

THE PREDATOR STATE: HOW CONSERVATIVES ABANDONED THE FREE MARKET AND WHY LIBERALS SHOULD TOO

James K. Galbraith

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The title of *The Predator State* is partly a misnomer. This short book by economist James K. Galbraith, son of the celebrated economist John K. Galbraith is, in fact, a well-timed critique of what the author sees as the conventional wisdom in economics and a call for an expansion of the role of the state. Only its subtitle reveals its true theme: *How Conservatives Abandoned the Free Market and Why Liberals Should Too*. I will deal with the good things about the book first before moving on to discuss my misgivings.

The book is predominantly aimed at American Democrats, but it is of interest to a wider audience for a number of reasons. The first is that, though the son does not write as elegantly as his father did, the younger Galbraith does nonetheless succeed in ably slaying a number of shibboleths. More than a few of these are held by the left. For instance, he claims that the USA is both more egalitarian and more efficient than the European Union taken as a whole. The idea that carbon permits should be globally distributed on a per capita basis is rubbished as impractical. Galbraith rightly observes that global trading in carbon emissions is unfeasible because governments in developing countries would be unable to enforce their quotas. Proposals linking labour standards to trade deals are similarly given short shrift.

Secondly, and most interestingly, Galbraith's perspective is close to what could be called 'left-wing public choice'. This sometimes makes for more informed and interesting analysis than economists like Joseph Stiglitz or Paul Krugman can offer. Stiglitz *et al.* still inhabit an intellectual universe where it is thought useful to view the policies of government

as those of a benign dictator maximising social utility. Galbraith, on the other hand, seems to understand that regulation is often driven by special interests rather than by government benevolence.

This brings us to the title of the book. Galbraith rightly notes that despite the free-market rhetoric espoused by Republican politicians, the New Deal institutions have endured and still shape the American economy. They 'survived Reagan quite intact'. According to Galbraith, the problem is not that the state has been captured by ideologues intent on dismantling it, but that it has been captured by plutocrats intent on exploiting it. The predator state consists of reactionary businessmen: 'the worst polluters, the flagrant monopolists, the technological footdraggers' (p. 148). They are unprincipled conservatives whose 'reason for being, rather, is to make money off the state – so long as they control it' (p. 132).

The question then is: how do we deal with *The Predator State*? Here the problems I have with Galbraith's analysis become more pressing. I will mention only the most striking.

The book is aimed at the general reader but economists who read it will spot that Galbraith is that rare beast, a long-run Keynesian. Unlike Stiglitz or Krugman, Galbraith holds that economic growth is dependent on maintaining high demand and high investment, and that inflation is a cost-push phenomenon. This viewpoint is deeply unconvincing and it vitiates Galbraith's practical proposals.

Furthermore, Galbraith applies public-choice-style reasoning in one direction only. The solution to the predatory state proposed by Galbraith is an alternative coalition of 'progressive groups' such as trade unions, consumer groups and 'more reasonable' businessmen. This is unsatisfying unless we believe, as Galbraith appears to, that individuals who profess to be progressive cannot be predatory.

Finally, the author makes a number of outright errors. Adam Smith is misrepresented as envisioning 'a vast trading empire, defended as necessary by military force' (p. 67). In fact, Smith described the British empire as a project which has involved 'immense expense, without being likely to bring any profit'

(*Wealth of Nations*, Bk. v, Ch. iii). Apparently utterly ignorant of the situation in countries like the UK, Galbraith claims in Chapter 11 that if the state provided universal healthcare, the market for private health insurance would disappear. Galbraith's discussion of economic freedom, derided as the 'freedom to shop', is laughably insufficient. And at other times he simply attacks a straw man of perfectly competitive markets that is irrelevant to the task of comparing how different real-world comparative economic systems actually function. This means that the book is simply too lightweight to convince anyone not already on the left. The chapter on planning which is central to Galbraith's alternative economic vision is woefully short on details or analysis.

Perhaps Galbraith will offer us a more detailed and intellectually rigorous manifesto for planning and regulation in the future. Until that time, we are left with a book which, though lively and provocative, is ultimately unconvincing.

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THE CULT OF STATISTICAL SIGNIFICANCE: HOW THE STANDARD ERROR COSTS US JOBS, JUSTICE, AND LIVES

Stephen T. Ziliak and Deirdre N. McCloskey

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'The quantitative parts of a science should not be notable mainly for their lack of common sense.'

(Ziliak and McCloskey, p. 126)

Imagine an Alexandrian disaster in which all empirical social science research disappears immediately. It seems like this

would be a disaster of epic magnitude, but Ziliak and McCloskey might argue that we probably wouldn't have lost much. The book under review is the culmination of about two and a half decades of inquiry by McCloskey and about a decade and a half of involvement in the project by Ziliak. It is an undertaking of truly McCloskeyan proportions: Professor McCloskey is an excellent writer of 'big-think' science, and Ziliak has proven to be a more than able collaborator. They seek to expose *t*-testing for statistical significance as the naked emperor of empirical research. Theirs is a plea for statistics rightly done. This book will some day be assigned in first-year econometrics courses. The essence of their claim is based on the strong statement they make on p. 9: 'Fisher-significance is by itself about precisely nothing'. According to Ziliak and McCloskey, statistical testing is precisely-measured sound and fury. And yet it persists, even at the highest levels of the discipline.

