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Current Developments in the Economics of Regulation

Sam Peltzman

The economics of regulation seems to have arrived at a crossroads. Activity in the field is burgeoning and threatening to burst the boundaries established by theoretical insights that just yesterday seemed adequate to guide research for some time. My task here will be to step back and try to set some of the new work in a longer perspective, a perspective which I think indicates that we ought to be cautious about what to expect from it.

I find it useful, like so many who are charged with summarizing the work of academic conferences, to proceed taxonomically, So, I will organize my remarks around three principles that I believe are suggested by the history of the economics of regulation. The first of these is that the theory seems to move in waves, in the sense that once-respected theoretical insights seem easily superseded by new ones. This partly reflects the muddled state of the field, where normative and positive issues tend to get indiscriminately run together and a common theoretical bond is lacking. The first identifiable wave, which held center stage up to about 1960, is sometimes called the "public-interest" view. It can be found in one form or another in the seminal work of Hotelling (1938) and in generations of text books on public utilities. The focus is on market failure, typically of the natural monopoly variety, as the stimulus for regulation. While the public-interest view of regulation as guardian against monopolistic inefficiency always viewed itself as more proscriptive than descriptive, it did claim some practical insights. Surely a finding that, for example, electric rates were not held down by regulation would represent a loss of innocence.

When the work initiated by Stigler and Friedland (1962) began revealing that regulation did not work as the public-interest view held that it should or did, the way was prepared for a new generalization: the cartel or "capture" view of regulation, whereby compact interest groups, usually of producers rather than consumers, were held to dominate regulatory decisionmaking. The public-interest view became more clearly a normative paradigm. Its underlying welfare economics remained a valid way of organizing discussion about what regulators ought to be doing, but any belief that regulators often did what they should was now severely tempered. For about a decade the capture model provided a major framework for the positive analysis of regulatory behavior. This work is not yet complete, as evidenced by Leone and Jackson's intriguing attempt to bring out the rent-generating elements for producers in pollution-control regulation (a form of regulation that appears, on its surface, to cut against the producer's interest). Nevertheless, the simple, straightforward view of regulation as a cartel enforcement device now is coming under serious question. Perhaps this is due partly to the lavish growth of forms of regulation that, like pollution control, seems to require much excavation before any cartelizing element emerges. More likely, it is due to a growing recognition that very substantial rent-dissipating elements (such as the perpetuation of excess capacity in railroads, which Levin's article documents so well) were an integral part, and not merely a sideshow, of regulatory activity in areas where the producer-protection model seemed most fruitful (for example, transportation).

While it may be premature to call it a new wave, this conference seems to confirm what is at least a new ripple. For want of a better term, I will call it "creeping realism" ("creeping" both because it is not a radical break with either the public-interest or producer-interest models and because, as I will argue, its analytical structure is so unevenly balanced that walking straight will be difficult). What it seems to me the creeping-realism literature is trying to do is to integrate some of the newer economics of political decisionmaking with some of the elements of the older normative economics of regulation. The older welfare economics is invoked to rationalize one or another form of regulatory intervention, but the resulting real-world institution then must bend to the realities of politics. And politics means coalition-building and log-rolling, so that nothing so simple as a "public" interest or a "producer" interest is going to predominate.

It is best to let a more precise characterization of this literature emerge from a few examples provided by this conference. The article of Leone and Jackson is a good starting point. Somewhere in freshman or sophomore economics the normative rationale for pollution control is typically spelled out, and then later our charges are told that the way regulators actually do things leaves much to be desired. Leone and Jackson see more here than a confused attempt to control pollution. If this were the whole story, we should see congressmen from districts with large paper mills (an industry with large pollution-control costs) opposing increased restrictions. They do not. Why not? Possibly they are implicitly bought off by the design of the regulation, the very design that seems laden with inefficiency to the conventional analyst. Leone and Jackson then find empirical support for this possibility. In effect, the regulators levy a higher implicit tax on new entrants than on established mills. With just a little growth in demand, the net effect of the regulation is to generate nontrivial rents for established mills. Thus, a potentially antagonistic and powerful interest is brought within the coalition served by the regulators. Of course, potential entrants are hurt, but you will not find the Association of Potential New Entrants listed in the Washington telephone directory.

