Robust Institutions

David M. Levy
Center for Study of Public Choice
George Mason University
dlevy@vms1.gmu.edu
June 29, 1999

For Jim's 80th

Abstract

I propose to show how to translate the economic analysis of institutions developed by tradition of political economic theorists who thought in "worst case" terms into the *lingua franca* of robust statistics. An institution will be defined as contingent upon a design theory and the difficulty we consider is the use of the institution by the designer.

The technical bridge between institutional robustness and statistical robustness is the the possibility of exploratory data analysis [EDA] with respect to the design theory. In an institutional context with EDA comes the recognition that the model is not completely specified, that we do not fully understand the structure of world *before* studying it. In a statistical context with EDA comes a non-normal error distribution.

The relationship between evolutionary institutions and robust institutions is discussed.

Introduction

Here is a historical-analytical puzzle. Why is it that J. M. Buchanan's worst-case philosophy of constitutional political economy sounds so similar to J. W. Tukey worst-case philosophy of mathematical statistics?[1] The use of heavy-tailed distributions as paradigm – without worrying overly much about descriptive accuracy as well as the "unrealistic" use of Leviathan models – are defended by the desire to avoid disaster.[2] Underlying this common

---

[1] D. F. Andrews, *et al.* (1972, p. 7C4): "Perhaps even more important than to say which estimators should be used, is to say which shouldn't. An important reason for not using an estimator is that it may behave catastrophically under some circumstances." G. Brennan and J. M. Buchanan (1984, p. 392): "The imputation of *homo economicus* motivation to actors in political roles may seen to violate ordinary notions about descriptive reality more than the comparable imputation to actors in the marketplace. But this difference need not provide any justification for replacing the model used for institutional comparison. It may be that judges seek to 'uphold the law' most of the time, that most government employees try to further their own conceptions of 'public interest' most of the time, and that elected politicians are genuinely concerned about promoting the 'good society.' But, even if this were admitted, institutional arrangements would surely be preferred which made these congruent with narrow self-interest on the part of the relevant actors. A model of human behavior in which the natural impulse toward self-interest, narrowly defined, predominates is a highly useful artifact in helping us to identify that set of arrangements that 'economize on love.'"

[2] J. W. Tukey (1960, p. 474): "In slightly large samples, there is ground for doubt that the use of the variance (or the standard deviation) as a basis for estimating scaling type is ever truly safe. ... If contamination is a real possibility (and when is it not?), neither mean nor variance is likely to be a wisely chosen basis for making estimates from a large sample." F. Mosteller and J. W. Tukey (1977, p. 14): "... a few straggling values scattered far from the bulk of the measurements can, for example, alter a sample mean drastically, and a sample s^2 catastrophically." Brennan and Buchanan (1984, p. 392): "Suppose you are hiring a builder to build you a house. ... For empirical purposes, therefore, the assumption you will make about the said builder is that he is honest: you would not deal with him if you genuinely believed otherwise. But now you proceed to your lawyer's offer to draw up a contract. And in this setting, the working hypothesis you make about the builder is quite different. For the contract-drawing exercise, you make the assumption that the builder *is* going to fleece you, not because you believe this necessarily is his objective but because this is the contingency against which you wish to guard."

concern is their questioning of the applicability of optimization considerations.[3]

I should think the common factor linking Tukey and Buchanan is the distant influence of the atheoretic view of human nature taught by the Greek historians.[4] How people behave is not, in fact, in correspondence with their own theories of how they ought to behave.[5] Buchanan's insight is explicitly informed by the constitutional theory of such careful students

---

[3]From the 1960s both expressed grave doubts about exclusive focus on optimization accounts. As early as 1962 Tukey explicitly questioned reliance on optimization considerations, Tukey (1962). The issue was implicit as early as 1960, Tukey (1960, pp. 449-50): "Some years of close contact with the late C. P. Winsor had taught the writer to beware of extreme deviates, and in particular to beware of using them with high weights. Using second moments to assess variability means giving very high weights to extremely deviate observations. Thus the use of second moments, unquestionably optimum for normal distributions, comes into serious question." Tukey (1977, p. 1): "**We do not guarantee to introduce you to the 'best' tools, particularly since we are not sure that there can be unique bests**." Summarizing a decade of work, Buchanan (1979, p. 281): " ... the maximizing paradigm is the fatal methodological flaw in modern economics."

[4]An account of common morality as false but useful theories of conduct is provided in Levy (1992).

[5]One illuminating passage from Thomas Hobbes' translation of the Melian dialogue will stand for it all. Thucydides(1975, pp. 381-82): "*Melians*: 'We think, you well know, a hard matter for us to combat your power and fortune, unless we might do it on equal terms. Nevertheless we believe that, for fortune, we shall be nothing inferior; as having the gods on our side, because we stand innocent against men unjust: and for power, what is wanting in us will be supplied by our league with the Lacedæmonians, who are of necessity obliged, if for no other cause, yet for consanguinity's sake and for their own honour, to defend us. So that we are confident, not altogether so much without reason as you think.'

