Contracts Do Matter: Robust Evidence of an Optimal Level of Legal Formalism in Developing Countries

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Abstract

Research on how political institutions impact economic growth consists largely of cross country regressions with severe econometric limitations. This paper looks at the question of how institutions impact household wealth, which is a different but arguably more useful question for analysis. Households in large surveys are independent random variables and do not choose their institutions, thus reducing the problems of small sample sizes, closely related variables and endogeneity that plague cross country work. Results show a strong quadratic effect of legal formalism on household wealth, contrary to the literature that shows no impact of formalism on growth. Household analysis also shows smaller impacts of property rights than found in the literature. Results are robust to inclusion of controls for other institutions, geography, economic indicators, historical factors and democracy.

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Contracts do Matter

Suppose a researcher plans to analyze the impact of different healthcare systems on cancer survival rates. What would a sound empirical approach look like? A first take would be to use country data on average survival rates, code different types of healthcare systems and put together cross country regressions. Regressions of survival rates on healthcare systems and country level controls could potentially estimate the impact, but econometric problems would be immense. The sample would be small, the units of analysis would not be independent and nearly all the variables would highly correlated aggregates leading to major endogeneity problems.

A good researcher might consider a second approach of using individual data of cancer patients. Controls could then be included for individual characteristics such as gender, socioeconomic status, age, education level, etc., along with the country level variables including the type of healthcare systems. Such an approach is akin to an experiment, where the individuals in each country are observations and the countries are the experimental groups. While some econometric concerns would remain, it seems clear that this second approach is stronger empirically than the first. The variable of interest is not endogenous to individual outcomes, the units of analysis are independent random variables and the sample would be larger.

The literature on institutions and economic development is surprising in regards to empirical methods; it relies almost exclusively on cross country regressions structured like the first example. Although not perfect, data is available at the household level that is comparable across countries, allowing researchers to carry out analysis similar to the second example. This paper carries out such analysis: regressions on household wealth in a sample of 64 developing countries with over one million observations are used to analyze the impact of institutions on household wealth.

This analysis sheds light on a continuing puzzle in the literature on institutions and development: the lack of empirical evidence that contracting institutions are important for wealth (Acemoglu and Johnson 2005). The new institutional economics view of economic history emphasizes the importance of contracts in terms of transactions costs and trading over time and in more complex ways (North 1994, Williamson 1975, 2002). Cross country research that measures the transactions costs of excess formalism, however, indicates that property rights are the key drivers of long term growth (Djankov, et al. 2003, Acemoglu and Johnson 2005, Acemoglu, Johnson and Robinson 2001). Other research links contracting institutions with differences in vertical integration and financial development (Du, Lu and Tao 2012; Acemoglu, Johnson and Mitton 2009), but shows no direct impact of contracting institutions on wealth.

This paper presents evidence that contracting institutions, measured by Legal Formalism, do have a significant impact on household wealth that is quadratic in form. The negative squared term captures the higher transactions cost of formalism, but the positive linear term indicates substantial benefits from formal institutions. The most likely explanation for these benefits is that formal institutions allow for more trades to take place among anonymous participants and over time (North 1994, Dixit 2004, Williamson 1975). There is a high cost of a lack of formal contracting institutions; it is the opportunity cost of trades that cannot be completed without them.

The evidence of a strong, robust effect of Legal Formalism is found by analyzing the impact of institutions on household wealth across 64 developing countries. The question of how institutions impact household wealth is slightly different from what has been studied in the literature, which analyzes the impact of institutions on aggregate growth. The motivation for this empirical approach comes from both theoretical and econometric arguments for household level analysis. In theory institutions impact micro behavior: contracting institutions allow for mutually beneficial trades to be consummated between individuals and firms (North 1994, Williamson 2002); property rights institutions lower uncertainty and encourage investment (Acemoglu and Johnson 2005). A household or firm level outcome is thus closer to the theoretical frameworks for how institutions impact long term development and worthy of careful analysis.

The econometric argument for household analysis was outlined in the cancer analogy above. Regressions on household wealth are much closer to the experimental ideal than cross country regressions and thus provide more useful estimates. The concern is often raised that regressions on household wealth and aggregate means are mathematically the same as regressions of mean wealth on the aggregate variables; however it is easily shown that this is not the case when household controls are included (Green 2013). The household level assessment of institutions can thus provide valuable evidence as to how institutions matter. The evidence here is that contracting institutions have a significant quadratic impact on wealth, which could not be easily identified in cross country regressions. This nonlinear estimated effect is robust across a number of specifications. Property rights institutions are estimated to have a small impact on household wealth.

Background

The literature on institutions and growth, following Acemoglu, Johnson and Robinson (2001), has focused on property rights institutions, especially given the evidence that property rights institutions are more important for long term growth (Acemoglu and Johnson 2005). The argument for property rights institutions is straightforward: private property must be protected for markets to function at all.

The literature focuses on investment; De Soto (2000), for example, is well known for arguing that land titles are critical for development because they provide greater incentive for investment that improves the productivity of the land. While property rights are important and necessary for some investments; contracts are necessary for firm investment of many forms as well. Complex contracts underpin most long term relationships that large firms need to commit resources to investment. The literature has understandably moved away from analyzing contracting institutions because the cross country evidence indicated insignificant linear effects at the country level. However, the evidence here is of a strong quadratic effect at the household level, which is not inconsistent with a weak linear effect at the country level.

