Critique of Law and Libecap Paper for Corruption and Reform Conference

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July 15, 2004

Professors Marc Law and Gary Libecap tackle a crucial set of events in the history of economic regulation in the United States. The Pure Food and Drugs Act of 1906 created a set of lasting regulatory authorities and enabled an administrative organization that has evolved into contemporary U.S. Food and Drug Administration (FDA). That agency now directly regulates commodities that aggregate from one-fifth to one-quarter of U.S. GDP. It also possesses some of the most striking gatekeeping powers in the U.S. government: the power to veto product entry, which is also the implicit power to regulate years of R&D and investment, to induce patterns of cooperative activity by firms without ever having to bring down an observable hammer upon them. Clearly the substantive focus of this paper is important, and generally neglected by political and economic historians.

There are some things to like in this paper, and there are some things much less satisfactory. I take it as my charge to focus attention on the latter, not the former. But let me list a few of the appealing properties of the argument first.

On the credit side of the ledger, the authors have conducted some original research and have produced a nuanced argument about the origins of pure food and drug regulation at the national level. Law and Libecap (hereafter LL) analyze both narrative and quantitative evidence and come to the very reasonable conclusion that a combination of public interest and capture accounts best explains the 1906 Act. The evidence adduced from congressional testimony and debate is novel and I congratulate the authors for doing this. There is also an appealing element to the research design here. Law and Libecap observe switches in votes on highly similar food and drug regulation measures from 1903 to 1906 and exploit this variation in their statistical analyses (Table 4). Having tried unsuccessfully to do this myself with a series of votes on what became the 1938 Food, Drug and Cosmetic Act, I commend the authors for their effort.

On the debit side of the ledger, I have a number of concerns to express. I think the principal weakness of the paper is its poor empirical substantiation of theoretically relevant claims. Too often in the empirical discussion, evidence is interpreted as for or against a theoretical alternative without sufficient consideration for alternative interpretations or readings of the data. In other cases the events described and explained are heavily overdetermined (the cheapest examples in the paper are those events where nothing happens, viz., that a particular proposal does not become law and we are left with the status quo). Let me offer here two examples of claims that I believe are both poorly supported by the evidence adduced in the paper, and amenable to powerful alternative explanations as well.

> **a. The Paddock Bill**: On pages 17 and 18, LL argue that the failure of the Paddock bill "reveals the efforts of various competing industry groups to mold regulation to their private benefit." They offer a narrative of the Paddock Bill under which it passes the Senate but not the House in 1892, despite what appeared to be

widespread support for the bill (at least as claimed by Paddock). They then conclude, by virtue of the House's failure to pass the bill, that "organized producer interests were able to prevent the Paddock bill from becoming law" (18).

On the face of matters alone, this account leaves much to be desired. Why would we expect the House to have passed the bill immediately when the Senate itself had failed to consider the bill twice over before passing a weakened version? Might not the weakened version of the bill softened the energies of the very interests who stood to benefit from the bill, thus easing rejection by the House?

More important, it strikes me that factors separable from interest groups politics were likely responsible for the evolution of bills and votes here. Consider that the House of Representatives in the late 1880s and early 1890s was, until the 1894 "midterms," closely divided between the Republican and Democratic Parties. The majority party went from Democrats in the 50th House (1887-1888), to Republicans in the 51st House (1889-1890), back to Democrats in the 52nd and 53rd Houses (1891-1894), and then decisively to the Republicans in the 54th House (1895-1896). It is also worth reflecting the both Democrats and Republicans became appreciably more conservative in their voting behavior during this short period.[1] This period saw the waxing of power of the gold Democrats, and witnessed the increasing cooperation of large business interests with the conservative ("Stalwart") wing of the Republican Party. The Republican presidents of this period (Harrison and McKinley) were much more tethered to large corporations than either their predecessors (Garfield, Arthur) or their successors (TR). As evidence for this claim, consider the drift in party specific medians for DW-NOMINATE scores observed in the House from the 49th to the 53rd House. The DW-NOMINATE first dimension scores for the Republican median rise from 0.440 to 0.486 (positive trends indicate a conservative movement). For the Democrats, they also rise appreciably from the 51st to the 52nd House (-0.444 to -0.362). (You can check my facts at Professor Keith Poole's VOTEVIEW web page, or at http://voteview.uh.edu/pmediant.htm.)