"*Athenians.* 'As for the favour of the gods, we expect to have it as well as you: for we neither do, nor require anything contrary to what mankind hath decreed, either concerning the worship of the gods, or concerning themselves. For of the gods we think according to the common opinion; and of men, that for certain by necessity of nature they will every where reign over such as they be too strong. Neither did we make this law, nor are we the first that use it made: but as we found it, and shall leave it to posterity for ever, so also we use it: knowing that you likewise, and others that should have the same power which we have, would do the same.'" The editor notes sardonically that the Athenians sound as if they had been reading Hobbes' *Leviathan* (p. 580).

of the Greek historians as Thomas Hobbes', David Hume's and John Stuart Mill's.[6] A little

more oblique is the fact that behind robust statistics in general stand the contributions of John

von Neumann: minimax loss theory and monte carlo method. These two contributions are

connected by a willingness to give up any hope of a unique solution in exchange for insight into

real problems.[7] First, von Neumann's minimax loss approach to decision making is absolutely

central to robust thinking. One can see this impact in A. Wald's notion of admissibility[8] and

in G. Box's use of the term "robust" to describe estimators which "insure" against risk by giving

up efficiency at the ideal case and in the development of explicit minimax estimation by P. J.

Huber.[9] Second, without the technology provided by the Ulam-von Neumann's monte carlo

---

[6]David Hume (1987, p. 42): "Political writers have established it as a maxim, that, in contriving any system of government, and fixing the several checks and controuls of the constitution, every man ought to be supposed a *knave*, and to have no other end, in all his actions, than private interest." The modern revival of this Humean point of view comes in James Buchanan and Geoffrey Brennan (1980).

[7]J. von Neumann and O. Morgenstern (1964, p. 42): "Indeed we shall in most cases observe a multiplicity of solutions. Considering what we have said about interpreting solutions as stable 'standards of behavior' this has a simple and not unreasonable meaning, namely that given the same physical background different 'established orders of society' or 'accepted standards of behavior' can be built, all possessing those characteristics of inner stability which we have discussed." S. Ulam (1991, p. 199): "The one thing about Monte Carlo is that it never gives an exact answer: rather its conclusions indicate that the answer is so and so, within such and such an error, with such and such probability ... it provides an estimate of the value of the numbers sought in a given problem."

[8]Tukey (1986, p. 289): "Then the work of Abraham Wald rolled back certainty still further when he showed that, insofar as procedures leading to definite actions were concerned, there could, in general, be no single optimal statistical procedure, but only a 'complete class' of procedures, among which selection must be guided by judgment or outside information."

[9]There is a tendency for modern economists in the Bayesian tradition to let the utility-theoretic appendix to von Neumann-Morgenstern stand for the whole book. G. Box's (1953) coinage of the term "robust" and his discussion of robust estimation in terms of an insurance metaphor is in the tradition of minimax thinking just as surely as A. Wald (1950) on admissible estimators.
Here is the authoritative comment of L. J. Savage on the second edition of *Theory of Games*

method there would be no Tukey-directed Princeton Robust Study.[10] Minimax loss thinking works when we cannot even assign probabilities to outcomes. With monte carlo methods we can find solutions where our analytical powers fails. Tools for a world where our theoretical insight fails. This is von Neumann's gift to robust statistics.[11]

And the link to the Greek historians? Von Neumann did not, as far as I know, write on the classics. But we are told that his thoughts as he lay dying turned to Thucydides' account

---

and Economic Behavior, Savage (1972, p. 281): "Contains, as a digression, the important new treatment of utility from which the treatment of utility in this book derives. The second edition contains more than the first on this subject, especially as a technical appendix. Also, the idea regarding multiple choices as single overall choices is discussed in great detail. Finally, the chapters on 'zero-sum two-person' games are mathematically intimately connected with the statistical minimax theory."

The close relationship between a minimax approach and an expected utility approach is doubtless responsible for Savage's comments on the work leading to Huber (1981) at Savage (1972, p. 291): "An important nonpersonalistic advance in the central problem of statistical robustness." The Huber family of estimators is an explicit solution to a minimax problem, e.g., Andrews, *et al.* (1972, p. 2C1), Huber (1981). Thus, the estimator LJS, Savage's contribution to Andrews *et al.* (1972, p. 2C3), is a "Huber estimates are maximum likelihood estimates for a gross error model ...", i.e., a distribution which is normal in the center, double exponential in the tails where the splicing parameter is estimated simultaneously. Its performance *vis-á-vis* the robustnik favorites – trimmed means, weighted quantiles and the redescending hampels – is a subject which would repay close study.

[10]S. Ulam (1991, p. 199): "The Monte Carlo method came into concrete form with its attendant rudiments of a theory after I proposed the possibilities of such a probabilistic schemes to Johnny in 1946 during one of our conversations. ... After this conversation we developed together the mathematics of the method. It seems to me that the name Monte Carlo contributed very much to the popularization of this procedure. It was named Monte Carlo because of the element of chance, the production of random numbers with which to play the suitable games." Ulam (1991, p. 200): "I felt that in a way one could invert a statement by Laplace. He asserts that theory of probability is nothing but calculus applied to common sense. Monte Carlo is common sense applied to mathematical formulation of physical laws and processes."

