

Legal Origins and Empirical Credibility

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1. INTRODUCTION

Modern empirical microeconomics is the methodological success story of the profession. Angrist & Pischke (2010) suggest the field experienced a “credibility revolution” starting in the 1990s. Because of this revolution, they argue, decision makers started taking empirical economic analysis seriously in policy discussions in a way they had not previously. Their argument explicitly invokes, endorses, and addresses Leamer’s (1983) famous criticism of empirical economics which basically dubbed the field a “con”.

In the midst of this revolution, the legal origins literature starting with La Porta et al. (1997, 1998) indisputably became a juggernaut in both scholarly and policy discussions. La Porta et al. (1998) is the most cited paper in all of economics since its publication.¹ Organizations like the World Bank used this research to formulate their recommendations concerning institutions to developing countries. From both a scholarship and a practical standpoint, it is not hyperbole to claim that this research has had more impact than anything else done by economists in the past two decades.

Surprisingly, the legal origins literature suffers from the frailties identified by Leamer and Angrist & Pischke. Scholarship on legal origins missed the credibility revolution altogether. Unfortunately, given the nature of legal institutions, the tools of modern microeconometrics, or statistical tools more generally, are not suited to shed any light on the relationship between legal origins, or even any specific legal institution, and economic or financial development. Thus, even when seemingly sophisticated approaches like the instrumental variables technique used in Acemoglu, Johnson, and Robinson (2001) or work using panel data techniques such as that described in Armour et al. (2009), we learn nothing about the causal impact of legal origins on financial market development.

The fundamental problem is one of statistical identification. That is, while the legal origins literature has identified a number of correlations between various legal institutions and economic outcomes, it is not possible to claim with any confidence that those correlations provide evidence of causality. The estimated relationships may merely be the result of an omitted variable bias. It is problematic then to draw policy conclusions about the kinds of institutions countries should adopt based on the econometric evidence in this literature.

The empirical tools that drove the credibility revolution in applied micro are not suitable for making causal inferences in this context as suggested in Klick (2010). Panel data approaches are unlikely to work since institutions change very slowly over time, whereas the intuition of panel data methods relies on sharp unexpected natural experiments. When there are sharp changes in institutions, for example as the result of conquests or wars, there is significant reason to believe many other factors are changing beyond legal rules. If these factors themselves are related to growth and development, as suggested by Olson (1984), it is not possible to isolate the effects of the institutions separately from the other factors. Similarly, any instrument that is powerful enough to influence legal institutions is surely going to have its

¹ La Porta et al. (1998) has been cited 1,183 times according to <http://ideas.repec.org/top/top.item.nbcites.html> and 1,667 times according to the ISI Web of Science (both as of 20 May 2011).

own independent effects on development. If the exclusion restriction is not met for the instrument, this instrumental variables approach fares no better than naïve regression analyses. To make matters worse, recent work by Sobel and Coyne (forthcoming) demonstrate how many of the commonly used institutional measures are themselves highly related over time, making it unlikely that we could ever parse out very precise policy recommendations about any particular institution even if we had plausible instruments.

Other modern approaches, like regression discontinuity designs and structural modeling, might hold out more hope at least as conceptual matters, but have as of yet not been used in this area. There are also reasons to be pessimistic about these methods in this context. With respect to structural modeling, we do not generally have particularly well defined theoretical models of institutional choice. Such models would be a prerequisite for structural modeling to work. Regression discontinuity designs, on the other hand, are likely to be constrained by data limitations. If a researcher starts with a dataset with a maximum sample size of one hundred or so countries, it is likely that any promising discontinuity would winnow the usable data to a handful of observations at best limiting both the statistical power of the analysis as well as confidence in its generalizability. That is, we would be left with little more than a qualitative case study without any claim to external validity.

This chapter questions the claim of La Porta et al. that their results are robust despite criticisms based on omitted variables stories in section 2. We then discuss the fundamental empirical problems in this setting in section 3, including the unsuccessful attempts to solve these problems in this set of articles.

