AMS Press, 1968, pp.88-89.

27 Statistician's Report, Census of the Commonwealth of Australia 1933, Vol.I, p.315.

as to whether part or full-time public relief work should be called 'unemployment' and as the Statistician reported, this would have been open to considerably variable interpretation.<sup>28</sup>

Finally a special official "Census" was instituted in 1939 for the purpose of measuring manpower potential for the War. It counted only males of British origin or naturalised Australians. The counting definition included persons on public sustenance and relief work and investigated the duration of unemployment as well as the number of weeks a person had been out of work in the 12 months prior to Registration. Apparently, no cognisance was taken of the Recommendations of the Committee of Statistical Experts of the League of Nations in 1938 which stated that the

occupation of persons working ... on unemployment relief projects is to be considered as a gainful occupation. A person who has recently exercised a gainful occupation is to be considered as still engaged ... even though, by reason of sickness, injury, vacation or inability to obtain work, he may at the time of the census, be temporarily not working.<sup>29</sup>

It was clear that the Registration system was mainly designed to gauge the potential stock of manpower that could be mobilised for wartime employment. Thus, no 'causes' of unemployment were asked for because the purpose for designing the measures was not directly concerned with obtaining information to help analyse and alleviate unemployment. The Registration concept is instructive insofar as it shows how relative official

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28 Ibid., p.247.

29 The recommendations are reprinted in ILO, The International Standardisation of Labour Statistics, Studies and Reports, Series N, No.53, Geneva, 1959, p.43.

operational definitions and codes are to social context and key events.<sup>30</sup>

### 5.3 Trade Union Unemployment Statistics 1891-1940: A Reassessment<sup>31</sup>

The first Commonwealth Statistician G.H. Knibbs blamed the deficiencies of Australian unemployment statistics for not being able to assess the likely effects of unemployment insurance schemes (which operated in some other countries) in the Australian environment:

At the present time there exist no sufficient data relating to Australian experience of unemployment to indicate what the effects of any such system would be. The necessary information on which to be found any proposals for practically dealing with this question could be obtained with proper inquiry.<sup>32</sup>

Following Knibbs' concern, trade union unemployment statistics were first officially collected and published by the Labour and Industrial Branch of the Bureau in 1913. Knibbs would have had some influence on the decision to collect these statistics but the purpose for doing so may not have had much to do with the possibility of designing a State unemployment insurance

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30 For the complete questionnaire see The Acts of the Parliament of the Commonwealth of Australia, National Registration Act 1939, No.11, pp.48-49. See Appendix V(A).1 for a complete breakdown of Australian Census counting and coding rules relating to unemployment over the period 1891-1939.

31 For an earlier assessment see Forster, C., 'Australian Unemployment 1900-1940', ER, 41, 1965, pp.426-50.

32 Parliament of the Commonwealth of Australia, Parliamentary Papers (hereafter, C.P.P.), 1910, II, p.1429.

system.<sup>33</sup> Figures for the years 1891-1912 resulted from an official inquiry into trade union unemployment in 1912. The Bureau of Census and Statistics conducted a survey based on questions circulated to trade union secretaries. Trade Union secretaries were directed to state the total number of members of their union and also, if available, the number of unemployed at the end of each year specified. In the absence of direct counts, estimates were accepted. The Statistician supplied union secretaries with four documents (forms L.-TU18-21). Use of the relevant form(s) depended upon whether the information which was to be returned from (a) the whole state (including branches); (b) the 'Head Office District' only; (c) an independent union having no branches, or from (d) a branch of a union. Although these forms differed in detail they still asked the same questions. As 1912 is approached more unions were able to supply data.<sup>34</sup> Initially, it was realised that some union officials harboured suspicions and were apprehensive about supplying information, "but this hindrance practically vanished, as soon as it was realised that the affairs of any

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33 E.g., whilst blaming the statistics Knibbs had earlier concluded: "It remains to consider to what extent this device [insurance] is applicable in making provision for involuntary idleness not due to physical disability, nor degeneracy of will. For this last, which is by no means a chimerical danger, it is apparently impossible to provide", ibid., p.1415, my emphasis. For an assessment of Knibb's political outlook and his influence on the collection of early labour statistics see Bambrick, S., 'The First Commonwealth Statistician, Sir George Knibbs', Journals and Proceedings, Royal Society of New South Wales, 102, 1969, p.131ff.

34 CBCS, Labour Bulletin, No.1, April 1913, p.20. For the union series to 1940 see idem., Labour Report, 34, 1944, pp.102-3. (The CBCS ceased collection and publication of this data after 1953).



single union would not be disclosed in the published results and that the investigations had no regard to the prejudices of any political designs, but were being made solely for general statistical purposes". The Bureau did not elaborate on the meaning of the phrase 'general statistical purposes'. It was noted that in "one or two cases, demands for the information had to be issued under the penal provisions of the Census and Statistics Act 1905". Many union officials did not appreciate the government's need to collect unemployment statistics, and were uncertain as to whether the information they furnished was in the best interests of their unions. The Bureau explained away these uncertainties as "misconceptions regarding the object and value of ... [its] investigations", neatly begging the question as to what the objectives were.<sup>35</sup>

The operational definition implied that an individual could not be considered officially unemployed unless he was a member of a trade union. This, of necessity, meant that an individual must have held a previous gainful occupation before being enumerated as unemployed. This conception was contemporaneous with the leading formal economic theory of unemployment developed by A.C. Pigou in 1913:

... the term unemployment should be used exclusively in relation to the sphere of work for wages. "It is ... contrary to usage ... to include among the "unemployed" those members of the professional, employer, or salaried classes, who are, either from choice or from necessity, from time to time unoccupied. Hence, unemployment means unemployment among the wage-earning classes. Furthermore, it means unemployment among

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35 CBCS, Labour and Industrial Branch Report, No.2, April 1913, pp.5-7, my emphasis.

those classes in respect of wage work.<sup>36</sup>

The primary orientation of the previously, gainfully occupied approach in this connection, was to assess the reserve of human resources potentially available for periods of peak production. Hence the former occupation requirement implicit in the statistics, above all, served the labour information requirements of employers.<sup>37</sup> The statistics may have also been useful to researchers at the time interested in confirming Beveridge's observation derived from English union statistics viz., that the "central paradox of the unemployed problem" lay in the need for a "reserve of labour" made up of people required only during boom periods.<sup>38</sup>

Statistics published initially related only to the total number of union members unemployed and estimates were combined with direct counts. In official commentary on the limits of the resulting data a particular level of analytic understanding is apparent. Although the Bureau was pleased with the outcome it was uneasy about the conceptual basis of the series which defined unemployment as a 'stock' at one point in time. Collection of unemployment statistics in this manner did

not take into account variations in  
employment throughout the year, due to

36 Pigou, Unemployment, op.cit., pp.13-14.

37 For comments on the employer bias of gainfully occupied concepts, see Moses, 'Labour Supply Concepts', op.cit., 1975, p.30.

38 Beveridge, W.H., Unemployment - A Problem of Industry [1909], London, Longmans, Green Co., 1930, pp.69-70.

seasonal activity and other causes ...  
 [Therefore it is] not safe to conclude that  
 the actual percentage returned as unemployed  
 in past years by trade unions at the end of  
 each year is equal to the average percentage  
 unemployed during the year.<sup>39</sup>

The Bureau asked if the union statistics provided a fair sample of the industrial population and concluded that while they did not give a true representation of unemployment throughout the whole society it "is not unlikely ... that particulars of unemployment are on the whole more generally available for those trades in which liability to unemployment is above the average of skilled occupations". This was qualified by the observation that building and engineering were heavily represented, but stable industries such as railways were hardly included at all. The Bureau concluded that the percentage given for unemployment was likely to both understate and overstate the real level of unemployment at the time, depending on the industry. Despite this limitation, the Bureau decided that for purposes of comparison and for showing long run trends, they were the "most satisfactory figures available" for that period.<sup>40</sup>

As from the first quarter of 1913 the C.B.C.S. began collection and publication of quarterly trade union returns and

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39 C.B.C.S., op.cit., April 1913, p.16.

40 Ibid., pp.16-17. Although 14 industry groups were represented, some industries (e.g. Agriculture, Railways, Shipping) were not sufficiently reported. For instance, it was argued that employment was stable in railways and that agriculture was subject to wide seasonal variations, hence distortions were believed to have been avoided by excluding them from group status. See ibid., p.20.

continued to do so throughout the period under discussion. The statistics were acquired using the same procedures but trade union secretaries were now also required to state the number of members unemployed on a specific date, for more than three days during the last week of the middle month of each quarter. Those out of work through lack of work, sickness, accident or other causes were distinguished for the first time.<sup>41</sup> These 'causes' were effectively concerned with superficial 'individual' causes rather than the more fundamental nature of unemployment.<sup>42</sup> The 'sickness' category contrasts with the then prevailing Pigovian conviction in economic theory which excluded from the definition of unemployment "those who are definitely incapacitated from wage-earning work by extreme old age, infirmity or temporary sickness".<sup>43</sup> Improvements made in collecting quarterly figures did not quell official discontent over the adequacy of the data. According to Gerald Lightfoot, a CBCS official:

It is true that the information thus obtained does not by any means throw light on the whole question of unemployment. In the first place it refers only to four specified weeks in the year, and, secondly it does not take into account unemployment

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41 CBCS, Labour and Industrial Branch Report, No.5, Dec. 1914, p.13 and idem., Labour Bulletin, No.2, 1913, p.100. This change accommodated the recommendations of the Int. Stat. Inst. and Int. Unemployment Association in 1912 - see note 24 above.

42 See Mills, Contemporary Theories of Unemployment, op.cit., 1917, who stated, in referring to U.S. union unemployment statistics: "[t]hough a compilation of individual causes of unemployment, such as lack of work ... is included, no investigation of the more fundamental causes is attempted" (p.166).

43 Pigou, Unemployment, op.cit., p.15.

lasting for less than three days during any of these weeks. The inquiry has however, advisedly been made in this form in view of the difficulty in obtaining accurate returns of any other description from the majority of the trade unions. Very few of the unions in Australia pay unemployment benefit or keep unemployment registers. The majority of unions allow, however, for a revision of the weekly subscription in cases where a member has been out of work for more than three days, and it is mainly for that reason that the questions have been drafted in their present form.<sup>44</sup>

These practical constraints on data collection moulded the operational rules for counting unemployed unionists. Practical problems had a strong influence on the concept of unemployment that the resulting statistics could be legitimately used to represent. Collection of quarterly figures in 1913 helped accommodate a seasonal concept of unemployment hitherto left unquantified.

In its submission to the International Conference of Labour Statisticians in 1925, the I.L.O. concluded that the "best source of regular statistical information concerning unemployment is obtained from systems of compulsory insurance". Owing to the dearth of insurance schemes in most Australian States this sort of statistical information was unavailable at this time. In such cases, the I.L.O. recommended, inter alia, that trade unions should be required to submit to official statisticians: total membership; numbers of members unemployed

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44 Lightfoot, G., 'Labour Statistics', Report of the Fourteenth Meeting of ANZAAS, XIV, Melbourne, 1913, p.540.

owing to lack of work, and monthly the total number and percentage, unemployed among the members of trade unions.<sup>45</sup> The Conference also resolved that the definition for compiling internationally comparable unemployment statistics "should exclude that ['unemployment'] due to sickness, invalidity and participation in trade disputes and should be limited to unemployment due to lack of employment or lack of work while in employment" and "include those persons not hitherto wage earners who seek to become so".<sup>46</sup> Australian union statistics compared quite well with I.L.O. strictures except that (i) the breakdown of 'causes' by sickness and invalidity was continued; (ii) the part-time unemployed and underemployed were not distinguished from those who were wholly unemployed; (iii) those persons not previously wage earners were not likely to have been included in the returns, and (iv) only quarterly figures were collected. These I.L.O. recommendations did not lead to any changes in Australian counting rules.

In its Second Progress Report tabled in 1926, the Royal Commission on National Insurance analysed union unemployment statistics as a preliminary to recommending various systems of social insurance. The Report began what was to be a gradual deterioration in official government respect for trade union

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45 I.L.O., Methods, op.cit., 1925, No.7, p.25 and p.36.

46 I.L.O., The Second International Conference of Labour Statisticians, Studies and Reports, Series N, No.8, Geneva, 1925, p.61. C.f., Methods of Compiling Statistics of Unemployment: Replies of the Governments, Series C, No.7, Geneva, 1922, p.26.

returns as 'fair' or 'accurate' measures of the 'real' level and trend of unemployment in Australia. After a long detailed consultation with 154 witnesses of whom 60 were trade union representatives the Commission formed the view that the "returns furnished by the trade unions are in most cases ... only based on the secretary's general knowledge of the [unemployment] position". Moreover, while "several trade unions keep an unemployment register ... the average unemployed member does not sign regularly and many do not advise when they obtain employment". Among other criticisms the Commission also perceived that most unions whose members were "casually employed" and perforce at greater risk of frequent unemployment, were not covered in the returns and union employment figures in N.S.W. "were said to include part-time workers in addition to the wholly unemployed". On the question of union statistics indicating seasonal fluctuations in unemployment, the Commission noted one of the weaknesses of quarterly figures previously highlighted by the I.L.O.: "they are point statistics and refer to a particular week only and not to the experience for the whole quarter". The Commission did not come out with a recommendation for more frequent data collection as did the I.L.O.<sup>47</sup> The Development and Migration Commission's analysis of unemployment statistics two years later, added to the disenchantment with union statistics. The Commission was informed by the Commonwealth Statistician that many of the

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47 Royal Commission on National Insurance, Second Progress Report, CPP, 1926, Vol.IV, pp.1413-4.

reporting unions "do not always give particulars as to causes" of their members' unemployment. It was important for the Commission to ascertain 'causes' because, in its analysis of "Unemployment and Business Stability in Australia", it confined the unemployment concept solely to "lack of work". This concept was entirely in accord with I.L.O. recommendations already discussed as well as economic theory at around about this time, which excluded reference to sickness or accident.<sup>48</sup> The Commission also criticised the usefulness of published union statistics for analysing seasonal fluctuations from place to place: "the publication of statistics of unemployment in industrial groups for Australia as a whole does not give a clue to seasonal fluctuations ... in various States".<sup>49</sup> The Statistician's Report for the 1933 Census called for an inquiry into union statistics but to no avail. The Statistician held that a check on the figures was required because of their possible inaccuracy; the variable number and the unequal and fluctuating membership of the reporting unions; the sex composition of the reporting unions; the different experiences of unemployment from quarter to quarter of the reporting unions singly and in groups, and their representing only a sample of the wage or salary earning group.<sup>50</sup> Finally, an official report

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48 E.g., Pigou, A.C., The Theory of Unemployment, London, Macmillan, 1933: "A man is not unemployed because he desires to work but is prevented from doing so by sickness" (p.4).

49 C.P.P., 1926-8, Vol.V, p.668.

50 1933 Census, Statistician's Report, Vol.III, p.303.



on unemployment insurance in Australia in 1937 concluded that "[s]atisfactory statistics as to the volume and extent of unemployment in Australia are not available. Trade union ... returns ... cannot ... be regarded as furnishing a proper measure of the degree of unemployment."<sup>51</sup> In his thoroughgoing treatment of union returns for the period 1900-1940, Forster also noted, without a full explanation, that in Labour Reports from 1927, the tone of CBCS discussion surrounding tabulated union statistics moved from one of enthusiastic affirmation to a stance characterised as "quite noncommittal" by 1939.<sup>52</sup> During the late 1920s and throughout the 1930s many private studies, on balance, heavily discounted the value of trade union unemployment statistics.<sup>53</sup>

Growing contemporary criticism of the series, in this reassessment, can be explained by three interrelated developments: increasing sophistication in public understanding of the nature and causes of unemployment, improvements in the analytic capabilities of officials and growing public policy concern to alleviate unemployment through some form of national social insurance. Experience of mass unemployment during the depression of the 1930s resulted in a general shift in social attitudes. The meaning of unemployment had also undergone a

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51 CPP, 1937, Vol.V, p.2662.

52 Forster, 'Australian Unemployment', op.cit., p.433.

53 See, e.g., Gifford, J.K., Economic Statistics for Australian Arbitration Courts, Melbourne, Melbourne University Press, Economic Series 3, 1928, pp.9-12; Walker, E.R., Unemployment Policy With Special Reference to Australia, Sydney, Angus and Robertson, 1936, pgs. 64 and 75; Ward, E.E., 'A Sample of Unemployment in Victoria', ER, 14, 1938, p.24.

subtle metamorphosis in the minds of public officials. Union officials may have also been subject to changes in attitudes as well. These changes would have fed-back into the way they co-operated with the CBCS in compiling 'accurate' returns. In any case, the unions were still returning their statistics in the same form during the mid-1930s as they had been two decades earlier. However, the content of the concept had changed. This is one reason why criticisms focussing on the 'accuracy' of the statistics were reaching a peak at the end of the period under discussion. Beneath the mere verbal similarities and methods of quantification were changes - inextricably bound together - in both (Schumpeterian) economic analysis (e.g. official movement away from a gainful occupation concept) and everyday economic thought. By 1940, concepts of unemployment operationalised for union returns in 1913, were falling behind contemporary events and notions of unemployment. Of course, economic historians may still find the series useful given these qualifications. This reassessment suggests that historians of thought might take more interest in series of numerical observations and document changes in ideas underlying statistical compilation and statistical interpretation.

#### 5.4 Statistics of State Labour Exchanges: A N.S.W. Case-Study

Experience in Australia with the design of unemployment statistics had similarities with early British experience where "much pioneering work in the quantification ... of unemployment was carried out by persons mainly concerned with the practical relief of the unemployed".<sup>54</sup> Labour exchanges were set up in

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<sup>54</sup> Harris, J., Unemployment and Politics: A Study in English Social Policy 1886-1914, Oxford, Clarendon Press, 1972, p.7.

NSW as early as 1892, and in all other colonies and States except Tasmania before 1905. Systems of registering and lapsing job applicants and the scope of resulting data varied between the States. Figures relating to labour exchange transactions for all the States of Australia were not regularly published before 1912 in Commonwealth statistical reports.<sup>55</sup> Only a case-by-case study of the measurement procedures and data presentation methods in each State can unravel the associated concepts of unemployment that were in vogue at different times. Officers of the CBCS seemed to acknowledge the differing interpretations given to unemployment in the States; early CBCS publications used the terms unemployment and unemployed sparingly in the discussion of these statistics under the rubric 'State Free Employment Bureaux'. All statistical tables were headed 'Applications for Employment', and no categories within the tables referred to unemployment. Lightfoot, a CBCS barrister, separated "statistics of the operations of employment bureaux" from "statistics of unemployment" and confined his ANZAAS discussion on unemployment to trade union statistics.<sup>56</sup>

Other unemployment concepts were operationalised in statistics designed by the NSW Bureau from its inception on

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55 The employment exchange statistics were first gathered together in CBCS, Labour Bulletin, op.cit., 1913, p.44 & ff.

56 Lightfoot, G., 'Labour Statistics', op.cit., 1913, pp.539-41. This view is further supported by the unemployment statistics published by the CBCS in the Monthly Summary of Australian Statistics (later Quarterly ...), which began in January 1912 (Melbourne). The only unemployment statistics published therein were union statistics. This publication strategy lasted throughout the period 1912-1940.

18/2/92 until 1940. The first Bureau secretary referred to men recorded on the books of the Bureau as "registered unemployed".<sup>57</sup> Bureau statistics were underwritten by changing notions as to what constituted unemployment over time. Bureau officials took various practical factors for granted when designing statistics, e.g., a person's ability to register. Registration was restricted to males and was conditional on the provision of certification of residence in the colony for at least 12 months, and/or (from 1897) providing evidence of an elector's right. Moreover, people 'sent to work' by the Bureau did not all find work directly through the Bureau; some found their own job and were advanced repayable travel passes to reach their destination. Those registered unemployed represented those people who were willing to take their chance in an official 'lottery'. The alleviation of unemployment, for the individual, was left open to the luck of the ballot system wherein "all applicants for employment irrespective of their physical capacity, trade, calling or fitness for the work were placed on an equal footing in their claim for consideration."<sup>58</sup>

Numerical data on registered unemployed published by the Bureau was set out in the following manner prior to 1900:

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57 'First Annual Report of the Government Labour Bureau', Legislative Assembly of NSW, Votes and Proceedings (hereafter LANSW, V&P), 1893, p.957.

58 NSW Dept. of Labour and Industry, 'The State Labour Bureau: A Retrospect', NSW Industrial Gazette, (hereafter NSW I.G.), Vol.2, 1912, p.675. For other minor rules influencing registration, see LANSW, Journals, 61, 1899, p.442, and Reeves, W.P., State Experiments in Australia and New Zealand, London, Vol.II, [1902], Macmillan of Australia, 1969, pp.218-219.

TABLE 5.1

NSW LABOUR EXCHANGE STATISTICS TO JUNE 1899

Year ending	Head Office		Branches	
	Registered	Sent out	Registered	Sent out
18/2/1893	18,600	8,154		
18/2/1894	12,145	10,349		
18/2/1895	13,575	16,380		
18/2/1896	14,062	20,576		
Intermediate period <sup>a</sup>	3,283	5,327	1,104	143
1896-1897 (June years)	6,427	13,718	1,253	534
1897-1898	4,167	7,817	715	288
1898-1899	3,843	7,228	686	224
TOTALS	76,102	89,549	3,758	1,189

a The intermediate period referred to was between 18th February and 30th June 1896.

SOURCE: LANSW, Journals, 1899, p.435.

