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Jon Elster, *France Before 1789: The Unraveling of an Absolutist Regime*. Princeton, NJ: Princeton University Press, 2020. ix + 263 pp. Figures, notes, and index. \$39.95 (hb). ISBN: 9780691149813.

Response by Jon Elster, Columbia University

I am grateful to William Doyle and Travis Wilds for taking the time to read and comment on my book on the ancien régime. They help me to see more clearly how some of my claims might have been better stated and better argued. In particular, I should have spelled out in more detail what I meant by “unraveling.”[1] At the same time, I disagree with some of their objections and am unable to respond to others because they are stated as peremptory judgments not backed by evidence or arguments.

Doyle and Wilds have different intellectual profiles. Doyle (whom I cite in my book) is a highly accomplished and highly regarded historian of the ancien régime and the French Revolution. He writes as a *traditional historian*, an expression that I use as praise, not as dispraise. Wilds is an intellectual historian, whose writings focus on the late-eighteenth and early-nineteenth centuries. Whereas Doyle writes as an insider to the subject, Wilds, like me, is an outsider.

DOYLE

Let me first comment on some minor matters of style. Doyle writes that my translations from French into English are “spectacularly wooden.” As neither is my native language, this assessment is quite plausible, but an example or two might have been more persuasive than this pronouncement ex cathedra.[2] The same comment applies to his reference to my “quite abstract conceptualization about human behaviour which in the eyes of many an empirical historian will often seem like long-winded expressions of the obvious”—quite possibly, but *show us*. Overall, he thinks the book is “rugged and “angular.” I can understand why it can appear in that way. When I taught in France, my assistant who read through the manuscripts of my lectures before I delivered them suggested I might want to “*arrondir les angles*.” I mostly did not; readers must judge for themselves.

On substance, I am surprised by a number of misattributions and misreadings. Let me cite and comment on some of them. For example, Doyle writes:

If only the crown had been more resolute, [a common] argument goes, preservative change could have come about without violence, and revolution avoided. This is the traditional *thèse royale*, and it seems no coincidence that Elster’s footnotes are loaded with references from historians such as Marcel Marion, Roland Mousnier and Michel Antoine, idolisers all of absolute monarchy and contemptuous of any opponents as little better than rebels. This approach features heroes and villains, and Elster shares the same ones. Louis XV and XVI (monarchs, after all!) are not in general blamed for what happened, apart from being too weak; but other individuals are.

I cite Marion, Mousnier, and Antoine on a number of empirical issues. I do not endorse their idolization of the absolute monarchs, and I criticize another historian (François Bluche) for his hagiography of Louis XIV (pp. 146-147). Following Fénelon (one of my few heroes, along with Vauban, Turgot, Malesherbes and the Abbé de Véri), I argue that Louis XIV was to blame for the misery that followed from the aggressive wars fueled by his massive egocentricity (pp. 120-21).

Doyle continues:

[T]he traditional institutional villains are the parlements, presented as corporate bodies of selfish, purblind and venal judges who blocked all attempts at reform in order to protect their own interests. Little allowance is made for the possible sincerity of magistrates when they clashed with the crown, or for the commitment of these professional jurists to the rule of law, not to mention the constant lack of solidarity among them.

Malesherbes, who knew the magistrates as well as any, referred to their “subtlety and falsity” (cited on p. 228). I attempted to assess his claim by considering remonstrances made by the *parlement* of Paris in the eighteenth century. My hypothesis was that most of their decisions were motivated by *interest* or *vanity* rather than by *reason* (the desire to promote the *public good*). Conclusive proof of the last motivation would require the existence of remonstrances that (i) remained unpublished (excluding vanity) and (ii) did not focus on issues where the magistrates had a material stake (excluding interest). On my count, out of thirty-three remonstrances submitted between 1715 and 1753, at most one satisfied both criteria (pp. 37-38). I fail to see how the lack of solidarity among the magistrates (which I discuss on pp. 180-81) would contradict my hypothesis. If anything, it would seem to confirm it.

Doyle claims:

[A]lthough in the introductory chapter the author briefly recognizes that royal authority could actually derive strength from the ability of subjects to resist, the point receives little later elaboration, and he endorses the traditional conclusion of the *thèse royale* that the fundamental mistake of the absolute monarchy was to abandon the reforms of Maupeou and Terray, which had smashed institutional resistance, and restore the old parlements.