[11]This must not be read as his *only* contribution. I remember the late Abraham Charnes saying that the Charnes-Cooper solution to the least absolute deviations regression problem depended upon devices from von Neumann.

of a particularly nasty moment in the war between Athens and Sparta.[12] The Greek historians are Berlin's hedgehog. They teach one lesson, the trick which always works. The world is not as we would we would have it. We ought to proceed as if *our* hopes are illusions.

What's Wrong with this Picture?

We now turn to develop a analytical linkage between Tukey's and Buchanan's worries about avoiding disaster. The bridge will be the recognition that our idealized characterization of the world could be false.[13] Of course the topics which concern Tukey and Buchanan differ. Of abstracting device is a specification of a formal language broad enough to express some of their common concerns but narrow enough to be tractable.

Consider the following picture which describes the performance of two artifacts – 1 and 2 – as a function of some theoretical information. For point of reference we indicate one possible state of the theory which we label J. We know that in the case when the theory attains state J that the performance of 1 is superior to 2. It is worthy of considerably reflection

---

[12]S. Ulam (1991, p. 102): "He was a great admirer of the concise and wonderful ways the Greek historians wrote. His knowledge of Greek enabled him to read Thucydides, Herodotus, and others in the original ... The story of the Athenian expedition to the island of Melos, the atrocities and the killings that followed, and the lengthy debates between the opposing parties fascinated him for reasons which I never quite understood. He seemed to take a perverse pleasure in the brutality of a civilized people like the ancient Greeks. For him, I think it threw a certain not-too-complimentary light on human nature in general." Ulam (1991, p. 244): "I will never forget the scene a few days before he died. I was reading to him in Greek from his worn copy of Thucydides a story he liked especially about the Athenians' attack on Melos, and also the speech of Pericles. He remembered enough to correct an occasional mistake or mispronunciation on my part." Hobbes' translation is provided in note 5 above.

[13]F. A. Hayek quotes Oliver Cromwell at (1960, p. 530): "'I beseech you, in the bowels of Christ, think it possible you may be mistaken.'" and then adds "It is significant that this should be the probably best-remembered saying of the only 'dictator' in British history!"

that serious discussions can be conducted considering *only* the performance of 1 and 2 at ♩.[14]

---

[14]Tukey (1977, p. vi): "**The greatest value of a picture** is when it *forces* us to notice **what we never expected to see**."
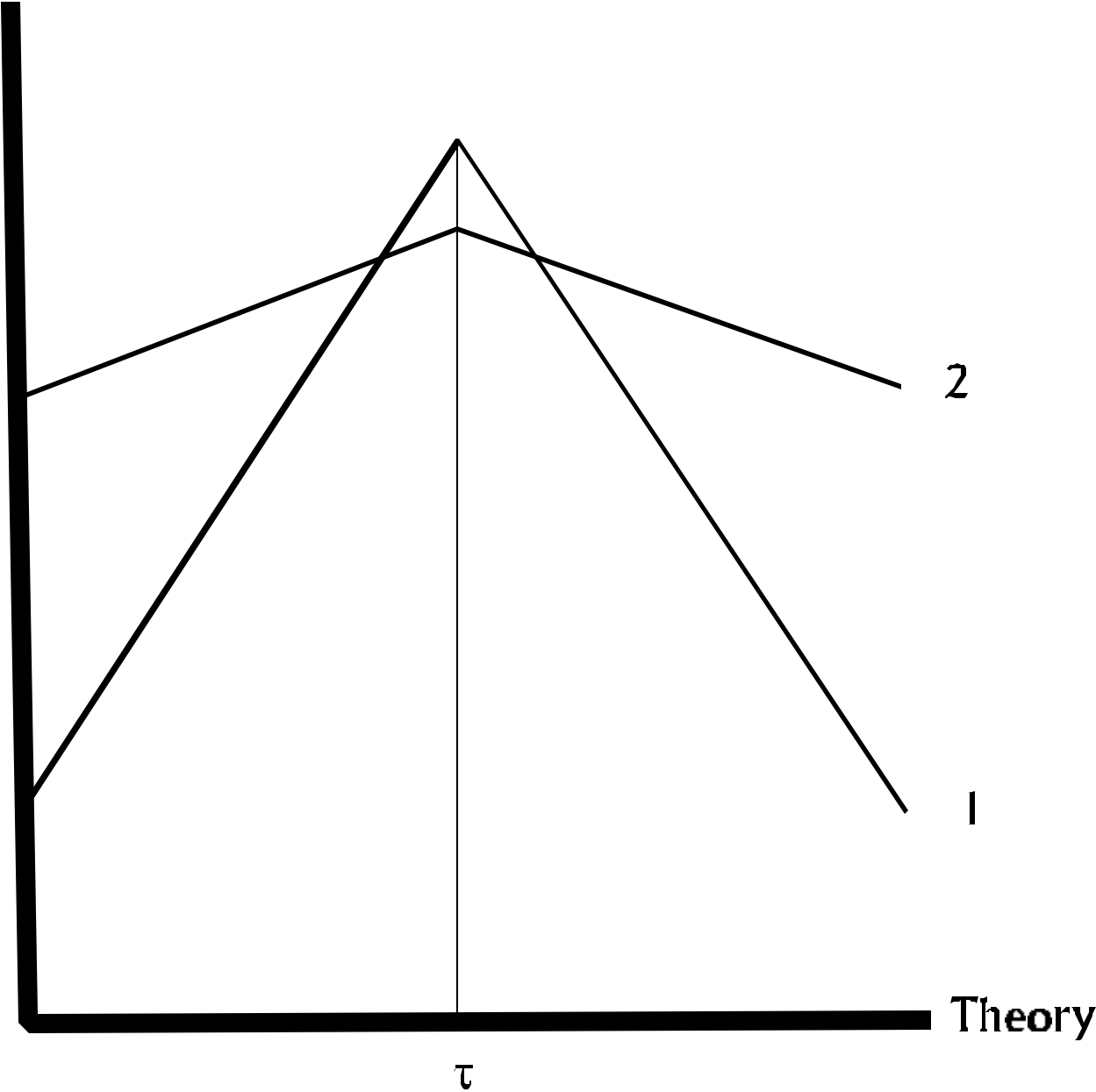
Good Stuff