2. THE ROBUSTNESS OF LEGAL ORIGINS

We assume that readers of this volume will be familiar with the claims of the legal origins literature. For the best summary of these claims, see La Porta et al. (2008). In addition to summarizing their research in this review, La Porta et al. address many of the criticisms of their work, especially the claims that legal origins is really proxying for or picking up the effects of some cultural effect. Effectively, these arguments are specific forms of the omitted variables bias problem referred to above. The major criticisms suggest that certain cultural attitudes, including those springing from religious beliefs, are correlated with (or perhaps partially cause or maintain adherence to) legal institutions as well as economic development indicators. While not denying that culture matters for development, La Porta et al. argue that these cultural effects are less important than legal origins and controlling for them does not remove the explanatory power of legal origin indicators in regressions of various development metrics.

To make their case, La Porta et al. (2008) provide Table 4 (p. 312) which presents regressions including various cultural indicators in their original model specifications in explaining the aggregate creditors' rights scores presented in Djankov et al. (2007). This sensitivity analysis shows that, generally speaking, the cultural variables do not generate statistically significant coefficients, and the negative effect of French and Scandinavian legal origins remains statistically significant and economically important in determining the rights of creditors in various countries as measured in 2002.² From this, La Porta et al. (2008) conclude 'Cultural variables, then, do not make much of a dent in the explanatory power of legal origins.' (p. 311).

² The original La Porta et al. papers (1997, 1998) examined creditors' rights and this examination was carried out on a broader sample of countries in Djankov et al. (2007).

This kind of sensitivity analysis is exactly the kind of critical approach suggested in Leamer (1983) where scholars were implored to present multiple specifications of their models so readers could judge the underlying robustness of the evidence, adjusting their confidence in any conclusions drawn accordingly. The idea is that in the absence of a credible identification strategy, which Leamer took to mean an experimental design, scholars should test the fragility of the results from their non-ideal designs. Intuitively, in a perfectly designed randomized experiment, specification changes will not change the estimation of treatment effects. Additional (or different) control variables may affect the precision of any estimates, but since randomization ensures there will be no omitted variables bias, the estimates themselves will not change in expectation. If results non-experimental results endure through various specifications, we gain confidence by extension from the experimental intuition.

However, La Porta et al. (2008) do not go as far as Leamer suggested. While they add a cultural variable separately in each specification, Leamer (and other Bayesians) would prefer to see multiple estimates including all the various permutations of the various variables that plausibly affect the outcome variable and might be correlated with the explanatory variable of interest. From these multiple estimates, Leamer suggests that scholars present the bounds of their coefficient estimates.

In Table 1 below, we recreate part of the La Porta et al. (2008) table 4.³ The coefficients presented differ slightly from those presented in their article because of some data revisions that have occurred in the meantime. We use the data conveniently provided by Andrei Shleifer on his website.⁴

The dependent variable is the aggregate creditors' rights score from Djankov (2007) which is a 0-4 scale, with a mean of 1.7, where a higher score represents more secure rights. The models also include indicators for French legal origin (mean = 0.59), German legal origins (mean = 0.14), and Scandinavian legal origins (mean = 0.03). The model also controls for 2002 per capita GDP in purchasing power terms (mean = 9048). The cultural variables examined are as follows: 1) % catholic which is an indicator variable taking the value of one if the largest proportion of the country's population practices the Roman Catholic faith (mean = 0.32); 2) power distance index which is a metric indicating the extent to which the individuals in a country submit to authority with a higher score indicating a more authoritarian regime (mean = 56); 3) individualism which captures a country's focus on individual rights and attitudes (higher score) as opposed to a more communal orientation (mean = 45); 4) uncertainty avoidance index which gauges a country's comfort with uncertainty with a higher score indicating a lower comfort level (mean = 66); and 5) masculinity measures the degree to which the country reinforces the traditional role of males with higher scores implying more differentiation in sex roles (mean = 51). Metrics 2 through 5 were developed by Geert Hofstede.⁵

Table 1:					
Replication of La Porta et al. (2008) Table 4					
	(1)	(2)	(3)	(4)	(5)
Dependent Variable: Creditors' Rights in 2002					

³ We omit some of the cultural variables examined but our main point about fragility applies when those variables are examined as well.

⁴ Available at http://www.economics.harvard.edu/faculty/shleifer/files/JEL_%20web.xls. For this analysis, we use the data listed as Table 4, but the income measure included in that worksheet of the spreadsheet is not the one used in the article's Table 4 (the article uses 2002 income but the worksheet includes 1995 income). For the income variable, we use the 2002 income presented in the Table 6 worksheet.