A key literature in institutional economics emphasizes the importance of contracts (Grossman and Hart 1990, North 1994). Led by Coase (1937), who pointed out that many economic decisions and actions are not made through the market, and solidified by Williamson (1975, 2002), who studied hierarchal economic relations in detail, this research argues that the creation and enforcement of contracts is critical for economic activity. North (1994) stresses the importance of institutions that "permit anonymous, impersonal exchange across time and space (p. 362)." The implication is that there may be high opportunity costs of poor contracting institutions.

Dixit (2004) captures this key insight in a game theoretic model dubbed "relation versus rule based governance." The model is a game where a continuum of players are positioned on a circle. When any two players interact they play a prisoner's dilemma game. There are two mechanisms that allow for cooperation. The first is social sanctioning, which is effective in small groups. This is modeled as each player having a neighborhood around them of people they trust and will cooperate with, while outside of this neighborhood they will not cooperate. The intuition is that reputation mechanisms can insure cooperation in small groups, villages for example, where interactions will be repeated, if not with the exact same player then with someone else who knows everyone's reputation. The second mechanism is a system for anonymous monitoring, similar in intuition to the credit rating system in the U.S. Here any player can pay a cost to learn the type of any other player. This system allows for efficient outcomes in large groups, but is generally too costly to implement in small populations. Thus there are several equilibria possible. For small populations the reputation mechanism is efficient and for large populations the credit system is efficient. As one moves from a small population to a large population there is an intermediate range where neither system is efficient. This model thus indicates excessive costs to formalism in homogenous, low population areas but gains from more formal mechanisms in heterogeneous areas with larger populations (Dixit 2004).

Research on contracts is often highly detailed and specific in terms of modeling transactions and therefore does not lend itself to easy measurement in aggregate. The leading indicator of contracting institutions is the formalism index developed by Djankov et al. (2003). They surveyed law firms in as many countries as possible about the intricate details of the process for collecting a bad check. The index is comprised of seven components, each of which is scaled from zero to one, where one indicates a formal institution and zero represents the "neighbor model." Djankov et al. (2003) and Acemoglu and Johnson (2005), who use the index as well, argue that the "neighbor model," which involves informal arrangements such as oral rather than written arguments, no opportunities for appeal to higher authorities, laypeople rather than professional lawyers, etc., is preferable economically because it indicates lower costs of simple transactions. They hypothesize that legal formalism will therefore have a negative impact on GDP per capita; Acemoglu and Johnson (2005) however find no significant effect of formalism in cross country regressions once they instrument for legal formalism and control for property rights.

It is a puzzle in the literature that contracting institutions have not been shown to have a large impact on economic development because they play a large role in the institutional economics view of long term growth (North 1994, Williamson 1975). The lack of significant findings on contracting thus far may stem from problems with measurement and identification. The formalism index was hypothesized to have a negative impact on wealth because it captured the costs of undue regulation and formality in the market. Indeed, research on legal origins and contractual formalism (Djankov, et al. 2003) along with the institutional economics view that transactions costs were important led to the World Bank's annual "Doing Business" survey. The survey, which began in 2003, initially measured formalism in much the same way as Djankov et al. (2003), along with other measures of the costs of doing business. After the first year, the "Doing Business" survey dropped the formalism index and added other aspects of business costs, presumably because they measure more directly the relevant aspects of contracts and regulation for business growth (or lack thereof). The 2012 report, for example, includes data on the number of procedures, cost in time and monetary cost of getting construction permits, getting electricity, registering a business, registering property, enforcing a contract, and several other aspects of the business environment.

Somewhat surprisingly, Legal Formalism does not correlate strongly with most of the variables measured in the doing business report. In the sample used in this paper, formalism correlates significantly with only six variables from the more than fifty in the "Doing Business" dataset. The variables that do correlate strongly with formalism are the number of procedures to start a business

(0.25) the time in days to start a business (0.26), the number of times in a year taxes are paid (0.23), the percentage of adults covered by public credit registries (0.28), the index of the strength of legal rights (-0.42) and the index of investor protection (-0.29). Notable variables that do not correlate strongly with formalism include costs and procedures for enforcing contracts, registering property, resolving insolvency, getting electricity, getting construction permits and registering a business. It is surprising that formalism is negatively correlated with the indices measuring the strength of legal rights relating to credit and the strength of investor protection since these variables measure formal legal mechanisms to encourage lending and investment.

There is some indication then that the index of Legal Formalism does not directly measure costs of doing business. While formalism is strongly correlated with some of the "Doing Business" variables, its quadratic impact on wealth is robust to inclusion of these variables, indicating that some other aspect of Legal Formalism has a strong effect on wealth. This effect likely stems from the greater possibilities for economic interaction that come with formalism. While there are extra enforcement costs from undue formalism; there are two clear benefits as well: more trades can take place, especially with anonymous partners, and more complex, long-term contracts can be written.

Acemoglu and Johnson (2005) argue that contracting institutions are not directly important for growth because individuals can find ways around poor contract enforcement. If the court system is inefficient and corrupt, for example, traders can rely on family networks and reputation and still trade. While valid, this argument runs counter to the key insight of the literature on contracting institutions (Williams 1975, North 1994), namely that complex contracts are an inherent part of any modern economy. Formalism is necessary for complex, long term contracts to be written and enforced; there is thus a high opportunity cost of poor contracting institutions.

