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literature and science are basically dreamers and storytellers' (p. 74), he advises them to '*use but don't love technology*' and 'recruit a better-prepared collaborator' to help with technology they find 'at all forbiddingly difficult' (p. 87). By doing so, they can make collaborators of 'whoever it takes for the project to succeed' (p. 93). He further encourages researchers to be adventurous and find niches that are not over-crowded with established experts or talented people: 'March away from the sound of the guns. Observe the fray from a distance, and while you are at it, consider making your own fray' (p. 46). He also points out that '[i]n the search for scientific discoveries, every problem is an opportunity. The more difficult the problem, the greater the likely importance of its solution' (p. 47). In his letter on scientific ethics, he stresses that only original discoveries count but warns against error: 'You will make mistakes. Try not to make big ones. Whatever the case, admit them and move on' (p. 239). On the basis of his own experience, Wilson advocates entrepreneurship, the performance of numerous short and easily performed experiments, and 'messing around' to see whether something interesting happens. As he puts it, 'Disturb Nature and see if she reveals a secret' (p. 83). A brilliant storyteller, he shares many valuable insights on how his scientific discoveries actually emerged, a detail seldom elaborated in scientific journals even though, as stressed in Wilson's *Consilience: The Unity of Knowledge*, this process contains most of the secrets of scientific success.

This book is a must read for any scientist, regardless of discipline and age. It is storytelling at its best and a declaration of love for science that not only emphasises both its challenges and rewards but brings the message home creatively and poetically:

> Whatever that might be, wherever located, however expressed, it begins as a phantom that rises, gains detail, then at the last moment either fades to be replaced, or, like the mythical giant Antaeus touching Mother Earth, gains strength. Inexpressible thoughts throughout flit along the edges. As the best fragments solidify, they are put in place and moved about, and the story grows until it reaches an inspired end. (pp. 74–75)

Wilson's characters come vividly alive, whether William L. Brown, a working-class guy with a first-rate mind and an inspiring devotion (bordering fanaticism) to science, entomology, jazz, good writing and ants; Corrie Saux Moreau, an ant fanatic with ants tattooed over her body, who with her courage, self-confidence and determination achieved a cover article in *Science* as a PhD student; or William H. Bossert, the brilliant mathematician with whom he worked on a general theory of chemical communication (engineering by natural selection) using first evolutionary biology, then biophysics and later natural products chemistry. His joy as a scientific naturalist is contagious, and the book's honesty frequently elicits a smile; for example, a distinguished researcher's comment that 'a real scientist is someone who can think about a subject while talking to his or her spouse about something else'. The book's positive message is perhaps best summarised by the following quotes: 'The universal nature of scientific knowledge yet to be revealed includes a near-infinitude of surprises' (p. 175) and 'First and foremost, I urge you to stay on the path you've chosen, and to travel on it as far as you can. The world needs you – badly.' (p. 13)

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The Limits of Inference Without Theory, by Kenneth I. Wolpin (MIT University Press, Cambridge, MA, 2013), pp. 192.

In this book, which is an extension of the 2010 Tjalling Koopmans lectures given in Yale in 2010, Ken Wolpin makes the case for using structural models in applied microeconomics using examples from his own line of work. He explains his favoured structural approaches to labour supply, educational choice, parental investments and job search, showing how structural models can be used to extrapolate from current data to future policy, and how 'reduced form' methods get answers that conflate a variety of deeper mechanisms under particular assumptions about those deeper mechanisms.

On one hand, this is a fine book. Ken writes well and clearly articulates the reasons why he favours particular structural models over others, what the current state of play is in the lines of literature he contributes to, and has picked topical examples from North America to make his case. One sees the real craftsmanship that went into the literature discussions in this book, and as a review of parts of the structural estimation literature, the book is excellent.

On the other hand, I found this book a disappointment in that it covers no new ground on the debate it says it wants to contribute to. Wolpin's basic point, that more assumptions get you greater leverage on your data, has been well chewed over many times already, in particular by his co-author Mike Keane in his 2010 Journal of Econometrics article on the same topic. The 'structural versus reduced form' debate in the US has raged for about 10 years now and seems to have reached an equilibrium in terms of arguments bandied around.

My disappointment comes from the disconnect between what the book promises in the introduction and what it delivers. In the introduction, the author promises 'to consider…the role of theory in drawing inference from data and the limits that eschewing the use of theory places on inference. My intention is to illustrate the applicability of Koopman's concluding remark to inferential empirical work in economics and the social sciences more generally'. Let us take this stated goal seriously (otherwise the review can stop here!).