⁵ Data available at http://www.geert-hofstede.com/hofstede_dimensions.php.

% catholic	-0.25 (0.22)				
Power Distance Index		-0.00 (0.01)			
Individualism			-0.01 (0.01)		
Uncertainty Avoidance Index				-0.01 (0.01)	
Masculinity					-0.02* (0.01)
French Legal Origin	-0.76*** (0.24)	-0.84** (0.35)	-0.92** (0.34)	-0.48 (0.40)	-1.02*** (0.37)
German Legal Origin	-0.25 (0.27)	-0.52 (0.45)	-0.56 (0.43)	-0.25 (0.45)	-0.28 (0.43)
Scandinavian Legal Origin	-1.10** (0.48)	-0.87 (0.58)	-0.87 (0.58)	-0.92 (0.55)	-1.69** (0.70)
Log(GDP per capita in 2002)	0.28*** (0.08)	0.19 (0.24)	0.32 (0.21)	0.22 (0.19)	0.20 (0.20)
Constant	0.00 (0.73)	0.65 (2.50)	-0.27 (1.80)	0.74 (1.65)	1.70 (1.88)
Observations	136	52	52	52	52
R-squared	20%	14%	15%	16%	19%

Note: Data come from http://www.economics.harvard.edu/faculty/shleifer/files/JEL_%20web.xls .

Heteroskedasticity robust standard errors presented in parentheses.

*** p < 0.01 (two sided test of zero null hypothesis)

** p < 0.05 (two sided test of zero null hypothesis)

* p < 0.10 (two sided test of zero null hypothesis)

In Table 2, however, we show what happens when more than one cultural control is used in any given specification.

Table 2:	
Robustness of La Porta et al. (2008) Table 4	
Dependent Variable: Creditors' Rights in 2002	
Catholic	-0.79** (0.36)
Power Distance Index	-0.00 (0.01)
Individualism	-0.00 (0.01)
Uncertainty Avoidance Index	-0.01* (0.01)
Masculinity	-0.02 (0.01)
French Legal Origin	-0.00 (0.39)

German Legal Origin	0.19 (0.39)
Scandinavian Legal Origin	-1.86*** (0.68)
Log(GDP per capita in 2002)	0.33 (0.22)
Constant	1.10 (2.44)
Observations	52
R-squared	29%
<p>Note: Data come from http://www.economics.harvard.edu/faculty/shleifer/files/JEL_%20web.xls . Heteroskedasticity robust standard errors presented in parentheses. *** p < 0.01 (two sided test of zero null hypothesis) ** p < 0.05 (two sided test of zero null hypothesis) * p < 0.10 (two sided test of zero null hypothesis)</p>	

We see that, in this specification, the Catholic indicator, which equals one if the largest proportion of the population practices Catholicism, is statistically significant and quite large in magnitude. In fact, it is of a larger magnitude than the effect of having a French legal origin. The French legal origin indicator is no longer statistically significant and, in fact, is a pretty precisely estimated zero effect. The effect of German legal origin, while still statistically insignificant, now has a point estimate of the opposite sign from the one this variable had in the La Porta et al. regressions. The Scandinavian legal origins indicator now carries a negative coefficient with a point estimate that is much larger in magnitude than that estimated by La Porta et al., and it is still statistically significant.

These results belie La Porta et al.'s claims of robustness. Note that we are not simply saying that the legal origin variables lose their significance due to multicollinearity or a loss of degrees of freedom. The point estimates themselves change substantially and, in some cases, actually experience sign reversals. Also, as suggested by the Catholic variable above, in some specifications, the cultural variables can be statistically significant and suggest practically large effects, though we are quick to point out that the cultural variables do not generate robust effects either.

One response could be that it was appropriate for La Porta et al. to leave out the other cultural variables when they test a new one, given that they found those variables to be statistically insignificant. Such an approach would be incorrect.⁶ While one might be forced into a trade-off between a better specified model and a more parsimonious one when sample sizes are small, a lack of precision does not seem to be a large problem here. Further, even when one makes such a trade-off, it is important to keep misspecification problems in mind when conclusions are drawn. Also, La Porta et al. retained the German legal indicator variable and the income variable in their models despite their general lack of statistical significance, perhaps suggesting a lack of concern regarding degree of freedom or precision problems.