While there is clearly some efficiency cost to too much formalism, the literature has perhaps overemphasized the costs of legal formalism. A key function of institutions is to "permit anonymous, impersonal exchange across time and space (North 1994 p. 362)," which may necessitate formal enforcement mechanisms. Williamson (1975, 2002) argues that contracts are critical for hierarchal relationships, which are foundational for the growth of firms. Fafchamps and MInten (2001), for instance, argue that economies in many African countries are characterized by pervasive market exchange similar to the "neighbor model." The result is a lack of long term contracts and hierarchical relationships. These features are indicative of a low level of economic development and poor institutions however, not excessive costs of formal contracting. The evidence here indicates an optimal level of Legal Formalism, which indicates both costs and benefits of formal contracting institutions.

Methodology

One of the first concerns raised about household level analysis of aggregate institutional variables is whether it is really different at all from aggregate analysis. If the key regressor is at the aggregate level, the argument goes, then the coefficient of interest is identified from the aggregate variance in the dependent variable and there is no difference between household and aggregate analysis. It is easily shown that this argument is correct only when there are no household level control variables. Consider a 1 x n dependent variable y at the household level and let X be a 2 x n matrix of independent variables where X_1 is a household level variable and X_2 is an aggregate variable.

We begin with the least squares estimator

$$\beta = (X'X)^{-1}(X'y) \qquad (1)$$

Expansion of (1) gives

$$\beta_2 = [X_1'X_2X_2'y - X_2'X_1X_1'y]/\det(X'X) \qquad (2)$$

where β_2 is the coefficient of interest on the aggregate variable. Now consider two cases. In the first case, let y and X_1 be the aggregate means of the household variables. In the second case, let y and X_1 remain household variables. In case one, expansion of each part of (2) gives:

$$X_1'X_2X_2'y = [X_{2c1}\Sigma_{c1}X_1/j+\ldots+X_{2cm}\Sigma_{cm}X_1/j]*[X_{2c1}\Sigma_{c1}y/j+\ldots+X_{2cm}\Sigma_{cm}y/j] \qquad (3)$$

$$X_2'X_1X_1'y = [X_{2c1}\Sigma_{c1}X_1/j+\ldots+X_{2cm}\Sigma_{cm}X_1/j]*[\Sigma_{c1}X_1\Sigma_{c1}y/j^2+\ldots+\Sigma_{cm}X_1\Sigma_{cm}y/j^2] \qquad (4)$$

Where c stands for country and there m countries, each of which has j households. In case two, we have

$$X_1'X_2X_2'y = [X_{2c1}\Sigma_{c1}X_1+\ldots+X_{2cm}\Sigma_{cm}X_1]*[X_{2c1}\Sigma_{c1}y+\ldots+X_{2cm}\Sigma_{cm}y] \qquad (5)$$

$$X_2'X_1X_1'y = [X_{2c1}\Sigma_{c1}X_1+\ldots+X_{2cm}\Sigma_{cm}X_1]*[X_{11}y_1+X_{12}y_2+\ldots+X_{1n}y_n] \qquad (6)$$

It is clear that the first term in the numerator of β_2, $X_1'X_2X_2'y$, differs across the two cases only by a factor of j^2. That is (3) is equivalent to (5)/j^2. However, the second term in β_2, $X_2'X_1X_1'y$, is substantively different across the two cases. The first part, $X_2'X_1$, differs only by a factor of j, but the second part, $X_1'y$, is truly different. In case one $X_1'y$ consists of the country means of X_1 times the country means of y. Expanding these terms gives an expression that includes each x_{1i} multiplied by y_i and by y_j for all other j in the same country. In case two, the $X_1'y$ term consists of each x_{1i} multiplied only by y_i. Thus this last term differs substantively across the two cases. It follows that in general β_2 is different in the two cases and thus regressing regional variables on household data is not equivalent to regressing regional variables on regional means so long as household controls are included.

Analyzing institutions at the household level is therefore fundamentally different from country level analysis; the regressions on household dependent variables with household controls estimate a different coefficient than that estimated in country level regressions. The analysis here asks a slightly

different question than what has been analyzed before: how do institutions impact household wealth? This question is worth asking because institutions in theory should impact micro level behavior and because the impact of institutions on household can inform policy.

The cross country literature estimates the following equation:

$$y_c = \alpha I_c + X'_c\beta + \mu_c \quad (7)$$

where y is the dependent variable of interest, usually GDP per capita or growth, I is the independent variable of interest and X is a vector of controls (the c subscript indicates a country). Ideally, equation (7) estimates the average impact of institutions on GDP per capita, conditional on country characteristics. The endogeneity of institutions at the country level necessitates instrumental variables, which are quite scarce. Endogeneity concerns also limit the number of controls that can be included; specifically, other macroeconomic indicators such as inflation, trade and aid are not usually considered. Cross country regressions invariably have small samples, non-random and related units of analysis and a host of characteristics, both observed and unobserved, that may impact both institutions and GDP per capita. Estimates of (7) in the literature are thus a far cry from the statistical ideal and are generally treated with a justified skepticism.