The first disappointment is the parochialism of this book. One could mistakenly deduce from this book that structural estimation only survives in a small corner of economic academia occupied by Wolpin and his friends. One does not learn anywhere in this book that structural models are the norm in much of macroeconomics, in work on auctions, in mechanism design, in structural I/O, and many other economic literatures. Disciplines outside economics do not get a look-in at all, despite the promise in the introduction about 'social sciences more generally'. Almost as if a classic focusing illusion is at play, the issue of reduced form versus structuralists is seen solely through the lens of a particular strand of the applied microeconomic literature. The title of the book is therefore too lofty, as it should have been something like 'The merits of structural estimation in applied microeconomics'.

The second reason for disappointment is that Wolpin makes no attempt to get at the core of the difference between structural and reduced-form in the literature he talks about. In the introduction, he merely says that in a structural model 'the relationships that are estimated are explicitly intended to be invariant to policy'. Taken literally, all that a paper would need to do to count as structural is then to say 'we interpret the found relationships as invariant to policy and applicable elsewhere'. It is clear that that is not really what Wolpin has in mind. Thus, on a case-by-case basis, he nominates certain estimation models as reduced form and others as structural.

From his examples, it is clear that what Ken means by structural is the assumption that individual agents rationally maximise a discounted stream of utility functions themselves dependent on stable preference parameters, augmented by a whole set of issue-specific ancillary assumptions to make the estimation tractable. Reduced form is then primarily the absence of the requirement that particular choices are maximising a given function. At core, Wolpin thus makes the assumption of each individual as a homo economicus, whilst the reduced form models do not. When Wolpin talks about the usefulness of theory he therefore talks about the usefulness of the homo economicus assumption in learning something policy-relevant from data.

If Wolpin had put it in this way, it would have been easier to have a real discussion about the merits of structural versus reduced form based on an understanding of the history of economic thought: we could had had a discussion about the appropriateness of the homo economicus abstraction depending on circumstances, as well as an honest assessment of the importance of the ancillary assumptions made for tractability in particular applications. Wolpin would have had the centuries-old literature on the homo economicus assumption to draw from.

Lacking such a clear distinction as to what counts as structural and what not, the book fails to construct a reasonable case for reduced form. And I think there is a reasonable case: one can still hold as an ideal that economists should one day come up with a behavioural model of the system to get predictions and deduce optimal policy (which was Tjalling Koopman's basic point: no conclusions without assumptions), but reject the homo economicus assumption used in applied microeconomics as worse than not even bothering with behavioural assumptions. Within that view, reduced form estimation is essentially a particular way of waiting for better assumptions and theories to come along. That reduced form coefficients are then 'wrong' and 'uninterpretable' as seen through the lens of the homo economicus assumption does not prove they are wrong and uninterpretable under different, yet to be fully articulated, behavioural assumptions. By not articulating this alternative, Wolpin only preaches to the converted when he talks about the importance of his brand of theory.

The third disappointment is that Wolpin is uneven in his evaluation of the relative merits of the two approaches. He, rightly in my view, mocks the large degree of ad-hockery and unreasonable assumptions in the reduced-form literature, for instance via the ill-understood 'conditional independence assumption' made in matching models or the equally dubious 'no other variable is endogenous' assumptions made as a matter of course in nearly all regression-based approaches. Yet, that critical eye is missing somewhat from his treatment of the disadvantages of the structural approach.

What demerits of the structural approach does Wolpin conveniently gloss over? As a prominent example, take the results of the Keane and Wolpin (1997) dynamic choice model, which pops up in many places in the book as it has been a real boon for them career-wise. The reader is told on page 97, once again, the kind of conclusion that nearly always comes when you apply that particular structural model: 'Keane and Wolpin (2010) find that unobserved heterogeneity is the most important of the initial conditions in accounting for the variance in [educational choice] behaviours'.

Where does this conclusion come from in the data, you may wonder? It essentially comes from the finding that there is a lot of persistence in behaviour: people do not change their choice of partner, jobs, education stream, residence or occupation very often. To marry the empirical regularity of this persistence to the principle of rational maximisation gets you two basic possible interpretations. The first is that life was essentially already over at conception as there is something fixed (and usually unobserved) about you that must have predetermined what education, job and partner you were going to end up with and stick with. The second is that there are shocks later in life to you with a strong element of persistence: lock-in effects with partners, jobs and education streams (via unobserved slow-changing stocks).

Why does the Keane and Wolpin (1997) model invariably tell you life is largely predetermined at conception (fixed types)? Because, mainly for analytical convenience, they assume that there are no persistent shocks later in life. Where does that particular rabbit go into the hat? It is in the assumption of independent identically distributed shocks every period. That technical assumption, coupled with persistence in the data, is what forces the users of this model to, time and again, conclude that life was largely predetermined at the first moment you came into the data.