Questions of 'statistical significance' are fundamentally questions about the existence of an effect rather than measurement of an effect, as one can see from the rhetoric of empirical social science. It suggests a McCloskeyan theory of regression analysis in the spirit of her 'A-prime, C-prime' theory of economic theory. McCloskey argues that for every set of assumptions A producing conclusions C, there exists a set of assumptions A' arbitrarily close to A that will produce conclusions C' arbitrarily far from C. One can say the same thing about regressions. For every set of regressions R that produce a conclusion C, there exists a set of regressions R' arbitrarily close to R that produce a set of conclusions C' arbitrarily far from C. One 'finds' or 'identifies' an effect based on a *t*-ratio. The *t*-ratio is mute on whether the identified effect is quantitatively significant.

Reading their book is a bit like reading Old Testament prophecy. After a moment's reflection, the insight is obvious: the standard error of a regression coefficient tells us about the properties of the sampling distribution, not about the magnitude of the estimated effect. The 'significance' of the coefficient can be altered by changing *n*. 'Significance' can change radically even if the coefficient doesn't budge as long as

one changes the sample size. Adding more degrees of freedom leads to greater significance; reducing the number of degrees of freedom reduces it.

But it is more than a jeremiad. Two obvious questions the authors address concern how the sciences were brought to such a lowly state and how it persisted for so long. I interpret their argument to rely in part on a view of a tragic historical accident: Ronald A. Fisher (the Cambridge statistician and villain in their story) was writing at a time at which the intellectual atmosphere was particularly conducive to it. It was a complement to neopositivism and falsificationism in the sciences, with Fisher positing that the key question concerns whether one should reject or not reject various null hypotheses (p. 149) even though falsificationism and hypothetico-deductivism 'are illogical, erroneous deductions' (p. 150). One factor they do not discuss is the role of socialism in social science, in the idea that an economy could be successfully and scientifically managed and fine-tuned.

They offer an intellectual history of statistical inference which I hope will be fleshed out in greater detail in Ziliak's in-progress biography of W. S. Gosset. In Chapter 18, they discuss Pearson's 'three-sigma' rule, which is basically the idea that a single statistical test is sufficient in the neopositivist framework, an inversion of the traditional work of science which looked at all the evidence. They offer a long and digressive discussion of the intellectual history of significance testing, with Fisher clearly in the role of the villain and Gosset clearly in the role of the hero.

Significance testing makes for a very easy, very quick way to assess output: if $p < 0.05$, you've found something. If $p > 0.05$, you haven't. I would also argue that the rhetoric of analysis and evaluation in government, education and the business world is partially responsible, as well. There is nothing wrong with developing measurable standards, but the standards themselves rarely have a loss function. So what is one to do here? According to Ziliak and McCloskey, it appears that there is a simple solution: re-build empirical research from the ground up. A key to understanding social phenomena is to understand exactly what we mean when we are carrying out empirical investigations.

Our research is often hard-boiled and half-baked, and bad science based purely on significance testing reduces quality of life. They discuss, for example, the Vioxx drug research (pp. 28–31) that led to several deaths. The key problem was one first of lying about the data, apparently, but second it was a problem of using the wrong metric – statistical significance – as a measure of the impact of Vioxx on heart attack probability. This is part of the social search process whereby more suitable ways of doing things emerge. At the same time, however, it is tragic that deaths (which could have been avoided) occurred.

Numbers are mysterious, and they seem objective. Significance testing has the virtue of looking scientific while excusing the analyst from taking responsibility for his or her conclusions. Even in epidemiology (p. 162), 'Statistical significance came to mean "epidemiological significance." Statistical insignificance came to mean "ignore the results."' They are adamant that statistical significance is not the same thing as clinical or economic significance, even though it is treated as such by scholars and commentators. What Ziliak and McCloskey would prescribe is a sophisticated contextual analysis of scientific results, which requires serious economic thinking. One gets the impression from reading Ziliak and McCloskey that the obsession with statistical significance is a type of pseudo-intellectual fast talk.

In spite of the claim of objectivity (p. 45), significance testing boils down to an appeal to the authority of Ronald A. Fisher, but Ziliak and McCloskey hope to move the scientific question beyond 'do we have enough data to determine whether the effect is or isn't zero?' They do not mince words in their contempt for the significance-testing paradigm: 'You might as well use a table of random numbers or a late-night phone call to a foreign-language psychic network to make your scientific decisions' (p. 54). For science to proceed we have to think about analysis like economists, comparing the marginal costs and marginal benefits of different threshold choices. In comparison to 'testimators', Ziliak and McCloskey argue that 'Scientists make a judgment about a threshold and get on with it' (p. 54) with the goal of being 'quantitatively

persuasive, not to be irrelevantly mechanical' (p. 55).

