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Book reviews

Arthur Lupia and Mathew D. McCubbins, *The democratic dilemma: Can citizens learn what they need to know?* New York: Cambridge University Press, 1998. xiv + 282 pages. $64.95 (cloth); $19.95 (paper).

In closing the Series Editors' Preface to this book (p. xi), the editors write: "The book thus provides a pathbreaking analysis of information and persuasion in contemporary politics and lays the foundation for exciting future interdisciplinary empirical work". I cannot disagree more strongly. *Inter alia*, Lupia and McCubbins (LM) (i) claim to introduce new assumptions from cognitive science (p. 18) and to use a "definition of rationality [that] departs from the definitions commonly employed by economists and political scientists" (p. 23), but in fact undertake classically rational choice reasoning; (ii) claim to develop a "unique" and "novel" model with similar conclusions but in fact just tweak an existing signaling model; and (iii) assert a variety of properties for the canonic cheap talk signaling model and the existing literature that are demonstrably false.

The "democratic dilemma" to which LM refer is that people may be incapable of making "reasoned" political decisions (i.e., decisions predicated on accurate predictions of their consequences) due to a paucity of information. The central thesis of the book is that in fact individuals do not have to be fully informed to make "reasoned" choices; so in particular the empirical observation that people are largely ignorant about politics does *not* imply that political outcomes are necessarily at odds with what would be chosen were everyone fully informed. This is hardly a novel perspective; e.g., Popkin, 1991; McKelvey and Ordeshook, 1986 (which is not, contrary to the claims of LM (p. 5), a signaling model). Thus the authors want to go further to identify "specific conditions under which people who have limited information can make reasoned choices" (p. 5) and, to do this, they purport to offer a theory that is "unique in that it consists of a novel combination of assumptions, results in a novel set of findings, and produces important and *testable* implications about the political consequences of limited information" (p. 43). Unfortunately, except cosmetically, their theory and their results are neither unique nor novel.

Due both to reasons of space and my comparative advantage (such as it is), I shall exclusively confine my discussion to the first (and core) part of the book, the theory (and even here, I shall have to be selective, looking mostly at Chapter 3 the central theory chapter). The second part adds some experimental and survey results to the existing literature examining the predictions of strategic information transmission models, and a conclusion reflects on what the authors believe about the information aggregation properties of various democratic institutions.

Chapter 2, the first theory chapter, is concerned with "How people learn". LM claim to want to integrate aspects of rational choice theory and cognitive science but, as far as I can tell, such integration amounts to little more than mixing the jargon. LM write, for instance, that "The first new assumption [that LM are to use] is that *learning is active and goal oriented*. This is one of the most basic lessons of cognitive science" (p. 18, emphasis in the original). It might be added that it is also a staple of rational choice theory: see for instance, Grossman et al. (1977) or the text by Hirshleifer and Riley (1992: Ch. 5). So while the attribution to cognitive science is just fine, it is certainly not a novelty for rational choice modeling. Similarly LM's "second new assumption incorporates the process by which people use limited information to draw complex inferences. Cognitive scientists call this process *connectionism*" (ibid.). Unfortunately this assumption is never articulated. However, LM do claim that "without a process like connectionism, reasoned choice requires encyclopedic information"; along with the examples used to illustrate what is meant, this claim suggests that the assumption is simply that individuals are capable of making inferences. What exactly is "new" here is left a mystery. Nevertheless, LM seem to view these "new assumptions" as leading to a distinct conception of rationality (p. 23). They write that "many of [economists' and political scientists'] definitions confound rationality and omniscience, assuming that rational actors must be limitless calculators. ... [In contrast] we define rationality to mean all human behavior that is directed toward the pursuit of pleasure and the avoidance of pain" (p. 23). The first claim is at best misleading. The entirely standard use of rationality in contemporary economics and political science requires individuals to have consistent (i.e., at least acyclic) preferences over consequences and to choose more preferred over less preferred feasible alternatives; in particular, rationality imposes no constraints on how individuals respond to or evaluate uncertainty *per se*. Were LM correct in their assertion, for example, there would be a negligible literature on the foundations of expected utility and other theories of decision making under uncertainty; yet theorists have been worrying about this in droves at least since 1944. Given their phrasing, I suspect that what LM intended to write was that rational choice theory typically presumes *logical*

