

## BOOK REVIEW FEATURE: TWO REVIEWS OF *IDENTIFICATION FOR PREDICTION AND DECISION*

By MANSKI (CHARLES. F.) (Cambridge, MA: Harvard University Press: 2007. Pp. 348. £55.00, cloth., ISBN: 978-0-674-02653-7)

### Review 1

‘Another brilliant Manski book.’ This will be a common, and reasonable, reaction to the latest output of his prolific research agenda to study the limits of identifiability. This book follows his earlier *Identification Problems in the Social Sciences* (1995) and two technical monographs<sup>1</sup> and, like the earlier book, it is ‘intended to be broadly accessible to students and researchers in the social sciences’. It is meant, in the author’s phrase, to describe ‘the next generation’ of work on the general problem of missing data leading to lack of point identification or to ‘partial identification’. It also represents his methodological views or ‘fresh worldview to guide empirical researchers in the social sciences’, as he puts it in the preface.

Manski deals with a very broad range of topics, not all which are ordinarily thought of as involving missing data, and he almost always has interesting and novel insights, making his book a rich and absorbing read. His topics include non-response in surveys; decomposition of mixtures; response-based sampling; selection problems; treatment response; decision making with unknown objective functions; measuring expectations; and even the simultaneous equations problem that started the identification hare running. And although the book is about identification he does not entirely omit discussion of inference. After all, if you have shown that something is (partially) identified, why not explain how to estimate it?

The basic idea is that the economist observes a frequency distribution that differs in some systematic way – the observed data are incomplete or some is missing – from the distribution that he would like to observe. The question is what, if anything, can be said about the latter distribution knowing the former, without making incredible assumptions.

Before offering some comments it is worth making a remark about the relation between the research agenda of this book and the traditional identification agenda. ‘In many fields the object of the investigator’s inquisitiveness is not just a “population” in the sense of a distribution of observable variables, but a physical structure projected behind this distribution, by which the latter is thought to be generated.’ (Koopmans and Reiersol, 1950, p. 165)

<sup>1</sup> *Partial Identification of Probability Distributions* (2003) and *Social Choice with Partial Knowledge of Treatment Response* (2005).

This is how Koopmans and Reiersol thought back in 1950; identification consisted of going behind the observed distribution of data to the physical structure that is assumed to have generated it. By physical structure it seems reasonable to suppose they had principally in mind an economic model. Although you can cast Koopmans and Riersol's perspective as one of coping with missing data – for example if only you could observe quantities demanded and supplied at alternative not necessarily equilibrium prices then you can work out demand and supply curves which provide the physical structure – it is not perhaps the natural way of looking at it. The present book places much less emphasis on the 'physical mechanism', and much more on missingness mechanisms. The typical problem is that of going from one frequency distribution not to a physical structure but to another frequency distribution. (I shall illustrate this with two examples shortly.) In this it looks more like the large and increasing number of recent books by statisticians and biostatisticians on the subject of missing data (Little and Rubin, 2002; Daniels and Hogan, 2008) than it does a traditional econometric identification text. Indeed, going from one frequency distribution, that of the data in your sample, to another, that in the 'population' from which your data is presumed to originate, is the essence of the problem of statistical inference. It has nothing particularly to do with econometrics.

Two examples, both mentioned in Manski's book, may make our discussion concrete. The first is the Piliavin and Sosin example discussed on p. 41. At a certain date there were 106 homeless men, of whom 64 could be located six months later; and of these 64, 21 were no longer homeless. The observed data are the 64 men out of 106 of whom 21 had found a home. The desired data are all 106 men and whether each had found a home. Interest is in the fraction of homeless men who are housed six months later – a measure of the rate of leaving homelessness. But we do not observe this number, all we can see is the fraction of the men who could be traced who found a home.

A second example is the recent study of wages by Blundell *et al.* (2007) The desired distribution is that of the wages that each of 187,467 individuals in the UK between 1978 and 2000 could have earned had they worked in the paid labour market; the observed distribution is that of the wages of those who did work. Interest is in changes in the distribution of wages, as measured by the interquartile range, that cannot be attributed to changes in the composition of the work force.

As is by now well known, Manski's point of view depends on what he formalises as The Law of Decreasing Credibility. This states that: 'The credibility of inference decreases with the strength of the assumptions maintained' (p. 3). And since he wants credible inference he wants weak assumptions, preferably using only the empirical evidence.<sup>2</sup>

How would Manski's agenda work out in the case of the two examples sketched above? Out of the 106 homeless men 42 could not be found after six months, so there are 43 possibilities. Either none of the missing men had found a home or one of them had, or two of them had, ...or all 42 had. So the fraction of all 106 men exiting homelessness was either 21/106 or 22/106 or ... or 63/106. One can bound the true

<sup>2</sup> 'Inference using the empirical evidence alone sacrifices strength of conclusions in order to maximise credibility.' (p. 62)

fraction to an interval. That is all one can say using the empirical evidence alone. But note that one can be more precise if one asserts that the fraction of the missing men who had found a home was the same as the fraction of the observed men who had found a home – a sort of missingness at random. In this case the fraction of all 106 men who found a home would be exactly 21/64. Inference becomes much more precise than without this assertion – indeed we have point identification – but at the expense of a possibly incredible (to whom?) assumption.