This table officially represented the efforts of the "Statistical Branch [of the Bureau] to compile and tabulate the most accurate statistics concerning the state of the labour market". The 'state of the labour market' was indicated by those who "passed through the books of the Bureau" and not the number of men registered at any one time. In this connection, during the 1890s there was no official operational definition of a stock of unused labour supply (let alone demand) being used to compile these statistics. In the minutes of the evidence taken before the Select Committee on the Workings of the Government Labour Bureau in 1893, it was asked: "Do you not strike a man's name off [the books] when he goes out to work? [Answer] ... No".<sup>59</sup> Registered unemployed figures are cumulative and show

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59 LANSW, V&P, op.cit., 1893, pp.967-68 and p.978.

that those 'assisted and sent to work' usually exceed the number registered. Some applicants were assisted more than once in a full year. The volatility of the labour market at this time made it impossible to count a stock of unemployed labour.<sup>60</sup> Part-time work would not have complicated registration figures so much as the 'assisted and sent out' series - the more often casual work became available, the greater would be the number sent out. The underlying formal theory of unemployment seemed to be of the necessary 'labour reserve' kind. All the Bureau had to do was give some semblance of order to labour mobility. In this respect the NSW Bureau reinforced Charles Booth's idea that a "system of industry will not work without some unemployed margin" and anticipated Beveridge's view that State labour exchanges were the "headquarters of a compact mobile reserve of labour".<sup>61</sup>

In early 1899 the policy problem of those remaining on the registers (even if they were not visibly distinguished in the statistics) became pressing. An Advisory Board was constituted to help the government cope with "the difficult question of the unemployed". When the Board reported in September 1899 it recommended a stock measure of those registered and instigated a number of administrative changes which influenced the count of

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60 LANSW, Journals, 1899, p.441.

61 Booth, quoted unapprovingly by John A. Hobson, 'The Meaning and Measure of "Unemployment"', Contemporary Review, 67, March 1895, p.418, and Beveridge, Unemployment [1909], op.cit., p.203.

registrants.<sup>62</sup> In the period 1901-2 the NSW Labour Commissioners designed more detailed statistics of the registered unemployed at a certain date. The statistics were the outcome of an administrative process that was gradually evolving toward a definition unemployment based on the notion of a Beveridge-type mobile labour reserve supposedly available at any current wage rate. Registered applicants were classified according to their usual occupation and physical strength. Coding categories used to compile published statistics led to a new official distinction between 'eligible' unemployed (employable; willing to work) and 'unemployables'.<sup>63</sup> Only those fit for work were now counted as registered and eligible. Eligibility excluded any person with a job who was searching for a more satisfactory one. The balloting system was dispensed with. Men were notified by mail if a job was available; jobs were meted out in order of registration and people were selected according to the category of skills required by employers. Rather than employers having to obtain detailed information about prospective employees at first-hand, job applicants were now

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62 NSW I.G., 1912, pp.675-676. The first recommendation was not heeded for long because, five years later, the figures returned by the NSW Bureau and published in the CPP were still highly approximate compared with the statistics from most other States. In South Australia and Victoria registered unemployed were counted as a "balance on the register", see Appendix V(A).2.

63 See the Report of the Labour Commissioners, LANSW, V&P, 1904, p.746.

rigorously screened, graded and counted by officials.<sup>64</sup> These innovations anticipated the theory of unemployment associated with State labour exchanges advanced by William Beveridge in the 1909 edition of Unemployment. For Beveridge, a labour exchange had to be structured and administered in such a way that it "appeal[s] to the individualist because by diminishing chances in the labour market it gives more decisive influence to individual merit". Further, the unemployment registration system of the exchanges should be organised in such a way as to "squeeze out the very lowest class of men".<sup>65</sup> In summarising the Reports of the NSW Labour Bureau from 1906-1912 specifically on attempts therein to design unemployment statistics, Fowles' contemporary comment is worth repeating here: "Labour statisticians ... are [now] inserting new columns with new headings; the workless are being classified; the inescapable residuum is being analysed".<sup>66</sup>

The statistical output of the NSW Bureau soon fell into disarray; the data did not 'fit' the official labour supply

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64 For new registration procedures see Labour Bulletin (Sydney), 2, April, 1902, p.77. For the statistical returns see, e.g., ibid., 5, July, p.272. A female-cum-servants State labour exchange was established in Sydney on 10/3/02 and closed 31/1/06. For a summary of the figures for this period, see the Annual Report of the Director of Labour, LANSW, Parliamentary Papers, I, 1906, Appendix F. Figures were collated and kept separate from male job applications. A completely revamped Women's Employment Agency was established in 1914, but persons remaining on the register were counted at a point in time. The statistics were still kept separate from male applications; see NSW I.G., June 1914, pp.1253-1254.

65 Beveridge, op.cit., p.208 and p.206.

66 Fowles, E.H., 'On Unemployment', Report of the Meeting of ANZAAS 1911-1913, XII, Melbourne, pp.456-7.



concept. At the end of September 1903, 16,283 men were "on the books as unemployed" but only 933 were reporting to the exchanges monthly as seeking work. Re-registration commenced in 1903 and was again necessary in 1911 when the number on the books was 11,212 whilst those reporting regularly only totalled 273. Poor lapsing procedures and the unwillingness of eligible workers to consider the work and wages offered through the Bureau accounted for this divergence.<sup>67</sup> By late 1912 the number of men 'eligible' was transformed into a stock of men "waiting for work" on the last day of each month, given current wage rates and conditions of employment.<sup>68</sup> With the onset of the First World War, official labour supply and unemployment concepts were overtaken by events and altered almost beyond recognition. The Minister of Labour established a flexible self-registration unemployment system based upon the use of post-free letter cards available from post offices throughout NSW. The cards were prefaced by the statement that: "I hereby notify that I am unemployed and I forward the following particulars for consideration in connection with my desire to

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67 LANSW, V&P, 1904, p.748 and Parliamentary Papers, 1911-12, II, p.609, 612 and 1912, II, p.187f.

68 NSW I.G., 1, 1912, p.2. The rationale for this kind of operational concept can be found, duly formalised, in the Pigovian conceptualisation of unemployment: "A man is only unemployed when he is both not employed and also desires to be employed ... Desire to be employed must be taken to mean desire to be employed at current rates of wages ... A man is not unemployed because he would like to work if the current wage were £1000 a day but does not so like when the current wage is 5 shillings a day." Pigou, Theory of Unemployment, op.cit., 1933, pp.3-4; see also a similar conceptualisation in idem., Unemployment, op.cit., 1913, p.16.

secure employment". By the end of the first month of its operation the scheme resulted in an additional 3,636 males registered as unemployed. Five months later, 8367 had registered in this manner.<sup>69</sup> After the War, statistical compilation evolved into the following form:

TABLE 5.2

## NSW LABOUR EXCHANGE STATISTICS: 1919

Industry	Persons			Surplus	
	Registered for Employment	Sought by Employers	Sent to Employment	Supply	Demand
Building	151	130	90	21	-
Manufacturing	230	182	114	48	-
Domestic	211	540	302	-	329
Transport - Land, Rail & Tram	7	1,195	328		1,188
- Other	46	23	19	23	-
- Sea	18	1	12	17	-
Rural	129	178	197	-	49
Mining	32	3	2	29	-
General Labouring	472	141	143	331	-
Miscellaneous	58	20	23	38	-
Total December	1,354	2,413	1,230	507	1,566
Total November	1,873	2,459	1,880	1,059	610
Increase	-	-	-	-	-
Decrease	519	46	650	-	449

SOURCE: NSW I.G., 15, Jan. 1919, p.48.

This presentation enabled calculation of unemployment as a surplus labour supply for the whole month. It formally related

69 NSW I.G., 6, 1915, p.533, 534. For later cumulative statistics only, see, pp.664-66.

the statistics to simple concepts of demand and supply. The change was occasioned by two updated functions of NSW labour exchanges: to "bring together intending employers and persons seeking employment [and] to make known the number of unemployed in the State". However, 'the number unemployed in the State' in terms of those remaining registered unemployed at a certain time is not shown by published statistics.<sup>70</sup> This system of compiling numerical observations was not in accord with a resolution of the ILO (1925) which recommended that State labour exchanges should give (a) the number of workpeople registered on a given day of the month as seeking work, with the total number of vacancies remaining unfilled on the same day as well as (b) the number of workers' applications registered and vacancies filled during the month.<sup>71</sup> Method (a) was important for intranational and international comparison. It was used by most States except Queensland and NSW.<sup>72</sup> Method (b) was demonstrably an indication of the success of the measures taken by NSW authorities to administer the continuous flow of labour through State labour exchanges. Moreover, this administrative function, implicit in the form of statistical collation was

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70 N.S.W. Incorporated Acts, (Sydney), IV, No.16, S21, 1918. For relevant data see, NSW I.G., 18, 1920, p.33; 23, 1923, p.18 and 32, 1927, p.101.

71 Resolutions of the Second International Conference of Labour Statisticians 1925, reprinted in ILO, Studies and Reports Series N, No.25, Montreal, 1943, p.68, my emphasis.

72 See the Labour Report, e.g., 1929, No.20, p.119, where those "on live register at the beginning of period" at State Labour Exchanges throughout Australia, are "exclusive of NSW" from 1927-1930, my emphasis.

consistent with Beveridge's earlier account of the aims of State exchanges: to increase labour mobility; facilitate the speedy filling of vacancies and to abolish chance job selection.<sup>73</sup> Despite calls by two official inquiries for stock measures of type (a) from all labour exchanges in Australia, the NSW authorities had no clear brief to design these measures. They had no responsibility, prior to State-wide establishment of government relief works, for the unutilised stock of labour left on the books as unemployed. The exchanges seemed to give primacy in their activities to employer needs. This accords with the way the statistics were designed and prevailing official economic thought leading up to 1930 that governments had no special, overriding responsibility for mass unemployment, c.f., for the decasualisation of labour and alleviation of seasonal unemployment.<sup>74</sup> The Conference of Commonwealth and

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73 Beveridge, Unemployment, op.cit., pp.210-218. At this time it was internationally recognised that one of the principal desiderata for compiling unemployment statistics was to help "throw light on the efficiency of measures ... for ... prevention and relief" of the problem; ILO, Report Prepared for the International Conference of Labour Statisticians, April 1925, Studies and Reports, Series N, No.7, Geneva 1925, p.9. This is also emphasised by Pribram, K., 'The Scope of Labour Statistics', ILR, 14, July-December 1926, pp.481-482. NSW methods did not completely live up to this requirement.

74 See CPP, 1926-28, IV, p.1429 and V, pp.672-674. The underlying economic interpretation was that a given, balanced budget level of annual government expenditure could be used to counter seasonal unemployment in the interests of "industrial stability". This view is regularly appended to statistics in Labour Reports, e.g., No.18, 1927: "definite provisions [must be] made for the postponement of public works during times of economic activity with a view to reserving such works for periods of slackness in employment generally" (p.135).

State ministers in 1929 was preoccupied with the question "of the organisation that was necessary to deal with what might be termed normal unemployment caused by seasonal fluctuations". The Conference agreed that "the establishment of [more] effective labour bureaux" was one practical method of dealing with the problem.<sup>75</sup>

The statistics did not stand up to serious scrutiny when the phenomenon of mass unemployment started to appear. Statistics showing how the exchanges 'brought employees and employers together' were to become increasingly insignificant when notified vacancies were far outrun by the number of registered applicants. Registration at NSW exchanges became a prerequisite for social aid and relief work.<sup>76</sup> Many skilled workers previously not availing themselves of the State exchange service registered as unemployed and this increased statistical coverage.<sup>77</sup> Discussion appended to the published statistics of the exchanges during the 1930s concentrated on the numbers sent to employment rather than those who were still looking for work

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75 CPP, 1929, II, p.1438, my emphasis. Later Conference Proceedings and official reports attached to the Proceedings delete all references to labour exchange activity and concentrate on reinforcing the prevailing pattern of economic thought that "financial stability is within the control of Governments, ... [whereas] the restoration of employment is not directly within the capacities of Governments", ibid., 1929-31, II, p.172.

76 See Colley, A.G., 'Unemployment Relief in New South Wales', Australian Quarterly, June 1939, pp.87-95. For a list of the legislative changes which would have influenced statistical returns, see Labour Report, No.25, 1934, pp.108-9.

77 For a discussion see Colley, A.G., 'NSW Unemployment Statistics', Australian Quarterly, March 1939, pp.96-100.

at the end of each month. Emphasis was on labour turnover - on employment provided rather than the magnitude of any unemployment problem. This was merely an extension of the methods used to present the statistics which, as before, aggregated applications, vacancies and placements during the whole month.<sup>78</sup>

Statistical tables began to appear in the Gazette during 1936, which, in addition to 'registered-sought-sent' statistics, showed the number of males registered at labour exchanges at the end of each month including relief workers.<sup>79</sup> This method of designing and publishing the statistics of NSW exchanges continued unchanged into the 1940s. The Ince Report on Unemployment Insurance in Australia called for a "system of Employment Exchanges ... for the administration of any scheme of unemployment insurance" and recommended that "the existing Government Employment Exchange Service in the States should be developed and extended for this purpose".<sup>80</sup> The idea of counting registered unemployed as the number remaining on the registers of labour exchanges served key policy and social reporting functions in the post-war unemployment insurance system administered by the Commonwealth Employment Service and Social Security Department. Thereafter, the measure became

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78 See NSW I.G., 40, 1931, p.22 and 47, 1935, p.37.

79 Monthly figures back to June 1936 appeared in ibid., 52, 1937, p.825, and half-yearly dating back to June 1934 in the Official Yearbook of New South Wales, Sydney, e.g., 1938-39, p.645. C.f., Annual Reports of the NSW Relief Council where statistical tabulations exclude those on full-time relief work, e.g. NSW I.G., 51, 1937, p.8.

80 CPP, op.cit., 1937, p.2696.

institutionalised in official statistical publications and reports.

### 5.5 Epilogue (1)

Present-day users of the three series of statistics discussed in this brief study cannot overlook the following, complexly interrelated changes: (a) changes that took place in operational counting definitions and (b) changes in official interpretations or meanings conferred on the numerical data. Both categories place limits on the temporal comparability of the data. Further, changes in category (b) suggest additional but immeasurable discontinuities in the data considered in its intellectual context. When taken together, changes in categories (a) and (b) reflect changing limits on official 'knowledge' of Australian unemployment through time.

Forster used the Census and union unemployment series as one "basic benchmark" to generalise about "the functioning of the economy" and "the process of Australian economic development" over the period 1900-1940, and he passed over many of the changes documented here. He hypothesised that "the real changes come with the 1940s"; that "the war marks a real discontinuity in the series" and that the statistics can "be looked at broadly, without worrying too much about differences in definition".<sup>81</sup> Counting rule changes only partly responded to major historical events connected with changes in unemploy-

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81 Forster, 'Australian Unemployment', op.cit., pgs. 427 and 434.

ment in Australia over the period reviewed. For example, union and Census rules did not change markedly during the experience of mass unemployment in the late 1920s and early 1930s. Counting rules were influenced by official interpretative and analytic insights associated with presentation of the data in statistical reports and their perusal by various official inquiries. These insights, in turn, arose out of shifting, immeasurable attitude, intention, or value-related preconceptions about unemployment. Also, it is not obvious, for instance from union statistics, that meanings conferred on unemployment in Australia were unchanging over periods of time even whilst counting definitions remained unaltered. Moreover, the design of labour exchange statistics in NSW showed that the content of the concept unemployment was changeable. Legislative developments and variable administrative functions influenced the number who were returned as registered unemployed. In the 1930s official attitudes to unemployment altered and the union system of data compilation instituted in 1913 was gradually outmoded. Labour exchange methods were becoming more acceptable as the Commonwealth Government contemplated moving toward accepting responsibility for those left on State labour exchange registers. New meanings or patterns of (Schumpeterian) economic thought were emerging beneath the counting rules. These factors led to immeasurable, category (b) discontinuities in all the series surveyed here.

Connections (including lagged connections) between statistical design and more formal economic theories of unemployment prevalent at different times were difficult to make. A wide gulf between 'pure' theory and actual practice was



evident. Any connections therefore seem rather strained. Some points of similarity between counting definitions and formal background theories of unemployment were noted. In particular, theories of the 'gainful worker' and the 'necessary reserve of labour' provided the rationale for some forms of operationalism. On the other hand, conditions and hours of work were usually regarded as parameters. Census questions were not tight enough to exclude those with 'excessive' wage aspirations. In some cases, data presentation methods left open the question as to whether physical disabilities could count as a 'cause' of unemployment. Finally, 'frictional', transitional or between-jobs activity was neither excluded, nor distinguished, by any of the operational definitions of unemployment over the period under review.<sup>82</sup>

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82 C.f., Keynes, General Theory of Employment, op.cit., 1936, where it is found analytically "convenient" to exclude 'frictional' unemployment from his formal definition of involuntary unemployment (p.15, see also p.6).

## APPENDIX V(A).1

## EXPLICIT UNEMPLOYMENT CONCEPTS IN AUSTRALIAN CENSUSES 1891-1939

YEAR	Relevant Question Number(s)	Definition for Householder	Relevant Coding Scheme Alterations	Operational Definition for Enumeration
1891 (April 5th)	N.A.	Unemployed on census date	By usual occupation	Unemployed with a former gainful occupation on census date.
1901 (March 31st)	7	If not at work for more than a week immediately prior to census (except in cases of leave of absence).	Same, except unable to follow usual occupation because of sickness or infirmity is separate question.	Unemployed with or without gainful occupation for more than a week immediately prior to census.
1911 (April 3rd)	14(b)	If out of work on census date.	- duration - occupation usually followed.	If out of work for more than a week but less than a year prior to census date.
1915 (June 30th) (War Census)	8	Males > 18 yrs & < 60 yrs out of work for more than one week prior to census date.	-	As for householder
1921 (April 2nd)	16(c)	If out of work at time of census.	-number of days since last employed -by cause- scarcity of employment, illness, accident, strike, lockout, old age, "other".	"
1933 (June 30th)	17(vi), Same (a)&(b)	Same	Causes include 'other involuntary'.	"
1939 (July) (National Register)	8(d) & 9(a)&(b)	Unemployed males of British origin > 18 yrs & < 65 yrs (including sustenance & relief workers) on registration date.	-not by cause -number of weeks unemployed in the last 12 months -period since last employed in any occupation (other than sustenance or relief work) in months, weeks, days.	"

Sources: Census questionnaires and Statisticians' Reports 1891-1939.

## APPENDIX V(A).2

RETURN OF REGISTERED UNEMPLOYED FROM STATE LABOUR EXCHANGES  
TO COMMONWEALTH GOVERNMENT 1905

U N E M P L O Y E D   P E R S O N S

(Particulars furnished by the Premiers re the number  
of unemployed registered in their  
respective States)

States	Number Unemployed	Remarks
New South Wales	1,300 to 1,400	The approximate number actually unemployed in Sydney and suburbs.
Victoria	900	Number registered on 31st October was 1,391, but experience shows that about one third of the men are not available when called on. It is, therefore, believed that approximately 900 men represented the number of unemployed on the date mentioned.
Queensland	356	Number registered during month of August.
South Australia	413	Seven hundred and forty-nine registered during month of September, of whom 336 have since been employed, leaving a balance of unemployed on 16th October, 1905 of 413.
Western Australia	Less than 500	Number registered on 4th October, 1905, was 1,768, but from a telegram dated 30th October, it appears that the number actually unemployed is comparatively very small.
Tasmania	—	There is no labour bureau in Tasmania, as the unemployed difficulty hardly exists there.

Source: Parliament of the Commonwealth of Australia  
Parliamentary Papers, 1905, Vol.II p.1397.

V(B) NEW ZEALAND UNEMPLOYMENT STATISTICS:  
A STUDY IN CHANGING STATISTICAL DESIGN  
AND INTERPRETATION TO C.1960

Additional Abbreviations Hereafter

<u>AJHR</u>	<u>Appendices to the Journals of the House of Representatives</u>
<u>JDOL</u>	<u>Journal of the Department of Labour</u>
<u>MAS</u>	<u>Monthly Abstract of Statistics</u>
<u>NZA</u>	<u>New Zealand Archives</u>
<u>NZG</u>	<u>New Zealand Gazette</u>
<u>NZOYB</u>	<u>New Zealand Official Yearbook</u>
<u>NZPD</u>	<u>New Zealand Parliamentary Debates</u>
<u>NZS</u>	<u>New Zealand Statutes</u>
<u>NZSR</u>	<u>New Zealand Statistical Report</u>

## 5.6 COUNTING THE UNEMPLOYED IN NEW ZEALAND BEFORE 1900\*

The first documented evidence of an 'official' count of persons unemployed was in the province of Otago in 1861. The alluvial gold boom in the provinces was near cessation and many labourers wanted to find alternative work. The subsequent measure was based on the collection of (totalled) signatures from 1,010 workmen. As a petition, the signatures were presented to the Provincial Council.<sup>1</sup> This measure can be regarded as quasi-official because it was designed to help Council policymakers serve some Divine law. The petition did not name unemployment or refer to any 'problem'. Nor did it define very clearly what the list of signatures was supposed to indicate. The petition's tone suggested submissiveness to authority. The Council was urged to "take a golden opportunity" to employ "all hands able and willing". The explicit purpose for designing the measure from the workmen's point of view was stated as being their "sacred duty". The signatures on the petition names those workers who were "available to help set afloat public works" and develop the natural resources of Otago.<sup>2</sup> Motivations for creating the measure may have been related to the Deity. More concretely, another implicit motivation can be ascribed to it - simply the hope of obtaining

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\* See the 'Preamble' to Part V, p.263 (above) for the framework of questions used to organise the following New Zealand study.

1 For extracts from the petition and related commentary refer to Sutch, W.B., Poverty and Progress in New Zealand, Wellington, Reed, 1941, pp.61-63. Curiously, no reference is cited by Sutch. As well, all details concerning the petition are deleted from Sutch, Poverty and Progress in New Zealand: A Re-Assessment, Wellington, Reed, 1969 (see p.63).

2 Sutch, Poverty, 1941 edn., op.cit.

work in order to ensure subsistence. During the 1860s and 1870s the concept of unemployment remained unrecognised by governments of the day, or was otherwise left implicit.

In the context of the recession conditions in the mid-1880s the first government policy measure of unemployment appeared under the guise of those "unemployed who have applied for work" to the Immigration Department. A total of 558 men applied for work between 9 May 1884 and 11 October of that year; were recorded by usual occupation and duration in New Zealand and totalled in a Memorandum of the Christchurch Immigration Office (refer to Table 5.3). This measure was limited because of its localised nature. Although it is not clear from the records, one reason for counting and tabulating those who applied for work appears to have been an administrative check on the employment prospects of migrants and could have been used as part of the information base on which to decide whether to assist more migrants to New Zealand. From Campbell's suggestion it may be inferred that, since the measure was designed to show an applicant's usual occupation and duration in New Zealand, it was used to allow prospective (usually private) employers to discriminate among the applicants without having to obtain information about them at first-hand.<sup>3</sup> Discrimination was not based simply on occupational skills but also, according to Campbell, on the length of time the applicant had been in the country. Newly arrived migrants were probably preferred by

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3 Campbell, R.J., "'The Black 'Eighties' - Unemployment in New Zealand in the 1880's", AEHR, 16(1), 1976, p.75.