Table 3 provides the results of a more systematic robustness analysis. For the analysis, we ran all permutations of the regression of creditors' rights in 2002 on four core variables that were included in every regression, namely French, German, and Scandinavian legal origins indicators and log GDP per capita in 2002, and all possible combinations of the cultural variables included in La Porta et al. (2008)

⁶ See, for example, Cameron and Trivedi (2005) p. 286.

Table 4. These cultural variables include the ones investigated above – Catholic, Power Distance Index, Individualism, Uncertainty Avoidance Index, and Masculinity – as well as the six other metrics investigated by La Porta et al. (2008) in Table 4. These other metrics come from the World Value Survey and represent the fraction of the country’s respondents agreeing with the following statements: 1) child obedience is important (mean = 0.37); 2) child independence is important (mean = 0.51); 3) parents must do their best for children (mean = 0.75); 4) parents must be respected regardless of the situation (mean = 0.77); 5) family life is very important (mean = 0.89); and 6) strangers can generally be trusted (mean = 0.28).

The Table provides the following information: 1) the estimated value of the coefficient in a regression that includes none of the cultural control variables (baseline); 2) the mean value of the estimated coefficient (mean); 3) the maximum value of the estimated coefficient (max); 4) the minimum value of the estimated coefficient (min); 5) the average standard error (SE).

	Baseline	Mean	Max	Min	SE
French Legal Origin	-0.85***	-0.05	1.12	-1.04	0.59
German Legal Origin	-0.30	0.07	1.14	-0.82	0.70
Scandinavian Legal Origin	-1.03**	-1.63	-0.56	-2.57	0.82
Log(GDP per capita in 2002)	0.25***	-0.04	0.41	-0.36	0.31
Catholic		-0.86	-0.25	-1.25	0.49
Power Distance Index		-0.02	0.00	-0.03	0.01
Individualism		0.00	0.01	-0.01	0.01
Uncertainty Avoidance Index		-0.01	0.00	-0.02	0.01
Masculinity		-0.02	-0.02	-0.03	0.01
% agree child obedience is important		0.20	1.68	-1.80	1.85
% agree child independence is important		1.93	2.70	1.10	1.16
% agree parents must do their best for children		0.10	2.78	-2.74	2.76
% agree that parents must be respected		-0.93	0.43	-2.17	1.34

regardless					
% agree family life is very important		1.90	3.47	0.27	2.77
% agree strangers can generally be trusted		0.12	1.97	-1.29	1.47

This sensitivity analysis solidifies the claim that the effect of French legal origin on creditors' rights is not robust. The average effect in a given specification is very close to zero (-0.05) and swings from a maximum of 1.12 to a minimum of -1.04. The German legal origin effect is equally fragile. Only the Scandinavian legal origin effect exhibits any stability with all coefficients bearing a negative sign. By way of comparison, the Catholic cultural indicator, along with a number of the other cultural metrics, always generates a negative sign and exhibits a lower degree of volatility. This is surprising given La Porta et al.'s (2008) claim that the legal origin effects are robust and maintain their importance when including cultural controls.

Examining the bounds of effects may be misleading, however, since it could mask a general importance behind some extreme outlier results. Table 4 provides statistics regarding how frequently a coefficient was statistically significant (at the 5 percent level), and how frequently it took a given sign in the robustness regressions, as well as the average t statistic the coefficient generated.

	%(Significant)	%(Positive)	%(Negative)	Average T	Number of Specifications
French Legal Origin	0.05	0.45	0.55	0.75	2048
German Legal Origin	0	0.50	0.50	0.50	2048
Scandinavian Legal Origin	0.51	0	1	1.96	2048
Log(GDP per capita in 2002)	0.00	0.40	0.60	0.45	2048
Catholic	0.24	0	1	1.771	1024
Power Distance Index	0.21	0.00	1.00	1.67	1024
Individualism	0	0.63	0.37	0.29	1024
Uncertainty Avoidance Index	0	0.00	1.00	0.91	1024
Masculinity	0.05	0	1	1.69	1024
% agree child obedience is important	0	0.61	0.39	0.33	1024
% agree child	0.23	1	0	1.69	1024

independence is important					
% agree parents must do their best for children	0	0.50	0.50	0.50	1024
% agree that parents must be respected regardless	0.00	0.05	0.95	0.74	1024
% agree family life is very important	0	1	0	0.68	1024
% agree strangers can generally be trusted	0	0.55	0.45	0.36	1024