The wage example is much more interesting to an econometrician, because the very distribution of interest, that of the potentially counterfactual wages – what she would have earned, in equilibrium, had she worked – is defined by economic theory. Anyone who finds the theory, hallowed by time and tradition though it is, incredible would not leave the starting post with this paper and although praised by Manski it is a very long way from inference using only ‘the empirical evidence’. The basic idea here is that if you observe 100 people of whom 90 work and 10 do not, so you have 90 observed wages, you can bound some of the quantiles of all 100 wages. One extreme possibility is that all 10 unobserved wages lie below the 90 observed ones. This implies that you can never bound from below the 10th percentile of the full distribution – it could be anywhere below the smallest observed wage – but you can bound from below the 11th percentile – it cannot lie below the smallest observed wage, being equal to it if all unobserved wages are below the observed ones and greater than it otherwise. The authors exploit this statistical type of reasoning to bound interquartile ranges of the full distribution, and they then consider the additional precision gained by further more or less (in)credible assumptions, such as that the median wage of those who work is greater than that of those who do not. But, unlike missingness at random in the homelessness example, these further assumptions tend to have strong economic content and one can imagine much of our profession agreeing on their credibility or not though outsiders may be sceptical. Moreover they put some of these extra assumptions to the test, in so far as they are able. This paper is a far cry from letting the data speak for themselves.

Turning to the Law of Decreasing Credibility itself we might ask what the author means by credible. He supplies the answer, ‘An assumption is credible to the degree that someone thinks it so’ (p. 48). This raises the question of who are to be the judges of credibility? Who will reject an article because its assumptions are incredible? This criterion appears to be a recipe for methodological conservatism and seems to introduce an alarming degree of subjectivity into an econometric analysis. Reliance on the opinions of unspecified judges of credibility sits rather awkwardly with a philosophy which emphasises using only the, objective, empirical evidence.

This methodological point of view also, of course, clashes sharply with the recommendations of Friedman, as Manski is well aware. Friedman (1953) would say that the credibility of assumptions is of little relevance and that what matters is the quality of predictions from a model. ‘Truly important and significant hypotheses will be found to have “assumptions” that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions...’ Friedman argues in his famous essay on *The Methodology of Positive Economics*. But for Manski the credibility of assumptions is central, and predictions seem to play little role in judging a model, notwithstanding their appearance in his book’s title. To adapt an example of Friedman, Manski would apparently reject the formula  $s = \frac{1}{2}gt^2$  to

explain the speed at which apples fall from a tree, even though it predicts well, on the grounds that the assumption of a vacuum is clearly incredible.

Nowhere is this view clearer than in Manski's take on rational expectations, of which he disapproves heartily. 'It is not credible to suppose, without substantiation, that expectations are either literally or approximately rational.' (p. 277) One consequence of this is that the econometrics of dynamic stochastic general equilibrium (DSGE) models, currently an important and active area of applied econometrics, fails to get a mention in his book. More generally, what used to be known as structural modelling is not a part of the Manski agenda, which could be said to be reduced form with a vengeance. It seems strange that the one-time distinguished (and latterly Bayesian) statistician Milton Friedman advocated a methodology in which possibly unlikely economic assumptions play the central role; while economist Manski advocates a methodology in which the central role is played by a purely statistical device (bounding) and economic theoretical assumptions have, at best, a walk-on part. And to add to the paradox Friedman's greatest early piece of applied econometrics, *A Theory of the Consumption Function* (1957), dealt with a missing data problem. He wanted to observe permanent income and permanent consumption but could only observe measured income and measured consumption, permanent income was missing data. He dealt with this not by producing bounds on the consumption function but by adopting strong economic hypotheses, such as the proportionality of permanent income and consumption, and seeing whether the implications of these were consistent with a wide variety of economic data.

The reader will have noticed that I have misdescribed the two examples and written that, in the homelessness example for instance, the object of interest is the fraction of all 106 men who had found a home within six months. Actually, the object of interest is the fraction of homeless people in some presumably larger 'study population' that find a home within six months. This number is interpreted as the probability of finding a home within six months by those, including Manski, who believe in objective probabilities. And the end points of the identified set, namely  $\{21/106, 63/106\}$  are regarded as estimates of theoretical bounds on the probability of interest, estimates that will vary over repeated samples from the study population. My reason for ignoring this version of the problem is that the extension of the problem to fractions in imaginary repeated samples from ill-defined (how do you randomly sample the homeless?) study populations, though it is the traditional way of thinking of course, violates the law of decreasing credibility for this reader. You do not need to believe in objective probabilities to find the algebra of set identification useful, as both my examples demonstrate.