TABLE 5.3

## STATISTICS OF THE CHRISTCHURCH IMMIGRATION OFFICE 1884

H.—23.

SESS. II.—1884.  
NEW ZEALAND.

## UNEMPLOYED WHO HAVE APPLIED FOR WORK

(MEMORANDUM RELATIVE TO THE NUMBERS OF).

*Laid on the Table of the House of Representatives by leave of the House.*

## MEMORANDUM by the IMMIGRATION OFFICER, Christchurch.

Mr. Elliott.

I FORWARD list of applicants for work to the 7th October. As the Public Works Department are now giving the orders, this will be the last list I shall have to send.

The total number of applicants since the 9th May, 1884, has been 558; of these—

46 have been in the colony over 25 years.				140 have been in the colony over 5 years.			
47	"	"	20	37	"	"	2
51	"	"	15	25	"	"	1
185	"	"	10	27	"	under 1	"

The trades or occupations of the applicants are as follow:—

Occupations.	No.	Occupations.	No.
Bakers ...	5	Labourers ...	412
Basketmakers ...	1	Millers ...	2
Blacksmiths ...	4	Millwrights ...	1
Bakermakers ...	2	Miners ...	2
Bootmakers ...	2	Moulders ...	4
Bricklayers ...	3	Painters ...	11
Brickmakers ...	3	Packers ...	1
Butchers ...	5	Platelayers ...	1
Carpenters ...	33	Plumbers ...	2
Cabinetmakers ...	2	Plasterers ...	2
Clerks ...	8	Sawyers ...	3
Carriage-smiths ...	2	Sailmakers, &c. ...	2
Coach-trimmers ...	1	Saddlers ...	2
Compositors ...	1	Storemen ...	5
Coopers ...	1	Stonemasons ...	4
Cooks ...	2	Survey assistants ...	2
Drapers ...	1	Strikers ...	1
Engineers ...	4	Waiters ...	2
Engine-drivers ...	3	Warehousemen ...	1
Fitters ...	3	Well-sinkers ...	1
Gardeners ...	7	Wheelwrights ...	1
Grocers ...	3	Total ...	558

Immigration Office, Christchurch, 11th October, 1884.

J. E. MARCH,  
Immigration Officer.

By Authority: GEORGE DIBSBURY, Government Printer, Wellington.—1884.

SOURCE: AJHR, 1884, II, H-23.

employers at this time because of their willingness to work hard. This was reinforced by - if not the practical rationale for - the classificatory scheme used by the Immigration Department. The measure primarily served the labour requirements of private employers.

After the 11 October 1884 the Immigration Department dispensed with its measures as immigration fell away sharply. The best way to get rid of the unemployed throughout the 1880's was to export them: "the busiest industry in New Zealand was shipping - not shipping cargo, but shipping human beings - out of New Zealand anywhere to escape destitution and unemployment".<sup>4</sup> As recession conditions set in, responsibility for measurement passed over to the Public Works Department. As it turned out, the Public Works Department did not systematically count men who applied for work. One official statistical return during this period totalled men engaged on public works and referred to those men as 'unemployed'. Although, inverted commas appearing over the term unemployed suggest some uncertainty as to whether all those on public works were still unemployed, apparently since some work was described as purely for relief and other work "would have been undertaken in any case" (see Table 5.4). Lloyd Prichard noted that in the 1880s "although the figures do not provide exact information as to the total unemployed, they do measure the efforts made by the authorities to relieve unemployment when no system of

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4 Sutch, Poverty and Progress, 1941, p.83. The annual net loss of migrants reached - 2744 by the end of 1885; NZOYB, 1894, p.71.



## TABLE 5.4

## STATISTICS OF THE PUBLIC WORKS DEPARTMENT 1887

D.—6.

SESS. II.—1887.  
NEW ZEALAND.

PUBLIC WORKS ON WHICH UNEMPLOYED ARE ENGAGED  
(LIST OF).

*Laid on the Table by the Hon. Mr. Mitchelson, with the Leave of the House.*

LIST OF WORKS on which "UNEMPLOYED" are engaged, and the Number of Men in each case who were at Work on the 28th November, 1887.

MARBOROUGH.					
1. Kaituna-Clarence Road	...	...	...	...	109
CANTERBURY.					
2. Kaituna Quarry	...	...	...	...	17
3. Drainage, Addington Workshops	...	...	...	...	9
4. Irrigation reserve, bywelling, &c.	...	...	...	...	19
5. Hagley Park improvements	...	...	...	...	30
6. Mount Somers Railway extension	...	...	...	...	10
7. Stone-breaking at Addington	...	...	...	...	12
OTAGO.					
8. Completing formation, Otago Central Railway	...	...	...	...	7
9. Hindon Road, formation	...	...	...	...	48
10. Catlin's River Railway, formation	...	...	...	...	79
11. Seaward Bush Railway, platelaying	...	...	...	...	40
Total	...	...	...	...	380

\* These are the only items that are purely relief-works, as the others would have been undertaken in any case.

Public Works Department,  
Wellington, 2nd December, 1887.

JOHN BLACKETT,  
Engineer-in-Chief.

[Telegram.]

The General Manager, N.Z. Railways, Wellington.  
Number of "unemployed" now working at Sentry Hill is twelve.  
Wanganui, 2nd December, 1887.

C. B. HANKEY.

Approximate Cost of Paper—Preparation, oil; printing (1,275 copies), 14s 6d.

By Authority: GEORGE DIBBON, Government Printer, Wellington.—1887.

SOURCE: AJHR, 1887, II, D-6.

unemployment insurance existed".<sup>5</sup> In the New Zealand Parliamentary Debates (NZPD) of 1884 a petition was said to have been "presented on behalf of the Oamaru unemployed" who were engaged on public relief works.<sup>6</sup> The way in which the Public Works Department measured unemployment excluded those who were not able to obtain government relief work. By inference from the method of measurement, only those on public relief works were 'problems' for the authorities. This is one reason why they were counted. Public work schemes could not be relied upon to give agreeable, permanent work. Furthermore, there was considerable 'stigma in applying' to the Public Works Department. The unemployed were described as "men who were really unfortunate enough" to ask for and obtain jobs on public relief schemes.<sup>7</sup> The count of men engaged on relief schemes was highly localised. Those who were actually registered were probably fortunate enough to arrive at the site of public works ahead of others. Because the measure was discontinued when public monies were exhausted in 1890, one implication is that the measure was designed largely for administrative purposes to assist public authorities to keep account of the cost of public works in relation to numbers employed - the objective being to keep the numbers on public works high and their wages low

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5 Lloyd Prichard, M.F., An Economic History of New Zealand to 1939, Auckland, Collins, 1970, p.181. Occasionally, numbers employed on public relief works were tabulated on a regional basis. See, e.g., AJHR, 1890, D-10.

6 Vol.50, November, p.363, my emphasis.

7 NZPD, Vol.47, September 1884, p.407.

relative to similar work in the private sector.<sup>8</sup>

The creation of the Bureau of Industries answerable to the Minister of Labour led to more systematic collection of labour statistics from June 1891. One of the objectives of the Bureau was to "compile statistics concerning the conditions of labour generally".<sup>9</sup> The new measures were designed in a similar fashion to those of the Immigration Department some seven years earlier. The first secretary of the Bureau, Edward Tregear, found that when he first commenced his job there was a "pressing difficulty ... of 'unemployed' labour in the colony".<sup>10</sup> The class of labourers in the towns expanded in the 1890s partly because of technological developments in agriculture and the cessation of public works in rural areas. The concept of unemployment at this time was regarded with extreme caution by the authorities. This is evidenced by the inverted commas that appeared around the word in the Bureau's reports to the Minister from 1892 to 1895 inclusive.<sup>11</sup> Ostensibly, no-one was sure how to define the 'problem', or to assign responsibility for the 'problem' in what was believed to be a self-regulating economy, so the concept remained exceedingly fuzzy. The statistics that

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8 Relief work wages were kept to about one-half the level obtainable for similar work in the private sector during the 1880s. Refer to Campbell, 'The Black 'Eighties', op.cit., pp.75-6.

9 AJHR, 1892, H-14, p.1.

10 Ibid.

11 AJHR, ibid., H-10, e.g., p.14; 1894, H-6, e.g., p.4 and 1895, H-6, p.7. See also the Journal of Commerce and Labour, No.3, 1893, p.35 and 52; and even as late as AJHR, 1903, H-11, p.11 and JDOL, August 1905, p.717.

were developed did not show those who were out of work. (Doubtless, Garraty's more universal comment on the measurement of unemployment before 1900 holds equally for the New Zealand case: "the jobless had seldom been counted systematically before the idea of 'unemployment' was conceived".)<sup>12</sup> One hundred and eight-nine agencies of the Bureau were initially established throughout the country in 1891 and were administered by government officers and local policemen. All males, usually irrespective of age, state of health or disability who declared themselves in want of work were recorded by the Bureau officers (by usual occupation and age). Since no archive material from the early activities of the Bureau survived the Hope Gibbons Fire in Wellington (1952) only published material can be drawn upon to understand the official methods of statistical compilation that were used from time to time. It was reported to the Select Committee on the Working of Government Labour Bureau in New South Wales, Australia, that, in New Zealand, "on the last day of each month, each of the officers in charge of an industrial district, forwarded to the Bureau of Industries at Wellington, a schedule stating the number and names of the unemployed registered at the branch offices" including details as to the action taken to deal with the registrants in question. Parenthetically, it was also noted that if "employment has been found for the men in the town or district where they have been consigned, the receiving district reported the fact in the monthly return".<sup>13</sup> It is doubtful whether those remaining on

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12 Garraty, Unemployment in History, op.cit., p.167.

13 Legislative Assembly of New South Wales, Votes and Proceedings, Vol.8, 1892-3, p.1037.

the registers of the Bureau were actually counted and totalled say, at the end of each month or year. The first published figures are cumulative and relate only to men assisted and/or placed during the whole year. Table 5.5 is an example of the way the Bureau's statistics were set out in published form for the first twenty years of its operation.

TABLE 5.5

MEN ASSISTED BY THE BUREAU OF INDUSTRIES 1891-1912

Year	Total <sup>a</sup>	Married	Single	Dependants	Private Work	Govt. Work
1891-92 ...	2,593	1,054	1,539	4,729	1,730	863
1892-93 ...	3,874	1,808	2,066	7,802	2,518	1,356
1893-94 ...	3,341	1,836	1,505	7,942	1,019	2,322
1894-95 ...	3,030	2,007	1,023	8,883	894	2,136
1895-96 ...	2,871	1,880	991	8,424	708	2,163
1896-97 ...	1,718	1,084	634	4,719	652	1,066
1897-98 ...	2,035	1,163	872	4,928	544	1,491
1898-99 ...	2,115	1,178	937	4,759	638	1,477
1899-1900 ..	2,147	1,115	1,032	4,471	486	1,661
1900-1 ...	3,124	1,326	1,798	5,432	519	2,605
1901-2 ...	1,830	713	1,117	2,747	396	1,434
1902-3 ...	3,704	1,492	2,212	5,934	580	3,124
1903-4 ...	2,860	777	2,083	3,085	1,216	1,644
1904-5 ...	3,130	953	2,177	3,425	1,960	1,170
1905-6 ...	6,712	2,027	4,685	7,351	1,929	4,783
1906-7 ...	7,393	1,427	5,966	4,187	2,718	4,675
1907-8 ...	6,305	1,440	4,865	4,408	2,977	3,328
1908-9 ...	10,391	2,538	7,853	7,510	4,190	6,201
1909-10 ...	8,506	2,987	5,519	10,164	5,059	3,447
1910-11 ...	7,102	2,181	4,921	8,454	4,251	2,851
1911-12 ...	5,735	1,407	4,328	4,233	3,450	2,285
Totals ..	90,516	32,393	58,123	123,587	38,434	52,082

Notes: <sup>a</sup> The total assisted is exclusive of dependants.

SOURCE: AJHR, 1912, H-11, p.iv.

Many of those assisted were sent to "work in outdistricts" much of which was "heavy work in bush districts where [they had to] toil unremittingly or starve".<sup>14</sup> The incentive for a person to apply for work was not strong unless starvation threatened and because of the 'without work - nothing' philosophy of the Bureau. New immigrants applying to the Bureau, especially the Auckland branch, were not assisted and instead were told to "take to the country".<sup>15</sup> In addition, men "trained in some calling not required in the colony ... or perhaps well-fitted for some business that has already too many followers in the colony" were not counted nor were those who used private labour exchanges.<sup>16</sup> An "unemployed summary" in numerical form was also published from December 1892 in the Journal of Commerce and Labour (five months later it was retitled: Journal of the Department of Labour). Nine column headings made up this return: (1) locality (2) number married (3) single (4) number of dependants (5) number sent to private employment (6) number sent to government works (7) number of months unemployed (aggregated) (8) causes of failure to get work: slackness of trade (9) sickness. Totals referring to the unemployed, as in AJHR, count males assisted by the Bureau exclusive of dependants.

It is significant that the Bureau was not moved to count, tabulate and make public the number of men remaining on its

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14 AJHR, 1892, H-14, p.1.

15 AJHR, 1893, H-10, p.14.

16 NZOYB, 1894, p.371.

books. This might have been the case because branches of the Bureau functioned as regional labour exchanges. Abstracting from additional administrative complications involved in trying to obtain a macro-level total, a count of the stock of unemployed labour would have had no practical administrative value. The cumulative totals of men assisted were flow measures of bureaux activity and workload. From 1899 the monthly figures published in the JDOL were accompanied by figures showing the number of persons who were sent to employment or assisted by the bureaux more than once during the period in question.<sup>17</sup> Of course, publication of these figures did not necessarily enable calculation of numbers remaining on registers at a point in time. The "assisted more than once" statistics were to be used to point out that officials had to deal with a sometimes recurring unemployment problem occasioned by temporary employment and/or placements that were later found untenable by employers and/or employees.

Men who could not be found work were evidently regarded as a residual by the authorities and were not counted. The data was used to help "observe the movements of labour and to endeavour to equalise its distribution in proportion to the demand ascertained to exist in the various towns or districts throughout the colony".<sup>18</sup> From casual observation, officials in rural areas reported to branches in major towns the extent of demand for workers and men were recruited from the "surplus

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17 JDOL, January 1899, p.9.

18 Legislative Assembly of New South Wales, op.cit., 1892, p.1037.

labour pool" found thereabouts.<sup>19</sup> Men left uncounted or unplaced might have been described as the very young, old, sick and disabled. However, at this time Tregear ruled this out.<sup>20</sup> By 1903 it was finally made clear that "the sick, the maimed, the inebriate, the lazy" were not counted since they were more properly "subjects for the charitable aid societies or the Commissioner of Police rather than the Labour Department".<sup>21</sup> It was most likely that, prior to 1900, the numbers of those remaining on the books were unpublished or more precisely, were not compiled, because officials feared that publication would fuel the cause of those who pressed for more public works via what Tregear called a process of "persistent agitation".<sup>22</sup> He added that his duty was to ascertain the "real facts". Tregear's 'facts' were numerical observations - labour statistics. They were purposive, operational constructs underwritten by value judgements. Superficially, the Bureau's placement measure was an advertisement as to the usefulness and efficiency of the public employment exchange which helped legitimise the continued operation of the Bureau. The measure showed the extent to which the Bureau serviced the public in contrast to a similar service provided by private labour

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19 AJHR, 1895, H-6, p.7.

20 Refer to Tregear's discussion on "Labour in New Zealand", NZOYB, 1894, p.371ff; see also an official report along similar lines expressing the same sentiments by the Minister in Charge of the Bureau of Industries, Edward Reeves, entitled "The Present Position of Labour in New Zealand", JDOL, Vol.4, 1896, pp.158-73, especially p.167ff.

21 AJHR, 1903, H-11, p.ii.

22 AJHR, 1894, H-6, p.4.



exchanges. It also served the purpose of indicating how many new employment opportunities were taken rather than the opportunities that did not exist. Once again the placement measure, including its mode of classification and presentation, served employer needs and labour market demand requirements. The "pressing difficulty" which Tregear spoke of in his first report was one to be identified within the individuals concerned because they were not suited to contemporary labour market requirements: "Many of the 'unemployed' ... could be sent ... to an institution where ... they could ... [be] trained to habits and duties fitting them for the general labour market".<sup>23</sup> In part, the Bureau's measure of men assisted was an administrative check necessary to obtain recompense from those found employment. (These men were given railway and travel passes for which they were usually obliged to pay after commencing work in rural areas).<sup>24</sup>

The conclusion here must be that the Bureau did not actually intend to measure unemployment - those who were regarded as being without work during the period under review. Thus, W.P. Reeves understood that, with reference to the workmen placed by the Bureau: "many were not in distress and hardly to be classed as unemployed".<sup>25</sup> In his report to the Minister of Labour in 1895, Tregear reinforced this view when he admitted

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23 AJHR, 1892, H-6, pp.2-3.

24 For details of various forms of assistance afforded by the NZ bureaux see Legislative Assembly of NSW, op.cit., p.1038.

25 Reeves, W.P., State Experiments in Australia and New Zealand, Vol.II, op.cit., p.223 n.I.

in response to a rhetorical question: "What is the exact number of unemployed at present in the colony?"; that it was "incapable of being answered by any person". At best it could only be estimated through rough "guesses" by officers of the Bureau who had local, first-person knowledge of labour market conditions.<sup>26</sup>

A second method of counting the unemployed, actually the first count according to a specific, unambiguous operational definition prior to 1900, evolved through the quinquennial Censuses conducted by the Registrar General's Office (then a division of the Internal Affairs Department). There were hints of unemployment notions in sections on occupations in some earlier Censuses (e.g., codes entitled "no specific occupation"). The first full population Census of the colony of New Zealand taken in 1862, and even before that in a New Munster Census taken as far back as 1851, included a code for those without rank or occupation.<sup>27</sup> The 1891 Census concentrated on delineating the skill composition of the labour force and distinguishing employers from employees. This Census did not attempt to ascertain whether employers and employees were working or not.<sup>28</sup> The term unemployed first appeared in the

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26 AJHR, 1895, H-6, p.6. Recent comments by the N.Z. social historian, W.H. Oliver, reinforce all this: "we know very little about employment in nineteenth century New Zealand, ... even less about unemployment". 'The Origin and Growth of the Welfare State', in Trlin, A.D., ed., Social Welfare and New Zealand Society, N.Z., Methuen, 1977, pp.4-5.

27 See NZG, 8th June 1852, No.13. For a history of the NZ Census prior to 1900 see the Statistician's Report attached to the Census of the Dominion of New Zealand 1916, (Wellington, Govt. Printer) 1920, pp.1-8.

28 Census of the Colony of New Zealand, 1896 (Wellington, Govt. Printer).

1896 Census. The 1896 Census recorded for each occupational group the number of non-Maoris reported "out of employment" at the Census date including males and females younger than 15 years and younger than 90 years of age apparently irrespective of their mental or physical condition. It was evident that this first Census measure was based on a fairly loose concept of 'out of employment'. The concept was left open - its precise boundaries being left to the subjective evaluation of respondents. Even so, this first Census measure succeeded in narrowing down and operationalising a concept of unemployment.<sup>29</sup>

#### 5.7 THE EVOLUTION OF UNEMPLOYMENT STATISTICS IN NEW ZEALAND:

##### 1900-1940

##### (a) The Census (continued) to 1926

Compared with the Census which preceded it in 1896, the 1901 Census rendered the idea of unemployment more ambiguous. The Census volume for 1901 directed the user of unemployment statistics by the following phrase: "for details of unemployed see Table XIV".<sup>30</sup> This table turned out to be a list of occupations arranged alphabetically and contained an unemployed category totalling 58 males. Conceivably, this category was designed for those out of work but without a previous gainful occupation. In this Census then, the official 'unemployed'

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29 Census of the Colony of New Zealand, 1896 (Wellington, Govt. Printer).

30 Census of the Colony of New Zealand, 1901, Wellington, Govt. Printer, p.324.

class did not seem to refer to wage and salary earners out of employment - only those new entrants to the labour force without work.<sup>31</sup> This use of the term unemployed may be contrasted with the mode of statistical tabulation recommended at the Conference of Statisticians on the 'Census of Australasia 1901' held in Sydney during March 1900. E.J. von Dadelszen represented New Zealand at the Conference. The relevant resolution 17(B) read:

... subsidiary tables [should] show ...:-

(1) The ordinary Occupations of the unemployed of either sex, a person to be considered as unemployed if out of work for more than a week prior to the Census.

(2) Employers of labour and persons in business for themselves, relatives assisting, and wage earners; no persons to be considered as belonging to one of the foregoing divisions who is not actually employed.<sup>32</sup>

Tabulation of New Zealand Census results appeared to overlook resolution 17(B)(2). In other words, the Census listed some (potential) wage earners as unemployed whereas according to 17(B)(2) no person could be considered a wage earner who was not actually employed. Admittedly, Conference deliberations and resolutions did not clarify how to classify unemployed wage earners who had not been previously occupied.<sup>33</sup> In the 1906 Census the unemployed category referred to wage earners out of

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31 The 1901 Census showed 9,826 people "out of employment"; ibid., Table XI, p.379.

32 Legislative Council of New South Wales, Journals, Vol.62 II, 1900, p.982, my emphasis.

33 For the minutes of deliberations see ibid., pp.975-991.

work as for the 1896 form of statistical classification. The unemployed category in the list of occupations was deleted without explanation.<sup>34</sup> The 1916 Census tried to observe the severity of unemployment by including a code for number of weeks unemployed: "If wage earner state in weeks period out of work since last employed". The resulting data was tabulated by the Statistician to create unemployment subgroups by duration.<sup>35</sup> The next major change in the design of the Census statistics occurred in 1926. As before, unemployment was defined as being out of work for more than one week prior to the Census date. The coding scheme and method of presentation of the unemployment measure were altered so as to enumerate 'causes' of unemployment. The 1926 Census was the first to include unemployment as a component in the title of a volume: "Unemployment from sickness and other causes". In the preamble to this volume it was stated that the purpose of the newly designed measures was to "widen the present inquiry on sociological lines into the extent and causes of unemployment".<sup>36</sup> The measures either focussed on personal inadequacies as the cause of unemployment (sickness, accident

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34 Census of the Colony of New Zealand, 1906, Wellington, Govt. Printer. In Table XIV showing "Occupations of the People" 3138 persons were returned with "no occupations" (p.451); a total of 9561 wage earners were returned as unemployed; 9902 persons were returned in an "indefinite" class and added to "relatives assisting but not receiving wages", (Table XIII, p.441).