In the Senate, too, there was considerable change during this period. The 51st Senate saw the Republicans with a healthy 51-37 majority. The 1890 midterms reduced this to 46-41, and by the 53rd Senate the Democrats were in control for only the second time since the War. One also notices appreciable rightward movement in the Republican party's median Senate voter in this period (0.339 to 0.381 on the first-dimension DW-NOMINATE party median) and an even more noticeable shift among Senate Democrats (-0.581 to -0.470).

It seems that a better and more generalizable explanation for the failure of regulatory legislation in the 52nd House, then, lies in larger partisan and ideological trajectories buffeting national politics. These gave rise to two forces that help to explain the dearth of legislative activity across numerous domestic policy fronts in this period: (1) a conservative shift among dominant factions of both of the major parties on

[1] For the Democrats, these trends were halted with the rise to dominance of the populist-Bryan wing in the 1896 campaign. But this only shows that conservative Democratic voters and politicians left the party and became either inactive, or became progressives and progressive Republicans.

economic issues, and (2) increasing electoral uncertainty and policymaking as election-to-election shifts in seats became more variable and as partisan control of both chambers was up for grabs. (This second factor would, I think, induce risk-aversion by party elites, given high electoral turnout.) Given that the 52nd Congress came into a tightly contested general election year, it is not altogether surprising that reform legislation was dropped.

Whatever LL's positive case (or the case for or against the alternative possibility I have proposed here), it is striking that in an analysis of congressional voting neither partisanship nor ideology play any role. With these factors absent, the interpretive claims need at the very least to be attenuated.

b. The 1903 Vote. Upon conducting a regression in which Senate voting on reconsideration of a pure food and drug bill in 1903 is the dependent variable, LL claim that "the fact that the coefficients for our proxies for patent medicine interests and blended whiskey interests are both negative and highly significant suggests that opposition by these two groups played an important role in blocking regulatory reform….Overall, we interpret these regression results as implying that producer rather than reform interests were the critical constituencies shaping Senate voting in 1903" (page 23).

This claim, too, is quite problematic. To claim from (A) negative coefficient estimates on constructed proxies for liquor interests that (B) these interests played an important role in blocking reform and that producer interests were the "critical constituencies" in the process is an unwarranted leap. (I deal with the construction of the liquor proxies below, and so delay discussion of issues of measurement until then.) For now, it strikes me that the authors' data leave us a fair distance short of their conclusion.

Consider first that, at least as measured by the McFadden R-squared statistic, and from the insignificance of most coefficients, that the explained variation in this regression is quite low. The one rather large coefficient estimate in both models is that for the blended whiskey interests proxy, but this variable is zero for all but a maximum of six senators. Even with a large coefficient value, then, this variable can be doing little explanatory work. Beyond this, the other explanatory variables in the model are either marginally significant or, more importantly, strike me as not likely robust to the addition of partisan and ideological controls.

This brings up a second point: from the standpoint of spatial voting theory (see Poole and Rosenthal 1997), this and the other voting regressions of the Law and Libecap paper seem rather poorly specified. Even the inclusion of partisanship, or the inclusion of simple ideology estimates would add much to the regression and would tell us just how phenomenal (or epiphenomenal) the reported results are. Again, partisanship and ideology measures represent more generalizable mechanisms (James 2000, Poole and Rosenthal 1997) with which one can explain an array of legislative outcomes. (If LL believe that these factors are somehow endogenous to their story, then such an argumentative tact would require different research. In other words, it behooves them to show this, and to make a direct argument about the composition of parties and ideological coalitions in Congress.)

A third point is necessary and I state it as a question: Suppose that the counterfactual deletion of blended whiskey interests from Illinois, Indiana and Ohio would have changed six votes in 1903. In two senses, would this have changed the outcome? First, would these six votes have been enough to convert a minority into a majority? We do not know in the most simple sense because Law and Libecap do note give us the vote aggregates, and because we cannot tell much about the predicted voting probabilities that would occur were the shift to occur. What we can tell from the first moment of the voting variables in Table 1 is that these variables probably made little difference to the outcome of the 1903 vote: if I have my calculations right, it was 28 for, 60 against. A full seventeen senatorial votes would have needed to shift to convert the outcome.