Both claims are wrong. What Doyle calls “the point” is identical to what he later refers to as “the best insight in the book, that ‘the *impotence of omnipotence* may be the central paradox of the ancien régime.’” I elaborate “the point” on pages 150-54. I also defy him to find a single sentence in the book that supports the *thèse royale*. In the absence of citations from my book, at least some page numbers would have been useful to locate these “endorsements.”

Doyle also states:

[I]t is surprising that an author so keen, in the context of his wider three-volume project, to analyse earlier representative institutions, has only used one of Russell Major’s lesser treatments of their operation in the sixteenth and seventeenth centuries. Elster has certainly consulted Joseph Droz’s unjustly forgotten *Histoire du règne de Louis XVI*. He believes it influenced Tocqueville. But that must have been before publication, since Droz’s posthumous work only appeared two years after *L’Ancien Régime*, and only just before Tocqueville’s own death.

The first objection is idle. It would have been valid only if Doyle had cited specific works by Major that go against what I write about earlier Estates-General. That comment also applies to my “omissions” of Van Kley, George V. Taylor and Robert D. Harris. How, *exactly*, did these omissions lead me astray? The second objection is misinformed, as the first edition of Droz’s work appeared in 1839-42. Tocqueville admired Droz, who was his colleague at the Académie Française, and it stands to reason that he had read that edition and perhaps discussed it with Droz. Although I should have checked whether the first edition contained statements similar to those I cite from the later edition, Doyle should have verified whether the later edition was the only one (and perhaps conducted a similar check himself).

Finally, Doyle writes:

When he mentions the *cahiers*, or grievance books as he calls them, that vast body of evidence which nobody studying revolutionary origins neglects, he follows Tocqueville in seeing the *cahiers* of the Third Estate as facilitating outbursts of anger against the constraints of timeless economic and social structures, a call for change.

Once again, I defy Doyle to cite chapter and verse to document passages where I follow Tocqueville in viewing the *cahiers*, as Doyle calls them, in this perspective. I follow George Taylor (one of my “omissions”) in viewing Tocqueville’s use of the grievance books as highly selective, as “illustration rather than proof” (Taylor, cited on p. 213 n.). Doyle may have confused Tocqueville’s references to the grievance books with his brief comment on what he called the “grievance (*grief*) of liberty and equality as well as of money” (cited on p. 6). In this comment, Tocqueville makes no reference to the grievance books. Here, he does not address “timeless structures,” but the psychological effects of *changes* that, on the one hand, improved the material situation of peasants and commoners while, on the other hand, making them subjectively worse off. Hence these grievances or “griefs” were both the result of change and the call for (more) change. In my opinion, this “Tocqueville effect” constitutes a major theoretical innovation with wide application.

WILDS

Wilds’s main claim is:

France Before 1789 applies rational choice theory, anathema to the sociologist, to the case of the ancien régime, leveraging a bevy of cognitive and behavioral concepts to describe the actions and inactions of its groups. The particular brand of rational choice theory Elster has developed through his long career mitigates the thin economism of the basic model by building in agents’ emotions, cognitive mechanisms and beliefs. [...] The degree to which you find the results stimulating will hinge on how well you tolerate the simplifications of rational choice. [...] In *France Before 1789*, rational choice thinking facilitates judgments with all the elliptical, universalizing character of the *grand siècle* writers whom Elster cites as a kind of supplementary critical apparatus. A short section called “Necker Cause of the Revolution?” examines the emotional and cognitive motivations driving Jacques Necker’s decision to raise funds through loans rather than taxes, summarily attributing them to an “extreme vanity and desire for popularity.”

My stated intention was to focus on *choice* as the key explanatory variable (pp. 8-20). Simply stated, *rational choice* and *emotional choice* form the two species of the genus *choice*. I do not “build” emotions into “the thin economism of the basic model,” as some economists have

indeed tried to do, but recognize emotional choice as irreducible to rational choice. I certainly do not try to understand Necker through the prism of rational choice.[3] Emotions subvert rationality in many ways, notably by leading to irrational belief formation (pp. 13-14) and to disregard of consequences. I cannot see why this line of argument should be anathema to the sociologist. The sociology of emotions is alive and well.[4] The fact that mainstream rational-choice theory has fallen into discredit (in some circles) due to its excessive ambitions and misplaced precision does not undermine the obvious appeal of the rough-and-ready idea of choosing means that are appropriate to one's ends (p. 11).