35 Census of the Dominion of New Zealand, 1916, Wellington, Govt. Printer.

36 Census of the Dominion of New Zealand, 1926, Wellington, Govt. Printer, Vol.X, p.1.

or injury, strike action) or on the superficial cause defined as "lack of employment not due to strikes". While the first set of causes may have had some utility for an analysis of the employment situation the sources for the solution of these causes of unemployment were implicit in the coding system, i.e., responsibility was shifted on to the individuals concerned to rehabilitate or change themselves. The measurement and analysis of the causes of unemployment could be extended almost without bounds provided it could be kept at the level of the individual. Officially it was contended that the Census "provides data as to unemployment in conjunction with such interesting relevant facts: conjugal condition, nationality, length of residence, age, religion".<sup>37</sup> One of the enumerated causes - "lack of employment not due to strikes", did not help identify more fundamental causes of unemployment; like contemporaneous Australian Censuses it did not help users of the resulting data to work-out whether lack of employment was due to a choice on the part of respondents not (say) to accept work at prevailing wage rates due to higher wage expectations, and it did not distinguish lack of full-time employment from part-time or underemployment and so on. Some enumerators actually found that this Census investigation into causes was far too sophisticated for respondents. Some reported that "the question on unemployment was answered in very few cases" while another suggested that "unemployment questions should, with advantage be deleted".<sup>38</sup>

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37 NZOYB, 1926, p.721, my emphasis.

38 'Precis of Suggestions, ETC., in Enumerators Reports: Census 1926', NZA, S 12/9/49.

As it happened, the 1926 Census was the first and only N.Z. Census to consider 'causes' of unemployment.

(b) Union Unemployment Statistics 1925-30

Some scope for comparison between unemployment statistics collected from the Census and from other sources was permitted after 1925. A resolution adopted at the International Conference of Labour Statisticians, Geneva, April 1925, decided that there should be more international uniformity in labour statistics. The relevant resolution (No.77(3)) read:

Where statistics based on unemployment insurance, compulsory or voluntary, are not available, it is desirable to obtain from workers' organisations the following information:

(a) Monthly, the total number of unemployed on a given day and the percentage they form of the total membership covered by the enquiry.

(b) Annually, the number of workers covered by the enquiry as a percentage of the total number of workers in the corresponding industries or occupations.<sup>39</sup>

Although this resolution may have had some influence on the decision to collect and publish union statistics, other events leading up to the decision suggest a number of other influences as well. These may be briefly outlined from archive material. The Government Statistician, Malcolm Fraser, started a discussion on union unemployment statistics with the Minister of

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39 ILO, The International Standardisation of Labour Statistics, Studies and Reports, Series N (Statistics), No.25, 1943, p.67. For comments that reflect the influence of this Conference on New Zealand labour statisticians, refer to NZOYB, 1926, p.768.

Internal Affairs on 12/6/1925. Fraser showed the influence of international obligation (he only mentions the League of Nations in this connection) and pressure from researchers in universities requesting unemployment statistics. He further supported his case with reference to the Census and Statistics Act 1910:

Although the collection of unemployment statistics is specially enjoined upon the office by Section 21, Paragraph C of the 1910 Act, for various reasons, [upon which he did not elaborate], collection thereof has not been undertaken in New Zealand. The time and circumstances now seem opportune.<sup>40</sup>

On the fifth of August 1925, the Minister approved Fraser's request to collect statistics from unions. Fraser noted in his subsequent memorandum to the Minister:

I have to thank you for your intimation of the ... 6th instant, ... re the approval for the collection of statistics of Unemployment among Trade Unions and for the publication (commencing not earlier than November of the current year) of these statistics.<sup>41</sup>

Fraser then wrote to the Solicitor General. The forms used by Australia's Commonwealth Bureau of Census and Statistics were discussed.<sup>42</sup> The schedule subsequently gazetted for quarterly returns is reproduced in Table 5.6. Form U.-1, which was used to collect the statistics from union secretaries is reproduced in Table 5.7.

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40 NZA, CS 22/50/5, bracketed insert added. In 1910 the first Census and Statistics Act was passed. Included in the Act was statutory provision, though not apparently an obligation, for the Statistician, "subject to the directions of the Minister, [to] collect annual statistics in relation to ... Employment and non-employment". NZS, Wellington, Govt. Printer, 1910, No.64 S21, p.324.

41 NZA, CS 22/50/5.

42 Ibid., on 17/8/1925.



TABLE 5.6

GAZETTED UNION SCHEDULE FOR UNEMPLOYMENT STATISTICS 12/9/1925

## SCHEDULE.

Full name of union:

Area covered by union's activities:

Questions	Replies.	
	Male.	Female.
1. What was the total number of members of your union on [Date]?		
2. State, so far as you know, the total number of members of your union who were unemployed for <u>more than three days</u> during the week ended [Date], on account of lack of work [In absence of exact <u>figures</u> , <u>approximate</u> or <u>estimated</u> figures will suffice]		
3. If available please specify separately the number of members of your union who were unemployed for more than three days during the same week on account of -		
(i.) Strikes or lockouts .. ..		
(ii.) Sickness .. ..		
(iii.) Accident .. ..		
(iv.) Other reasons .. ..		
4. In your opinion was employment in your trade in the area covered by your unions' activities during the week ended [Date] good, moderate, or bad?		
5. Please state "Yes" or "No" as to whether any trade dispute <u>causing stoppage of work</u> occurred, or was in progress, in your trade in the area covered by your union's activities during the months of [Months].		
6. Were there any special circumstances (e.g., short time, overtime, changes in rates of wages or hours of labour, &c.) affecting employment in your trade in the area covered by your union's activities during the three months referred to in question 5? If so, please append particulars ..		

I hereby certify that the above is a correct return to the best of my knowledge and belief.

[Signature and official designation of  
union official supplying information.]

Address:

Date:

As witness the hand of His Excellency the Governor-General,  
this 12th day of Sept., 1925.

[Signed]

Minister of Internal Affairs.

SOURCE: NZA, CS 22/50/5.

TABLE 5.7

QUARTERLY RETURN SCHEDULE U-1 SENT TO UNIONS 1925-30

Questions	Replies	
	Males	Females
1. What was the total number of members of your union on _____? ( <u>Exclude unionists who, though remaining on the rolls, are known to have left the trade</u> ).		
2. State, so far as you know, the total number of members of your union who were unemployed on account of lack of work for more than three working-days during the week ended on the date mentioned in question 1. ( <u>In the absence of exact figures, approximate or estimated figures will be of some value. If the figure quoted is an estimate only, that fact should be definitely indicated</u> ).		
3. If available, please specify separately the number of members of your union who were unemployed for more than three working-days during the same week on account of - (i) Strikes or lock-outs in your trade ... (ii) Sickness ... .. (iii) Accident ... .. (iv) Other reasons, excluding lack of work (specify): _____		
4. In your opinion, was employment in your trade in the area covered by your union's activities during the week mentioned in question 2, good, moderate, or bad?		
5. Were there any special circumstances (e.g., short time, overtime, changes in rates of wages or hours of labour, &c.) affecting employment in your trade in the area covered by your union's activities during the three months immediately preceding the date mentioned in question 1? If so, please append details:-		

SOURCE: Ibid.

In November 1925 Fraser requested approximately 200 trade unions (all with more than 20 members) to collect quarterly numerical information about their members on the subject of unemployment. In some cases the request may have been a token gesture since some unions would have been collecting similar information about their members for their own reasons - for example, some unions paid their members an unemployment benefit and others reduced union dues for unemployed members. Fraser did not deem it necessary to make clear the reasons why the State wished to collect union statistics. For instance, in a patronising letter to union secretaries he intimated that:

There is no need for me to stress the importance especially to unionists but also to the community generally of adequate Dominion statistics of unemployment; and I feel convinced that I can rely on your hearty co-operation in the matter of supplying a similar return each quarter promptly ...<sup>43</sup>

The coding system on the return form was similar to 1926 Census questions on unemployment. When the data was published, unionists not reporting to work because of sickness, accident, strikes and other reasons excluding lack of work, were not counted as unemployed. For example, according to the New Zealand Statistical Report (NZSR) for 1927: "Persons ... idle

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43 Ibid., on 11/11/1925.

for three days or less, or idle because of illness, accident, or industrial dispute in which they are directly concerned are not deemed unemployed".<sup>44</sup> Four other comments can be made about coding rules and resulting official presentation of the numerical data. First, the method of presentation differed from the 1926 Census in that, as already noted, data from Question 3(i) through (iv) on Schedule U.-1 (Table 5.7) was not tabulated with or added to data from Question 2 in published statistics. (See, e.g., Table 5.8).

TABLE 5.8

UNEMPLOYMENT AS ESTIMATED BY INDUSTRIAL UNIONS:MAS TABLE HEADINGS

Unemployment by (a) Industrial Groups (b) Industrial Districts	Reporting Unions		Persons unemployed for more than Three Days during the Week on account of Scarcity of Employment.			
	Number of Unions	Membership of Unions at End of Week. Males Totals	Males	Totals	Percent. of Un- employed to Total Members	Corres- ponding Percent. 16/11/29

SOURCE: MAS, January 1931, p.46.

44 NZSR, for 1927, (published: 1929), p.xvi.

Second, question 5 in the gazetted regulations (Table 5.6) read: "Please state 'Yes' or 'No' as to whether any trade dispute causing stoppage of work occurred, or was in progress, in your trade in the area covered by your union's activities during the months of ...". This question was deleted from Form U.-1 (Table 5.7) without explanation. It might be inferred that the Statistician originally wished to ascertain whether unemployment due to 'lack of work' in one trade was 'caused' by a stoppage of work in an allied or cognate activity. That is, though not 'directly' related, a stoppage in one trade may have repercussions for a whole industry. In any case the question was not reprinted on the official questionnaire perhaps because it was thought superfluous considering that question 3(i) on U.-1 seemed to allow for returns of work stoppages. Fourth, there was clearly some dissent in official circles as to the usefulness of Question 5 on U.-1 as it related to wage rate changes and alterations in hours of labour.<sup>45</sup> Actual inclusion of these questions on the questionnaire showed that official analytical skills and understanding of unemployment had progressed to the point where at least there was some vague recognition of connections between wage rates and hours ('causes') and the level of unemployment (one 'effect').

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45 For instance, Dr. Neale, an official in the Statistician's office expressed the view that Question 4 on the Australian CBCS union unemployment form L.-TU.19 ["Please state 'Yes' or 'No' whether any change in rates of wages or hours of labour occurred or was in progress in your district and in your trade ..."], seemed superfluous. He argued that it should not be retained on the New Zealand forms. NZA, CS 22/50/5 on 22/5/1925.

Union unemployment data was presented by occupational group and district at quarterly intervals and published in the MAS and in some other places such as NZOYB and NZSR from May 1926 until early 1931. They were last published in MAS in January 1931 (see Table 5.8) and recorded union unemployment up to 15/11/30. The union statistic was putatively designed in order to calculate an official 'unemployment rate' defined as that which related 'the number of persons unemployed to the number subject to the risk of unemployment'.<sup>46</sup> However, this crude rate was poorly designed to reflect the size of the population that was available for work and at risk of unemployment. It was confined to using numbers from Question 2 to relate to those at risk, *i.e.*, total union members (from unions surveyed). Furthermore, the survey of unions covered 47% of all registered male unionists in New Zealand.<sup>47</sup> Some undetermined number of union members belonged to more than one union.<sup>48</sup> From Question 2 on form U.-1 it may be reasoned that the published statistics were not exclusively compiled from direct counts of unionists unemployed. The statistics also incorporated union secretaries' estimates of unemployment within their unions. It is not clear in the published data whether the statistical tabulation includes or excludes approximate or estimated figures returned by union secretaries. Official comments lend to the interpretation that estimates were simply added to direct counts

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46 NZOYB, 1927, p.875.

47 MAS, May 1926, p.ix.

48 NZSR, op.cit., 1927, p.xvi.

in the statistical tabulations: "the estimates of the numbers unemployed rests on individual judgements (based on a rough knowledge of the facts) of the union secretaries, such judgements probably on the whole being equally liable to error of excess as of defect".<sup>49</sup>

The estimates by trade unions did not have any immediate government policy implications. The statistics were obtained as a matter of convenience by the Statistician and used in publications as a measure of the level of, and trends in, unemployment. The numbers were supposed to represent something important - exactly what, in other than union circles, was left vague and unstated. In its first discussion of unemployment statistics the NZOYB mentioned in passing that: "statistics of unemployment should shed light on the efficiency of measures adopted for the prevention and the relief of unemployment".<sup>50</sup> In the case of the trade union measures, any direct count, as opposed estimate, of unionists unemployed would have been motivated by a natural desire on the part of unions involved to check their efficiency in dealing with the plight of their own members. There was no other inducement for union secretaries to directly count their unemployed members or for union members to report their joblessness unless some return was provided by the union (e.g., a relief benefit based on a union insurance scheme or provision for the reduction of union dues). This interpretation of the purpose behind the measures is supported

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49 MAS, op.cit., 1926, p.x.

50 1926, p.768.

by the fact that they were discontinued in November 1930 when it was likely that, because so many unionists were jobless, union finances were exhausted.

(c) Labour Exchange Statistics to 1930

In the meantime up to 1920, the Labour Department bureaux registration system still produced published statistics that totalled those assisted to employment.<sup>51</sup> This method of statistical compilation appeared consistent with the administrative aims of the bureaux - the numbers were used, demonstrably, to stand-in for administrative competence and efficiency in dealing with job applicants. For example, the women's bureaux established in March 1908 was dispensed with in 1921 because "it was considered that the number of engagements made by these bureaux had not for several years justified the expenditure involved, [and] it was decided to close them as separate offices". Placement figures were key criteria legitimising expenditure. The women's bureaux figures were invariably presented under the following four column headings: Occupation; Applications from Employees; Applications from Workers; Workers Placed.<sup>52</sup> In all other statistical tabulations

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51 For example, see AJHR, 1912 H-11, p.xiv.

52 See JDOL, Vol.24, 1916, p.180. Some branches, however, used different headings at different times. In one case the Dunedin branch column headings for November 1916 anticipated future developments in labour exchange statistical compilation recommended by the ILO (1925), (to be referred to anon). In addition to the above four headings, a fifth heading totalled "Applications from Employers: Vacancies remaining" and a sixth: "Applications from Workers: Workers not suited last month". Ibid., p.689. In the last column heading, in particular, is found a hint of a counting rule which some years later was to become the accepted official measure used to gauge unemployment.



for the period up to 1926, no calculation was made, or can be made, of those remaining on registers. Lapsing procedures were not spelt out in bureaux reports. The purposes for designing the registered and placed measure was, at least partly, to justify continued public support for bureaux services.

During 1921 the Labour Department's bureaux registration system had been altered to allow for totalling of unplaced unemployed applicants remaining on the books of the Department at the end of each week. No definite legislative reason was apparent as to why these weekly figures began to be compiled. Published figures along these lines dated back to the week ended 13 August 1921. However, no figures were published in this form prior to 1926. The data was acquired by the Statistician after his request addressed to the Secretary of Labour on 26 May 1925:

Could you also supply me with a table showing for each week for the Dominion as a whole, as far back as available down to the present date:-

- (a) The total number of new applications during the week for employment.
- (b) The total number of applicants "placed".
- (c) The total number of applicants remaining on your books at the end of each week?

The Statistician was given permission to publish figures on unplaced applicants on 6 August 1925.<sup>53</sup> Figures first appeared

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53 Fraser to Secretary of Labour and Fraser, Memorandum to Minister of Internal Affairs 12/8/25 thanking Minister for permission, NZA, CS 22/50/5.

in MAS, January 1926.<sup>54</sup> Two clues may be detailed as to the influences acting on New Zealand labour statisticians during the early 1920s relating to decisions taken to count unplaced male applicants during 1921 and to publish this data from early 1926. First, although there was no clear association between the Labour Department and the Census and Statistics Department (then a branch of the Internal Affairs Department), it may be noted that the British Empire Statistics Conference (1920) passed resolutions No.83-85 which called for greater international consistency, improvement and extension of unemployment statistics. The New Zealand Statistician, Malcolm Fraser, who was a member of the Conference Labour Statistics Committee, supported these resolutions.<sup>55</sup> Second, a resolution passed at the International Labour Conference at its first session (Washington, 1919) invited the governing body of the ILO to form an international commission

empowered to formulate recommendations upon the best methods to be adopted in each State for collecting and publishing all information relative to the problem of unemployment, in such form and for such periods as may be internationally comparable

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54 See p.39. The NZOYB, 1926, p.774 contains figures from the end of the first week of September 1921, and NZSR for 1924, (1926), p.137 contains figures back to the week ended 13 August 1921. No statistics using this method of compilation appeared in AJHR during the mid-1920s. It may be added that only in exceptional instances did the New Zealand Gazette publish labour statistics of any kind from 1860-1960 other than occasional Census returns.

55 AJHR, 1920, II, H-12, p.5. The Conference was held at the Board of Trade from 20/1 to 26/2/1920. For a more complete report on deliberations and resolutions of the Conference and of the Labour Statistics Committee in particular, see Commonwealth Parliamentary Papers (Australia) 1920-21, Vol.IV, pp.864-901.

... Advantage was taken of the presence at the Third Session of the International Conference (Geneva, 1921) of a large number of delegates and technical advisers who were particularly interested in the questions of the statistics of unemployment, for the purpose of bringing them together and obtaining their opinions ...

The Commission had presented a report on its work to the Session of the Governing Body of the International Labour Office, which was held in April 1921, and after approval of the proposals of the Commission by the Governing Body, the Director of the International Labour Office had, on 16 September 1921, communicated to the Governments of the Members of the International Labour Organisation, among other things, a draft definition of involuntary unemployment, and draft tables for use in compilation of unemployment statistics.<sup>56</sup> [See Table 5.9]

TABLE 5.9

WEEKLY STATISTICS OF VACANCIES FILLED AND  
APPLICATIONS OUTSTANDING (ILO:1921)

(To be furnished monthly: all occupations and industries taken together.)

Week of the month	Applications by			Applications outstanding at end of month	
	Work-people	Employers	Vacancies filled		
	during the week			From employers	From work-people
1st from .. to ..					
2nd from .. to ..					
3rd from .. to ..					
4th from .. to ..					
5th from .. to ..					

SOURCE: ILO, ibid., p.58.

56 ILO, Methods of Compiling Statistics of Unemployment, Studies and Reports, Series C, No.7, 1922, pp.7-8, my emphasis.

This may be compared with the earliest New Zealand tabulation where vacancies notified and vacancies unfilled are not counted.

TABLE 5.10

## ACTIVITIES OF LABOUR DEPARTMENT'S EMPLOYMENT BUREAUX, 1921-22

First Complete Week of	Number of Applicants "placed"	No. of Applications remaining on Books at End of Week					
		Auckland	Wellington	Christchurch	Dunedin	Other Branch Offices	Total
1921.							
September	159	220	525	108	85	198	1,136
October	182	214	408	151	181	225	1,179
November	132	195	389	197	172	187	1,140
December	120	232	304	113	98	132	879
1922.							
January	75	240	307	131	132	117	927
February	89	384	341	86	104	284	1,199
March	79	370	399	82	55	273	1,179
April	117	404	335	111	86	248	1,184
May	125	444	468	68	76	392	1,448
June	164	526	497	239	89	400	1,751
July	123	482	467	153	71	640	1,813
August	122	441	454	178	82	265	1,420
September	213	376	350	264	83	226	1,299

SOURCE: NZOYB, 1926, p.774.

So, overall, in the light of the two international obligations discussed here, it seems that a desire to standardise New Zealand unemployment statistics and definitions in relation to international practice was one reason why the Labour Department's statistics were redesigned to give totals of unplaced male applicants from 1921.

During the mid-1920s the confused state of parliamentary debate over the extent of unemployment reflected the inherent inadequacies of statistics available. In the NZPD during 1926 a question was asked about what the government intended to do in

the Auckland region where 209 women and 1000 men were alleged to have been out of work. The Prime Minister replied by mentioning how many people had been placed by the Auckland Bureau of the Labour Department; he concluded that he was "supplying the House with the information as he received it".<sup>57</sup> The government expressed minimal responsibility for those remaining on the Labour Bureau's registers at this time. Voluntary committees were set up to provide additional help in an employment exchange capacity. However, many committees were disbanded with the government's approval when many hundreds of applicants were still unplaced.<sup>58</sup> In an earlier debate, some concern was expressed for the difficulties faced by women who could register for work in some centres but who could not be placed by the government's (male) placement service. Voluntary committees had to be set up for this purpose. In any case, the Minister of Labour did not agree that the government placement service would be required to help women because he "did not know that there were many cases" where women were unemployed.<sup>59</sup> Further heated debates were to follow about the numbers of unemployed as a depression beckoned. At first, government members denied responsibility for the 'problem' and remained largely uncommitted to its alleviation. When unemployment remained unrecognised, questions about the phenomenon were answered in terms of placements or terms of the number of new employment

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57 1926, Vol.210, p.2.

58 Ibid., p.897.

59 NZPD, 1926, Vol.209, p.814, my emphasis.

positions filled. When unemployment was explicitly recognised it was regarded as seasonal, periodical or related to the voluntary choices of certain groups in society to "live extravagantly". At this time the official statistics of unemployment had what can be called pre-policy or descriptive uses. There was no clear idea what the policy problem was and, until 1926, the official operational definitions were accepted without question. Official figures were occasionally used by politicians as vindicators of policy inaction or indictors of certain social groups as the case demanded. The figures were eventually challenged because they were not in accord with some people's experience of the extent of unemployment as a problem when measured according to the official operational definition. It was asked: "why should the Honourable Minister not take notice of unofficial figures? ... Surely the claims of humanity are sufficient without having to be embodied in an official return".<sup>60</sup> This illustrates that measurement procedures are decision-making activities in their own right - no matter how unconscious the producers of a measure are of their political role. Measurement of unemployment during this period of New Zealand's economic and social history influenced definitions of the 'problem' for policymakers and the solutions or lack of solutions proposed.