The household specification is:

$$y_i = I_c'\delta + X'_c\lambda + Z'_i\gamma + \mu_i \quad (8)$$

where the i subscript indicates a household and Z is a vector of household controls. δ, the vector of coefficients of interest, is the impact of institutions on wealth, conditional on both country and household characteristics. δ thus differs fundamentally from α in (7) because it is conditional on household characteristics. (8) is not plagued by endogeneity in the same manner as (7) because households clearly do not choose their institutions. (8) can then be estimated with OLS and include many institutional variables, country controls and interactions. To alleviate concerns that there may still be some endogeneity or omitted variables problems, instrumental variables estimation of (8) similar to the cross country literature can also be considered for comparison. Indeed, (8) can be compared directly to (7) by weighting the household sample so that each country has an equal probability of being in the sample and dropping the household controls. While such comparisons are informative to establish validity (Green 2012), full household estimation is preferred for the myriad reasons presented earlier.

Using household survey data from many surveys adds some complexity in terms of estimating appropriate standard errors. Each survey includes weights and survey characteristics that are easily accounted for with standard survey estimation tools; however, these weights are not designed for the

combination of many such surveys into one sample. The major concern with the combination is that the standard errors need to be clustered by country to appropriately account for the fact that the country variables differ only between countries. A simple way to account for such clustering in this situation is to treat each survey as though it were sampled in a first stage, out of all the possible countries and years that could have been surveyed.[2] This correction maintains the representative nature of the survey data for each country while also appropriately clustering the standard errors by country. Since the country-years were not actually randomly selected, the results here are not strictly representative of all possible country-years that could have been sampled. However, this problem is common to all cross-country regressions.

Data

The household data come from Demographic and Health Surveys funded by USAID.[3] These randomized household surveys have been carried out in a uniform fashion in many countries; the full sample consists of 64 countries, many of which have multiple surveys ranging from 1993 to 2010. The outcome of interest is a wealth factor score, which is created through minimum distance factor analysis of indicator variables for asset ownership.[4] Such factor scores are commonly used in development economics as reliable indicators of household well-being in the absence of solid income data (Deaton 1997). The wealth variable is standardized. The household controls consist of indicators for living in an urban area, having a female household head and for the highest education level completed by the household head. Table 1 summarizes the household data.

Table 1: Summary of Household Data

Variable	Obs	Mean
Female	1704700	0.23
Urban	1704700	0.43
No Education	1704700	0.26
Incomplete Primary Ed.	1704700	0.25
Complete Primary Ed.	1704700	0.13
Incomplete Secondary Ed.	1704700	0.16
Complete Secondary Ed.	1704700	0.11
Higher Ed.	1704700	0.09

[2] The data cover an 18 year period (1993-2010) and roughly 150 possible developing countries. There are thus 2700 possible "country-years" that could have been included.
[3] www.measuredhs.com
[4] See appendix 2 for details on the factor analysis.

This paper seeks to estimate the impact of country level institutions on household wealth. A good starting point is to consider how much household variation in wealth can be explained by country characteristics. Table 2 presents initial regressions along these lines. The first only includes the household controls from Table 1. Each variable excepting the indicator for female household head is significant and has the expected sign and the r-squared is 0.436. The second regressions adds in country fixed effects to give a rough estimate of how much variation in household wealth can be accounted for by time invariant country characteristics. The household variables all remain significant in the second regression, although most have smaller magnitudes, and the r-squared is 0.702, indicating that country level variation can account for around 26 percent of the variation in household wealth.[5] This paper identifies which country institutions and other characteristics are significant determinants of household wealth in developing countries.

Table 2: Household Regressions

Variable	(1) - Only HH controls	(2) - Country Fixed Effects
Female HH Head	0.039	**0.039**
	(0.025)	**(0.008)**
Urban Residence	**0.914**	**0.681**
	(0.042)	**(0.030)**
Incomplete Primary Ed.	**0.233**	**0.118**
	(0.053)	**(0.012)**
Complete Primary Ed.	**0.439**	**0.259**
	(0.062)	**(0.016)**
Incomplete Secondary Ed.	**0.575**	**0.383**
	(0.051)	**(0.015)**
Complete Secondary Ed.	**0.915**	**0.555**
	(0.051)	**(0.023)**
Higher Ed.	**1.098**	**0.776**
	(0.047)	**(0.034)**
Observations	1704700	1704700
Country Years	190	190
Countries	64	64
R-squared	0.436	0.702

[5] It is worth noting that estimates of the impact of education level on wealth may be subject to bias due to unobserved ability of the household head. Corrections for this problem in this data are not readily available, so the estimates of the impact of education should be treated with some skepticism. However, the country level estimates coefficients are robust to the exclusion of the education variables.

Country Data

Property Rights and Geography

The leading indicators of property rights institutions at the country level are the Executive Constraints variable from POLITY (Acemoglu and Johnson 2005) and the measure of Expropriation Risk from Country Risk Services (Acemoglu, Johnson and Robinson 2001). Freedom House scores are also commonly used (Easterly and Levine 1997) and Betancourt and BenYishay (2010) argue that the Personal Autonomy and Individual Rights measure from Freedom House is the best performing country level variable measuring property rights institutions. There are some other measures available as well, including a Heritage Foundation index of property rights that is also considered. The results are robust to different property rights measures, so Executive Constraints is used in most specifications to be consistent with the literature.