Second, would a bill as strong as the 1906 Pure Food and Drugs Act have passed in light of this shift? Again, for two reasons, I sincerely doubt it. Note initially that the 1906 Act passes 63-26. What exactly the partial derivatives retrieved from the Table 3 regressions can tell us about the aggregate difference made by the theortically relevant variables is, then, severely limited. Second, there was an evolution of content from 1903 to 1906 that needs to be explained (see Young 1990 and Carpenter 2001 for relevant narratives).

Fourth, it strikes me that the authors could take advantage of the structure of the Senate in this case. Each pair of Senators has an identical constituency. If we are truly observing constituency pressure, then we should observe a high (higher than average?) degree of within-state homogeneity in voting, controlling for split senatorial delegations. This is something that can be tested directly.

As it stands, however, the paper lacks these more nuanced and rigorous analyses of the data. Few of these concerns would require the costly collection of new data, and so I would urge such revisions (among others) upon Professors Law and Libecap

These are two points (there are others), where inferential claims advanced in Professors Law and Libecap's paper seem rather weak. In LL's defense they might argue that, with limited data, what we see in the paper is all they can do. But then it seems all the more compelling to voice the circumspection appropriate to historical and scientific inquiry. And as I have noted, some checks on the data seem cheap in both the data-collection cost sense and in the sense that the recommended auxiliary analyses are technically rather simple to execute.

<u>Some other concerns</u>:

1. *The Model of Policymaking*. The paper seems to rest on a model of policymaking that is rather implicit. The implicit model seems to be that, once a constituency is in place, a law will be enacted. When we observe the failure of pure food and drug laws to pass nationally, the reason must be that no broad-based constituency was behind the bill (see, e.g., claims on pp. 16-17, 21, 23). An observed lack of passage means that the requisite constituency

(broad-based, composed of "ordinary citizens," or other) was not in place. In fact quite different processes may be at work.

Consider the possibility that a broad-based constituency could be in place, but that a reform law benefiting the constituency would not be enacted anyway. This can happen easily even when the median voter in a legislature (or electorate) truly does prefer a change to the status quo, by virtue of supermajoritarian requirements (filibuster and cloture procedures are the best example in current politics). It might also happen, however, when other matters bump food and drug regulation from the congressional agenda. Or suppose that committee gatekeeping power (a la Krehbiel) results in no reform bills reaching the floor, or only watered down or extreme instantiations of the relevant reform reaching the floor. The composition and evolution of the congressional agenda needs to be considered here.

2. *Differentiation of muckraking versus other journalism.* I think the authors should take a bit more care to distinguish between "ordinary" (or non-muckraking) journalism and muckraking. To call Sinclair and Adams "muckrakers" seems problematic, in two quite different senses. Sinclair, as the authors' note, was writing his book less as an expose of food safety and more as a critique of labor conditions in the packing and canning plants. In this respect, Sinclair's book joins a longer tradition of labor radicalism and critique that predates "muckraking" as it is normally conceived. So too, I think few historians of journalism would regard *Collier's* as one of the principal outlets for muckraking journalism.

3. *Did Muckraking Organize?* The claim that muckraking journalism broke the stalemate by galvanizing previous unorganized consumer groups seems historically problematic. The journalism of 1904-1906 may have given these arguments or votes additional weight, but my own work and that of James Harvey Young makes it pretty clear that the relevant organizations were already in place by 1904 (cf. the National Consumers' Union, the GFWC and the AMA). More generally, I think it is difficult to argue that the organization of "ordinary citizens" was much affected by muckraking. This is a period in which ordinary citizens (if this is at all a useful concept) were organized less into groups, and more into parties (although this was beginning to change, as Skocpol and Clemens have argued). It is quite possible that eligible voters may have been swayed by the crises of 1905 and 1906, but this could happen without political organization taking place (consider, for instance, citizen votes against tax increases (or against politicians who have increased taxes or spending), which can often take place even when those voting are not politically organized on an issue-specific basis).