I note also that in the wage example of Blundell *et al.* the discussion of bounds and possible restrictions on the relation between observed and unobserved wages could be easily thought of as exactly analogous to my discussion above of the 106 homeless men, in which the objects of interest are actual frequency distributions in a given real population, not 106 men in Minneapolis but 187,467 adults in the UK over a specific interval of time. It does not really appear to add anything to their analysis to think of their wage distributions as probabilities. Even the section of the paper that deals with inference about bounds relies largely on the bootstrap in the context of a multinomial model, a procedure which has a well-known Bayesian interpretation, (Lancaster, 2004).

Indeed, so far as I can see the wage paper looks like a fine piece of subjective Bayesian econometrics in which the authors conduct a subtle analysis of the effect of alternative prior beliefs about the unobserved wages.

One of the most interesting parts of the book is the later chapters which deal with his newer work on the interpretation and use of reported expectations, intentions and beliefs. Manski rightly remarks that economists have traditionally been sceptical of using the statements of individual agents about their beliefs and behaviour, preferring instead to use only objective data about prices, incomes, purchases and the like. This scepticism goes back at least to the 1940s when Machlup (1946) and others criticised Hall and Hitch's (1939) work that asked businessmen how they set their prices and received some apparently disconcerting answers. Manski documents that this aversion has recently decreased and that there is a growing volume of work in which agents are interviewed and asked 'expectations' questions. He leaves the impression that work taking seriously the answers to such questions is a recent development, but he neglects at least one earlier paper. In this work, Lancaster and Chesher (1983), unemployed people gave answers to the questions 'what wage do you expect to make in a new full time job?' and 'what is the smallest wage you would accept in such a job?'. If you maintain the simplest version of the optimal job search model and interpret these questions as asking for the reservation wage,  $\xi$ , and the mean wage given that it exceeds  $\xi$ , then Chesher and I showed that one can deduce parameters of the underlying physical mechanism, in Koopmans and Riersol's phrase. This paper takes seriously the answers to expectational questions, as Manski urges us to do but also shows the power of theory, when combined with these subjective opinions, to inform us about structure. Manski argues that 'economists have hardly begun to use probabilistic expectations data in econometric analysis of decision making'. But, as this paper shows, that agenda began at least a quarter of a century ago.

Praiseworthy as Manski's plans for serious econometric analysis of expectations and intentions data are, economists continue to display a curious passivity about data collection (though some economists do get to add questions to existing surveys). I would have expected to find them applying for funding to ask the questions suggested by our theories; to ask the unemployed person what is the smallest wage he would accept in a full time job, as the sociologists and economists of Political and Economic Planning did in collecting the data used by Lancaster and Chesher in the 1970s; to ask the woman how many hours she would work at a given wage or if child care was available at a given price and so on. But it seems that much of the data analysed by Manski and his colleagues is gathered by non-economists and for a variety of different reasons. This may be cheap but it can lead to apparent absurdities like the question, discussed by Manski in chapter 14, asked by the Current Population Survey of the US Bureau of the Census, namely 'Looking ahead, do you expect to have any (more) children?'; with answers 'Yes, No, Uncertain'. The word expect suggests that the arrival of children is comparable to a thunderstorm or other acts of nature, partially predictable maybe but wholly out of anyone's control. The questions seems comparable to, say, 'Do you expect it to rain tomorrow?', though it is described as providing information about intentions. Economists would surely want to ask instead about people's plans for children, plans which, of course, may or may not be realised. Perhaps political considerations to do with the acceptability of family planning

motivate this choice of word by a government agency but it does not make life easy for an econometrician.

And this remark points to an omission in the author's generally extensive literature survey. As is well known, subjective Bayesians analyse data by showing how prior beliefs are changed by evidence into new or posterior beliefs. An important part of this process is finding out what the prior beliefs of your client or subject are, a problem that goes under the heading of elicitation of beliefs. 'Elicitation is the process of formulating a person's knowledge and beliefs about one or more uncertain quantities into a (joint) probability distribution for those quantities,' (Garthwaite *et al.*, 2005, p. 680). The paper from which this quotation is taken is one of several recent surveys of a large and growing literature dealing with this problem of eliciting beliefs, which is relevant to econometric work but is ignored in this book. If economists wish to collect data about beliefs then the Bayesian literature would appear relevant.

To summarise, this is indeed a brilliant book and a rewarding read for any econometrician, full of insights and thought-provoking remarks. While it will be clear from the tone of my comments that I find Manski's new 'worldview to guide empirical researchers' to be unhelpful, a less grandiose interpretation of his contribution, in which the emphasis is on the construction of bounds from incomplete data, has clearly proved to be a valuable development for frequentist econometricians.<sup>3</sup> Many will enjoy this book as much as I did.

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<sup>3</sup> Bounds are an automatic by-product of an analysis based on likelihoods.