Sometimes creeping realism means a partial revival of a public-interest, or at least a non-producer-interest, view. For example, minimum capital requirements have long been recognized as a potential restriction of entry. Munch and Smallwood and their discussants focus on the possible benefits for consumers of insurance, who might otherwise be left stranded in a bankruptcy. In effect, the minimum capital requirement substitutes for the high costs facing the untutored consumer in evaluating the financial capacity of the insurer.

The last example I want to cite is the article of Willig and Bailey. Their wholly normative paper breaks with the venerable tradition of separating allocative from distributive issues. They want to guide a regulator who wishes to set prices that take simultaneous account of efficiency and income distribution norms. What about the old story that redistribution is better done by explicit money transfers than by the Public Service Commission? Willig and Bailey are silent on this. Though I am persuaded by Gary Becker's (1976) argument that the old story is wrong, I suspect that Willig and Bailey's silence is deliberate. For all our past hand-wringing about how messy it is, regulators persist in allowing distributive considerations to intrude on, and even dominate, their pricemaking. Even the pure theorist, Willig and Bailey seem to be saying, had better adjust to this reality.

The approach of Willig and Bailey illustrates my second principle of the economics of regulation, which is that practice leads theory. The full-blown development of the welfare economics of natural monopoly lagged the growth of electricity and telephone regulation. The capture models lagged the most notable empirical counterparts—ICC regulation of trucking, organization of the Civil Aeronautics Board, oil import quotas, and so on. The creeping-realism literature seems to be lagging two events, one practical and the other intellectual. The practical antecedent seems to be the growth of the whole panoply of safety, environmental, and consumer-information regulation, whose very existence or whose practice has often proved difficult to rationalize theoretically. The intellectual precursor seems to be the perceived difficulties with earlier models about which one or another theory did seem to have a lot to say. For example, if as the capture model holds the ICC is in business to organize a trucking cartel and enforce rate-making collusion among the railroads, why does it harm both captors by perpetuating excess railroad capacity?

Of course, intellectual and real-world developments are related here. The newer forms of regulation, such as consumer protection and pollution control, began their great growth at about the same time the capture model was making its biggest splash in academia. However, the Second Principle was at work, and most students of regulation did not rush to apply the capture model to the new regulations. Instead, they seemed willing, for a time, to try to understand the new regulations within the framework of the older public-interest model. When it became clear that the newer agencies were no more dedicated to Pareto optimality than their ancestors, the first reaction in the literature (still the dominant one) was to tell the new regulators to mend their ways. Kneese's 1971 work on the potential gains from effluent fees comes immediately to mind. The next reaction was more analytical. Much of what I have called creeping realism seems to be focusing on the new regulation. It seems to say that if the public-interest model doesn't explain how the new regulation works and if the capture model has been found wanting in explaining important aspects of the behavior of those agencies to which it seems most applicable, we now have all the more reason to develop a new synthesis.

However, we ought to embrace any new model of regulation with some caution. Implicit in much of what I have so far said is, perhaps, another principle of the economics of regulation: that the currently fashionable theory is usually wrong, or at least misleading. This conference provided more examples of this Third Principle than I have already touched upon. For example, none of the literature spawned by Stigler and Friedland argued very strenuously against the notion that natural-monopoly problems were an initial impetus to regulation of utilities. The story seemed to be that once the clamor for regulation had been heeded, the industry was able to assert itself in the mundane activity of the new agencies. However, Jarrell (1978) argued that the facts are more compatible with the view that industry interest was present at the creation of these agencies as well. The article of Fuss and Waverman, though working from a much different perspective, also ends up questioning the natural-monopoly rationale for telecommunications regulation. The elaboration of the producer interest in pollution regulation by Leone and Jackson serves to update this story.

As I have pointed out, the producer-protection model has met its own share of skepticism, both at this conference and elsewhere. Levin's article adds new insights into work begun by Keeler (1974), Friedlaender (1971), and others. But, these have a common theme. The ICC's attempt to preserve excess capacity in railroading is an integral part of its history, and not some aberration of a cartel manager manque. It constitutes a huge tax on the wealth of railroad owners for the benefit of a few shippers. Moreover, Keeler pointed out that U.S. railroads are not alone in facing such a tax. Similarly, Munch and Smallwood's discussion of the potential benefits to at least some consumers of capital requirements was echoed at the conference in discussions of similar benefits from more explicit entry control, like licensing. Occupational licensure may have been the first form of regulation to which economists applied the producer-protection model. Adam Smith deserves paternity here, as in much else. His descendants, like Friedman and Kuznets and Kessel, gave the model considerable empirical content. However, the realist challenge intrudes here too. The potential empirical importance of a consumer interest in shaping licensing regulation was debated at the conference and is the subject of ongoing work by Holen, Leffler, and Gaston and Carroll. It is, however, too early to tell whether this work will end up confirming my Third Principle, which is that the predominant licensing-as-cartel model is wrong.