Public officials were the first to set about analysing the new 'registered unplaced' figures forthcoming from the Secretary of Labour. The Secretary emphasised that applicants for employment from whom the Department of Labour had not heard for

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60 NZPD, e.g., 1926, Vol.210, p.31 et.passim, and p.61.

fourteen days were excluded from the figures; statistics of private labour bureaux were not included since the unemployed could register at both public and private exchanges so that any inclusion of private data would have resulted in an unknown degree of duplication. In official publications the notion of seasonal unemployment evolved for the first time out of the Statistician's analysis of figures of unplaced applicants, who were now regarded as the unemployed:

A marked seasonal fluctuation will be observed. The number of unplaced applicants is always at a minimum at Christmas time, when there is a great demand for labour for shearing in the country, and in connection with Christmas orders in the wholesale, retail, and manufacturing establishments of the cities and towns. Unemployment continues to be small until the conclusion of the grain and threshing seasons, but after March, with the closing of many of the freezing works, the tendency to rise is greatly augmented, until the maximum for the year is reached some time in June or July, about which months it is customary to engage additional labourers on the more important public works.<sup>61</sup>

Not until 1929 does the Secretary of Labour begin to analyse, in any great detail in his Reports, the phenomenon of seasonal unemployment.<sup>62</sup>

(d) Unemployment Statistics During the Depression Years of the Early 1930s

As the depression worsened, the Prime Minister called the National Industrial Conference (March 1928). The Conference

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61 NZOYB, 1926, p.775, my emphasis. See also NZSR, 1927, p.xvi.

62 AJHR, 1929, H-11, p.2.

aired much concern about the existence of wide-spread unemployment; the problem was brought to the centre of public consciousness. A milestone was passed in New Zealand's social history: "The employers, farmers, Chamber of Commerce and manufacturers of New Zealand had agreed that the care of the unemployed was a function of the State, and Coates had given his mandate to work something out ...".<sup>63</sup> The Bureau of Labour was responsible for measuring unemployment and responded at about the same time by loosening procedures for registration. From March 1928, jobless males could register at post offices or by post to officers of the Labour Bureaux - both requiring two week renewals. Many postmasters acted as employment agents.<sup>64</sup> If Coates was to be successful in 'working something out' as Sutch put it, he needed to know the magnitude of the 'problem'. The statistics that were published before 1928, particularly prior to 1926, were not able to bear either credibility in the ever-growing public attention or the soon to follow policy decisions partly based on them. Bureaux statistics were criticised by one parliamentarian: "A greater effort has been made this year to get at the actual number of unemployed; but ... there are hundreds of unemployed ... who are not registered".<sup>65</sup> Following a government announcement that work would be offered to all unemployed males registered the number registered as unplaced

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63 Sutch, W.B., Colony or Nation? Economic Crises in New Zealand From the 1860s to the 1960s, Sydney, Sydney University Press, 1966, p.47.

64 See NZOYB, 1932, p.727, 728.

65 NZPD, 1928, Vol.217, p.536.



rose from 2,466 at the end of September 1929 to 6,263 only a fortnight later.<sup>66</sup> The promise of relief brought out the previously hidden unemployed and gave them an incentive to register. A further factor which during 1929 and 1930 undoubtedly caused a larger proportion of the unemployed to register was the policy of engaging all men for public works and local bodies' relief works through the bureaux.<sup>67</sup>

Coates set up a Special Unemployment Committee which made recommendations that culminated in the Unemployment Act (1930). The Act made male registration with the Labour Department an essential preliminary for obtaining placement or relief. However, no relief was given without work in return until 1933. The Committee's report, tabled in the House during 1930, included "a review of the statistics as to the unemployed". It recommended that an unemployment board, when instituted, "have power to make recommendations ... as to the desirability of obtaining and publishing statistics of unemployment ... which it may consider helpful in dealing with the unemployment situation".<sup>68</sup> In a review of the Committee's Report, Fisher noted that a comprehensive unemployment insurance scheme for New

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66 AJHR, 1930, H-11B, p.19.

67 For an admittance of this influence on the statistics see NZSR for 1930, (1932), p.xvii.

68 AJHR, 1930, op.cit., pgs. 1, 8. The Unemployment Act 1930 (henceforth UA) provided for establishment of an Unemployment Fund (UF); an Unemployment Board (UB) and for compulsory registration of all men 20 years of age or older. Registration was necessary for the purpose of levying wages to finance the UF. The Employment Bureaux of the Labour Department were transferred to the control of the UB.

Zealand was rejected by the Committee because of the short-run difficulties involved in building up 'an adequate statistical basis for unemployment'. He pointed out that

objective knowledge of this kind is essential if unemployment is to be removed and not the least important of the functions suggested for the Board is the regular collection and publication of the results of such statistical investigation.<sup>69</sup>

As an aside, it may be noted that the Committee had no time for trade union unemployment statistics which were available. It did not consider admitting them as data to background its first report:

the Committee ... [has] dealt only ... with those on Unemployment Registers. It is well known that the employment offered by the Labour Department is mainly of an unskilled character, and that skilled tradesmen are not in the habit of registering when seeking new jobs. Whatever the number of unemployed there are unregistered the Committee are not in a position to deal with them, and the following statement is confined entirely to those on the labour bureaux registers.<sup>70</sup>

In one sense this suggests that public policymakers felt their responsibility to the unemployed extended only to those left on labour bureaux registers. In another sense, it implicitly casts aspersions on the reliability and coverage of union statistics - inasmuch as the statistics were biased toward well-unionised and skilled tradesmen. This probably added weight to the official case for their deletion from statistical reports after 1930. A consensus emerged that the problem of unemployment was best

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69 A.G. Fisher, 'Unemployment in New Zealand', ER, 6, 1930, pgs. 119, 123.

70 AJHR, 1929, H-11B, p.3.

observed and comprehended 'objectively' only when quantified by a public authority. By 1932, the NZOYB was able to point out that "more reliable statistics as to the extent of unemployment [are] now available as a result of the activities of the Unemployment Board".<sup>71</sup> The UA provided for the administrative machinery whereby more comprehensive statistics could be collected. The concept remained narrow insofar as it only included males of 20 years or older who were eligible for placement or relief work under the UA. Applicants off the unemployment register for over three months were treated as new registrants on next application and thereby added to the total unemployed. Renewed applications at shorter intervals than three months (whether or not temporarily employed) were regarded as continuously on the register.<sup>72</sup> In summary, the new statistics of unemployment occasioned by the UA were used for the purpose of administering a new policy. They helped define and interpret a 'relief' problem rather than an unemployment problem directly. The measure helped build up the concept of the relief problem in order to fit the solutions perceived appropriate by public policymakers. Its informational value for the public, and for future researchers interested in the extent of unemployment in the 1930s was incidental if not misleading. Thus one contemporary economic researcher during the early 1930s complained: "The figures ... give no analysis of the unemployment problem in New Zealand; its absolute volume; its

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71 1932, p.723.

72 AJHR, op.cit., 1930, p.22.

distribution or its causes".<sup>73</sup> The statistics of unemployment at this time changed to suit their active use in the policy process in the light of new political and administrative difficulties. Unfortunately, as a consequence, the unemployment series published by the UB during its tenure in the 1930s were far from consistent. The measures underwent several changes involving mainly changes in definition: included and/or excluded in the measures from time to time were men on full-time and/or part-time government relief work. In an assessment of the level of unemployment for the 1930s Westrate complained that, whether "unemployment in New Zealand was exceptionally severe ... is difficult to judge" because "the statistics are incomplete" and "there are differences in definition of the concept of 'unemployment'".<sup>74</sup> If changing operational definitions in the early 1930s are anything to go by, prevailing economic thought about unemployment in official circles (and probably in society at large) was severely shaken. The numbers registered and unplaced were counted as were those given sustenance without work from December 1933, but data coverage was limited in that it did not include people younger than 20 years of age or women. Women's employment exchanges were set up but no count was made of people registered throughout the country. There was little incentive for women to register because the government "found [it] practically impossible to

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73 Murphy, E.W., 'Some Aspects of the Unemployment Problem in New Zealand', M.A. thesis, University of New Zealand (unpublished), 1930, p.6.

74 Westrate, C., 'Unemployment in New Zealand', ER, 33, May 1956, p.141.

subsidise female employment without in some way prejudicing normal employment".<sup>75</sup> The prevailing official view during the early 1930s was that the 'surplus' urban population on government relief works was still unemployed, simply because it was a charge on the UF. For instance, appended to the UB report for the week ended 10/6/33 containing sections headed "Numbers of Unemployed" was a caveat that the statistics included men who were "engaged on work of a definite reproductive and developmental nature in connection with the Dominion's primary industries".<sup>76</sup> The UB argued that "it was imperative" to retain men 'employed full-time [44-48 hours per week] on relief work on the unemployment register'.<sup>77</sup> The NZOYB usually reinforced this view or alternatively emphasised that the amount of employment which actually existed, rather than that which did not, deserved more analysis.<sup>78</sup> The UB faced increasing placement difficulties as the Depression worsened. A disincentive was added to male registration as it became necessary to remove men from their families and send them back to the land for long periods. This policy, together with the dictum that "all money must be worked

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75 AJHR, 1932 III, p.23. A total of women registered in the four main cities at the end of 1933 appears in AJHR, 1933, H-35.

76 Similar comments were added to the statistics in reports dating back to the week ended 1/10/32. See NZA, C.S. 22/50/24.

77 AJHR, 1933, H-35, p.6.

78 NZOYB, e.g., 1929, p.858ff.

for" (up to late 1933) was "immutably imposed by natural economic law".<sup>79</sup> The principle of paying sustenance without work, which was adopted towards the end of 1933, was rendered necessary by the increasing difficulty of finding work of a suitable nature particularly in the vicinity of larger cities and the difficulty of finding work for men who were unable, from old age or physical condition, to undertake ordinary labouring work. Eventually the UB found itself in financial difficulties. Among other changes in the regulations governing relief measures, the statistics produced for the UB were subject to direct political pressure and manipulated for political ends. Whilst it may have been the case that the numbers remaining on the register during 1932 and 1933 "mounted to proportions which were beyond the grasp of most people ... [and appeared as] mere numbers in a statistical return",<sup>80</sup> the numbers meant much more to public policymakers. More unplaced applicants and shortage of finance forced the government to ration relief work; reduce relief work wages; increase unemployment levies on wages and salaries; pay sustenance without work and surreptitiously change the definition upon which unemployment was measured so as to reduce the cost of relief.<sup>81</sup> There were frequent complaints about political interference with official statistics. At this time, the statistics were not sufficiently institutionalised to

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79 AJHR, 1932, op.cit., p.23.

80 Sutch, Poverty and Progress, op.cit., 1941, p.134.

81 Lawn, G., 'Unemployment Relief in New Zealand', ER, 7, 1931, p.305 alluded to tighter informal administrative practices governing unemployment registration but did not elaborate. There were no NZG statements at this time regarding tougher, formal registration regulations.

be shielded from the vagaries of day-to-day politics. As well, the Census was not taken in 1931 for reasons of economy and this lends weight to the view more recently expressed by MacRae and Sinclair that: "The Coalition Government of 1931-5 had no wish to collect statistics revealing the full extent of unemployment".<sup>82</sup> This is reinforced by parliamentary debate over postponement of the Census. Debate centred around unemployment statistics. According to H.E. Holland, whereas the number of unemployed in the Dominion around the time of the 1926 Census was 711 (going by those remaining on bureaux registers), the 1926 Census "showed that 10694 persons registered themselves as being out of employment. That in itself demonstrates the need for having reliable figures which the Census provides".<sup>83</sup> J.A. Lee later complained that information gathered on the number of unemployed in 1932 "was shrouded in secrecy" and that official statistical tables "appeared to have been drawn up in such a way as to conceal the real position".<sup>84</sup> A question was addressed to the Acting Minister of Employment about the removal of rural Maori males from the register in the last quarter of 1932. The Minister, in reply, stated that the idea of unemployment for the Maori differed from that of the non-Maori;

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82 MacRae, J. and Sinclair, K., 'Unemployment in New Zealand During the Depression of the Late 1920s and Early 1930s', AEHR, 15(1), 1975, p.36. The Act postponing the Census contained no official justification for the legislation. See NZS, 1930, No.26, p.103.

83 NZPD, 1930, Vol.226, p.843. See also pp.840-842.

84 Ibid., 1932, Vol.234, p.170. C.f., also a call in parliament for the development of new methods for counting the unemployed: ibid., 1933, Vol.235, pp.1105-6.

that there were "difficulties in opening the Unemployment Fund too widely to Maoris". Since the UB "had to take into account the number of registrations and money available", and because "expenditure could not continue ... unless the registrations fell", then one solution was to make it more difficult to register, and Maoris were the first to be struck-off and/or stopped from registering. By succumbing to a temptation to use the statistic to suit the short-term book-balancing interest of the UB as an administrative agency this event marked the nadir in the evolution of this unemployment measure in New Zealand.<sup>85</sup>

Methods used to code and present unemployment statistics during the mid-1930s were continuously redesigned thereby adding to official confusion as to the magnitude of unemployment as well as to a more basic question viz., the meaning and definition of unemployment. The NZOYB distinguished those "totally unemployed" from those obtaining full or part-time relief work under the UB's scheme No.5.<sup>86</sup> By way of contrast, the MAS from January 1935 included as unemployed both (a) the number of new and renewed applications for employment at government employment bureaux and (b) men in employment but remaining on the UB register. Column (b) was annotated with a comment that:

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85 Separate registers were kept for Maoris but from 1933 onwards no figures on Maoris were published or evident in NZA. See AJHR, 1933, H-35, p.5. The question and reply are recorded in NZPD, 1932, Vol.234, pgs. 319, 412, 413.

86 E.g., see NZOYB, 1932, p.729.



All men on the Unemployment Board's Scheme 5 are included as applicants remaining on the books. Most of these men are in partial employment only. Some of them are, however, in full-time employment, part of their wages being paid under Scheme 5. Other men in full-time employment, whose remuneration is wholly or partly paid out of the funds of the Unemployment Board are not included in the above table.<sup>87</sup>

To add further confusion to the contemporary meaning of the statistics and to add irreconcilable discontinuities to the series published in the MAS, the numerical data was compiled and presented in two different ways later in 1935 (refer to Tables 5.11(A) and 5.11(B) below).

TABLE 5.11(A)

APPLICANTS FOR RELIEF ON REGISTERS 8TH JUNE 1935

Week ended	Number of New and Renewed Applications for Employment	Number of Place-ments	Number of Applications remaining on the Books at End of Week					
			Urban Areas				Rest of Dom.	Totals
			Auck-land	Welling- ton	Christ- church	Dunedin		
8 June 1935	1504	250	10194	6190	6212	2094	22518	47208

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87 MAS, Vol.22, January 1935, Table II, p.30.

TABLE 5.11(B)  
NUMBERS OF APPLICANTS NOT PLACED, AND NUMBERS  
IN RECEIPT OF RELIEF, 8TH JUNE 1935

Date	Registered Unemployed Unplaced or Ineli- gible for Relief	Number of Men a Charge on the Unemployment Fund			
		In receipt of Sustenance without Work	On Rationed Work	In Full-time Subsidized Employment*	Total
8th June 1935	3,025	11,262	25,188	16,791	53,241

\* Employed in industrial undertakings, or on farms, gold prospecting, &c., with earnings subsidized from the Unemployment Fund. Men employed on relief works by Public Works Department are included.

SOURCE: MAS, 26th October 1935, p.28.

This presentation distressed the UB even though it appeared to allow users of these statistics wide choice as to which categories of relief and so on, would legitimately constitute unemployment. UB Commissioner, J.S. Hunter, wrote to the Government Statistician expressing his desire "not to have published ... the actual figures of those engaged full-time in industry whose employment is made possible by subsidy from the Unemployment Fund". Moreover, "[o]ne of the main reasons for discontinuing the collection [and publication of these] ... figures is that such figures ... have been used without cognisance of their true connotation".<sup>88</sup> Hunter no longer

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88 Unemployment Board Commissioner to Statistician, NZA, L 27/1/15 on 25/11/1935.

wanted those who were working full-time with the help of a subsidy from the UF to be tabulated among statistics headed 'unemployed', despite the fact that only a few years earlier any other interpretation would have been entirely out of court. In a highly accommodating reply the Statistician agreed to this change of definition because

the policy of the Unemployment Board is directed towards the diminution of unemployment by the rehabilitation of workers in industry, with the assistance of the Unemployment Fund. While the employment of these men is a direct result of the Board's activities, it is not proper to regard them as 'unemployed' since the men are in full-time employment, in many cases, at their normal trade or occupation ... The exclusion of these men from statistics of unemployment is in accord with international practice, and it is obviously desirable that New Zealand ... statistics of this nature should be compiled in conformity with international usage.

The Statistician recognised that the "more important point" at this time was "the definition of the 'true' unemployment". He appreciated that "there is something to be said for various connotations of the term 'unemployment'" and was "quite prepared to meet the wishes of the Board in this respect".<sup>89</sup> Accordingly, tables in the MAS were altered. The December 1935 edition tabulated unemployment statistics in the following way - finally excluding all reference to those on full-time public relief work. (See Table 5.12).

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89 Ibid.

TABLE 5.12

REGISTERED UNEMPLOYED MALES.--SUMMARY FOR 8TH JUNE 1935

Date	Registered but not Receiving Relief	Receiving Part-Time Relief			Total Unemployed
		Scheme No.5 (Rationed Work)	Sustenance without Work	Total	
8th June 1935 ..	3025	25043	11262	36305	39330

SOURCE: MAS, 23rd Dec., 1935, p.28.

A new official concept of unemployment was emerging out of the turmoil of the Depression. In comparison with earlier editions of the MAS during the 1930s these tables clearly come out with an unambiguous definition of unemployment and an actual total labelled unemployed males. The operational concept changed to a degree that moved the Statistician to comment: "figures ... comparable with those previously published are not available".<sup>90</sup>

(e) New Counting Definitions 1936-1940

A new chapter in the official design and interpretation of numerical observations relating to unemployment opened with the government's commitment to job creation expressed through new legislation under the Employment Promotion Act (1936). The UB was disbanded by the new Act and responsibility for collection of unemployment statistics reverted to the Labour Department.

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90 NZSR for 1935, Wellington, Govt. Printer, 1937, p.xix.

New factors came into play which influenced, and changed, counting rules associated with registration statistics in particular. The Act emphasised promotion of employment rather than provision of temporary relief. In general terms it directed the authorities to actively make "arrangements with employers or prospective employers for the employment of persons who are out of employment".<sup>91</sup> Registrations for women (20 years or older) and juveniles (between 15 and 20 years old) were allowed in addition to those previously kept on adult males. However, no figures for females appear, or are included in any published statistics prior to 1940. Payments of money, meals, clothing and so forth for women and unemployed girls through the Women's Employment Committee were increased in 1936 especially when the recipient was living away from home. It may be noted that, according to one official commentary, during 1936-38 "unemployment among women and girls was not serious and there was a distinct shortage of certain types of female labour".<sup>92</sup> Overall, sustenance without pay was unequivocally accepted from 1936; sustenance rates were increased in early 1936 and this attracted to the unemployment registers "a section of the community which before had not availed itself of ...

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91 New Zealand Statutes, Govt. Printer, Wellington, 1936 No.3, S35, p.44. The State Placement Service was inaugurated as a result and its activities were carried out through the medium of four District Employment Offices (in the four main centres) and forty four offices of the Social Security Department (fifteen Registrars and twenty-nine District Agents).

92 New Zealand Social Security Department, The Growth and Development of Social Security in New Zealand 1898-1949, Wellington, Govt. Printer, 1950, p.36.

assistance".<sup>93</sup> Women and juveniles (over minimum school leaving age of 15 years) were afforded State Placement Service assistance and unemployment registration became (for a time) a prerequisite for sickness benefits. Men suffering from ill-health were regarded as unemployed and counted as such. Sickness allowances were actually given to men registered as unemployed and later these allowances were extended to men who, although not registered, were in necessitous circumstances.<sup>94</sup> Prior to 1936 men were not able to receive sustenance unless they were fit to undertake manual work. They may have been counted as unemployed but in the class labelled 'ineligible for relief' (see Table 5.11(B)). Even so, the fact that they could not receive sustenance from the UB (instead from voluntary relief organisations and Hospital Boards) would have mitigated against their registration with the UB as unemployed. After 1936 with sustenance provided for ill-health, unemployment registration was more personally worthwhile. Furthermore, according to a circular sent to all district Labour Department placement officers during 1937, Maoris were also now accepted as unemployed registrants. Certifying officers of the Department were directed to "determine [the] eligibility [of Maoris] for relief in accordance with the same rules of eligibility as apply to Europeans".<sup>95</sup> In the final analysis, all these legislative and administrative changes led to significant

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93 AJHR, 1939, III, H-11A, p.3.

94 N.Z. Social Security Dept., op.cit., 1950.

95 NZA, L 1/1/12, on 20/7/1937.

alterations in counting rules for unemployment statistics and implied corresponding changes in prevailing thought patterns about unemployment.

By mid-1937, "unemployment had become in the minds of the people a matter of economic mismanagement".<sup>96</sup> It was also officially accepted, again unequivocally, that public works were an avenue for employment so that people on public relief work were not now counted in statistical tabulations headed 'unemployed'. Ruth estimated that 20,000-25,000 "unemployed relief workers" underwent a change in status in the statistical returns during 1936.<sup>97</sup> The new official outlook followed hard on the heels of political and legislative changes and was accompanied by increased efforts to count and classify accurately the unemployed. Finer distinctions were drawn among those registered as unemployed who were now grouped according to a whole host of variables and also according to why they were not working. A number of new categories of joblessness and degrees of 'employability' appeared in the extensive review of unemployment carried out by the Department of Labour on the 30th September 1937. (For the official survey form see Appendix V(B).1 which details the lengths to which officials went to screen and classify job applicants in order to help place them in suitable occupations). This was one of the earliest official surveys of registered unemployed in New Zealand -

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96 Rosenberg, W., Full Employment: Can the New Zealand Miracle Last?, Wellington, Reed, 1960, p.11.