Now, I must confess that I have always found it intellectually disconcerting of Keane and Wolpin to not mention the importance of that assumption whenever they tell policy makers and others about their findings, and am bothered by seeing it again in this book. You see, almost no-one they talk to is as versed in their models as themselves, and hence if they don't religiously remind their audience about the technical rabbits they have put into their hats to get their 'amazing results', their audience's only choice is to take it or leave it on faith. And it is not good enough to say that criticizing their own model is a job for others because the few that understand how these models really work are usually co-authors or PhD students of the originators of these models and hence have a vested interest.

What goes for the 'hidden importance' of the iid assumption goes for many elements that happen in the basement of the actual estimation of these structural models. Often, for instance, particular aspects of the data are 'impossible' according to the structural model and one hence has to somehow ignore or massage away inconvenient data points, such as people with negative incomes (a real nuisance when one specifies incomes as log-normal, such as one finds on page 115) or people with multiple jobs and multiple partners (a real nuisance for structural occupational-choice models or marriage-models in which people serially search for one match, such as the model on page 116). In the basement of many structural models there is a whole bag of tricks one needs to employ to get reasonable outcomes and massage inconvenient data.

Now, there is nothing inherently unusual about these issues as all applied social science is hampered by the fact that we do not describe the whole socio-economic system, or even the part we research, and that our models thus invariably violate some aspect or other of reality that therefore needs to be swept out of sight, but when talking about the (de)merits of different approaches it is crucial to bring these elements to the fore and openly discuss them for structural approaches make more assumptions and hence need more data-massaging. Part of the appeal of the reduced-form literature is that they have less to hide and that it is harder for them to hide anything because their estimation codes run in the dozens of programming lines rather than the thousands of lines typical for structural approaches. That is a real and important benefit of reduced form estimation that Wolpin does not bring to the fore.

Finally, there is the issue of actual policies and the whole process via which policies get chosen, implemented and updated. Wolpin basically refuses to get down and dirty with the whole business of politics by giving the reader a very pristine view of policy, that is, that politicians and bureaucrats have a very well-defined policy change in mind and wonder what its effect might be on whole groups of people, eager to ‘get it right’ and be ‘informed’ by the outside researcher. Within that caricature, termed *ex ante* policy evaluation, structural estimation brings as its advantage that one can come up with all kinds of ‘policy simulations’ within the toy-world one has just created and estimated, allowing one to find optimal policies.

Needless to say, this caricature of policy development has little bearing on policy-land. Not merely will policy makers be incapable of appreciating the finer details of the assumptions and methods of economic research and thus be unable to consume complicated results, but policy will be made up on-the-hoof, implemented differentially according to the different interpretations by thousands of people, and continuously updated and amended as effects start to become clear and adjustments are made both nationally and locally. It is against a background of that kind of dynamic process, where research output competes for attention on the basis of how quick results can be gotten and how easy they are to understand and explain, that structural estimation must compete with reduced form.

Yet, the trials of policy practice also gives additional arguments in favour of structural estimation, for instance that it forces its adherents to be much more careful with data, to have an appreciation of inter-relationships and to learn to interpret data much quicker, all of which are important for economists close to policy.

In short, Wolpin gives an excellent overview of the merits of a facsimile of structural estimation in a particular corner of economics versus a facsimile of reduced form estimation in the background of a facsimile of policy processes. In that self-constructed shadow world, structural estimation is right and reduced form is muddled and wrong. The pros and cons in a non-pristine environment and in the wider literature are undiscussed, which is disappointing and limits the appeal of this book to those already of the right faith.

As to the outside value of this kind of exercise, I find myself agreeing with John Rust on this topic when he reacted to the aforementioned 2010 Mike Keane article on the superiority of structural versus reduced form (here: gemini.econ.umd.edu/jrust/research/keane_comments.pdf): ‘It really isn’t productive to criticize the status quo in economics these days, nor is it productive to try to “market” the virtues of structural estimation. Criticism only encourages practitioners to “rally around the flag”. I think it is equally a waste of time to engage in salesmanship. A better way to sell a tool to an uneducated consumer is to demonstrate that it is not too expensive or hard to use, and that it can do important things that would be difficult or impossible to without it.’

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The Great Recession: Lessons for Central Bankers, by Jacob Braude, Zvi Eckstein, Stanley Fischer and Karnit Flug (MIT Press Cambridge Massachusetts and London England 2013), pp xii + 380

This volume brings together selected papers from a conference on *Lessons from the World Financial Crisis* organised by the Bank of Israel in March/April 2011. Participants were from central banks and international institutions, giving the volume a strong policy-orientation. It presents the experience and perspective of a variety of countries, both advanced and emerging, mainly those which avoided a financial collapse. The central bank focus reflects the important role these institutions played in dealing with the crisis, but it does confine the focus to monetary and financial policies.

Given the two-year gap before the volume was published, one might question whether it had