However, if that principle has any validity, I believe that its lesson may be especially important for the new realist literature, as well as for some of the more institutionally innocent work that seeks new applications for market-failure models. The realist literature seems to be organizing around the following pattern: Select an area where producer protection has seemed important (for example, licensing, minimum price, or entry regulation); then show that there is a potential market failure that makes it credible for a coalition of producers and consumers, not merely the producers alone, to seek regulation. Or, reverse the pattern: Select an area, such as pollution, where market failure had seemed the most compelling force for regulation; then show how regulation of the market failure can be structured to serve a producer interest at the same time, and thereby enhance the political survival value of the regulatory institutions. This is an interesting research strategy that, I believe, deserves encouragement. Indeed, I have argued elsewhere (1976) that diversification of the interest groups served by a regulatory agency, rather than specialization, ought to be the common pattern.

There appears to be a dangerous asymmetry in the way this research is carried out: One part of the model (the cartel element) gets serious analytical treatment, while the other (market failure) is hardly developed beyond the point of vague possibility. Needless to say, the danger of this asymmetry increases with the emphasis placed on market failure as the source of regulation.

I can best elaborate on this sweeping claim by making another: Conjuring up rationales for regulation is too easy a sport; perhaps it deserves a Pigovian tax. On a few minutes' notice, any competent economist could apply an externalities model to fashions in clothing, a natural-monopoly model to stereo equipment (after all, don't they advertise amplifiers powerful enough to drive all the speakers on your block?), or a producer-protection model to laws against heroin. The only reason for taking some applications more seriously than others is our sense of their empirical importance. This, I would argue, is precisely what led us 10 or 15 years ago to take producer protection seriously as an important element in regulatory behavior. From the beginning the focus of that literature was on the importance of the measurable features of that phenomenon, not simply on the legal possibilities embedded in, say, the Civil Aeronautics Act or the Interstate Commerce Acts; so the literature developed by estimating the number of firms excluded from one or another market (for example, Jordan's work on airlines), the size of rents to existing firms (Friedman and Kuznets [1954] on the AMA), the gap between competitive and regulated prices (Keeler [1972] on the CAB). The range of sophistication in this literature is, to be sure, very wide, but most of it seemed to point in the same direction of pinning down the empirical magnitudes. And the increased technical virtuosity seems to have resulted in better or more credible estimates of the size of the regulatory effects. Perhaps the air-transportation literature, starting from Keyes's work in the early 1950s, best illustrates these points. By now any new entrant to any part of the capture literature is conditioned to worry not only about the potential or directional effect of regulation, but about its size. Note, for example, the procedure of Leone and Jackson. After stating the rather novel possibility that the EPA is creating positive rather than negative producer rents, they immediately give a crude estimate of magnitudes (capital gains of $200 million versus losses of about 1 percent of this). Had the numbers been reversed, I doubt that the theoretical possibility would be given much attention.

This degree of concern for empirical relevance does not, however, seem to carry over to our treatment of market-failure issues. To be sure, some of the relevant issues have received analytically sophisticated treatment attentive to empirical importance. A case in point is the treatment of scale economies in two of the articles presented here. Levin's on railroad costs makes precise the nature of economies of scale in this industry, and then proceeds to measure the magnitude of the costs of excess capacity resulting from restrictions on exit when there are traffic-density economies. The compelling result is not the theoretical possibility of suppressed density economies, but their very large magnitude. Similarly, the main motive to Fuss and Waverman's work seems to be concern with the extent of scale economies in telecommunications; that is, with how the economies can be measured in the multiproduct-firm, and how big the economies are. Again, the interesting result is the sense of empirical proportions conveyed by the work—the suggestion that scale economies may not be so extensive as heretofore believed. Both these articles add to previous work. Examples that come to mind are Keeler's (1972) and Friedlaender's (1971) work on railroad costs, in the case of Levin, and Christensen and Greene's (1976) work on electric utilities, in the case of Fuss and Waverman. However, even in the economies-of-scale literature no one will accuse us of great haste in this search for empirical relevance. Public utility commissions were being created for almost a century before this empirical literature became established, textbooks were written about their operation, and Hotelling made it evident in the title that his classic article on optimal pricing applied to railroad and utility rates. This whole enterprise was based on a presumed but undemonstrated belief in the importance of scale economies in particular activities.