4. *How Credence-Good Economies Might Lead to Producer-Sought Regulation without Rent-Seeking.* The authors repeatedly claim that the involvement of producers in the regulatory struggle is supportive of the revisionist (Stiglerian) school of regulation (pp. 3, 17-19, 37-38). Credence good economies may give rise to producer-oriented claims for regulation that are not necessarily rent-seeking. Consider a two-producer economy of the sort represented by Akerlof that confronts a lemons problem. If the consumers' cost of verification rises and one firm is "good" (high type) whereas the other firm is not (low quality type), one can easily imagine a pooling equilibrium in the advertising market where both firms converge to claims of high product quality and where the bad firm drives out the good by virtue of production

cost superiority. But now suppose[2] that the good firm had some political voice of the sort that it could support regulatory measures that would be partially quality-revealing (e.g., the content or chemical labeling of medicines, foods and other consumer products). Such a regulation might improve consumer welfare by rendering consumers better informed at low cost. But would we then argue that the regulation was "producerist" or "Stiglerian" because the "better" firm sought regulation that would enhance its profits? This would be a questionable application of rent-seeking theory, at least if we are taking the concepts of "rent" and/or "capture" seriously and rigorously. In this simple example, one can imagine that labeling regulation sought by the high-quality firm does not give the high-quality firm any rent whatsoever. There is, to be specific, no source of supply controlled by the firm; any economic advantage from the regulation is *induced* through consumer choices, net of equilibrium price. Yet the high-quality firm would find it worthwhile to expend costly activity in favor of the regulation.

5. *Construction of the Liquor Variables*. The construction of the liquor interest proxies does contain some drawbacks that should at least be admitted openly or defended. First, the idea that there is genuine continuous variation here (or that it is continuous variation that matters more than the mere presence or absence of industry) seems problematic. I wonder whether a dummy variable for the presence of blended or straight whiskey would do as well as the authors' current measures. The second is the claim that "blended whiskey and straight whiskey" were produced in different states. Is this *absolutely* true? If not, then the measurement of these proxies commits LL to the assumption that *no* straight whiskey was produced in Illinois, Indiana or Ohio, and that *no* blended whiskey was produced in Kentucky, Maryland, Virginia and Pennsylvania.

<u>Nit-Picking Points</u>:

(p. 21) what is meant by bureaucratic interest groups? Labor associations? Actual associations of government officials? Or just the agencies themselves?

(p. 25) It's worth pointing to the widespread readership of Sinclair's book (over one million copies sold in its first year of printing).

(p. 28) Adams had considerable help from Wiley, so his achievement (positive or no) should be considered as much bureaucratic strategy as muckraking.

(p. 30) On "…Wiley was not a disinterested observer." I argue in my own book that although this is certainly true, Wiley was *seen* as a disinterested observer in the press and by many politicians and political elites. The aura of neutrality can be a powerful political weapon.

(p. 34) The term "industry" seems to be used a bit monolithically here.

[2] This entire theoretical discussion is intuitive and could benefit from formalization.

Some Concluding Thoughts:

While we're at it, I think the authors ought to take a small amount of space and reflect on the generally weak state of affairs in analyses of the political economy of regulation. Neither the "public interest" theory of regulation (whatever that is) nor the Stiglerian/capture theory has performed all that well across a wide array of empirical settings. Public interest theory, for one, has no theoretical account of *politics* to offer. Capture theory seems largely incapable of explaining much of the social regulation (and much of the economic) regulation over the past half century. This would include labor-safety regulation (see studies by John Scholz), environmental regulation (see studies by Sanford Gordon and others), nuclear energy regulation (Balogh), pharmaceutical and medical device regulation (see studies by Mary Olson and myself), and others. Careful studies such as Lawrence Rothenberg's have shown that capture and rent-seeking accounts perform poorly even in empirical arenas – here trucking regulation at the now-defunct Interstate Commerce Commission – where one might have predicted success. Or, in cases where rent-seeking accounts of the creation of regulatory agencies used to have currency, more careful analyses have since come along – I think of Scott James' now definitive studies of the creation of the ICC and the Federal Trade Commission – that have, for all intents and purposes, left rent-seeking accounts in shreds.

In short, the paper would be better represented (and concluded) as a critique of existing studies of regulation. If the authors are right about this discussion, then either both public interest and rent-seeking theories are in need of significant revision, or some third/fourth alternative needs to be considered.

All in all, then, I think a more nuanced, circumspect version of the paper would make for a fine addition to the volume.