omniscience (i.e., that rational agents in the theory are capable of computing the logical consequences of any given set of assumptions). Now this is true but it most certainly does not imply individuals possess *general* or, in particular, *informational* omniscience nor that such omniscience is necessary for "reasoned choice" (it is also a technical assumption and not an empirical claim about the capacities of real people; but this is a separate issue). Moreover, despite their remark that "their" definition of rationality is distinct from the usual one in rational choice theory, the fact is that the theoretical arguments of subsequent chapters are, as will become clear momentarily, *exactly* the arguments of a fully fledged rational choice model with logically omniscient and fully Bayesian, expected utility maximizing, agents.

LM go on in Chapter 2 to develop a theory of attention and the value of information. This amounts to nothing more than a series of more or less trivial observations dressed up as "Theorems". For instance, it is evident that in any binary choice under uncertainty, receiving more information about the likely consequences of either decision need not alter my decision. If this is the case and I turn out to have made the correct choice *ex post*, then receiving the additional information was unnecessary to my making a "reasoned choice"; on the other hand, if I turn out to regret my decision *ex post*, then the additional information was insufficient to guarantee a "reasoned choice". This observation is elevated to a theorem (p. 27): "more information is neither necessary nor sufficient for reasoned choice". While correct, it is hardly a surprise and certainly not novel (again, examples can be found in Hirshleifer and Riley, 1992). Similarly, if "we assume that there are thousands of stimuli to which a person could attend [and] we assume that the opportunity cost of paying attention to a particular stimulus in this case is substantially higher than in the case where there are only one or two potentially relevant stimuli [then we have:] *Theorem 2.2: People have an incentive to ignore many stimuli*" (p. 19). Although there are several close competitors in the book (e.g., Theorems 4.1, 4.2), this particular "theorem" has the distinction of revealing absolutely no distance between assumptions and conclusion.

Despite the unnecessary formalizations and distracting references to cognitive science, the discussion of Chapter 2 turns out to be moot since, as already indicated, LM go on to use entirely orthodox (and well-understood) game-theoretic models of strategic communication. Before going on to examine the central theory chapter, Chapter 3, consider the following simple signaling model.

There are two players, a Sender [S] and a Receiver [R]. R has to choose an action from {x,y} and there are two states of the world {X,Y}. The game begins with Nature privately revealing to S whether S is a "friend" (S = F) or an "enemy" (S = E) and, with probability k = 1, the true state. Both S and R

have a common prior of $0 > b > 1$ that the true state is X and R believes that S is a friend with probability c. After Nature's decisions, S makes an assertion to R concerning the true state following which R chooses one action or the other. Payoffs are then distributed according to the table below, in which the first [second] number is S's [R's] payoff and $s,r > 0$.

	R's action		**R's action**	
	x	**y**	**x**	**y**
true state = X	s,r	0,0	–s,r	s,0
true state = Y	–s,–r	0,0	s,–r	–s,0
	S is a "friend" (F)		**S is an "enemy" (E)**	

There are two possible Bayesian equilibria here. One is a pooling equilibrium in which S conveys no information and R chooses the action that maximizes her expected utility conditional on the prior belief, b; the other is an informative (but not separating) equilibrium in which S = F always reveals the true state, S = E always claims the opposite of the true state holds, and R always chooses the action implicitly recommended by S. The pooling equilibrium always exists; the informative equilibrium exists if $c \geq \max\{b, 1-b\}$ and only if $c \geq \frac{1}{2}$. Thus information can be credibly conveyed in equilibrium only if S is sufficiently confident that R's ordinal preferences over actions in each state are the same as hers (this is the direct analogue of the Crawford and Sobel (1982) result that preferences over outcomes have to be sufficiently close in a spatial cheap talk model for any equilibrium information transmission).