97 Ruth, N., 'Full Employment in New Zealand', ER, Vol.26, 1950, p.100.

probably given impetus by a recommendation (No.45) of the 1935 ILO Conference to the effect that special official inquiries be made on unemployment from time to time with respect to duration of unemployment and occupational histories of persons concerned.<sup>98</sup> More significantly from the point of view of published unemployment statistics, three return forms were sent to all Placement Service offices and were to be used for collating unemployment statistics from 29th February 1938. The forms: E.P.324, 325 and 326 are reproduced below:

TABLE 5.13

NEW ZEALAND PLACEMENT SERVICE: ENROLMENT RETURNSEnrolments Return E.P.324

Industry	Of Disengaged in Search of Work			Of Employed in Search of Better Work		
	New	Renewals	Total	New	Renewals	Total
...	...	...	...	...	...	...

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98 For a list of relevant ILO resolutions at this Conference, see C.P.P., (Australia), 1934-7 II, p.732. See especially Recommendations No.44, 46, and 47 headed "statistics of unemployment". Other official censuses and surveys of registered unemployed were carried out by the Labour Department during the late 1930s in New Zealand. For details see NZA, L 1/1/12.



E.P.325

Placement Office:

Week ended:

(Note: Once a man has been included in this return he must not under any circumstances be included again during the same year).

Reasons for Enroling	Number of New Enrolees	
	During Week	To date
No previous employment	...	...
Cessation of employment	...	...
From gaol	...	...
From hospital	...	...
No previous employment since recent arrival from Australia	...	...
No previous employment since recent arrival from overseas (other than Australia)	...	...
Still in employment	...	...
Other reasons	...	...
Totals	...	...

Summary TablesE.P.326

Weekly and Cumulative Totals of Individuals and Enrolments to Date

	During Week	To date
Number of unemployed individuals enrolled	...	...
Number of employed individuals enrolled (in search of better work)	...	...
Total number of individuals enrolled	...	...
Total number of new and re-enrolments	...	...

SOURCE: NZA, L 1/1/12.

Form E.P.326 in particular, reveals that those remaining enrolled on the registers of the Placement Service were only regarded as unemployed if they were not enrolled as gainfully occupied and in search of better work. Also, E.P.325 shows that

the statistical returns excluded re-enrolments, so that any persons who were sent to work more than once in a given year did not have to renew their applications and this avoided any possible duplication. A separate count of persons who had never been in paid employment was also an innovation - but no statistics were actually published along these lines.<sup>99</sup>

The confidence inspired by increasing placements through the Service during 1937 and 1938 caused some placement officers to advertise their success locally. Over the period 1892-c.1925 placement figures were published and interpreted as the criterion of administrative success in dealing with the unemployed and more especially in satisfying the needs of employers. A new official attitude began to emerge in early 1938 when the habit of advertising local placement services was reproved by Head Office. In a memorandum to all placement officers the Director of the Placement Service wrote:

A particularly undesirable feature has become increasingly evident in local placement publicity - the claiming of "record placements". It should be obvious to any thinking officer that mere numerical indications of placements effected is not in any way a true factor of comparative efficiency or successful effort; indeed it may actually be the reverse ... In making a true comparison such factors as suitability of applicants; duration of employment ...; age of candidate, prospect of advancement ... must be taken into consideration, and no placement officer has the information which would enable him to make a true comparison.<sup>100</sup>

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99 This new code for those not previously gainfully occupied is evident in the archives for the 1930s and may have been prompted by a 1935 ILO conference recommendation on unemployment statistics. See C.P.P. (Australia), op.cit., 1934-7, No.44(2)(c), p.732.

100 NZA, L 1/1/12 on 11/2/1938.

Here was a case where the official interpretation of numerical observations ("placements") based on a counting rule that had remained almost unchanged since 1892 was altered in line with a new policy emphasis. The new acceptable official stance was redefined insofar as the needs of those placed were as important if not more so, than the labour requirements of employers.

In 1939 the Labour Department changed its method of presentation and its definition of registered unemployed applicants in order to exclude people unfit for work. Two purposes for the change were made explicit: (i) to present the data in line with international practice and (ii) help the government assess the "real number of unemployed".<sup>101</sup> Again, the concept and measure underwent a subtle metamorphosis in line with public policy concern to gauge labour supply for the expansion of public works. It illustrates how statistics of unemployment were redesigned in interaction with prevailing employment policies which they helped formulate. A circular from the Department of Labour addressed to the Census and Statistics Department headed "Unemployment Return For 11 March 1939" was set out as follows:

Registered but not on relief	649
On sustenance awaiting placement	-
On Scheme No.5, relief	77
	<hr/> 726

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101 AJHR, 1939, H-11A, p.20. Although no reference is made in this Report to any specific international practice or obligation, it was likely that League of Nation's recommendations would have influenced the decision not to count those unfit as unemployed (or, alternatively, as those with the potential, if work was available, to be gainfully employed). See League of Nations: Statistics of the Gainfully Occupied Population: Definitions and Classifications Recommended by the Committee of Statistical Experts, 1938, II A.12, Studies and Reports on Statistical Methods, No.1, Geneva, 1938.

On sustenance, unfit for employment  
for health or other reasons, but  
being offered relief from the  
Employment Promotion Fund 7,256.

The circular dated 9/5/39 explained that "[t]his is the last statement of unemployment in the Dominion that will be presented in this form". The Employment Promotion Act 1936 had been repealed, and, under the new social security legislation (Social Security Act 1938) no provision existed for the Labour Department to continue its purely unemployment relief activities. More significantly perhaps, the official circular emphasised that, thenceforth, "the incidence of unemployment will be reflected by the numbers in receipt of unemployment benefits provided under the Social Security Act of 1938".<sup>102</sup> The activities of the Employment Division of the Labour Department were now restricted solely to promotion of employment and placement work. From 1/4/39, payment of sustenance was superseded by unemployment benefit under the new Act.<sup>103</sup>

The Labour Department's Report for the year ended March 1939 expressed the notion of unemployment in a different way, although the statistical presentation reinforced the idea that those unfit for work were not to be counted as unemployed. The "true" number of unemployed, according to the Report was "represented by the total number who are without a contract of employment - i.e. sustenance men, Scheme No.5; and those who are registered for work but ineligible to receive relief for

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102 NZA, CS 22/50/24, my emphasis.

103 For details see NZOYB, 1940, p.846.

various reasons. Men in full-time employment ... cannot be classed as unemployed".<sup>104</sup> Refer to Table 5.14 which is the series of numerical observations around which this interpretation revolved:

TABLE 5.14

"Numbers of Unemployed" In AJHR 1939

(1) Four Weekly Period	(2) Registered but not on Relief, a- waiting Expiration of Qualify- ing Period	(3) On Scheme No.5 Rationed Work Relief	(4) On Susten- ance await- ing Place- ment	(5) Total No. of Unemployed on Register	(6) Unfit for Work for Health or Other Reasons
1938					
15 Jan.	1051	2827	4178	8056	8000
12 Feb.	1004	2735	3502	7241	8000
12 Mar.	1086	2735	2874	6695	8000
9 Apr.	1135	2799	3281	7215	8000
7 May	1244	2848	4222	8314	8000
4 June	1301	2812	4608	8721	8000
2 July	1551	2032	3240	6823	8000
30 July	1364	1244	1321	3929	8000
27 Aug.	881	913	360	2154	8000
24 Sept.	771	743	61	1575	8000
22 Oct.	726	519		1245	7964
19 Nov.	507	519		1026	7583
17 Dec.	407	510		917	7285

SOURCE: Ibid., Table VIII, p.20.

Aside from the suspect nature of returns in column (6) where there was an unvarying round-figure total for nine-months, it may be noted that those considered "unemployed" in column (5) were exclusive of those in column (6). Clearly, this redefinition of unemployment excluded many thousands of workers regarded as unfit for work for health reasons - but it is not

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104 AJHR, op.cit., 1939, p.3.

made clear in official reports exactly what may have constituted legitimate 'other' reasons in column (6). That is, no clear counting rules are evident for column (6) which obscures their meaning. Finally, it is notable at this point that, in comparison with labour exchange statistics and their interpretation in New Zealand prior to 1930, unemployment was not now discussed in official reports around numerical observations relating to the number of job positions filled through the medium of Labour Department exchanges.

For the first time in the NZSR in the section headed "Statistics of Unemployment" a new table appeared in 1939 that, going by the official Department of Labour definition communicated to the Census and Statistics Department on 9/5/39 (already discussed), now served as the best official measure of unemployment. It was set out in the following manner:

TABLE 5.15

UNEMPLOYMENT STATISTICS: NZSR 1939

"Numbers of Males in Receipt of Unemployment Benefit  
Under the Provisions of the Social Security Act,  
April, 1939, to March, 1940."

Date	Number in Receipt of Benefit.	Date	Number in Receipt of Benefit.
1939--April 29 ..	4,915	October 28	6,015
May 27 ..	5,565	November 25	4,924
July 1 ..	7,036	December 30	5,042
July 29 ..	8,009	1940--January 27	4,200
August 26 ..	8,066	February 24	3,995
September 30	6,805	March 30 ..	4,053

SOURCE: NZSR, 1939, p.23.<sup>105</sup>

105 Initial amounts of benefit are detailed in NZOYB, 1940, pp.632-633. For a more recent discussion of benefits for unemployed persons from 1939, see Te Heu Heu, R., 'Assistance For Unemployed People', in Palmer, G., ed., The Welfare State Today, Wellington, Fourth Estate Books, 1977, pp.317-318.

In addition to the possibility of gaining placement, registration as "disengaged" with the Department of Labour was necessary to obtain unemployment benefits. One qualification necessary to be counted among those in receipt of a benefit was a minimum sixteen year age limit - whereas registration as disengaged included fifteen year olds as well. Many other qualifications for the receipt of a benefit would have affected or acted as an incentive or disincentive, as the case may have been, for registration as disengaged. (Although, numbers registered as disengaged were not officially viewed as those unemployed at this time). In the context of the new official stance from April 1939, counting rules for enumerating those in receipt of unemployment benefit were as follows. That the person:-

- (1) is over sixteen years of age and unemployed (registered with the Department of Labour as 'disengaged');
- (2) is capable of undertaking and is willing to undertake suitable work;
- (3) has taken reasonable steps to obtain suitable employment (the benefit may be withheld if unemployment is deemed the fault of the applicant);
- (4) has resided continuously in New Zealand for not less than twelve months, and
- (5) reports weekly to the Labour Department District Office which is equivalent to re-enrolment as disengaged.

Further, a seven day waiting period was usually necessary between application for benefit and its receipt. Married women were entitled to the benefit only if their husbands could not

maintain them. The rates of benefit were reduced when the applicant or his wife earned more than a certain minimum amount.<sup>106</sup> If all efforts to find employment for an enrollee failed, he may have qualified for an unemployment benefit or emergency benefit or, in certain circumstances for subsidised employment. The payment of unemployment benefit and inclusion in the statistics of Table 5.15 was regarded as a last resort. The District Employment Officer was to ensure that "no person is referred to the Social Security Department for unemployment benefit until they are satisfied that every conceivable step has first been taken to effect placement of the worker". Lastly, persons enrolling as disengaged who were over sixty years of age were instructed to apply for age benefit rather than unemployment benefit.<sup>107</sup>

The new measure of unemployment contained in Table 5.15 referred to official 'involuntary' unemployment. The number of persons on unemployment benefit invariably fell short of those registered disengaged because of some of the above qualifications. In any case, unemployment benefit could only be obtained when the applicant overcame a number of bureaucratic obstacles. This often included an involuntary unemployment test, which, under Section 54 of the Social Security Act 1938 made special provision for the treatment of persons who rendered themselves voluntarily unemployed, i.e. "leaving their

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106 Department of Labour, Office Manual (dated 1/11/53) Vol.III (with amendments to 1/10/63) Section J, Appendix, paragraph Nos. 2, 3, and Appendix I, para. 9, 11.

107 Ibid., Section J, App. para. 4, and App.I, para. 16.



employment without good and sufficient reason or refusing to accept suitable employment offered to them".<sup>108</sup> The resulting numerical observations in Table 5.15 were interpreted in terms of a social policy (financial relief or security) problem more than a problem for economic policymakers concerned to mobilise unused labour supply. Not all unemployed persons willing to work at then current wage rates were able to secure unemployment benefit and/or actually applied for a benefit. All this is not to say that the official 'line' of interpretation evident in the NZA, viz., that the number on unemployment benefit best represented the "incidence of unemployment" was not consistent with international practice. Indeed, this New Zealand interpretation anticipated an ILO report on the "international standardisation of labour statistics" in 1943 which maintained that, above-all, "[g]ood labour statistics are essential to both the function and administration of progressive social policy".<sup>109</sup> It is arguable, however, that use of unemployment benefit statistics, on their own, to indicate the incidence of unemployment implied an extremely narrow definition of unemployment if not a complete fixation with the social policy problem to the exclusion of all else. By 1944 the statistics presented by the Social Security Department evolved into the following form:

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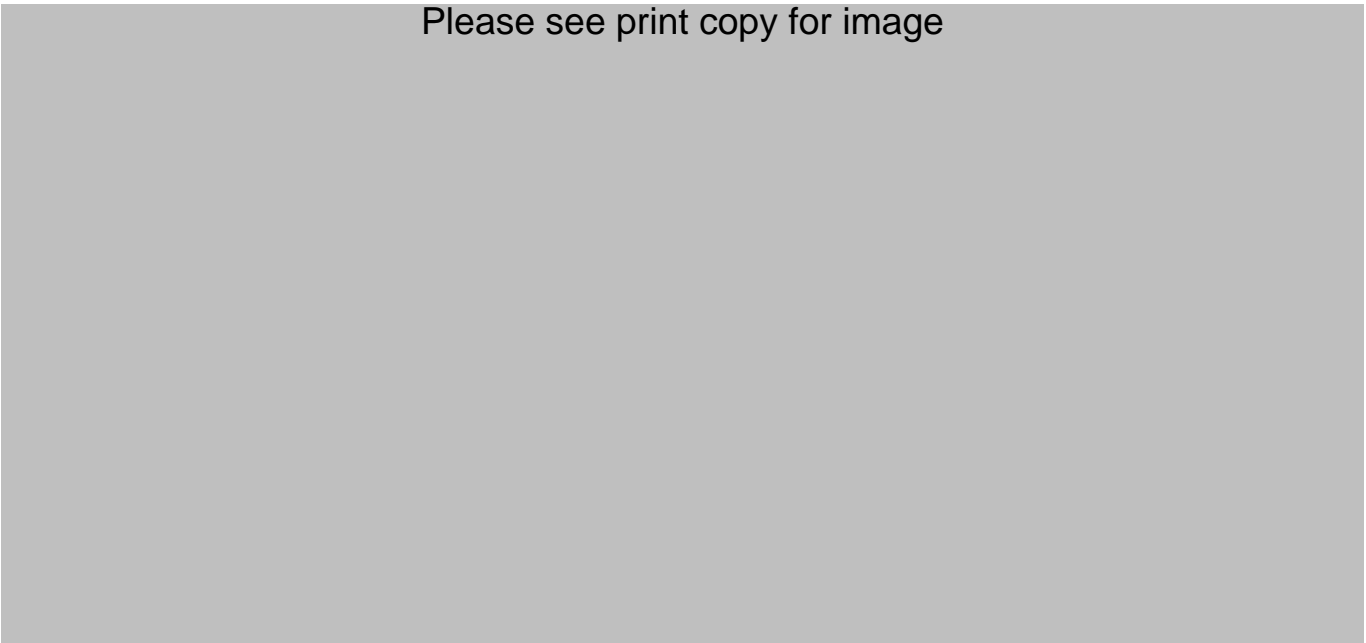
108 Ibid., Section J, App. para. 6.

109 ILO, The International Standardisation of Labour Statistics, Studies and Reports, Series N (Statistics) No.25, revision No.19, Montreal, 1943, p.i, my emphasis.

TABLE 5.16

"NUMBER OF APPLICATIONS FOR UNEMPLOYMENT BENEFITS"31/3/43-31/3/44

Please see print copy for image



SOURCE: AJHR, H-9, 1944, p.1.

These numerical observations, far from measuring labour supply at any point in time, were concerned solely with social policy. Little wonder that use of figures such as these to stand-in as proxies for unemployment fell from favour in official circles after 1945.

(f) The Census of 1936

Treatment of unemployment in the 1936 Census reinforced the Department of Labour's position that relief workers could no longer be counted as unemployed. These workers were excluded from returning themselves as unemployed by the new coding system. The concept of unemployment was measured under the rubric of an occupational status - evidently a humane official response to the Depression experience. At the time of Cabinet

approval for the 1936 Census, the Prime Minister, J.G. Coates, in apparent ignorance of the Census coverage of unemployment in 1926, addressed a note to the Minister in Charge of the Census and Statistics Department: "should not attention be given soon to the possible usefulness of adopting our NZ Census to cover some immediately prominent points - e.g., unemployment ...?". In a patient reply the Statistician, J.W. Butcher, detailed the Census coverage of unemployment in 1926. Despite his obeisance to the politicians, Butcher voiced some concern about delving into the question of unemployment in any great sophistication or detail in the 1936 questionnaire. He was keen to simplify the unemployment questions, since, with regard to 1926 Census questions on this matter, they had

met with only indifferent success, lack of knowledge of the facts, carelessness, ignorance and even ill-will quite seriously affecting the data. As 1926 was a year of comparative prosperity it will be appreciated that continuation of depression conditions in 1936 would probably greatly intensify difficulties in this direction, although it is obvious that questions relating to unemployment must be included in the questionnaire.<sup>110</sup>

As it turned out the 1936 Census unemployment questions proved quite different from 1926. Respondents were asked the number of weeks since finding steady employment; whether (1) wholly unemployed (and/or on sustenance), (2) partially unemployed (but not on relief) and (3) on part-time relief. Users of resulting data could make up their own minds what categories could be

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110 NZA, S 55/1/2, letter to the Hon. A. Hamilton, 21/1/1935 and reply by Butcher, 7/2/1935.

considered unemployment, although, the Statistician had in mind both (1) and (2), going by the terms used. A covering note on the questionnaire directed respondents not to consider themselves unemployed if their position was due to strikes or lockouts. As well, working days lost due to sickness, accident or injury could apparently be taken as a dimension of (1) or (2) by respondents. The concept of unemployment was still open in this Census. There was still much scope for variation in interpretation (e.g. hours which constituted 'part-time' and eligible age).<sup>111</sup> As far as Maoris were concerned the official stance was motivated by a desire to preserve putative "simplicity in form" in the corresponding Maori Census of 1936. No unemployment question appeared in the Census form relating to the Maori labour force.<sup>112</sup>

In summary, a collection of legislative developments during the middle of the 1930s may have had considerable bearing on the numbers registering as disengaged at the Labour Department, on the numbers receiving unemployment benefit, as well as on those returning themselves as unemployed in the Census. The Census questions were open enough for variable subjective interpretation. Certain pieces of social legislation would have helped change social attitudes to retirement age, school leaving age and as to what constituted a full-time job (in terms of hours in a full working week). In a revealing statistical

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111 New Zealand Census of Population and Dwellings 1936,  
Wellington, Govt. Printer.

112 New Zealand Maori Census 1936, Wellington, Govt. Printer,  
Vol.III, p.2.

analysis, Ruth argued that "in New Zealand the 'enforced unemployment' of the 'thirties was replaced with 'desired unemployment' in the forties".<sup>113</sup> Developments influencing absolute numbers in the statistical returns from 1936 included more general acceptance of the forty hour week and fifteen year old minimum school leaving age. Another influence which deserves attention is the hastening trend toward earlier retirement given incentive by provisions of the Social Security Act. According to Ruth's data on old-age benefits and sickness benefits, in 1926 30.2% of males over 65 years of age received pensions; in 1934 this figure was 42.3% and by 1939 the same proportion was 60.9%.<sup>114</sup>

#### 5.8 New Operational Concepts to c.1960

Events surrounding New Zealand's involvement in the Second World War led to an ever-increasing demand for labour and prompted the creation of new definitions and measures of unemployment. The government attempted to do everything possible to encourage people to work - so much so that by March 1942 the Labour Department Placement Service did not find it necessary to include an unemployed category in the statistical background to its report. Unemployment was discussed in the context of the numbers receiving unemployment benefit through the Social Security Department at this time. Categories existed

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113 Ruth, 'Full Employment in NZ', op.cit., 1950, p.103.

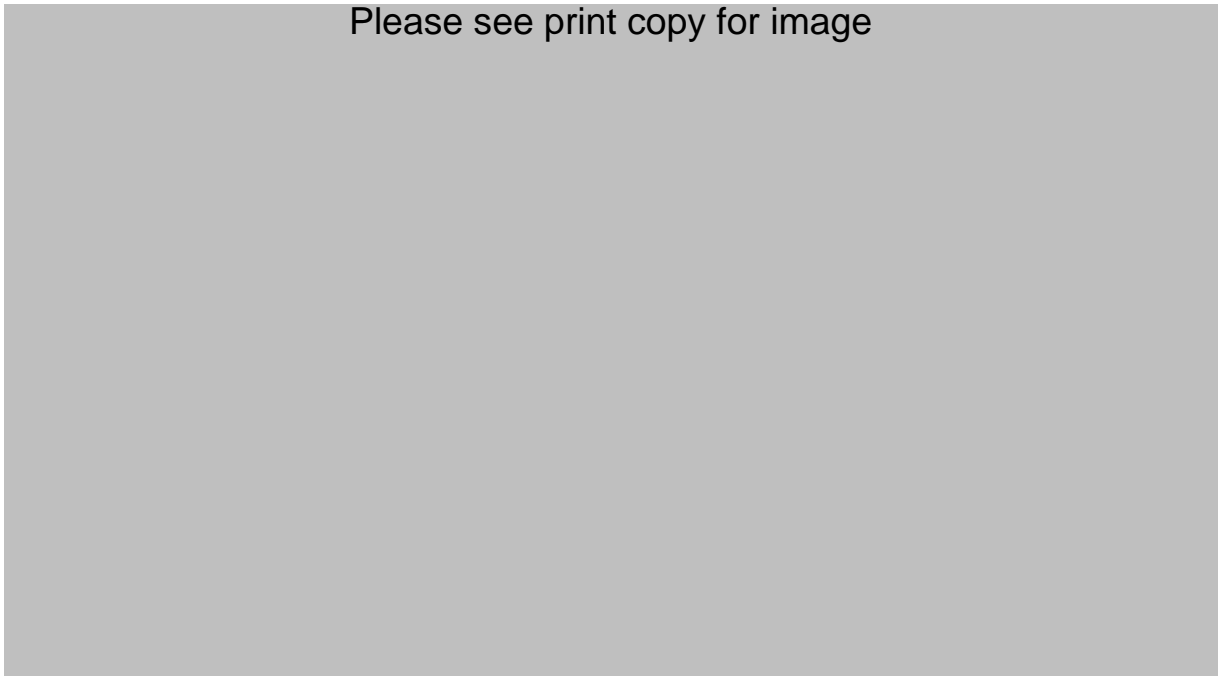
114 Ibid., p.101 Table II. For details and discussion of other influences, see Ruth, p.101ff.

for people "enrolled for employment" ranked according to the distance they were prepared to move to gain work. Those fit for light work and unemployables were counted but no discussion was provided to explain these categories.<sup>115</sup> Enrolments, both male and female were coded according to their "industrial fitness" (see Table 5.17).