Wilds's objections to rational choice theory do not seem to rest on intellectual grounds only:

The [rational-choice] framework could certainly be debated on its merits. But given its prevalence, it feels more pertinent to ask, in a Nietzschean or sociological vein, *who* finds it useful to think with, and why.

Sometimes, it may indeed be appropriate to sociologize a theory. The question "Why do many scholars believe in the theory of rational expectations?" (to cite just one example) is just as pertinent as the question "Why do many people believe in astrology?" Elsewhere I have proposed five putative explanations of the strange prevalence of hyper-sophisticated rational-choice theories.[5] Whether my hypotheses are right or not, the persistence of such theories over time poses an explanatory puzzle. However, the explanandum is interesting *only* because intellectual analysis shows that the theories are nothing more than science fiction with no empirical relevance. Thus, I would expand Wilds's statement to read "given its prevalence *and falsity*, it feels more pertinent to ask [...] *who* finds it useful to think with, and why." I would not, however, prejudge that the answer is to be found in the *usefulness* of the theory. Functional explanations and conspiracy theories, for instance, seem to owe much of their appeal to their ability to provide a sense of *meaning*.

Before one can proceed to a causal analysis of the origin and maintenance of a theory, it must be criticized "on its merit." My teacher Raymond Aron said in an interview that Pierre Bourdieu, whom Wilds cites several times, "tends to sociologize everything" rather than "keep[ing] the discussion on an intellectual level."[6] In some cases one can perhaps both sociologize and criticize, but to do only the former, as a substitute for the latter undermines the core value of academic work: the search for truth. In the last statement cited above, Wilds seems close to committing this fallacy.

I have criticized forms of rational-choice theory as varieties of "hard obscurantism." As far as I understand it, Wilds' conceptual universe falls within "*soft* obscurantism," often derived from "French Theory," of which Bruno Latour is a prominent example. The reference to how "Latour's account of how congeries of networks snarl to produce 'modes of existence'" is, to me, meaningless. References to the "'given' quality (Hunt) or 'profundity of generational unity' (Koselleck) of pre-Revolutionary time" may be meaningful if unpacked, but as they stand, they are too opaque. I am curious to know how my "economically inflected account of ancien régime temporality contrasts in interesting ways" with the last two writers, but I am not told *which* interesting ways these are. Nor did I realize I had proposed an "account of ancien régime temporality" or know what it would mean to do so.

I note that Wilds, too, approves of the idea of the impotence of omnipotence. The idea is not original with me: it derives from the work of Thomas Schelling, who emphasized the need for threats and promises to be *credible*, in the sense that the agent must have an incentive to

carry out the threat or keep the promise if or once the time to do so arrives. The incentive could be loss of reputation for failing to do what one has threatened or promised to do, or the presence of an institutional agent that would prevent one from renegeing. In an analysis of why the English kings, unlike the French monarchs, were able to keep their promise to return the principal of a loan at a specified time and with a specified rate of interest, Douglass North and Barry Weingast (cited on pp. 154-55) argue that the key difference was the existence in Britain of a strong parliament whose agreement was needed for changes in the contracts. Successive French ministers of finance tried to create sinking funds that would serve as a guarantee, but they were routinely raided when money was short, as it always was (pp. 150-52).

HISTORY AND SOCIAL SCIENCE

I mention the last argument not only for its intrinsic interest, but also because it shows that a marriage between social science and history can be fertile rather than sterile, as both discussants seem to think. Wilds asserts that I lavish “a great deal of science on a point of common sense,” and Doyle, as noted, that I produce “long-winded expressions of the obvious.” Now, one cannot persuade someone that an argument is interesting by thumping the table and assert that it *is* interesting. In the rest of this response, therefore, I shall not address myself to the discussants but to other readers who might be curious to reflect on the possibility of a fertile union.

In my view, history (including contemporary history) and psychology (including behavioral economics) are the pillars of social science that provide the foundations for anthropology, economics, political science, and sociology (p. x). I shall sketch three instances of cross-fertilization of history and social sciences (psychology and sociology) to illustrate this claim.