This game and the equilibrium results are due to Sobel (1985) who uses the set up as a stage-game (setting $b = \frac{1}{2}$ purely for expository convenience) in a repeated Sender/Receiver game in which the payoffs vary stochastically through time (for another signaling model in which S's ideal point is drawn from the real line and is private information to S, see Austen-Smith, 1995). LM cite Sobel (1985) in the introduction to Chapter 3 only as one in a list of past contributions. This is odd, especially as the *only* differences between the game above and LM's basic model, about which they make their "uniqueness" and "novelty" claims quoted earlier, is that the probability that S is informed, k, can be less than one and the prior, b, can be any number in (0,1) (both Sobel and LM allow some asymmetry in payoffs, but this, like the technical convenience of setting $b = \frac{1}{2}$, is substantively irrelevant). In the Appendix to Chapter 3, LM show (in Proposition 3.1) that the informative equilibrium above is also an equilibrium to their perturbation of the game,

stating the necessary condition $c \geq \frac{1}{2}$ as Theorem 3.1 and apparently not recognizing the tight connection to Sobel (1985).

Now it is a *defining assumption* of non-vacuous cheap talk games that the Receiver believe the Sender is better informed, i.e., that $k > 0$; for if $k = 0$ then by definition the Receiver can learn nothing from the Sender and will ignore any claims to the contrary (LM report this defining assumption as "Theorem 3.2"). Thus informative equilibria depend on R conditioning on the event "S is informed" and, given all types of sender have the same strategy sets and given that k is positive, the parameter k only modifies the boundary conditions defining the equilibrium set of the game. Specifically, in Sobel's game (where $k = 1$ and I relax the technical convenience of $b = \frac{1}{2}$) the set of (b,c) pairs in (b,c) space for which the informative equilibrium exists is the triangle formed by the vertices (0,1), (1,1) and (1/2,1/2); when $1 > k > 0$ (LM's variant), the set of (b,c) pairs for which the observationally identical equilibrium exists is also a triangular region with the same vertices and upper boundary, but with the side boundaries bowed inwards and (loosely speaking) with the degree of curvature decreasing in k. There are no other behaviorally distinct equilibria. To all intents and purposes, therefore, the LM model is strategically and consequentially identical to Sobel's 1985 stage-game. (If informed and uninformed senders have different strategy sets, however, this is not true: Austen-Smith, 1994).

It is also odd to read LM's claim that "In the standard cheap talk model, people cannot deceive one another *in equilibrium*" (p. 49, emphasis in the original); in discussing the second, informative, equilibrium to the game above, Sobel observes that "E always lies to R" (Sobel, 1985: 561) and R is always deceived. Moreover, *any* cheap talk model involving at least one type using a mixed strategy in equilibrium (of which there are myriad) necessarily involves successful deception in equilibrium. To see this consider the following cheap talk game (borrowed from Austen-Smith, 1992) in which S is known to be informed for sure ($k = 1$), R can take one of three actions and the prior belief on the true states is uniform ($b = 1/2$). Payoffs are given below with S's payoff the first number.

	R's action		
	x	**y**	**z**
true state = X	3,3	1,0	0,2
true state = Y	3,0	1,3	0,2

Here there are two equilibria. The first equilibrium is a pooling equilibrium in which S conveys no information and R chooses z since this maximizes R's expected payoff. The second equilibrium is semi-pooling: if S learns the true state is X then S reports this surely to R; if S learns the true state is Y then S reports this to R with probability ½ (S lies 50% of the time); if R hears a message that Y is the true state, R chooses y surely and is not deceived; if R hears that X is the true state, then with probability 1/3 R chooses x and with probability 2/3 R chooses z. Thus R is successfully deceived by a lie from S with probability 1/6. It follows from this example that LM's assertion above is false. Moreover, their further claim that a necessary component of a theory involving deception in equilibrium is "a principal [R] who does not have and cannot obtain knowledge about the speaker's incentives" (p. 73) is likewise erroneous: the only thing R does not know in the preceding example is the true state of the world since R possesses complete knowledge regarding S's incentives, state of knowledge and motivations.