The incentives, though, encourage the irrelevant and the mechanistic. Scholarship is a series of short-run goals, and what is in the scholar's short-run interest is not necessarily in the long-run interest of good science. Finish the dissertation. Get a job. Get tenure. Get promoted. This is part of the process that creates path dependence in scientific methods. At the same time, however, over the very long run what is very sexy today is unlikely to be very sexy tomorrow. Ziliak and McCloskey make this point specifically on p. 32, arguing that significance testing brings about various professional niceties but that 'it is not science, and it will not last'. This provides an intriguing set of prescriptions for the ambitious scholar. The cynic, who is perhaps focused on the luxuries that come with a successful career rather than on doing good science, sees nothing wrong with the existing apparatus. The scientist, on the other hand, is presumably more interested in truth.

The book's penultimate chapter offers a meta-economic and sociological analysis of the factors that help explain these trends. There are implications for editors in that we are in part caught in a transitional gains trap, and we cause a capital loss for people who have built reputational capital on significance testing. They refer to the careerist view of significance testing as 'amused cynicism' (p. 59). Again, the question is about whether one is doing science or whether one just has a job. They argue that 'the philosophical atmosphere of 1922–62 was perfect for the fastening of Fisher's grip on the sizeless sciences' (p. 149). A future jaunt into intellectual history, and a chapter I hope we will see in Ziliak's forthcoming book, will relate the Fisherian paradigm to the zeal for central planning, particularly in the New Deal.

Perhaps there is some overkill in their support for their point. On pp. 57–59, they offer a very long bibliography of studies on this, suggesting that their point is not new. This is perhaps understandable because Ziliak and McCloskey have spent about two and a half decades as voices crying out in the wilderness. They summarise the findings of their earlier studies of the use of statistical significance in economics journals; to the shame of the profession,

the statistical analysis in our flagship journal – the *American Economic Review* – is, in their estimation, anti-scientific. Towards the end of the book they offer a multi-chapter biographical treatment of Karl Pearson, Gosset and Fisher that explores the personalities in the debate. I assume these chapters will be fleshed out in greater detail in Ziliak's forthcoming book.

They offer several explanations for the seeming inefficiency of the intellectual marketplace, one of which is a Great Man theory whereby they argue that, without Fisher, this perhaps would not have happened. Another possibility, of course, is that the Fisherian programme is best, but they are persuasive in their demonstration that the Fisherian programme is *not* the best. Third, they argue that Path Dependence is at work. It is hard to switch off the path, and the large switching costs suggest that significance testing is okay. It is, quite literally, good enough for government work. At the same time, though, path dependence can sometimes be a just-so punt for phenomena we do not understand fully.

They argue in favour of an explanation relying on 'High Modernism', suggesting that 'Fisher had the good fortune to be born just as the prestige of mechanical methods in all fields, from mathematics to automobile manufacturing, was coming to a climax' (p. 243). They argue further that significance testing is an expression of what Robert Merton has called 'the Bureaucratization of Knowledge' (p. 243). In my notes on p. 243, I note that this looks an awful lot like what F. A. Hayek has called 'scientism'. Then they invoke Hayek and scientism on p. 244.

An important factor here is that there are incomplete markets for statistical analysis. The feedback mechanism is very noisy. The FDA can't go out of business, for example. What matters for regression output, according to some scholars, is just the column of *p*-values, and the feedback mechanism rarely tells researchers otherwise. The authors are pessimistic; they believe that there is little in the institutional infrastructure of science that will correct these systematic errors.

Ziliak and McCloskey close with a dramatic exhortation: 'The textbooks are wrong. The teaching is wrong. The seminar you just attended is wrong. The

most prestigious journal in your scientific field is wrong' (p. 250). What they have produced is the culmination of a long and detailed research agenda and proposes a new framework for empirical science. It is something we would perhaps do well to take to heart.

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THE CULT OF THE PRESIDENCY

Gene Healy

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In Virginia, two weeks before the presidential election, I found myself in conversation with an older lady. She said, in a disconsolate southern drawl, 'I do not believe we have in America today the same quality of people we had at the Founding.'

'But there were rogues even then,' I protested. 'Plenty of them. And there *are* plenty of good people today. It is not Americans who have changed. What has changed is the office of the presidency, and what we have come to expect from him.'

'Yes,' she said with resignation. 'He has become an Emperor.'

Gene Healy's new book, *The Cult of the Presidency*, is the story of how we arrived at this point.

The early Americans understood all too well the trouble a country could end up in at the hands of a whimsical and all-powerful ruler. They had a keen appreciation for both human fallibility and the corrupting effects of power. Their insight into human nature, as well as the historical antecedents, led the Founders to want to limit the powers of the new president, and particularly to limit his capacity to wage war. The Constitution they ultimately wrote separated the power to authorise or initiate war from the power to direct it and at the same time gave the presidency a modest role.

The presidency that Healy describes in its first one hundred years is