- *Agency bias*. When a surprising, important, and harmful event occurs, the human mind has a tendency to explain it by human agency rather than seeing it as the product of blind causality, “sound and fury, signifying nothing.” As noted, conspiracy theories owe a great deal to this “search for meaning.” To my knowledge, psychologists have not paid much attention to this mechanism. Historians of subsistence riots in the ancien régime knew, however, that peasants tended to attribute high prices of grain or bread to hoarding by merchants or speculators rather than to the vagaries of the weather (pp. 67 and 222). Moreover, officials sometimes interpreted the simultaneous break-out of conspiracy-based riots in many different provinces as itself the work of a huge conspiracy (p. 222). In my ongoing work on America before 1787 I have also found rumors of a conspiracy to spread and foment the rumor of a conspiracy. I believe psychologists could benefit from the work of historians to improve the understanding of the causes and consequences of agency bias.

- *Reactance*. Conversely, historians could benefit from the work of psychologists to improve the understanding of the frequently erratic behavior of the French kings. Their *reactance* – a tendency to disregard advice (even good advice) that would encroach on their sense of autonomy – was so prominent that others could manipulate it to their own benefit (p. 148-9). Drawing on psychological studies of reactance, one might look for evidence that the kings also delayed decisions when they felt *crowded*, as when drivers about to leave a parking lot tend to do so more slowly if they can see that other drivers are circling to take their spot. In a biography of François Mitterand, we read that “In the 1950s, as Interior Minister, he had once received the Algerian nationalist leader, Ferhat Abbas. After Abbas had waited in an

anteroom for an hour and half, an aide went in to find the cause of the delay. Mitterrand was reading the cartoons in *France Soir*.”[7] Perhaps he was just carrying on a tradition.

- *Préséance*. Sociologists have studied obstacles to class consciousness, understood as the capacity for collective action. These include spatial isolation of class members (Marx) and a high turnover rate (Tocqueville). The study of the ancien régime shows that to these impediments one can add the obsessional desire for rank or *préséance* within classes (p. 43). Moreover, it can teach us how this obsession also defined antagonistic relations within and between *institutions*, such as the *parlements*, guilds, provinces, or collegiate churches (pp. 44-45). Saint-Simon asserts that the royal government deliberately encouraged conflicts of *préséance* within the *parlement* of Paris, and Julian Swann explains how resistance to its claim of *préséance* among all the high courts prevented their union (cited on p. 44). These cases could enrich the database of the scholarship on institutional amour-propre.[8]

More generally, I conceive of progress in the social sciences as the addition of *new mechanisms to the toolbox of explanations of human behavior and human institutions*. Although this is the explicit aim of many social scientists, it is rarely acknowledged as such by historians. I believe, however that they contribute to this collective project in two ways. First, good historians are (by definition, in my book) implicit theorists. Their eyes are drawn to cases that suggest new generalizations or provide compelling counterexamples to existing ones. Second, some historians state the mechanisms on which they rely, such as the “Tocqueville effect” or Paul Veyne’s “micro-political illusion.” In fact, as I write in the book, “Tocqueville and Veyne are the founders of the union [of history and social science] that I am trying to practice.”

NOTES

[1] Doyle overstates the case for the prosecution when he writes that my “approach leaves the impression that substantially little changed over time.” On pages 4-5 of my book, I enumerate ten dimensions of change. I ought, however, to have tried to integrate them better, a task I may undertake in volume 3. In what follows, all otherwise remarked page references are to *France Before 1789*.

[2] I take it, however, as a tacit acknowledgement that my renderings are *correct*. That’s something.

[3] My attribution to Necker of vanity and the desire for popularity is indeed summary, as any summary tends to be, but is based on the testimonies of half a dozen contemporaries, including Necker himself, and on the appreciation by several historians.

[4] See Jonathan H. Turner and Jan E. Stets, eds., *Handbook of the Sociology of Emotions* (New York: Springer, 2008).

[5] Jon Elster, *Explaining Social Behavior: More Nuts and Bolts for the Social Sciences* (Cambridge: Cambridge University Press, 2015), pp. 475-79. I say “putative”, because hard confirmatory evidence is lacking.

[6] Raymond Aron, *Thinking Politically: Liberalism in the Age of Ideology* (London: Routledge, 2020), p. 33. Bourdieu sociologizes “everything,” that is, except himself.

[7] Philip Short, *Mitterand: A Study in Ambiguity* (London: The Bodley Head, 2013), p. 273.

[8] See, for instance, Laura E. Little, "Envy and Jealousy: A Study of Separation of Powers and Judicial Review," *Hastings Law Review* 52 (2000): 47-122.

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