Property rights institutions have one commonly used instrument in country level regressions to account for endogeneity: settler mortality rates from Acemoglu, Johnson and Robinson (2001). Although it is by no means perfect, the settler mortality instrument is the best available for this type of research. It is used here to test for the endogeneity of property rights institutions in household level regressions.

The main result from Acemoglu, Johnson and Robinson (2001), confirmed in later work (Easterly and Levine 2003, Rodrik, Subramnaian and Trebbi 2004) is that once property rights are identified through instrumental variables there is no significant impact of geography on income. However, Green (2012) shows that with adequate geographic controls, there is no need to instrument for property rights in a regression on household wealth and geography has a significant impact on household wealth. The main hypothesized impacts of geography come from disease environment (Sachs 2003) and access to world markets (Collier 2004). The disease environment is captured here through two variables: one that measures the maximum afternoon humidity and another measuring the mean temperature for a country. Access to markets is measured by the percentage of the population within 100km of a coast or navigable river.

Table 3 presents six regressions on household wealth with property rights and geography controls. The first three regressions use the Settler Mortality instrument in IV estimation to compare with the literature that finds instrumentation necessary (Acemoglu, Johnson and Robinson 2001). The findings of Green (2012) are confirmed here; when controls for geography are included it is no longer necessary to instrument for property rights institutions. This result is most clearly seen by comparing columns (3) and (4), which both indicate an insignificant impact of Executive Constraints with or without

instrumental variables. Columns (5) and (6) present similar regressions with Expropriation Risk as the dependent variable and similar results. Tests of endogeneity of (3) and (5) (not shown) indicate no evidence of endogeneity. These initial results provide support for the argument that institutions are exogenous to household wealth. They also show a small impact of property rights institutions and a significant effect of geography. Results are similar with different measures of property rights (not shown). Initial results thus indicate a smaller impact of property rights on household wealth than that reported in the literature on GDP.

Table 3: Property Rights and Geography

Dependent Variable: Wealth	IV(1)	IV(2)	IV(3)	OLS(4)	IV(5)	OLS(6)
Executive Constraints	**0.704**	**0.554**	0.172	0.032		
	(0.385)	**(0.276)**	(0.207)	(0.021)		
Expropriation Risk					0.135	**0.192**
					(0.157)	**(0.035)**
Maximum Humidity	**-0.044**	**-0.035**	**-0.022**	**-0.015**	**-0.015**	**-0.015**
	(0.013)	**(0.010)**	**(0.007)**	**(0.003)**	**(0.004)**	**(0.003)**
Pop. w/n 100km of Coast/River		**0.614**	**0.690**	**0.656**	**0.545**	**0.608**
		(0.261)	**(0.110)**	**(0.099)**	**(0.166)**	**(0.110)**
Mean Temperature			**-0.056**	**-0.038**	**-0.055**	**-0.056**
			(0.020)	**(0.011)**	**(0.012)**	**(0.013)**
Female Household Head	-0.113	-0.054	0.020	**0.039**	**0.057**	**0.037**
	(0.104)	(0.073	(0.046)	**(0.020)**	**(0.020)**	**(0.020)**
Household in Urban Area	**0.455**	**0.551**	**0.755**	**0.867**	**0.864**	**0.872**
	(0.227)	**(0.164)**	**(0.127)**	**(0.043)**	**(0.049)**	**(0.038)**
Incomplete Primary Ed.	-0.051	-0.017	0.131	**0.166**	**0.195**	**0.118**
	(0.186)	(0.151)	(0.095)	**(0.036)**	**(0.043)**	**(0.039)**
Complete Primary Ed.	**0.302**	**0.286**	**0.341**	**0.358**	**0.283**	**0.230**
	(0.144)	**(0.110)**	**(0.057)**	**(0.049)**	**(0.100)**	**(0.041)**
Incomplete Secondary Ed.	**0.366**	**0.360**	**0.430**	**0.557**	**0.434**	**0.439**
	(0.125)	**(0.104)**	**(0.053)**	**(0.044)**	**(0.044)**	**(0.034)**
Complete Secondary Ed.	**0.523**	**0.528**	**0.657**	**0.833**	**0.663**	**0.630**
	(0.176)	**(0.144)**	**(0.097)**	**(0.057)**	**(0.074)**	**(0.038)**
Higher Ed.	**0.626**	**0.666**	**0.822**	**1.011**	**0.869**	**0.886**
	(0.187)	**(0.154)**	**(0.115)**	**(0.047)**	**(0.058)**	**(0.041)**
Observations	1256944	1256944	1256944	1588204	1229541	1453773
Country-years	131	131	131	174	125	150
Countries	40	40	40	57	37	43
R-squared	.	0.0172	0.5532	0.5321	0.5736	0.5730

Contracting Institutions and Corruption

Contracting institutions receive far less attention in the cross country literature, likely because Acemoglu and Johnson (2005) provide evidence that they are not significant determinants of long term growth. This paper uses the index of legal formalism from Djankov et al. (2003) that is also used in Acemoglu and Johnson (2005). This index ranges from 1-7, where one is closest to what Djankov et al. (2003) term the "neighbor model" and seven indicates a high degree of legal formality and intervention from higher levels. An index of "procedural complexity" from the World Bank, introduced as well in Acemoglu and Johnson (2005), is also considered with very similar results. [6]

While Djankov et al. (2003) and Acemoglu and Johnson (2005) expect a negative linear impact of legal formalism, it is likely, as argued previously, that there are benefits to formalism as well. There is naturally some need for formal institutions and there are clear cases (high population density, high degrees of ethnic segregation/ low levels of trust) where more formalism should be more efficient. Linear and quadratic specifications for legal formalism are thus both considered here.