TABLE 5.17

PERSONS "ENROLLED FOR EMPLOYMENT": MARCH 1942

Please see print copy for image



SOURCE: Ibid., p.4.

In comparison with previous official strictures against publishing statistics of enrolments which included people actually employed but looking for better jobs, the statistics in Table 5.17 from the 1942 Report included "[m]any ... persons ... in employment and ... registered with the Service merely for the

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115 AJHR, 1942, H-11A, p.4.

purpose of obtaining more suitable employment".<sup>116</sup> There is no way that users of these statistics are able to distinguish the number out of employment in order to calculate a stock of unused labour. Numbers on unemployment benefit paid through the Social Security Department during 1942 provided the official measure at this time, despite the fact that not all persons who were out of work could obtain a benefit.

During 1942 the government introduced the Industrial Manpower Emergency Regulations. An immediate effect was to change the official meaning of unemployment. The regulations required individuals in specified age groups to register for employment if they were not already working. During 1944-5 these regulations were used to cast the net for more labour wider and eventually they covered all males 18-59 years of age and all females 18-40 years of age. The official objective in constructing the measure was to locate and register every civilian liable to direction into employment of national importance and "such power to direct individuals can necessarily only be exercised after locating those individuals".<sup>117</sup> Thus, Labour Department officials began to count many people who were voluntarily unemployed. They interpreted the statistics to imply that these people could be compulsorily directed into work. The numerical observations measured a potential reserve

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116 Ibid., p.5.

117 AJHR, 1945, H-11A, p.31. For the regulations on compulsory registration of persons for employment and the powers conferred on District Manpower Officers to direct registered persons into compulsory employment, see Statutory Regulations 1944, Wellington, Govt. Printer, 1945, No.8, pp.32-3.

of labour. They were designed as a direct response to policy and more important, as a response to events of the day. A social consensus and political commitment existed; the measure was designed and numbers used and analysed in connection with wartime work policies including expansion of civilian production industries which were necessary for support of the armed forces. Unemployment was rarely mentioned in connection with the registration statistics in official statistical publications or in available archives. The key file for applicants at employment exchanges and other potential workers was termed the N.S.134 "manpower control register".<sup>118</sup> The wartime observations are a clear example of how the meaning of statistics as well as their form of compilation depends on the context - time and place - of their interpretation. Indeed, the measure was discontinued and N.S.134 dispensed with in late 1945. In this case, the War pointed out the relative nature of interpretations of labour supply, unemployment and their numerical counterparts.

The end of the War and the Employment Act (1945) heralded new, fully-fledged official counting rules for unemployment that remained largely intact, understood and credible in the eyes of policymakers and the public over the rest of the period under review. The Employment Act spoke of people in need of "occupational readjustment" instead of people unemployed.<sup>119</sup> The words unemployed and unemployment notably returned to the

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118 See NZA, L 27/1/45, 24/9/1945, para. ixff.

119 NZS, 1945, No.9, p.50.



Labour Department Report for 1945 where it was claimed that in the Department's approximation, about 8000 workers on special government schemes previously of national importance, now represented "the irreducible number of unemployed marginal workers". These workers constituted the "core of unemployment" at the time because they could "not now find employment in industry".<sup>120</sup> A complete alteration of official definitions took place in early 1946 with respect to many thousands of these 'unemployed marginal workers', referred to in the Departmental Report for the year ended March 1945. These special, subsidised, relief workers on "Scheme No.13" were not to be included in the return of disengaged persons - according to a circular sent out from Head Office on 6/2/1945 (even though the AJHR Report written just a few months later referred to these workers as unemployed). Head Office also saw fit to send out another circular to the effect that "such workers should not appear in the Return of Disengaged Persons" on 8/7/1947. These considerations must be set against the way in which these workers were to be classified and counted, together with accompanying official interpretation:

CLASS

1. Fit for heavy local or district work.
2. Fit for heavy work but exempt from distant work.
3. Fit for manual work but not of a heavy nature.
4. Unfit or fit for the very lightest of work only. All those regarded as "unemployable" should be included under Class 4.

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120 AJHR, op.cit., 1945, pgs. 58, 59.

Granting some ambiguity in the term 'fitness' it appeared that these special nuances of classification were designed in order to deal with persons, possibly ex-returned service-people, once directly involved in the overseas war effort. There was certainly an official desire not to count or class these persons as ever unemployed or disengaged, even whilst, "under normal circumstances [there were] no men of Class 4 [actually] ... employed".<sup>121</sup>

According to a circular to all national Employment Service Officers dated 24/9/1945, the "E.S.5 register [of] all persons disengaged and seeking employment" had to be revised in order to cope with changes in the official definition of unemployment brought about by circumstances relating to the War. Separate unemployed sections of the register were to be kept for males and females and each of these were to be subdivided into two main subsections which made up "current" unemployment:

(a) The Fit Sub-Section: i.e., including the cards of persons who are fully fit for the work in respect of which they are enrolled with the Employment Service;

(b) The Semi-fit Sub-Section: i.e., including the cards of persons who are not fully fit for the work in respect of which they are enrolled, and of those who are virtually unemployable.

Each of the two main Sub-Sections of the Unemployed Section of the Register is to be further sub-divided thus:-

(a) Returned servicemen from the present war;

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121 NZA, L 1/1/12, Memoranda to all District Manpower Officers, 6/2/1946 and 8/6/1947.

- (b) Home servicemen from the present war;
- (c) Other adults, i.e., persons who have not seen service in the present war;
- (d) Juveniles.<sup>122</sup>

These coding changes were evidently part of a drive toward a supposed "scientific appraisal of all workers seeking engagement".<sup>123</sup> The National Employment Service in the Labour Department set out to help, in particular, returned service-people in their search for new occupations. Employment opportunities which the War provided disappeared and those people disengaged had to search for other work. It was at this point that the 'seeking work' concept became part of the vocabulary of labour statistics in New Zealand. The concept implied involuntary unemployment (although no reference was made to wage rates sought). Males and females fifteen years or older were counted as 'disengaged' if they sought work for thirty hours or more per week - and, of course, if they were without work when they enrolled with the Employment Service. In the published statistics people unplaced were divided into semi-employable (20-59 years) and full-employable (20-59 years), juveniles (under 20) and workers aged 60 or over. The terms 'semi' and 'fully' employable possibly suited the physical characteristics of the immediate post-War labour force.<sup>124</sup>

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122 NZA, L 27/1/45, Circular No.I.M.195, 24/9/1945, para.(iii) and (iv).

123 AJHR, op.cit., 1945, p.16.

124 Ibid., 1946, H-11A, p.105.

Clearer administrative procedures for lapsing disengaged, unplaced applicants were also spelt out:

As enrolees are placed, self-placed, leave the district, die or disappear, their card should be removed from the current index and filed alphabetically in a further section to become known as the Lapsed Section of the register. The Lapsed Section requires no sub-divisions such as are provided for in the case of the Current Section.<sup>125</sup>

The fit and semi-fit subsections of the 'current' section of the register was purged at least once monthly to ensure that no applicants were counted when they should have been lapsed, or that no applicants were classified as semi-fit when they were fully-fit and so on. Enrolees were lapsed if they had not made contact with the Department for more than fourteen days. (Reporting dates were recorded on each registrant's enrolment card). Since purging and updating of the register only had to occur monthly, the statistical returns of disengaged persons (also termed 'the unemployed' in official reports and memoranda at this time) which were sent to Head Office fortnightly, may have been compiled more accurately at some district offices than others. Frequency of purging exercises varied between branches and this would have influenced the returns.<sup>126</sup>

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125 NZA, L 27/1/45, op.cit., para.(vii).

126 Occasional reference in NZA is made to lax departmental office procedures for lapsing enrolees and compiling statistical returns. E.g., a year after the new procedures were instigated, District Employment Officers were being "requested [by Head Office] to make a personal check on staff responsible for the returns and satisfying themselves that such officers know what is required and that the office organisation is providing the necessary data accurately", NZA, L 1/1/12, Circular No.38, 30/10/1946.

The form of bi-monthly returns was altered on 20/11/1945. Counting rules were clearly spelt out and carried with them a clear interpretation and definition of real unemployment:

This return should include all disengaged persons currently enrolled for employment, whether willing to accept employment offering or not, and irrespective of age or eligibility for unemployment or other benefit. It should not, however, include persons still in employment but who have registered for work with a view to changing their employment ... As a measure of "Real" unemployment the new return will also show the number of fully employable workers ... who have been disengaged for two weeks or more immediately prior to the furnishing of the return.<sup>127</sup>

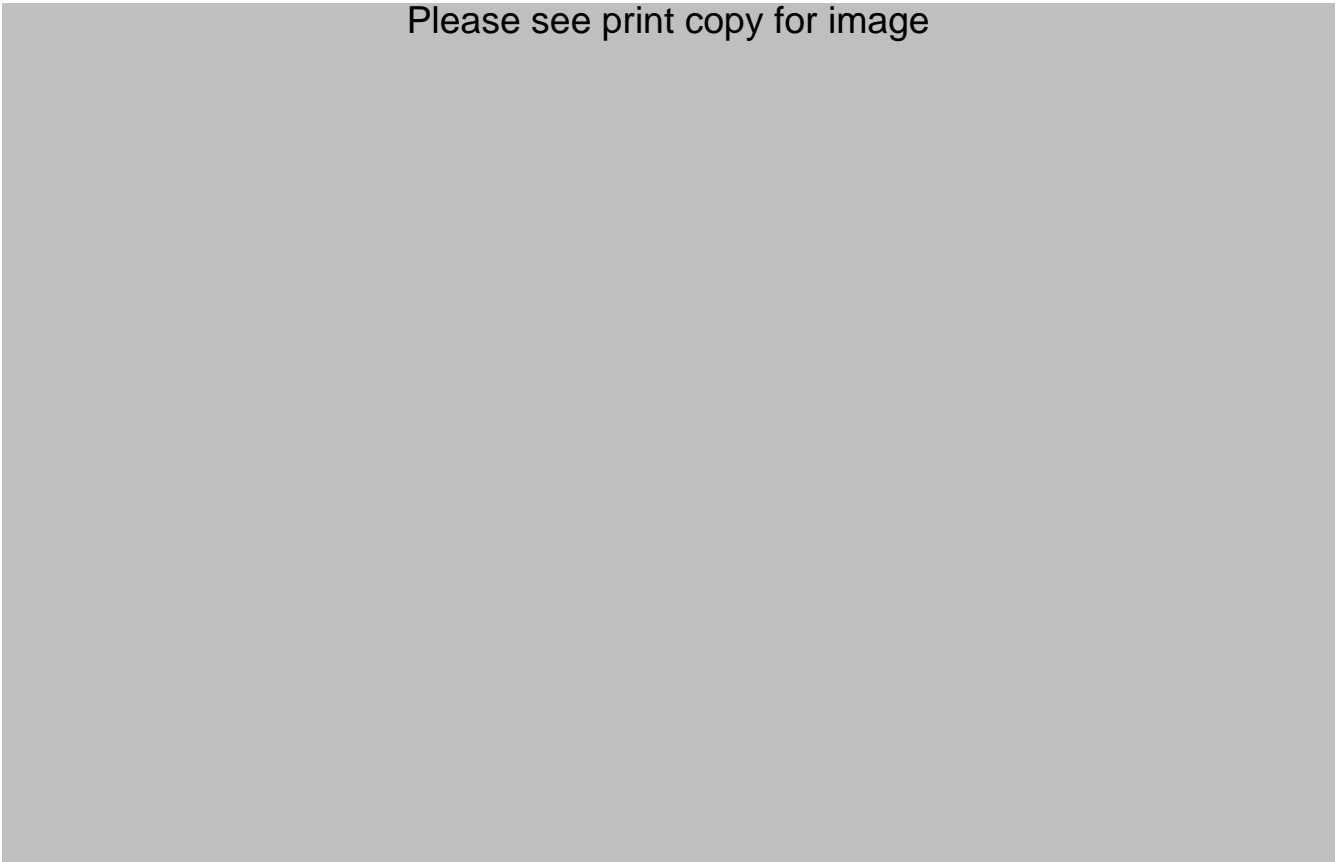
The statistics were published in the following form for 1945-46 (see Table 5.18).

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127 NZA, 27/1/45, Circular I.M.203, 20/11/1945, my emphasis. The Circular noted that a subsection containing cards of enrolees employed but in search of other work may be kept separate from other enrolees. However, at this time, it was stated that a need for such a subsection of this kind did not arise (probably because there were no enrolees in this category).

TABLE 5.18"DISENGAGED PERSONS ENROLLED FOR EMPLOYMENT" 30/9/45-31/3/46

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NOTES. (i) The above figures refer to all persons actually enrolled at each time point and therefore do not signify the numbers who have been disengaged for any length of time.  
(ii) \*Classification of enrolled persons into fully and semi-employable, &c., is not available prior to November, 1945.

SOURCE: AJHR, 1946 Vol.III, H-11A, p.150.

The appended 'note (i)' to Table 5.18 on the method of compilation, is misleading for users of these statistics to the extent that the actual 'length of time' required before enrollees were counted as disengaged was a minimum of two weeks on the register. From an administrative standpoint the two week time

reference was important because at any point in time a "suspense" section of E.S.5 recorded persons who had been referred to an employer for possible placement but who had not had their employment status (or otherwise) confirmed on form E.S.15 which was used for this purpose. Thus, those on the "suspense" register would not have been included in Table 5.18. As a consequence, more official concern about the possibility of leaving some genuinely unemployed persons out of the statistical returns led to awkward bureaucratic procedures for cross-checking the already laborious operation of compiling unemployment statistics at district offices:

On confirmation of engagement or otherwise the [suspended] cards are transferred to the "lapsed" or "live" sections respectively. With this [procedure] there is a danger of a card being taken out and omitted from the returns. As a safeguard ... a card to control the taking of cards from the "live" and "suspense" sections should be introduced.<sup>128</sup>

Early in 1945, the International Labour Office, anticipating the need for a new formulation of standards on unemployment statistics, sent out questionnaires and requests for detailed monographs on methods of compiling statistics in this field to certain countries, including New Zealand, in which new developments had occurred or in which one or more type of statistics of unemployment were especially well-developed. A report for the Sixth International Conference of Labour Statisticians was compiled from all these monographs.<sup>129</sup> (The

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128 NZA, L 27/1/45, Circular dated 22/2/1946.

129 ILO, Employment, Unemployment, and Labour Force Statistics, Report Prepared for the Sixth International Conference of Labour Statisticians, ILO, Geneva, 1948.

Conference was held in Montreal from the 4th to the 12th August 1947). The Conference Committee on Employment and Unemployment Statistics recommended the following changes in definitions and methods for designing unemployment statistics from those recommended at the 1925 ILO Conference. These may be compared with the form of New Zealand statistics at this time. First, on the question of duration of unemployment, the two week reference period used by the N.Z. Department of Labour appeared out of line in the light of resolution No.12:

Statistics of the "total number of unemployed persons" should include all persons, able to take a job if offered one, who are out of a job on a given day and have remained out of a job and seeking work for a specified minimum period not exceeding one week.<sup>130</sup>

This resolution defined unemployment mainly on the basis of the activity of each individual during a specified period. This was a departure from the 'gainful worker' concept commonly recommended by the ILO and used by most countries previously, according to which the classification of a person as unemployed was not related as strictly to activity during any specified time period. Debate around resolution No.12 was concerned with the impact this definition would have on the total number returned as unemployed. If, for example, the specified period taken was one week, it was alleged that "a considerable proportion of the unemployed would drop out of sight or, to be

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130 ILO, The Sixth International Conference of Labour Statisticians, Montreal, 1947, ILO, Geneva, 1948, p.54, my emphasis.



more exact, would be counted as employed".<sup>131</sup> Moreover this counting rule left a way open for the adoption of different specified periods within one week with consequent difficulties in making valid international comparisons. In the New Zealand case, at least as far as Labour Department Employment Service counting methods were concerned, the specified period (2 weeks) was well over the ILO minimum.

Counting periods used by the New Zealand Government Statistician, in contrast, anticipated the 1947 ILO recommendations in the 1945 Census. Emphasis in this first post-War Census was on an activity concept based on seeking work. The relevant question, appropriate to the times, covered all Pakehas "seeking work" or exservice-people" awaiting entry into work" on the Census date.<sup>132</sup> The 1951 Census retained this time reference but refined the counting definition somewhat by distinguishing on the questionnaire unemployment and (Keynesian, 'voluntary') frictional ("transitional, between-jobs") labour force behaviour (the latter was excluded from the Census count). Maoris were able to return themselves as unemployed for the first time in this Census. No separate Maori Census was taken. Waterfront strikers may have returned themselves as unemployed in 1951 since no strike code was provided (c.f., the 1926 and 1936 Censuses where the Statistician excluded those on strike from his enumeration of those legitimately unemployed according

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131 Ibid., p.17.

132 New Zealand Census of Population and Dwellings, 1945,  
Wellington, Govt. Printer.

to the operational definition of unemployment then in vogue). The seeking work proviso remained on the 1951 questionnaire.<sup>133</sup>

The four quinquennial Censuses from 1945 through 1961 were in fact the most comprehensive counts of the unemployed according to ILO concepts handed down in 1947. First, persons who were ill and could not accept paid work under any conditions if offered were excluded from Census unemployment counts. Second, persons merely subject to handicaps - such as being too old, too young or too inexperienced, lacking specific training needed for a given job, women who had family commitments to fulfil in addition to their work, persons partially crippled or suffering from similar difficulties - were not excluded. The Statistician did not exclude from the 'unemployed' group any who were seeking work and were able to take a job if offered one. Per contra, the Labour Department's measure of disengaged persons, especially during the middle and late 1940s excluded, among many others, those who were offered jobs but refused them on what was officially deemed unreasonable grounds (e.g., demanding a rate of pay higher than award rates). Furthermore, for the sake of comparison with the Censuses, the count of those on unemployment benefit with the Social Security Department excluded many persons who were ineligible for a benefit because, say, there was little prospect of their being re-employed or because employment officers considered them unsuited to ordinary

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133 New Zealand Census of Population and Dwellings, 1951,  
Wellington, Govt. Printer.

employment. However, following Census definitions from 1945-1961 if persons were seeking work and were able to take work if offered, they belonged just as properly in the total of unemployed. Wartime experience seemed to demonstrate that, for instance, employment of older and handicapped workers tended to be a function of the demand for labour. Many workers previously regarded as 'unemployable' in periods of heavy unemployment actually obtained employment during the Wartime labour shortage. So the Census broke new ground from 1945 by considering that a person was unemployed when actually seeking work, even though he may have demanded a rate of pay higher than he was likely to obtain, or was looking for a job for which he held inappropriate qualifications. Decisions as to 'reasonable' rate of pay and 'acceptable' job were highly subjective and partly moulded by social attitudes. Such decisions would have had some bearing on responses to Census questions. The New Zealand Census questions on unemployment from 1945 were completely consistent with international standards. The new Census counting definition of unemployment was

linked up with the right of the worker's free choice in a free economy. These decisions are parallel to the decisions of the employer in hiring. As the employer need not take any given worker - regardless of qualifications and regardless of the wages the worker is willing to accept - so the worker need not take any given job. Both can continue to seek what they require regardless of how unrealistic their aims may seem to others. Neither is held to any given standard of what is reasonable or "really deserved", or of the speed with which a job should be filled or accepted[;] ... workers must be included in the unemployed group no matter how low their productivity may prove to be or how unreasonable their economic aims may appear,

provided only that they are seeking work and able to take a job - but not necessarily any given job - if offered one.<sup>134</sup>

Lastly, the post-War New Zealand Censuses can be said to have fallen short of a more complete assessment of unemployment in that they did not distinguish those seeking full-time work from part-time job seekers (and/or under-employed workers). No hours were specified in relevant Census questions. By way of comparison, a clear and explicit interpretation appeared in Labour Department records during 1953 that persons were only unemployed (disengaged) if seeking work and enrolled for work for at least thirty hours per week. Thus, according to the Department of Labour and Employment Office Manual circulated on 1st November 1953 (with amendments to the end of the period under review), the term unemployment was applied "to all persons at any given time" who were (a) not in employment; (b) enrolled with the Department for employment and (c) available for and willing to accept a full-time job if offered one. In a covering note, full-time meant "any job requiring the attendance of the worker for 30 hours or more in each week".<sup>135</sup> As distinct from this alteration in the 'proper' official use of the term unemployed, no formal instructions were given in 1953 for any alterations in the method used to compile statistical returns of those unemployed. Conceivably, in contradistinction to ILO recommendations, stipulation (b), above, was still only

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134 ILO, Employment, Unemployment, op.cit., 1948, pp.13-14.

135 Office Manual, op.cit., Vol.III, Section J, Appendix I, paragraph 47, my emphasis.

operationalised in practice if the enrolee had been on the register for at least fourteen days as before. Time references differed between the interpretation of the term 'unemployed' ("at any given time") and the method of counting them (14 days minimum enrolment). Moreover, standing orders for preparation of statistical returns did not change over the period 1946-1960; at least no amendments appear in the Manual in this respect.<sup>136</sup>

One new influence on the level of unemployment as reflected by the statistical returns may be noted. According to the 1953 Manual, enrolees had to accept placement in "suitable" employment. Standing orders in operation during the 1950s were highly accommodating to employees' interests since it was stated that any job position must "have status and remuneration at least equal to that of [the enrolees'] previous normal employment" and that it "should be permanent with good conditions of employment". Apparently, anything less could have been rejected by a disengaged person who would then have remained in the total of involuntary unemployed on the register. As always, the variable interpretations of District Employment Officers would have come into play with regard to the issue of job suitability.<sup>137</sup>

Resolutions on the question of statistics of unemployment adopted by the 8th International Conference of Labour Statisticians (Geneva, November-December 1954) outlined a counting rule slightly different from the 1947 Conference. The

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136 Ibid., Section C, Appendix I, para.7-29.