Given the large literature on cheap talk signaling games in both economics and political science, one wonders why LM continue to insist that lying in equilibrium is impossible in these games. One reason might be a failure to appreciate that in cheap talk signaling games the meaning of any message or speech is determined endogenously in equilibrium and not by definition of the message space. For instance, Sobel's statement quoted above, that "E always lies to R", is predicated on an imposed interpretation of the message, essentially the same interpretation imposed by LM who write that a message means "I assert that x [respectively, y] is better than y [respectively, x] for the principal" (p. 241). However, being cheap talk games, it is perfectly legitimate to reinterpret the messages in Sobel's or LM's model (or the mixed strategy example above) as saying "I think the principal should do x [respectively, y]". Doing this leads to absolutely no change in either the equilibrium inferences drawn by the receiver or the set of equilibrium behaviours, but it is now literally the case that *no* type lies in equilibrium.

The same reason might also explain the confused and confusing discussion of an equilibrium refinement in the appendix to Chapter 3. As far as I can tell, the idea here is, first, to rule out pooling equilibria (although their discussion seems to focus only on ruling out equilibria in which all types of sender use a common mixed strategy with full support on the message space). Rather than just assume this directly on the grounds that they are interested only in informative equilibria when they exist, LM invoke Farrell's discussion of focal meanings in natural language to support (somehow) their ignoring pooling equilibria (p. 245). Second, LM go on to write "we focus on non-neologistic equilibria. In our model, a neologistic equilibria [sic] *requires* the speaker [S] and principal [R] to agree that the signal ["better"] means "worse" and

not “better” and that the signal [“worse”] means “better” and not “worse”. Focusing on non-neologistic equilibria is equivalent to assuming that words have focal meanings (Farrell, 1993: 319)” (p. 246, emphasis in original).

Strangely, LM nowhere define the concept of a “non-neologistic equilibrium”. On the one hand, this is irrelevant; since LM have already excluded pooling equilibria from consideration, the only remaining equilibrium is the informative equilibrium discussed above and, therefore, there is nothing left to refine. On the other hand, the quote above, along with the earlier discussion in the appendix and the subsequent formal argument regarding mixed strategy equilibria, suggests (at least to me) that LM believe Farrell’s (1993) neologism-proofness equilibrium refinement rules out equilibria in which (in the game above) S says “Y is true” and is interpreted by R as meaning “X is the case” and conversely. In fact, for Farrell’s neologism-proof equilibrium in a cheap talk game this would be just fine: the “natural” interpretation of the sentence “X is true” is an artifact of the development of the language and not a property of game theory; thus so long as the listener on hearing “X” acts as if “Y” and conversely, there is no equilibrium selection problem. This is not an issue for Farrell’s neologism-proofness refinement which is concerned with out-of-equilibrium messages: it is not (and was never) designed to deal with selecting equilibria in which a particular literal meaning, whatever it might be, is attached to a message sent *in equilibrium*. It is perhaps not surprising, therefore, that in apparently seeking to invoke neologisms to rule out such inferences, LM describe Farrell’s concept of a neologism incorrectly and, if we apply Farrell’s refinement correctly, it turns out that *all* the equilibria in the game are (in Farrell’s sense) neologism-proof.

LM offer two extensions of their model. One such is the possibility that with some nondegenerate probability Nature replaces the Sender’s message with the true statement after the Sender has spoken (p. 250). But since S can at best infer whether Nature intervened *ex post* and R never finds out, not only is it unclear what it has to do with “verification” it is also substantively identical to varying the product ck: the higher is the probability Nature reveals the truth to R the less incentive S has to lie when S is an enemy, which is identical to the comparative static result that the more frequently Nature chooses S to be a knowledgeable friend the less likely is deception to occur *ex ante*. The other two extensions (penalties for lying and effort required for sending a message) have the same effect; they convert a cheap talk signaling game into a costly signaling game. The main results here are that typically more information can be credibly revealed and the possibility of successful dissembling by the sender diminishes; neither result is novel by any stretch of the imagination (see any recent textbook on game theory).

Chapter 3 contains a variety of more-or-less striking inaccuracies regarding the state of the art in incomplete information games. Space precludes listing them all so one further illustration must suffice. LM suggest that, *inter alia*, the Feddersen and Pesendorfer result (1997; LM cite the 1995 working paper) on the information aggregation properties of large elections depend on an assumption "that people believe everything they hear" (p. 62). This is false. Not only do the Feddersen and Pesendorfer results not depend on this, their model no more assumes it than does the Sobel model used by LM. Individuals in Feddersen and Pesendorfer receive noisy signals about the true state of the world, and the quality and accuracy of these signals can vary from person to person; individuals are Bayesian, however, and so, exactly as discussed by LM for their setup, are appropriately skeptical about the extent to which any particular signal contains useful information.