Figure R. 2 is more robust than 1.

A heavily studied example is the efficiency of a least squares estimate of a regression

problem when the errors are independent normal. It would be hard to find a graduate econometrics text which does not develop the proof that under this condition the least squares estimate attain the Cramér-Rao lower bound and thus provide best unbiased estimates.[15]

What happens when the normality condition fails is somewhat less heavily discussed in the texts.[16]   Indeed, the student can be excused for not realizing that small deviations from normality create the situation pictured in Figure R. If **T** is the set of symmetrical unimodal distributions and **J** marks the special case of the normal distribution   then the efficiency properties of least squares estimation are as pictured by 1 compared with   robust regression techniques   pictured by 2.[17] The student generally does not come away from econometrics textbooks with the sure and certain knowledge that the efficiency gains from least squares *vis-à-vis* robust techniques are small at normality but the efficiency costs can be arbitrarily large as the actual distribution deviates from this ideal.[18]

---

[15]G. Chow (1983, pp. 22-24), H. Theil (1971, pp. 384-392).

[16]Chow (1983, pp. 88-90), Theil (1971, pp. 615-622).

[17]For the one-dimension case of a least squares regression (the sample mean) Tukey (1960), Andrews, *et al.* (1972). For the multiple dimension case Mosteller and Tukey (1977), R. Koeneker and G. Bassett (1978). Gib Bassett drew Figure R at a 1998 Public Choice Wednesday Seminar when he presented the results in G. Bassett and J. Persky (1998).

[18]Remarkably enough students often emerge from an econometrics course under the impression that the efficiency properties of least squares are something which can be proven from the Gauss-Markov theorem! This is obviously not the fault of the textbooks except (possibly) to the extent that the "linearity" restriction in the Gauss-Markov theorem is not developed in terms of a linear/nonlinear influence curve so the student does not recognize how from "linearity" we can immediately infer "unbounded." Influence curves play a critical role in Andrews, *et al.* (1972) and Mosteller and Tukey (1977). There is a certain delightful irony in *this* confusion because, as I shall

To develop intuition about the bridge between robust statistical considerations and constitutions consider the late central planning debate in which L. von Mises and F. A. Hayek stood against the profession. It was established to the satisfaction of almost all the participants in the debate, that a planned economy could be more efficient than a market *because* the planned economy would not be troubled with the sorts of public choice considerations which plague actually existing markets.[19] Perhaps the confidence with which economists pressed this case can be best judged by the widespread dissemination of the claim that for decades the planned economies's growth *exceeded* that of market economies.[20]

If normality is the ideal condition for least squares – the J in the picture – what serves as J for central planning? The public spiritedness of the new socialist managers: the benevolent despot assumption. Those who set prices would set aside any private interest to serve the public interest. And if this J failed? As I read historical record of the central planning debate, discussion of this possibility is a little sparse.[21] It only became obvious as the planned economies fell apart: the planners used their theory to fool the public and to make themselves arbitrarily rich.

The design J for central planning "forgot" to take into account of the interest of the

---

argue below, it reappears when those who work in the evolutionary tradition established by F. A. Hayek, who try to make an "efficiency" case ought to be trying to make a "robustness" case.

[19]Documentation for this reading of the debate is found in Levy (1990).

[20]The embarrassing numbers are pictured in Levy (1993) as well as a characteristic claim made about them on highest economic authority.