While corruption receives a fair bit of attention in the literature (Treisman 2007), there is not compelling country level evidence that corruption on the whole hinders economic growth. This lack of evidence may come from the lack of a reliable instrument for corruption, or it may be driven by the ambiguous average effect of different types of corruption (Treisman 2007, Green 2011). The leading measure of corruption is the corruption perceptions index from Transparency International. This variable is considered here, along with the control of corruption index from the World Bank.

Table 4 presents seven regressions focusing on contracting institutions and corruption. The first two consider linear and then quadratic effects of Legal Formalism; a strong quadratic effect is found. Columns (3) and (4) show similar results using the procedural complexity index. Equations (5) and (6) consider corruption as well; significant impacts of the Corruption Perceptions Index and the Control of Corruption variable are found. The last column adds an interaction of the Corruption Perceptions Index with an indicator for urban residence, which shows that corruption has a larger impact in urban areas.

The quadratic impact of Legal Formalism indicates a large impact of formalism on wealth that decreases as formalism increases. The marginal effect is zero when formalism is around 4.3, suggesting an optimal level of formalism slightly above the mean of 3.97. The estimated marginal effect of

[6] Both the Legal Formalism Index and the Procedural Complexity Index were measured only once; these variables thus vary across countries but not over time and the same value is used for each country, even if that country has multiple years in the sample. The same is true for any other time-invariant country characteristics. Time varying country variables, such as Executive Constraints, were matched by survey year.

formalism ranges from 0.55 standard deviations of wealth at the minimum level of formalism to -0.3 standard deviations at the maximum level of formalism.

Table 4: Contracting Institutions and Corruption

Dependent Variable: Wealth	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Legal Formalism	**0.080**	**0.730**			**1.191**	**0.550**	**1.157**
	(0.029)	**(0.341)**			**(0.370)**	**(0.330)**	**(0.367)**
Formalism Squared		**-0.081**			**-0.140**	**-0.064**	**-0.135**
		(0.042)			**(0.048)**	**(0.042)**	**(0.047)**
Procedural Complexity			0.018	**0.443**			
			(0.019)	**(0.173)**			
Proc. Comp. Squared				**-0.036**			
				(0.015)			
Corruption Perceptions Index					**0.232**		**0.164**
					(0.066)		**(0.079)**
World Bank Control of Corr.						**0.274**	
						(0.131)	
CPI*Urban Residence							**0.135**
							(0.055)
Executive Constraints	**0.050**	**0.059**	**0.049**	**0.052**	**0.055**	**0.058**	**0.057**
	(0.023)	**(0.023)**	**(0.021)**	**(0.021)**	**(0.030)**	**(0.027)**	**(0.029)**
Maximum Humidity	**-0.022**	**-0.020**	**-0.021**	**-0.019**	**-0.009**	**-0.018**	**-0.010**
	(0.004)	**(0.004)**	**(0.004)**	**(0.004)**	**(0.004)**	**(0.004)**	**(0.004)**
Pop. w/n 100km of Coast/River	**0.663**	**0.625**	**0.596**	**0.599**	**0.807**	**0.785**	**0.810**
	(0.101)	**(0.104)**	**(0.101)**	**(0.099)**	**(0.108)**	**(0.113)**	**(0.107)**
Mean Temperature	**-0.035**	**-0.046**	**-0.046**	**-0.052**	**-0.057**	**-0.039**	**-0.055**
	(0.011)	**(0.014)**	**(0.011)**	**(0.012)**	**(0.019)**	**(0.016)**	**(0.019)**
Household Controls	Included	Included	Included	Included	Included	Included	Included
Observations	1413581	1413581	1528267	1528267	1124279	1023117	1124279
Country Years	142	142	160	160	106	98	106
Countries	40	40	51	51	39	39	39
R-Squared	0.5553	0.5601	0.5537	0.5581	0.5829	0.5784	0.5851

Controls for property rights institutions and geography are included as well and are significant in all specifications. However, the magnitude of the Executive Constraints variable is quite small. A one point improvement in the Executive Constraints score is estimated to improve wealth by around .05 standard deviations. The magnitudes of the geography variables are similar; living in a country with five percent higher maximum humidity is estimated to decrease wealth by around .1 standard deviations. Having an additional 10 percent of a country's population close to a coast or river is estimated to

improve wealth by .07 standard deviations and living in a country with a two degrees Celsius higher mean temperature is estimated to reduce wealth by .08 standard deviations.

The household variables are not reported in Table 4 to save space, but all household controls are present and the urban residence indicator and the education variables are significant and consistent in magnitude through all specifications. The results on these variables are very similar to what is reported in Table 3. Also not shown are several interactions that are not significant. No evidence of quadratic effects of corruption was found, and interactions between corruption and formalism, corruption and Executive Constraints, formalism and urban residence and Executive Constraints and urban residence are all clearly insignificant.

The estimations in Table 4 indicate a strong quadratic impact of Legal Formalism on wealth, as well as significant but smaller in magnitude impacts of corruption, property rights and geography. The next step is to test the robustness of these results to additional controls for economic and historical factors.