Chapter 4 is a further restatement of some quite apparent properties of any standard signaling game, stated in terms of being "necessary and sufficient conditions for enlightenment [sic] in our model" (p. 69) and so on. And while the setting is slightly different, the model of Chapter 5 is essentially a small variant of Gilligan and Krehbiel (1989), in which one member of an informed committee (agent) makes a proposal to change the status quo platform to the uninformed House (principal) and, before the House chooses, a second member of the committee may make a speech to the House regarding the consequences of either choice: see also Banks (1993), whose results suggest the role of a third party (speaker) is superfluous, and, more generally, the huge corpus of work on mechanism design in economics.

The final part of the book "examine[s] the incentive effects of democratic institutions to ascertain when they promote the conditions for enlightenment and reasoned choice" (p. 206). The discussion here is quite informal and presupposes that the lessons from the 2-person/2-state/2-decision signaling game generalize to arbitrary environments. From a theoretical perspective, it is known that not all such lessons do so generalize and the settings in which generalizations fail seem important for politics: for example, other things being equal, more informed senders can be *less* capable of credibly conveying information in equilibrium (e.g., Shin, 1994) and, under some conditions, the presence of multiple speakers can *reduce* the amount of credible information transmission in equilibrium (e.g., Matthews and Postlewaite, 1995). Similarly, there is no consideration of any collective action problem in information acquisition or dissemination; of the coordination problems among multiple equilibria; of the extent to which other instruments like money interact with the informational aspects of any institution; and so on. Although much of what is said regarding the informational byproducts of candidate competition, for instance, is reasonable, I am hard put to find something that has

not already been observed. The literature already has work on the use of campaign contributions as signals (e.g., Lohmann, 1995); the role of candidate competition in leading to information revelation or otherwise (e.g., Harrington, 1992); the ability of large electoral systems to aggregate individual information (e.g., Feddersen and Pesendorfer, 1997); etc. While it is useful to be reminded of all this, I am not convinced there is any value added here.

More generally, throughout the book LM portray the existing literature, including numerous rational choice analyses, as a patchwork of special cases, whereas their claimed contribution is to provide a unified framework (see, for example, pages 7, 8 and 40). But the defining characteristic of the rational choice literature has always been the application of a quite self-consciously general methodological framework to ostensibly disparate problems of decision making. Inasmuch as the book is addressed to a broader audience than those working within the rational choice tradition, LM's representations are very misleading.

The residual contribution of this book is the addition of further empirical work on information games (Part II). Given how little is added by the authors' theoretical arguments, I think they should have concentrated their efforts on this empirical work.

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Peter Kurrild-Klitgaard, *Rational choice, action, and the paradox of rebellion*. Copenhagen: Institute of Political Science, University of Copenhagen, 1997. xxxi + 392 pages. DKR 80,00 (paper).

This volume does a masterful job of analyzing rebellion. Using a rational choice framework, Kurrild-Klitgaard explains why there are impediments to rebellion even when most people would like to overthrow the existing government, and explains how successful rebellions can be organized to overcome these impediments. Kurrild-Klitgaard is so methodical and so complete in his arguments that the book is slow going at first, as he carefully explains the rational choice paradigm as applied to rebellion, but his careful reasoning pays off later, when he gets to the core of his argument. He draws on his solid foundation and just as carefully explains why rational actors can organize and participate in a successful rebellion. He explains why political entrepreneurs will almost always be necessary for a rebellion to succeed, and describes what entrepreneurs must do to enhance the probability of launching a successful rebellion. He has extensive footnotes and a very complete list of references, and discusses in depth the objections that could be raised to his line of reasoning. The book shows an impressive level of scholarship. His analysis is so thorough that the book could be viewed as a handbook for subversives on how to organize a successful rebellion, cleverly disguised as an academic treatise.

The paradox of rebellion arises because even if there is strong public sentiment for a rebellion to overthrow the existing government, the outcome of a