This table makes it abundantly clear that the effects of the French and German legal origins indicators are not at all robust to the inclusion of La Porta's et al.'s (2008) chosen cultural indicators. The French indicator is statistically significant only 5 percent of the time, and the German effect is never statistically significant. Furthermore, each is effectively as likely to generate a positive coefficient as the negative coefficient the authors make so much of in their empirical argument. The Scandinavian legal origin effect is robustly negative, always taking a negative sign, and meeting the threshold for statistical significance in half of the specifications. Note that this does not necessarily imply that the true effect of Scandinavian legal origins is negative given the potential for omitted variables bias. It does provide some confidence that there is a systematic effect of Scandinavian, if there is no such bias. A similar claim cannot be made for the French and German indicators.

Contrary to La Porta et al.'s (2008) claims, some of the cultural variables do appear to have a systematic and potentially important effect (though, of course, the observed effect could suffer from omitted variables bias). For example, the effect of the Catholic indicator is always negative, and it is statistically significant one fourth of the time. The estimated power index effect is almost always negative, and it is statistically significant almost as often. The effect of the variable measuring the fraction of the population agreeing that child independence is important is uniformly positive, and it is a statistically significant predictor in about a quarter of the specifications. The one cultural variable that La Porta et al. (2008) suggest could be important, masculinity, is statistically significant just 5 percent of the time, but it does always generate a negative coefficient.

Our purpose in this analysis is not to suggest that the "right" specification includes these cultural variables or that we can be confident of any particular source of bias. We merely point out that this particular set of estimates, used by La Porta et al. (2008) to demonstrate robustness falls far short of this aim. Presumably, we could go through the other tables in that article which purport to show that the legal origins results endure when other hypothesized relationships are controlled for, and we could show fragility there too. We refrain from doing so, leaving the results above as an illustration of what we believe is a general phenomenon.

However, even if the results had been shown to be robust, we would still be skeptical of claims of causality. Robustness is a necessary condition for drawing strong causal inferences, but it is not sufficient. Results that are robust to the inclusion of a given set of covariates can still suffer from other sources of bias. Because the La Porta et al. results (really all of the results in this literature) lack a reliable research design, there is no confidence in their identification strategy and, therefore, no confidence in the causal inferences drawn.

3. WHAT'S THE PROBLEM?

The fundamental problem in modern empirical economics is one of statistical identification. That is, how can one isolate the causal effect of x on y . If one knew everything that affected y , and had data for all of those variables, this would be a trivial problem to solve. In that context, a regression that included all of those variables would yield causal estimates that would be unbiased. Unfortunately, economic theory is not nearly so well developed that any researcher could possibly know all of the important variables affecting the outcome of interest if that outcome is even moderately complex.

Further, even if theory or intuition did provide a well-developed guide, many of the important variables would be difficult to quantify or, at least, to get data that captured them. While proxies sometimes exist, they are never perfect.

If some important variables are left out of the empirical analysis, either due to a failure of theory or a failure of data collection, if any of those omitted variables are correlated with the included variables, the estimates for the included variables will be biased. That is, even in very large datasets, the estimated coefficients will not converge to the true parameter value. The estimates will be systematically too large or too small. While it goes by many names, endogeneity, reverse causality, sample selection, simultaneity, etc., this omitted variable bias problem is ubiquitous.

Intuitively, if an omitted variable is correlated with the outcome variable and a given regressor, the coefficient estimated for the regressor will represent both the true effect of that variable and some portion of the effect of the omitted variable.

While the omitted variable bias problem is wide spread, in some contexts it can be mitigated or eliminated. The credibility revolution referred to above came about due to research designs that were successful in addressing this source of bias. The basic idea behind most of the modern approaches to statistical identification is to mimic an experiment. In an experimental setting, explanatory variables (generally a "treatment") are randomized across observations or subjects. Because of randomization, any uncontrolled for determinant of the outcome will necessarily be uncorrelated with the analyzed determinants (specifically, administration of the treatment).