Economic and Historical Factors

There are a number of economic and historical factors that may impact household wealth in developing countries. As mentioned previously, an advantage of the household outcome is that macroeconomic indicators are not endogenous and can be included in the regression. Controls for inflation, exports as a percentage of GDP (indicating openness to trade), aid per capita and oil exports (percent of GDP) are considered. Data for these economic indicators comes from the World Development Indicators database.

Historical factors may be important as well and are often considered in research on institutions. Average GDP over the period of 1975-1980 (in log form) is considered as a baseline measure of country wealth. Given the argument that formalism is more important in areas with large populations and/or more heterogeneous groups (Dixit 2004), interactions of formalism with several other variables are considered as well. Interactions with population density and an indicator for living in an urban area test if formalism is more important in areas of high population. Heterogeneity among the population, which may necessitate more formal institutions, is commonly measured through ethnic fractionalization variables (Easterly and Levine 1997). Alesina and Zhuravskaya (2011), however, argue convincingly that segregation of people within countries is likely to be more important than simple fractionalization. They measure segregation of the population based on ethnic, linguistic and religious characteristics. Their country indices of segregation are considered here along with fractionalization as indicators of the degree of heterogeneity of the population.

The literature on contracting institutions discusses legal origin in detail. Djankov et al. (2003) show that countries with French legal origin have on average much higher degrees of formalism and use legal origin as an instrument for formalism. The exclusion restriction for this instrument, however, is not likely to be met because there are many possible impacts of French legal origin on modern development aside from the affect on formalism. Legal origin and an interaction of legal origin with formalism are considered here as independent variables.

Table 5 presents four regressions that add in economic and historical factors. The first, most striking result, is that the quadratic impact of Legal Formalism is robust. Both terms are significant in every specification, even when other interactions with formalism are included. The marginal impact of Legal Formalism at the means of all variables ranges from 0.53 to 0.74 standard deviations, which is consistent with previous estimates. The other institutional variables do not show such robust effects. Executive Constraints is significant in two specifications, with similar magnitudes to earlier estimates. The Corruption Perceptions Index is no longer significant once economic factors are included, suggesting that earlier estimated impacts of corruption were simply capturing economic policy or long run differences in income captured by the average GDP variable. The robust effect of formalism in the presence of this prior average GDP indicates that there are benefits to formalism across a range of income levels in developing countries.

The average GDP from 1975-80 is positive and significant. Other economic factors have mixed effects. Foreign Aid per capita has no significant effect on wealth and openness to trade, as measured by Exports as a percentage of GDP, is positive but small in magnitude. Openness to trade is interacted with formalism and the interaction is negative, suggesting somewhat surprisingly that greater formalism may limit the benefits of openness for household wealth. Although both the openness variable and the interaction are insignificant in most specifications, they are usually jointly significant. The magnitude of the effect is still relatively small, however. Inflation shows a relatively small and usually insignificant effect on wealth. The only other economic factor that does significantly and consistently impact household wealth is oil exports, which have a positive estimated impact but also a relatively small magnitude.

Historical factors are theorized to have a major impact on institutions. It is interesting that Legal Origin and the interaction of Legal Origin with formalism are not significant, nor are population density and its interaction with formalism (results not shown). There are, however, interesting effects of ethnic fractionalization and both ethnic and religious segregation. It has been argued that greater ethnic fractionalization leads to a more divided, conflictual society and thus inhibits economic development

(Easterly and Levine 1997). However, Acemoglu, Johnson and Robinson (2001) find no significant effect of ethnic fractionalization on long run growth. Alesina and Zhuravskaya (2011), argue that fractionalization can be overcome if societies are integrated and trust is developed. They claim that the greater problem for development is segregation, when countries are divided internally into several homogenous groups that are likely to conflict with one another. Alesina and Zhuravskaya (2011) present measures of ethnic, linguistic and religious segregation in many countries and show that greater segregation reduces trust, which then inhibits economic cooperation and growth. Data on trust is not available for a large number of countries, so direct impacts of fractionalization and segregation on wealth are considered.

Ethnic fractionalization is considered linearly in the second regression in Table 5 and has an insignificant effect. The third specification adds in ethnic segregation and an interaction between fractionalization and segregation (linguistic segregation correlates very highly with ethnic segregation so is not included). The direct impact of segregation is negative and significant, which is consistent with the findings of Alesina and Zhuravskaya (2011). Further, the interaction between fractionalization and ethnic segregation is positive and significant, which suggests that segregation is more harmful in less fractionalized societies. This finding is quite intuitive; countries with only two or three large ethnic groups that are highly segregated are likely to be less stable than countries that are more fractionalized with five or more smaller ethnic groups. The final regression in Table 5 adds in an interaction between ethnic segregation and formalism as well as religious segregation in quadratic form. The interaction has a positive, significant estimated coefficient, which shows that formalism can mitigate the problems of segregation. This finding is consistent with Dixit's (2004) model in which formal institutions are needed for anonymous trading among strangers. Religious segregation is significant in negative quadratic form as well, with small levels of religious segregation being harmful but larger ones having no effect or even a slightly positive impact. Interestingly, the geography variables are no longer significant once religious segregation is accounted for, suggested that there may be some relationships between religious segregation and geography. However, sorting out these relationships is beyond the scope of this paper.