Much of the current discussion of newly fashionable market failures seems to be at a similar stage of development. To be sure, vague beliefs are now enshrined in jargon and clothed in formal models which give them the correct ritual flavor and exclude the uninitiated. Perhaps a card-carrying member of the profession should not oppose this too loudly or even be entirely cynical about it. There is, after all, a gain in precision in talking about failure of information and insurance markets instead of about the cheated consumer. My worry is that the professional discussion here does not seem to be leading toward the next question: How important are the problems being discussed? Here, I would refer the reader back to Noll and Joskow's summary of the literature on health, safety, and performance standards. They review a welter of possible problems with unregulated markets that have cropped up in this literature, and a modest number of attempts to evaluate the effects of regulation. But they are as struck as I am by the lack of work on the empirical importance of the theoretical problems. At this stage in the development of economics, one should have hoped that the empirical question would be given priority. It does, after all, matter for how seriously we want to pursue the theoretical enterprise whether 1 percent or 50 percent of the sales of some product generated negative consumer surplus, just as it ought to have mattered to the early public-utility economists whether the output elasticity of total costs was 0.95 or 0.25. Nor do I mean to imply by this lapse into jargon that the theoretical enterprise has to be held captive to a sophisticated technology of data production and analysis. Precise point estimates are not required for getting at gross magnitudes. But, at least in the safety-health literature, we have not begun to find out if the theoretical problems we explore are worth talking about.

The literature on environmental externalities also falls into the "newly fashionable" category, and the situation here is a little better than in safety and health. The work by Lave and Seskin (1970) on health effects of pollution and that of Ridker (1967) and Crocker (1971) on land values is a start at defining the scope of the problem. But, again, compare the attention to this basic part of the problem with that given to theoretical problems which implicitly assume that there is a substantial problem. I refer here to the literature cited by Noll and Joskow on such matters as optimal control mechanisms for pollution and the attendant general equilibrium consequences. Similarly, the bulk of applied research on pollution control tends to avoid the issue of how important pollution externalities are, choosing instead to focus on such matters as the costs of various abatement policies. If someone today asserted that any substantial reduction in pollution would have trivial benefits, or that the resources spent in the name of pollution control had trivial effects on pollution, there would be no substantial concrete basis for laughing him out of court. Given this state of affairs, we may be seriously compromising our knowledge about pollution regulation. For all we know, this regulation may be only the disguised form of entry control Leone and Jackson describe, or a WPA project for the suppliers of control equipment, or something else that would call for a fundamentally different analytical framework than we have so far brought to bear on pollution regulation.

My point here is not to propose radically alternative models, since that would implicitly decide the crucial empirical issues. Instead, I am suggesting that, before we push our normative models into the newer areas of regulation or try to marry the normative models with economic analyses of politics, we not rush past an essential question: Are the welfare problems we are invoking trivial or sizable? We may well conclude that there is more reason to spend analytical energy on the externalities of automobile pollution than, say, those of automobile colors, in much the same way that we were able to conclude that there were at least some interesting scale-economy problems in regulated utilities. However, the justification for taking this sort of risk is much weaker today than it was fifty or one hundred years ago, when the tools of empirical analysis were far less developed.

I do not want to minimize the difficulties inherent in assessing the magnitude of the problems on which we focus our analysis. One need only read the discussion surrounding the Fuss-Waverman paper to see that there are still many difficulties to the measuring of scale economics. The corresponding measurement problems in the newer areas of regulation are going to be even more formidable, because we will not typically have something like balance sheets and income-expense accounts to start from. However, the biggest challenge, I suspect, will be to our imagination and flexibility in using analytical tools that we already have. If this is right, then I am more optimistic than Noll and Joskow, at least about the potential for success.