Experiments are generally not possible in social science and many policy situations. Random assignment of things like education or health care or political rights is viewed as immoral. Even if this morality constraint is satisfied, it is difficult to ensure random assignment if the subjects have other avenues to receiving the treatment or if the randomization can be undermined. Lastly, cost concerns may limit the ability of researchers to engage in broad social experiments.

To remedy this, modern empirical micro researchers focus on natural experiments or quasi-randomizations wherein some quirk of nature or policy leads some individuals (or jurisdictions) to be affected by a policy while other, presumably comparable, individuals (or jurisdictions) are not. The

outcomes for the affected group are examined before and after the quasi-randomization and this difference in outcomes is compared to the simultaneous difference in outcomes in the non-affected group over the same period. The so-called difference-in-difference measure represents the treatment effect of the policy intervention. A similar intuition supports the use of instrumental variables techniques wherein a particular instrument affects the regressor of interest but it does not directly affect the outcome studied. In a two stage process, wherein only the exogenous part of the variation in the regressor is used to explain the outcome (and the exogenous portion is separated from the endogenous portion by modeling the regressor using the instrument) and yield a causal estimate of the effect of x on y .

The use of these natural experiment or instrumental variable techniques is not possible in institutional settings like that found in the legal origins literature. First, because institutions only change slowly over time, and foundational factors like the source of origin of the system do not generally change at all, there is no experiment to examine. In some rare instances where a shock to the institutions might occur, such as happens after a war, it is very likely that the war itself has direct implications for most development indicators. In that case, it will be impossible to parse the effects of the war from the effects of the institutions. Any variable that has the power to change institutions almost certainly will have independent effects on lots of things having to do with the given society. By definition then, the instruments or experiment are not valid and there can be no confidence in any results coming from such designs.

Two other modern approaches offer some hope. Regression discontinuity designs focus on observations just above and below some decision threshold that determines treatment status. The intuition is that if the threshold was arbitrarily chosen (with some general range), subjects just above and just below that threshold are likely to be very comparable with the only difference being that the one group received treatment while the other did not. In the context of legal origins, such a design would require focusing attention on, for example, pairs of colonies where a given country planned to colonize both but for budget reasons chose only one and the choice of which one was decided essentially by a coin flip. Comparing the development of these two places would provide a very nice design for inferring the causal effects of legal origins. The limitation, of course, is that it is unlikely that we have enough information to say with confidence whether any given pair of colonies fits this bill. While historical records may provide clues about which colonies were being considered, it is unlikely that we can confirm that the choice among the set was truly random. If it was not random, the choice was likely influenced by unobservable variables which could have long lasting effects on development. Any estimate of a coefficient would contain part of these effects, depriving us of a causal inference.

Even if we could gain confidence about such pairs, they are likely to be very limited in numbers. This limitation would preclude us from having confidence that our estimated results would be generalizable. Over a large enough sample, idiosyncrasies will wash out, but there is no guarantee of that happening when sample sizes are limited.

One last tool of modern microeconometrics is structural modeling which attempts to use theory to adjust parameter estimates for any source of bias due to missing data. For example, if we are concerned that self-selection problems bias the estimated treatment effect of a job training program, a structural approach would use microeconomics to model the training enrollment decision and use that information in the estimation process to account for the unobserved heterogeneity that leads to the bias. In the legal origins context, one would need models of legal and institutional development to

address the omitted variables problem. While such models exist, they tend to be underspecified, suggesting that much more theory work needs to be done before this becomes a viable approach.

It seems then that the credibility revolution has passed the legal origins literature by primarily because the tools developed in modern empirical micro are not well suited for this application. Unfortunately, there does not appear to be a feasible alternative approach that provides any empirical confidence.

4. CONCLUSION

Despite its tremendous influence in both academic and policy circles, the empirical estimates from the legal origins literature are simply not credible. The cross sectional research designs used surely suffer from omitted variables biases. As an illustration of this, we showed how La Porta et al.'s (2008) results, which supposedly showed robustness, are extremely fragile. This is a symptom of a poorly identified relationship. While the legal origins literature raises important, perhaps among the most important, questions, these questions are not amenable to serious empirical examination.

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