[21]As far as can see only J. Schumpeter made the point, but he made it in such a way that it would not be easily understood, Levy (1990).

economic theorist who might just become a central planner. The result was without doubt the greatest historical disaster with which economists share complicity.[22]

I propose to generalize from this example with some technical machinery developed elsewhere to connect exploratory data analysis to non-normality.

Institutions and Design Theory

The building blocks of our account consist of a language $\mathbf{I}$ inside which we can write down a model which purports to describe an institution I. We shall suppose that each of the models of $\mathbf{I}$ is a linear estimating equation. The consistency condition we impose is that if $\mathbf{J}$ is a model, it is identifiable.[23] Thus, for each institution we associate a design theory. This conditionality we write as $I|\mathbf{J}$.

We define the institutional robustness with respect to the design theory $\mathbf{J}$ in terms of a loss function. There exists some finite bound, m, on the difference between the institutional performance with respect to a metric U when $\mathbf{J}$ holds – $U(I|\mathbf{J})$ – and when $\mathbf{J}$ fails – $U(I|\sim\mathbf{J})$. Thus I is robust if and only if:

---

[22]The professional reaction to the work of Warren Nutter ought to be studied as an hideous example of the reaction awaiting someone whose results do not fit with the reigning preferences of how the world works.

[23]In formal logic a model is defined as a collection of sentences from which all other sentences in the language cannot be deduced, C. C. Chang and H. J. Keisler (1978, p. 9). This consistency condition in econometrics fails when singular matrices occur, the multiple-dimension equivalent of division by zero. For my tastes Theil (1971) remains unsurpassed on the various aspects of the identification question, e.g., the link between colinearity and identification (1971, p. 152) and the problem of non-existing moments in popular simultaneous equation procedures (1971, p. 532). A "consistent estimator" is a well-defined term with a meaning unrelated to model-theory "consistency." "Identifiable" is as close to model-theory consistency as I think we can get.

m < U(I|**J**) - U(I|~**J**).

A robust institution will doubtless perform better when the ideal of **J** holds than when it fails but that is not the point. The point is the loss from such failure is bounded. A nonrobust institution can be defined by reversing the inequality.

We must go beyond a formal language and say how the design theory **J** could fail. Since we are assuming linearity and only requiring **J** to be an estimating equation, without being particular how it is estimated, failure of **J** occurs when we leave a real variable out of consideration. Thus failure of **J** is the same as model misspecification.

*EDA is possible iff **J** fails.* Let us consider more precisely how the design theory can fail. Let us write the traditional linear model which expresses a random variable Y_t as a function of a finite number (K) of independent variables X_{1t} ... X_{Kt} and an error term g_t.
Thus for t= 1...T

$$Y_t = \sum_{k=1}^{K} b_k X_{kt} + e_t$$

Suppose that g_t is normal. The Lindeberg-Feller conditions allow us to decompose a normal distributed random variable into normal and non-normal components.. Those components which are normally distributed, we label O_t and proceed to other concerns. Now, consider the infinite number (H) of non-normal components of g_t which the Lindeberg-Feller conditions allow. If each of these random variables X_{K+1t} ... X_{Ht} is real valued then there must be

infinitesimals $^*_{K+1 \dots} {}^*_H$ such that we can rewrite g_t in terms of the product of reals and infinitesimals. Thus, we obtain:

$$Y_t = \sum_{k=1}^{K} b_k X_{kt} + \sum_{k=K+1}^{H} d_k X_{kt} + h_t$$

While this formulation is not sufficient to establish the normality of g_t, it is necessary. If any of the $^*_k$ were non-infinitesimal and X_k were non-normal then this contribution to g_t would have to be a real-valued, non-normal random variable thus violating the Lindeberg-Feller conditions.

The definition of EDA proposed in Levy (1999a) is moving a variable from the infinitesimal * list to the real $\$$ list. At T_{N+1} but not at T_N, it may be possible to reject the hypothesis that $\$_{K+1}$ equals 0. We there consider two types of EDA: local EDA and global EDA. Local EDA: for some $T_2 > T_1$ the model expands from K non-infinitesimal contributions to K+1. Global EDA: in addition to local of EDA at T_1, if local EDA at T_N then local EDA at T_{N+1}.

Levy (1999a) establishes that if EDA is possible then the regression errors are non-normal. This is the bridge between robustness of institutional design and robust statistics. The ideal condition is violated if EDA is possible – something of real importance has been omitted from the model.

The robustness problem arises now. What is the consequence of this failure? A robust institution is one which puts a bound on the loss from such failure. The failure is akin to Popperian falsification: the model predicts that something is not of real important, but it is.

Against romance and exact micronumerosity. One of Buchanan's persistent themes has been the importance of comparing actual institutions against actual institutions, e.g., against the possibility of market failure one must compare the possibility of government failure.[24]

The link to robustness is obvious is we think about actually estimating an equation for an institution which does not exist. We have the case that $N=0$ – Goldberger's "exact micronumerosity" – which trivially resists identification.[25] Presumably the equation would be identifiable when $N>K$ so we know that EDA is possible. Thus, considerations of robustness are paramount in comparing existing institutions with a nonexistent "ideal" institution. We know that any J proposing to describe the ideal must fail for want of sufficient observations to make the identification.