To be more concrete, let me give a couple of examples of how we might frame questions that could get us closer to discovering the size of some of the problems I have mentioned.

• Are consumers behaving in a way consistent with the story that a big problem exists? For at least some goods there will be some objective measure of performance: accident frequencies of cars, failure propensities of insurers, injury frequencies of various occupations. Given these (or, more precisely, the relevant exogenous components), we can ask whether the good products or jobs sell at a premium to the bad and by how much relative to an independent estimate of the extra costs of the bad product. We will have an interesting problem if this premium is small, or if the good products do not drive out the bad.

• Does the political process act as if the problem is large or small? By now we are sufficiently wary not to take at face value the nominal intent of regulation. Nor can we easily interpret departures from this intent. Thus, suppose we found that, long after the establishment of a well-financed and amply empowered Consumer Protection Agency, as many consumers were being cheated or maimed as before. This could mean that the cheating and maiming was so small as to be practically irreducible. It could also mean that the title of the agency was hiding its objectives. If we found a large physical reduction in consumer fraud or injury, we would still have to evaluate its economic significance.

No study of the political process or the markets it regulates can escape such interpretive problems. Still, I believe we can gain something by looking for consistencies or regularities in political behavior, because the political process should not be expected to be indifferent to dead-weight losses in unregulated markets. These may be tolerated or even encouraged because they help "buy" another objective, but no rational model of political behavior would hold the dead-weight losses to be a good in and of themselves. Thus, if the dead-weight losses are large enough and inherent in unregulated markets, we ought to expect a consistent political response.

To uncover this consistency, I believe that the economics of regulation will have to give up some of its provincial focus on American institutions. The simple fact that many jurisdictions seem to persist in leaving large parts of transportation unregulated is telling (if crude) evidence that unregulated markets do not systematically generate large dead-weight losses. This is not to say that if regulation were ubiquitous the converse would be demonstrated. It could be that the forces making for regulation in the United States are so universally powerful that they can always overcome dead-weight losses of their own making. However, ubiquitous regulation would, I think, force us to take more seriously than otherwise the potential problems with unregulated transportation markets raised by advocates of regulation. My suggestion is that international comparisons of regulatory institutions can be a useful check or a crude screening device for selecting problems that may be worth pursuing. For example, we will take the possibility of market failure in electricity and telecommunications more seriously if every important country intervenes in these markets with seemingly appropriate institutions; we will be more skeptical if there is the same variety as in transportation. In view of the potential payoff to a modest analytical effort, there seems to me to be considerable scope for pursuing international institutional comparisons. If I am right, this strategy can also have obviously valuable spinoffs—for example, comparisons of the effects of apparently similar institutions.

These examples are more illustrations of a point than an agenda. The point is that deficiencies of data or analytical technique are not great enough to justify our neglect of the crucial empirical issues which have so far been ignored in analyses of the newer regulatory institutions. If this is so, perhaps we ought to impose that Pigovian tax on further proliferation of normative models or on their incipient marriage to the economics of politics until we begin redressing this neglect.

Such a reallocation of effort is not, of course, a substitute for a generally useful theory of regulatory behavior. It is entirely possible that when that theory is written it will so restructure our analysis of the regulatory process that the welfare problems that now tend to hold center stage will be pushed off to the side. We might, in hindsight, regret the time spent in worrying about their size. However, even though such a theory does not yet exist, research on specific forms of regulation has to look for some theoretical grounding. This is now being done by extending traditional normative models to the newer forms of regulation and by implicitly inserting specific allocative outcomes into the relevant objective function of regulators. Meanwhile, a good deal of the theoretical work on regulation has a similar motivation.

The promise of these analyses, either in enhancing our current understanding of regulation or in leading to a richer theory, rests on the importance of the problems around which they are organized. This is why I believe it is especially timely to divert some analytical energy to discovering the importance of these problems.

The increasing scope and spread of regulation and its impact on academic research make it almost obligatory for me to discuss policy issues. While my primary purpose in criticizing some of the focus of current research is to point out the unmet intellectual challenge, there are also related policy issues. At least some of the recent research seems to want to breach the wall between allocative and distributive issues that has stood so long in academic discussions of regulatory issues. The reasons for this are debatable, but, whatever their source, the infirmities of "make price equal marginal cost and send a check to the losers" advice are heeded at several points in these articles. Bailey and Willig discard this paradigm at the formal level, since their model starts with a marriage of allocative and distributive objectives as a given for the regulator. If one thing is clear from the discussion of excess capacity in railroads, it is the practical failure of our traditional advice. Finally, one of the motives to the marriage of the economics of regulation and the economics of politics is recognition of the practical link between economics and politics in regulatory policy.