137 See Ibid., Section E.

rule, duly summarised, was:

Persons in unemployment [:] ... all persons above a specified age who, on the specified day or for a specified week, were in the following categories[:]

- (a) workers available for employment whose contract of employment had been terminated or temporarily suspended and who were without a job and seeking work for pay or profit;
- (b) persons who were available for work (except for minor illness) during the specified period and were seeking work for pay or profit, who were never previously employed or whose most recent status was other than that of employee (i.e. former employers, etc.), or who had been in retirement;
- (c) persons without a job and currently available for work who had made arrangements to start a new job at a date subsequent to the specified period;
- (d) persons on temporary or indefinite lay-off without pay.<sup>138</sup>

The Conference also recommended regular studies of unemployment by duration.<sup>139</sup> No official duration studies were carried out in New Zealand during the 1950s. Methods of counting registered disengaged persons used by the Labour Department maintained the two-week time reference for statistical returns. As well, component (c) of the ILO recommended counting rule was not part of the registration count or New Zealand Census operational concepts from 1951. These international considerations may be regarded as insignificant, but they were nonetheless deviations

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138 ILO, The International Standardisation of Labour Statistics, Studies and Reports, New Series, No.53, Geneva, 1959, p.45.

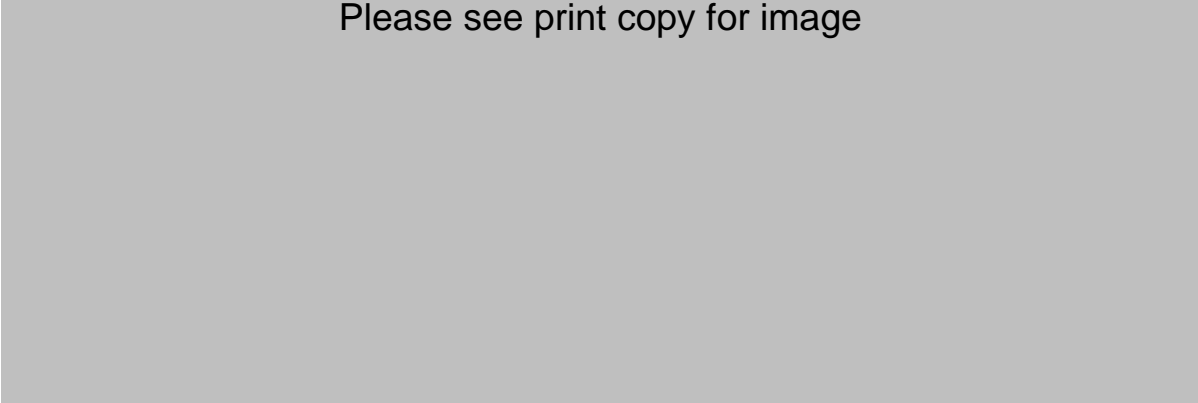
139 Ibid., p.46 No.11, and p.48 No.20.

from ILO resolutions aimed at standardising international counting rules. Their insignificance in the New Zealand case is arguably highlighted when numerical data showing the low level of unemployment during the 1950s is reviewed. For instance, the following return was typical of registration statistics during the 1950s:

TABLE 5.19

"DISENGAGED PERSONS REMAINING ENROLLED FOR  
EMPLOYMENT IN DISTRICTS, 1954-55"

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SOURCE: AJHR, 1955, Vol.II, H-11, p.39.

Also, unemployment recorded at relevant Census dates amounted to a small proportion of wage and salary earners and the labour force (also operationalised by successive Census counting definitions); (see Table 5.20).

TABLE 5.20

UNEMPLOYMENT AT CENSUS DATES 1945-61

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SOURCE: N.Z. Census of Population and Dwellings, 1945, 1951, 1956, & 1961.

From the early 1950s institutionalisation of the Labour Department registration measure, in particular, marked the end of official experimentation with definitions of unemployment. Registration statistics were thoroughly legitimated in the policy process during the 1950s when they were used as one monitor of full employment policies. Political commitment was given to the supervision of various economic statistics necessary for proper Keynesian economic management. The Labour Department Act (1954) stated that one of the functions of the Department was "to collect and publish information relating to employment and unemployment". At the same time the Department had to aim to "promote and maintain full employment".<sup>140</sup> Unlike some other governments at this time, no target level of unemployment was set or proposed in the New Zealand case.<sup>141</sup> This was probably superfluous in the light of statistics in Tables 5.19 and 5.20. According to the Reserve Bank, at one point during 1954 there were less than 1000 people registered as disengaged, whereas approximately 12,000 job vacancies were reported to district offices of the Department of Labour.<sup>142</sup> The registration measure designed by the Department may have acted as one criterion for the success or failure of full employment policies but full employment was not operationally defined. Garraty's comment in a more general context

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140 NZS, 1954, No.71, p.768, 769.

141 In a reply to a question from the United Nations Economic and Social Council, the N.Z. Government position was noncommittal when it came to setting targets for reducing unemployment. See, Reserve Bank of New Zealand, Employment of Economic Resources, Wellington, June 1954, pp.10-11.

142 Ibid., p.12.



summarises the international mood at this time: "[i]n the 1950s no nation would reject its commitment to full employment in principle ... but it was often difficult to persuade governments to state precisely what full employment meant to them".<sup>143</sup> The confidence inspired by the relatively happy state of affairs depicted by Tables 5.19 and 5.20 did not, however, cause economists and public policymakers in New Zealand to forget about unemployment, for careful Keynesian economic management required vigilant supervision of what during this period became known as key 'economic indicators' including leading unemployment series. The Reserve Bank used registration statistics in conjunction with vacancy statistics to build up the concept of "over-full employment". It warned that full employment must mean people gainfully occupied otherwise price stability may be threatened. It implied, as well, that the Department of Labour's disengagement statistics were misleading because they did not measure the amount of part-time unemployment (or underemployment).<sup>144</sup> The Reserve Bank report also suggested that unemployment statistics needed reinterpreting in response to potential inflationary effects and

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143 Garraty, Unemployment in History, op.cit., 1978, p.238.

144 RBNZ, op.cit., 1954. C.f., Rosenberg, W., who later maintained that the term 'over full employment' "was a semantic trick intended to make the employment situation the scapegoat for inflation". See 'The Functions of Full Employment', in Thompson, K.W. and Trlin, A.D., eds., Contemporary New Zealand, Wellington, Hicks Smith, 1973, p.24.

that it was perhaps better to have people 'temporarily disengaged' in the short-term so as to avoid inflation and help balance aggregate supply and demand. In this case the registration measure and its alter ego - vacancy statistics - was used and interpreted in a way which pointed out the need for action to keep the statistics at levels that were conducive to meeting other policy objectives. Just what the appropriate or 'right' levels were in the light of a need to compromise with other policy goals, was left unstated.

Near the end of the period under review registration of disengaged persons at the Department of Labour began to show marked increases (see Table 5.21). Parliamentary debate revealed that opposition members did not consider the level of unemployment measured by disengagements as appropriate. The government was accused of "building up the number on social security benefits to keep down the number of unemployed". It was alleged that older workers were encouraged to accept old-age benefit rather than register as disengaged and that many persons did not register even though they were unemployed.<sup>145</sup> Parliamentary criticism of the Department of Labour's statistics, though reasonable, appeared half-hearted during the 1950s.

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145 NZPD, 1959, Vol.320, p.1720. Some earlier misgivings by parliamentarians about the registration statistics may be found in ibid., 1958, Vol.316, p.231.

TABLE 5.21

DEPARTMENT OF LABOUR STATISTICS: "DISENGAGED PERSONS" 1949-59

	Disengaged Persons at End of Month		
	Males	Females	Total
Average for calendar year -			
1949 .. .. .	88	4	92
1950 .. .. .	34	4	38
1951 .. .. .	29	9	38
1952 .. .. .	33	14	47
1953 .. .. .	67	18	85
1954 .. .. .	48	10	58
1955 .. .. .	44	12	56
1956 .. .. .	240	19	259
1957 .. .. .	368	26	394
1958 .. .. .	733	52	785
1959 .. .. .	1,096	92	1,188

SOURCE: MAS, January 31st 1961, Table 14, p.16.

Apart from occasional parliamentary challenges to the meaning of registration statistics especially in relation to adequacy of coverage during the 1950s, the Department of Labour's method of compilation and publication remained unchanged throughout that decade. The consensus during this decade in public administration was that the "prospect of recessions seem remote" so "social engineering" programmes could be pursued with vigour.<sup>146</sup> The absence of recession ensured a decade of inertia and unquestioning acceptance of existing methods used to design unemployment statistics. By 1961, published Census information on occupations did not include the

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146 Green, W.A., Social Services in New Zealand, Study Group Report of the N.Z. Institute of Public Administration, Wellington Branch, Revised edition, 1955, p.35, and p.56.

term unemployment in any of the table headings.<sup>147</sup>

### 5.9 Epilogue (2)

In summary, the early 1960s marked a low point (until more recently) for official experimentation with, and general public debate on, the design and interpretation of unemployment statistics in New Zealand. State labour exchange methods, in particular, were legitimated in the policy process during the 1950s. Broadly speaking, they were actively used and interpreted as one monitor of Keynesian economic management. Before 1926 there is no explicit thought in official New Zealand reports as to the purpose for totalling registered unemployed in terms of numbers of unplaced workers at labour bureaux. Apparently no count was taken because it had no specific public policy or planning function in connection with unemployment relief; no perceived social information value and no role in relation to international statistical obligation and international comparison. From the mid-1920s to 1945 the labour bureaux registration count was redesigned on ad hoc bases and interpreted in connection with variable purposes and coding schemes. The connection of these bases and interpretations to pressing policy problems was sometimes identifiable, though hazy. After 1945, comparative stability in design and interpretation paralleled sustained social commitment to full employment policies. Standardised procedures for counting and classifying registered unemployed were often incorporated as a

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147 New Zealand Census of Population and Dwellings, 1961,  
Wellington, Govt. Printer.

check on those policies. Operational concepts in the New Zealand Censuses altered more gradually. While no quantitative estimate of the significance of these alterations is made here, it seems that conceptual changes limit the comparability of these Census statistics. Generally, New Zealand Census counting rules, as with their Australian counterparts, remained open enough to allow scope for wide interpretation on the part of respondents. The seeking work proviso appeared to be a significant step toward tightening the definition in Censuses from 1945 (see Appendix V(B).2).

Overall, concepts of unemployment in the minds of official statisticians did not remain static for very long periods. Nor, for that matter, did corresponding operational definitions remain static for very long periods. New operational definitions and associated layers of interpretation emerge in official statistical reports and archives almost continuously over the period reviewed. No single operational counting rule exhausted the whole meaning of the idea of unemployment. In Machlup's terminology, 'pure' concepts of unemployment have residual meaning and could not be fully exhausted by operational concepts. This is evident in the continuous attempts to operationalise unemployment in New Zealand which, in retrospect, have always left a non-operationalised residual meaning. Some aspects of this residual may have been operationalised at other times by other counting rules but these counting rules, in turn, left other residuals. The official process of designing and interpreting numerical data - at least with respect to unemployment in New Zealand - was open-ended and iterative and

responded to many and varied influences. This New Zealand study is perhaps one illustration of how the process of design and interpretation (in this case mostly by officials) of unemployment statistics is influenced, at a general level, by the time-place context. More specifically, each operational definition which was used to organise a set of numerical observations of unemployment, together with associated interpretation, marked-out the boundaries within which official thinking about the phenomenon functioned. Limits on official 'knowledge' and perception of unemployment (as a 'problem' or 'non-problem' and as to 'solutions' or appropriate policy responses) were dependent on historical factors (social context and key events) and inherited intellectual factors. Intellectual factors were at work in conceptualising and operationalising unemployment and in perceiving inadequacies of operational definitions. Some intellectual influences were evidently transmitted internationally (e.g., through the ILO). Most, if not all, intellectual developments were interrelated with changes in historical factors in New Zealand. These intellectual factors, or theories, were hardly recognisable as the formal economic theories of unemployment prevailing from time to time. In particular, in the case of labour exchange statistics which had direct public policy implications, these theories at work behind both operational counting rules and data interpretation more nearly approximated Schumpeter's everyday, commonsense aspects of economic thought. They appeared as a set of opinions or accepted generalisations on unemployment that came to prevail in a specific time-place context. These

opinions were formally expressed through legislative and official administrative strictures.

Once the process of data design and interpretation is analysed in detail, as in this case history, it becomes apparent that 'operationalising' unemployment involves far more than simple gathering of given numerical 'facts'. Official operational concepts of unemployment in New Zealand have been moulded by many influences through time; they have not been fixed. Instead, they have been continuously altered in an iterative and interactive design and interpretation process. New Zealand unemployment has assumed sometimes strikingly different operational forms through history. Forms of statistical design surveyed here show the continual readaptation of everyday aspects of economic thought on unemployment to both concrete circumstances and the purposes of officials and public policymakers.

APPENDIX V(B).1Review of Unemployment -Degree of Employability: NZ Dept. of Labour 1937

Bureau \_\_\_\_\_ Date \_\_\_\_\_ 193 \_\_\_\_

.....  
(Surname) (Christian Names)

Bureau No. .... Age (in years) .....

State whether single or married, (married to include divorced, separated or widower with dependants) .....

Usual Occupation .....

Alternative Occupation .....

Classification Required Under One of the Following:

Tradesmen

- A. Fit for normal sustained work in usual trade.
- B. Fit for normal sustained work in alternative occupation not requiring use of skill originally acquired.
- C. Fit only for normal sustained work in sedentary (mainly clerical) or routine light work (e.g., caretaker, lift attendant, unskilled light manual labour, etc.).
- D. Fit for light work such as "C" only occasionally or intermittently (regularity impracticable owing to physical or mental weaknesses).

Sedentary Workers

- E. Fit for normal sustained work in usual occupation.
- F. Those formerly fit for work of a sedentary nature involving fair degree of responsibility but now fit only for normal sustained sedentary or routine light work involving little responsibility or effort; (change due to advanced years, inferior health or mental powers).
- G. Fit for sedentary or routine light work only occasionally or intermittently because of condition in "F" above.

Manual (unskilled workers)

- H. Fit for normal sustained unskilled manual work involving average strength.
- I. Fit only for normal sustained light work such as "F" above and for same reasons.
- J. Fit for light work such as "F" above but only occasionally or intermittently (due to same reasons as in "F").

Unemployable

- K. Because of advanced years, reduced physical or mental powers through causes not within own control.
  - L. Because of result of anti-social habits (liquor, drugs, methylated spirits, disease, lack of desire to work, etc.).
-



I have examined the above worker, and for the purposes of the return required by Circular Memorandum E.P. 1937/13 and in accordance with the above classification, am of the opinion that the classification below correctly represents the workers degree of employability.

Classification ..... (State whether, A, B, C, etc.)

.....  
Signature of Examining Officer

SOURCE:    NZA, L 1/1/12 (1937).

APPENDIX V(B).2NZ Census Rules For Counting The Unemployed To 1961

<u>Date</u>	<u>Counting Rule</u> (as stated on questionnaire)
1896 (12th April)	Wage and salary earners out of employment on Census date <sup>+</sup>
1901 (31st March)	If not at work for more than a week prior to Census - (except in cases of leave of absence)
1906 (12th April)	Same.
1911 (2nd April)	Same.
1916 (15th October)	Same except unemployed asked to state number of weeks unemployed
1921 (17th April)	Same except duration deleted.
1926 (20th April)	Same except (1) not applicable to retired persons and (2) by cause; sickness, accident or injury and lack of employment <u>not</u> due to strikes.
1931	NO CENSUS TAKEN.
1936 (24th March)	Unemployed on Census date: (1) wholly unemployed or on sustenance (2) partially unemployed but not on relief <sup>#</sup> (excluding workers on strike or locked-out).
1941	NO CENSUS TAKEN
1945 (25th September)	If seeking work or awaiting entry to work on Census date.
1951 (17th April)	If seeking work on Census date excluding transitional workers between-jobs.*
1956 (17th April)	Same
1961 (18th April)	Same

Notes

- + Published statistics include males and females < 15 yrs and < 90 yrs old. Maoris are not included.
- # An occupational code was included for those on part-time relief.
- \* Maoris are included for the first time in the unemployment returns of this Census.

SOURCES: Census questionnaires produced by the N.Z. Census and Statistics Office (to 1936); the N.Z. Census and Statistics Department (to 1954); and the N.Z. Department of Statistics (from 1955) .

VI. AFTERWORD

This chapter completes these literary studies in 'political arithmetic'. These studies were by no means exhaustive. Insofar as any viewpoint was propounded here, these studies were carried out from what might be described as the outlook of a reluctant and sceptical a priorist. In one sense the studies that make up this thesis stand on their own as separate inquiries. They were followed through on three different levels and in three distinct areas of academic research. Apart from the brief overview of the role of numerical observations in the history of political economy, the first level of discussion was concerned with economic methodology and the knowledge-status of numerical observations in economic analysis. A second level of discussion was concerned with the role of numerical observations in the history of thought. Specifically, this level considered the design and interpretation of numerical observations by three seventeenth century political arithmeticians in a theory of knowledge context. The third level of discussion was broadly concerned with an aspect of economic history. It moved away from the role of numerical observations in a 'scientific' or formal economic analysis context. This third level was an exercise in intellectual history and it reflected on the official design and interpretation of numerical observations of unemployment over specific time periods in Australasia.

In another sense these three studies derive their common theme from my concern with the problem of justifying numerical observations, (including formal statistical constructs derived from 'raw' numerical observations), and the interpretations generated from them, as valid knowledge. Part I provided a sketch of the role and evidential status of numerical

observations in the history of political economy up to the period of more thoroughgoing statistical business cycle research and the dawn of Keynesian macroeconomics. It also provided a lead into economic literature on the epistemological status of numerical observations. The main body of literature on this question is usually identifiable as a subordinate theme in the scribblings of old and new generation Austrian economists. The theme arose originally from some Austrian economists' (1) mistrust, on a philosophical level, of aggregation in economic analysis; (2) their emphasis on the subjective appreciation of unique, micro-level particulars and (3) their elaboration of the problem of mistaking thought objects for objects of perception.

In the wake of popularity surrounding Keynesian macroeconomics Schumpeter remarked:

Perhaps the most important progress that has occurred in scientific economics is the vast increases ..., beyond the boldest dreams of past generations, ... of statistical information which was so great as to open up quite new possibilities for scientific research.<sup>1</sup>

The writings of a small number of Austrian economists - especially Hayek, Morgenstern and Machlup - gave direction to my studies concerned with some of the consequences for knowledge and limitations of knowledge, generated from vast increases in the availability of statistical information. Morgenstern raised questions about the notion of accuracy of economic observations as early as 1950, and a portion of Machlup's research over the last thirty years has been concerned with the methodological problems of processing 'empirical data' (numerical and non-

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1 Schumpeter, History, op.cit., p.1141.

numerical) derived from the operational domain to correspond with the concepts of formal economic theory. Hayek's critique of quantification and scientism in economic analysis dated from the mid-1930s but it was overshadowed by enthusiasm for aggregation following Keynes. More recently, Hayek's expression of concern for the limits of our numerical knowledge has proven more apposite in, roughly speaking, a period of relative disenchantment with Keynesian macroeconomics.

The variety of research suggestions arising from my reading of Austrian perspectives on the knowledge-status of numerical observations in the interconnected realms of (Schumpeterian) economic thought and economic analysis is illustrated by the foregoing studies. The suggestion which I chose to follow-up was the question of the character and meaning of 'empirical data' and 'empirical evidence' expressed in numerical form or, in short, the grounds of knowledge generated from numerical data. Theories of knowledge in general have been neglected by economists - especially by those economists trained in one or more of the 'positivist' traditions. Theories concerning the nature and limits of our numerical knowledge, in particular, have received hardly any attention. Austrian and subjectivist perspectives surveyed with reference to numerical observations in Part III(A) all represent challenges, in one way or another, to the 'positivist' traditions in economics which: (i) take numerical observations as 'given facts' or 'given data' and (ii) pass over the epistemological issues associated with the design and interpretation of numerical observations. A central issue foreshadowed in Part III(A) and investigated in III(B) was the problem of the uncertain correspondence between numerical

observations (or at another level: statistical constructs) and higher level abstract concepts that the observations are interpreted to stand-in for. Vast increases in the availability of numerical observations for use by economists in economic analysis - to design statistical constructs and to 'test' economic theories - has not removed the uncertainty involved in interpreting or conferring meaning on the data. This uncertainty emanates from the historical nature of all numerical observations and the heterogeneity of the phenomena which are operationalised, i.e., that which is observed and quantified. Expressed in other words, this is to say that all numerical indicators designed by fiat measurement contain inescapable, subjective, a priori elements. A concept of a numerical indicator was contributed in Part IV and was used to analyse the way early political arithmeticians designed and interpreted numerical observations (and formal statistical constructs) for special purposes to make reference to unobservable senses of reality and to legitimise these senses as 'real'. This indicator concept suggested a new way of investigating the contributions of seventeenth century political arithmeticians and, in particular, a new way of assessing the role of numerical 'facts' in their work.

Part V took one of the many other possible suggestions arising from the survey of Austrian perspectives in Part III. It attempted to shed light on some of the many factors involved in the historical process of operationalising unemployment in numerical form. The genesis of methods of operationalising unemployment studied here, when analysed from the standpoint of

intellectual history, was perhaps a tentative elaboration of what was meant at the close of Part III by: 'the continuous interplay on an operational continuum between data recording and its interpretation in conjunction with (i) different layers of theory and (ii) specific times and places'. These studies, among other things, also provided examples of the gulf between formal economic theories of unemployment and official practices of operationalising unemployment. Concentrating on the lower, 'raw' data end of the operational continuum, these studies illustrated the idea that all numerical observations of unemployment are theory or value-loaded and that this load (including informal economic thought patterns underlying statistical compilation, presentation and interpretation) could give clues as to the limits or boundaries surrounding official knowledge of unemployment at specific times in specific places. Theory used to design and interpret numerical observations of unemployment in Australasia contained complex mixtures of more formal economic-theoretic conclusions about what was to count as unemployment and (as highlighted in the New Zealand study), more informal, everyday economic thought.

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\*b Abbreviations for the main New Zealand sources can be found on page 306.

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