---

[24]Buchanan (1984, p. 11): "I have often said that public choice offers a 'theory of government failure' that is fully comparable to the 'theory of market failure' that emerged from the theoretical welfare economics the 1930s and 1940s. In that earlier effort, the system of private markets was shown to 'fail' in certain respects when tested against the idealized criterion for efficiency in resource allocation and distribution. In the later effort, in public choice, government or political organization is shown to 'fail' in certain respects when tested for the satisfaction of idealized criteria for efficiency and equity."

[25]A. Goldberger (1991, p. 249): "Econometrics texts devote many pages to the problem of multicolinearity in multiple regression, but they say little about the closely analogous problem of a small sample size in estimating a univariate mean. Perhaps that imbalance is attributable to the lack of an exotic polysyllabic name for 'small sample size.'"

Evolutionary Institutions

Suppose that an institution exists without a unique design theory, institutions which evolved by a process of trial and error.[26] It is suggestive that one institution which we have good reason to believe evolutionary – the practice of election by lot in Athens – can be shown to be statistically robust.[27] Indeed, since there is no design theory involved – only EDA on the part of the participants[28] – the robustness with respect to design theory is a matter of definition.

Moreover, many of the theorist with whom evolutionary insight has been credited – Hume and Smith in particular – clearly thought in terms of a utilitarianism based on "rules" not on "actions."[29] Figure R may be one way to express the concerns of those philosophers who

---

[26]F. A. Hayek is uniquely responsible emphasizing the importance of evolutionary institutions in the thinking of David Hume, Adam Smith. Hayek (1960) is perhaps his most systematic attempt. I am persuaded that most, if not all, of Hayek's insights can be translated into robust terms even when they do not fit so nicely in standard efficiency terms. Part of my confidence is that there is evidence, which is independent of Hayek's account, of robust considerations. Hayek points to Adam Smith as a formative theorist in the evolutionary tradition. Adam Smith's utilitarianism – which Hayek does not consider – seems to be have been made in terms of the well-being of majority, i.e., robust utilitarianism, Levy (1995). The same attention to median well-being is found in Malthus, cf. S. Hollander (1997). Tyler Cowen suggests that the discussion of "worst off" in J. Rawls (1971) might be a generalization of the Smithian approach as it allows *any* quantile to be selected.

[27]Levy (1989) discusses how Athenian practice used both voting and random representation. The Athenian understanding – from what we know about this at our distance – suggested that random representation ought to be employed in a bimodal context in which majority rule voting is nonrobust. Bassett and Pesky (1998) establish the formal equivalence of voting and estimation.

[28]Part of the exercise of the various studies in Levy (1992) is to give an explanation of evolutionary institutions in classical antiquity, e.g., the Athenian lot, the Homeric epics and the role of hero worship.

[29]Levy (1999b) gives an account of Smith's katallactic approach *vis-à-vis* the neo-classical "Robinson Crusoe" approach in terms of robustness.

defend "rule utilitarianism." While there exists some state of affairs J for which 1 is superior to 2, that is perhaps not how one wishes to bet.

Unfortunately, much modern thinking of evolutionary institutions has been expressed in terms of "efficiency," a formulation which encourages refutation. To establish an institution is efficient *without specifying* which model one is describing one must make the case that it is superior to the alternative for *all models* of **T.** I suspect that this case cannot be made for any interesting institutions.[30] Or if it were made then the institution ought to lose all its proponents! This case is drawn as Figure E.