The inference I choose to draw from the practical failure and the perhaps impending intellectual disintegration of the traditional policy advice of regulatory economists is that policy advice is not our strong point. An earlier generation of economists, with fewer policy problems tempting them, might have told us to stick to organizing the facts of the world intelligibly and systematically. If we follow this advice well, our impact on policy may increase. Since the impact is so small now and since "Policy Implications" is likely to remain the traditional conclusion to papers on regulation, this is a fairly safe prediction. Here again we are forced to proceed on less firm theoretical ground than we would like. This means that one day we may find out that the very categories in which we communicate with policymakers have little relevance for them. However, for now, any economist who wants to do "policy-relevant" work is forced to run that risk. If costs and benefits of the type we usually focus on are relevant for policy, the policymakers will inevitably have to deal with their magnitudes. Here I believe, economists have started to develop a methodology that can substantially reduce these policy-information costs.

In may cases the policymaker will, at least crudely, "know the score" without our help. Bankruptcies of short-haul, low-density railroads will, for example, get part of Levin's message across. However, consider the position of a politician whose constituency is not directly affected by the problems of short-haul, low-density railroads. He or she may be reluctant to vote for subsidizing these railroads, but fearful that without a "yea" vote a massive disaster will befall a political ally from another district. For such a swing voter, the sort of information provided by Levin's work, which pins down the consequences of the existing policy, can be far more valuable than *a priori* arguments about the desirability of free entry and exit. The other side of this is that policymakers looking for the "facts" will have to rely heavily on economists in these matters. The sport we have developed of debunking purely technical attempts at getting the "facts" is symptomatic. No competitor has succeeded in challenging our ability to organize data around a consistent theoretical superstructure in matters relevant for social policy.

Perhaps a better example of the policy payoff of our giving empirical content to theoretical issues is the current state of airline regulation. Congressional acceptance of deregulation and increased implementation of the basic principles of competition by the CAB provide a rare example of political endorsement of the professional consensus. But the professional consensus, backed by *a priori* arguments about lack of scale economies in the business and perhaps a few casual observations about experience in deregulated markets, was achieved well before policy began changing. What I want to suggest is that it took the weight of a fairly extensive empirical literature, able to generalize from the experience of deregulated markets and make precise the range of effects to be expected from deregulation, before policy changed—or at least before politicians felt able to use the work of economists to press for a change in policy.

Let me quickly recognize some of the risks of generalizing from this correlation between the flowering of an empirical literature and a shift in policy:

- The number of "swing" legislators susceptible to academic evidence is probably unusually large in the particular case of airlines, since the industry interest is not great in many congressional districts.
- The literature here is unusually well developed. One thinks immediately of the work of Keyes (1952), Caves (1962), Jordan (1970), Eads (1975), Keeler (1972), Douglas and Miller (1974), and the CAB's own economics staff, and fears that the list is incomplete. The quantity is matched by quality, and this combination may well be unmatched in the literature on the economic effects of regulation.
- There is probably by now a professional consensus, backed by a growing empirical literature, that regulation of exit from railroading is very costly. However, there has been little change in policy.
- Policy has changed in the same direction as a professional consensus, which had no strong empirical base. I am thinking here of the deregulation of stock-brokerage commissions. My casual judgment is that most economists would have "voted" with the Securities and Exchange Commission on this in the belief that the industry was structurally competitive. However, even if this is so, the direct role of economists in the process was peripheral.

In view of all these cross-currents, I am left less with any strong conclusions than with a tentative hypothesis: that the impact of economists on policy is indirect, and that the empirical support for their arguments weighs more heavily on policymakers than the arguments themselves. The justification for providing some future historian of ideas with more data with which to test this hypothesis is partly to make the sample more representative, but mainly because I think the choices are limited. The gap between theoretical possibility and empirical grounding has become so great in so much of regulatory economics that achieving a professional consensus, not to mention professional development, is going to compel us to look harder and harder at just how the world really works.

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