---

[30]Levy (1999b) argues that such evolutionary strategies as "tit-for-tat" are robust even if the "proof" of efficiency is simple hand-waving by supposing a zero rate of time preference.
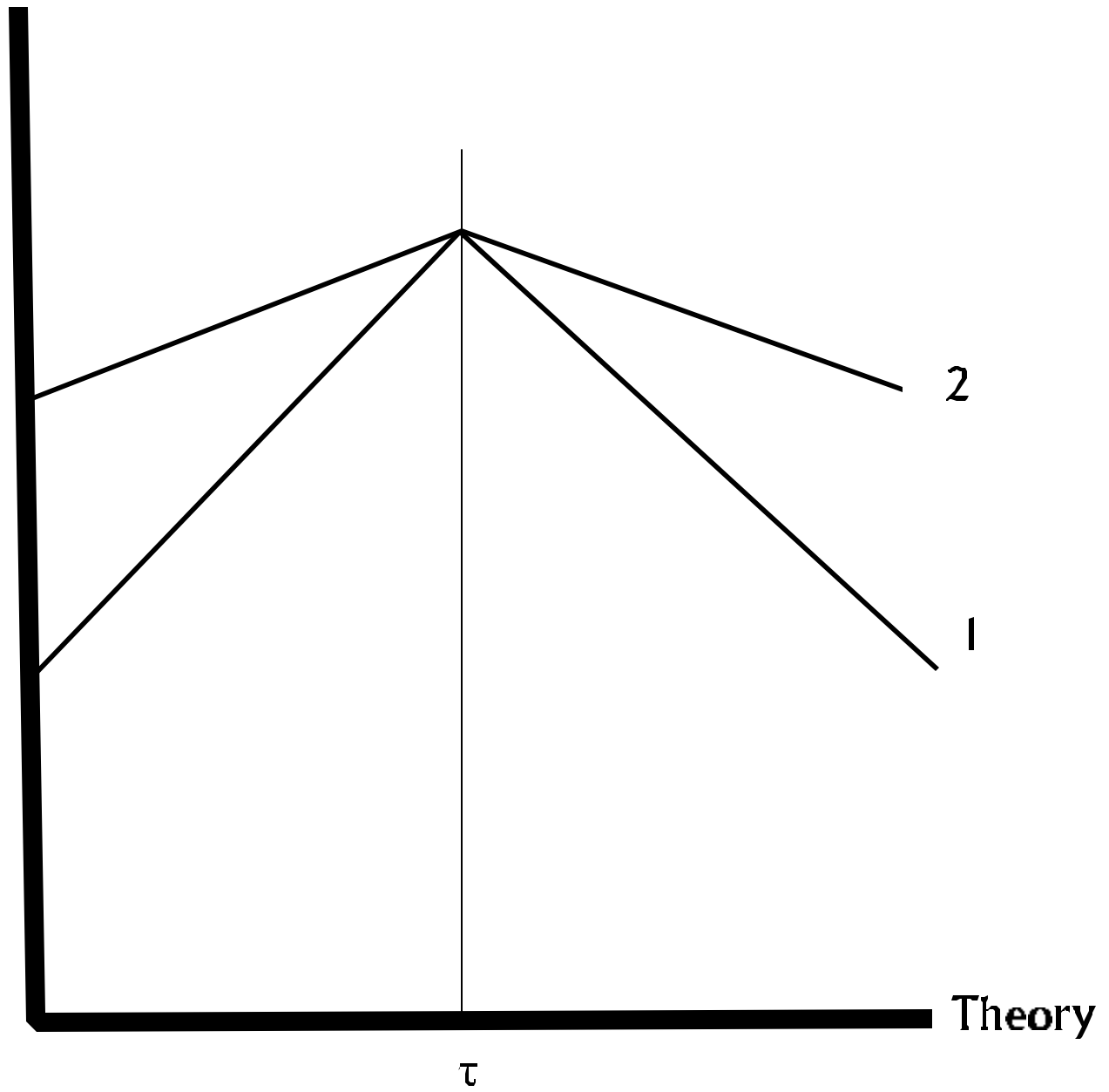
Good Stuff

Figure E: 2 is more efficient than 1.

In a statistical context such an argument would be the demonstration that an estimator such as 1 is inadmissable; there is *no* distribution for which it is superior.[31] In econometrics such proofs do indeed exist. The Gauss-Markov theorem establishes that with respect to the class of linear unbiased estimators, only least squares is admissible.[32]

As I read the record, von Mises and Hayek did try to establish that central planning was an inadmissible institution, there is no *model* – this is just a consistency condition, not a description of the world as we know it – for which planning was not dominated. Even Buchanan was not persuaded of this![33]

Conclusion

Inside economics the path of acceptance of both robust statistics and worst-case public choice thinking has been slow and controversial. The fact that the assumption of normality is still paradigmatic in econometrics[34] and the fact (or so I am told) that the benevolent despot assumption is still paradigmatic in public finance are, in my way of thinking, only one fact.

---

[31]A. Wald (1950) formalized the concept of admissablility using von Neumann-Morgenstern minimax devices.

[32]The reader who thinks I neglect the condition that the Gauss-Markov theorem requires that the error distribution have the first two moments might reflect upon Tukey's point, Tukey (1986, p. 411), that there is no way to distinguish between a Cauchy with the tails trimmed at $\pm 10^{-40}$ and the theoretical Cauchy. In the former case all moments exist, in the latter none exist.

[33]This remarkable bit of doctrinal history is provided in Levy (1990).

[34]Levy (1999a) was rejected by a less good journal than *Econometric Theory* with the comment that econometricians do not care about non-normality.

Best-case theorizing supposes the theorist has perfect insight into the structure of the world. EDA cannot be performed because we know the structure prior to study. This claim robust theorizing renounces. Without the assumption of perfect insight, the world is a messy place. But Tukey said all that really needs to be said about this a long time ago when he proposed that "commandments" to empirical workers be expressed in the negative, i.e., as "badmandments"

> The admonitions which follow are, indeed evil; they are not mere straw men or scapegoats. They are truly badmandments ....

> The great badmandment can be stated in all languages and to apply to any situation. In general allegorical language it reads:

> > IF IT'S MESSY, SWEEP IT UNDER THE RUG.[35]

---

[35]Tukey (1986, p. 199). This is an unpublished article from 1960 resulting from Tukey's year associating with behavioral scientists.

Bibliography

Andrews, D.F, P. J. Bickel, F. R. Hampel, P. J. Huber, W. H. Rogers and J. W. Tukey. *Robust Estimates of Location.* Princeton: Princeton University Press, 1972.

Box, G. E. P. "Non-normality and Tests on Variances." *Biometrika* **40** (1953): 318-35.

Bassett, Gilbert, Jr. and Joseph Persky. "Robust Voting." *Public Choice* (1998)

Brennan, Geoffrey and James M. Buchanan. *The Power to Tax: Analytical Foundations of a Fiscal Constitution.* Cambridge: Cambridge University Press, 1980.

Brennan, Geoffrey and James M. Buchanan. "The Normative Purpose of Economic 'Science.'" *The Theory of Public Choice–II.* Edited by James M. Buchanan and Robert D. Tollison. Ann Arbor: University Press, 1984.

Buchanan, James M. *What Should Economists Do?* Edited by H. Geoffrey Brennan and Robert D. Tollison. Indianapolis: Liberty Press, 1979.

Buchanan, James M. "Politics without Romance." *The Theory of Public Choice–II.* Edited by James M. Buchanan and Robert D. Tollison. Ann Arbor: University Press, 1984.

Chang, C. C. and H. J. Keisler. *Model Theory.* Second edition. Amsterdam: North-Holland, 1978.

Chow, Gregory C. *Econometrics.* New York: McGraw-Hill, 1983.

Goldberger, Arthur S. *A Course in Econometrics.* Cambridge, Mass.: Harvard University Press, 1991.

Hayek, F. A. *The Constitution of Liberty.* Chicago: University of Chicago Press, 1960.

Hollander, Samuel. *The Economics of Thomas Robert Malthus.* Toronto: University of Toronto Press, 1997.

Huber, P. J. *Robust Statistics.* New York: John Wiley, 1981.

Hume, David. *Essays Moral Political, and Literary.* Edited by Eugene F. Miller. Revised edition. Indianapolis: Liberty Classics, 1987.

Koeneker, Roger and Gilbert Bassett, Jr. "Regression Quantiles." *Econometrica* **46** (1978): 33-50.

Levy, David M. "The Statistical Basis of Athenian-American Constitutional Theory." *Journal of Legal Studies* **18** (1989): 79-103.

Levy, David M. "The Bias in Centrally Planned Prices." *Public Choice* **67** (1990): 213-26.

Levy, David M. *Economic Ideas of Ordinary People: From Preferences to Trade.* London, 1992.

Levy, David M. "The Public Choice of Data Provision." *Accountability in Research* **3** (1993): 157-63.

Levy, David M. "The Partial Spectator in the Wealth of Nations: A Robust Utilitarianism." *European Journal of the History of Economic Thought* **2** (1995): 299-326.

Levy, David M. "Problem and Solution: Non-normality and Exploratory Data Analysis." *Econometric Theory* (1999a)

Levy, David M. "Katallactic Rationality: Language, Approbation & Exchange." *American Journal of Economics and Sociology* (1999b): forthcoming.

Mosteller, Frederick and John W. Tukey. *Data Analysis and Regression.* Reading, Mass.: Addison-Wesley, 1977.

Rawls, John. *A Theory of Justice.* Cambridge, Mass.: Havard University Press, 1971.

Savage, Leonard J. *The Foundations of Statistics.* New revised edition. New York: Dover, 1972.

Theil, Henri. *Principles of Econometrics.* New York: John Wiley, 1971.

Tukey, John W. "A Survey of Sampling from Contaminated Distributions." *Contributions to Probabilities and Statistics.* Edited by Ingram Olkin, Sudhish G. Ghurye, Wassily Hoeffding, William G Madow, Henry B. Mann. Stanford: Stanford University Press, 1960.

Tukey, John W. "The Future of Data Analysis." *Annals of Mathematical Statistics* **33** (1962): 1-67.

Tukey, John W. *Exploratory Data Analysis.* Reading, Mass.: Addison-Wesley, 1977.

Tukey, John W. *Philosophy and Principles of Data Analysis.* Volume 3 of *The Collected Works of John W. Tukey.* Edited by Lyle V. Jones. Monterey: Wadsworth & Brooks, 1986.

Thucydides. *The History of the Peloponnesian War.* Translated by Thomas Hobbes. [= *Hobbes's Thucydides.*] Edited by Richard Schalatter. New Brunswick: Rutgers University Press, 1975.

Ulam, S. M. *Adventures of a Mathematician.* Berkeley: University of California Press, 1991.

Von Neumann, John and Oskar Morgenstern. *Theory of Games and Economic Behavior.* Third edition. New York: John Wiley, 1964.

Wald, Abraham. *Statistical Decision Functions.* New York: John Wiley, 1950.