Table 5: Robustness Checks: Economic and Historical Factors

Variable	1	2	3	4
Legal Formalism	**1.151**	**1.236**	**1.652**	**1.259**
	(0.324)	**(0.210)**	**(0.199)**	**(0.367)**
Formalism Squared	**-0.126**	**-0.145**	**-0.191**	**-0.187**
	(0.042)	**(0.026)**	**(0.026)**	**(0.048)**
Executive Constraints	0.021	0.028	**0.064**	**0.064**
	(0.024)	(0.018)	**(0.017)**	**(0.018)**
Corruption Perceptions Index	0.086			
	(0.059)			
Aid per capita	-0.001			
	(0.002)			
Oil Exports (% of GDP)	**0.018**	**0.020**	**0.032**	**0.038**
	(0.007)	**(0.006)**	**(0.009)**	**(0.011)**
Exports (% of GDP)	0.018	0.014	**0.031**	0.002
	(0.013)	(0.010)	**(0.007)**	(0.008)
Exports * Formalism	-0.004	-0.003	**-0.006**	0.002
	(0.003)	(0.002)	**(0.002)**	(0.002)
Inflation	-0.0003	**-0.0004**	0.0000	0.0001
	(0.0003)	**(0.0002)**	(0.0001)	(0.0001)
Average GDP per capita 1975-1980 (log)	**0.407**	**0.487**	**0.568**	**0.506**
	(0.071)	**(0.055)**	**(0.051)**	**(0.068)**
Ethnic Fractionalization		0.150	0.179	-0.057
		(0.188)	(0.168)	(0.237)
Fractionalization*Segregation			**1.866**	**1.470**
			(0.862)	**(0.858)**
Ethnic Segregation			**-1.437**	**-3.385**
			(0.533)	**(1.071)**
Formalism*Segregation				**0.825**
				(0.216)
Religious Segregation				**-2.203**
				(1.019)
Rel. Segregation Squared				**10.760**
				(4.057)
Maximum Humidity	**-0.014**	**-0.017**	**-0.012**	0.002
	(0.004)	**(0.004)**	**(0.004)**	(0.004)
Pop. w/n 100km Coast or River	**0.526**	**0.442**	**0.332**	0.077
	(0.120)	**(0.114)**	**(0.077)**	(0.134)
Mean Temperature	-0.021	**-0.021**	-0.009	-0.004
	(0.018)	**(0.010)**	(0.008)	(0.008)
Household Controls	Included	Included	Included	Included
Observations	1010233	1376656	1093682	869958
Country Years	97	136	112	95
Countries	37	39	32	28
R-squared	0.636	0.6541	0.687	0.6436

Democracy

Research on Democracy is broader but closely related to research on institutions (Acemoglu, Johnson, Robinson and Yared 2008, Green 2013). Democracy in general is associated with better institutions, but the impact of democracy on income is subject to debate (Boix 2011). Democracy is not expected to have a direct impact on household wealth, but it is possible that democracy may interact with property rights, contracting institutions and corruption. There are many measures of democracy available, with the POLITY score being the most widely used. However, the Executive Constraints variable is a component of the full POLITY score so both should not be used together. The indicator of democracy used here is developed by Cheibub et al (2009) and designates countries as democracies "if the executive and the legislature is directly or indirectly elected by popular vote, multiple parties are allowed, there is de facto existence of multiple parties outside of regime front, there are multiple parties within the legislature, and there has been no consolidation of incumbent advantage." The direct nature of this indicator allows for simple interactions to be calculated; the interactions indicate clearly if there is a different effect of formalism, for example, in democracies versus non-democracies.

Table 6 presents three specifications considering interactions with democracy. The first regression adds the democracy indicator and an interaction with formalism. The interaction is positive, indicating that formalism has a slightly stronger effect on wealth in democracies. The second specification considers an interaction between democracy and oil revenues under the hypothesis that democratic countries are more likely to redistribute oil wealth. Surprisingly, the reverse effect is found. The interaction between oil revenues and democracy is negative and significant; oil exports are thus predicted to have a smaller impact on wealth in democracies. This result may be due to non-democracies using oil wealth to maintain power to a greater extent than democracies redistribute the benefits of oil exports (Tsui 2011). The third model in Table 6 interacts democracy with ethnic and religious segregation. The negative impact of segregation comes from lower trust and therefore fewer economic transactions (Alesina and Zhuravskaya 2011). These interactions test if democracy can encourage cooperation among segregated groups. The interaction of democracy with ethnic segregation is clearly not significant, but the interaction of democracy with religious segregation is positive and significant. Democracy is thus estimated to reduce the negative impact of religious segregation on wealth.

Table 6: Democracy

Dependent Variable: Wealth	1	2	3
Legal Formalism	1.196	1.239	1.498
	(0.230)	(0.224)	(0.404)
Formalism Squared	-0.156	-0.165	-0.218
	(0.030)	(0.029)	(0.055)
Executive Constraints	0.049	0.062	0.090
	(0.037)	(0.036)	(0.027)
Democracy Indicator	-0.530	-0.520	-0.615
	(0.187)	(0.172)	(0.186)
Democracy*Formalism	0.105	0.119	0.101
	(0.051)	(0.045)	(0.037)
Oil Exports (% of GDP)	0.024	0.056	0.043
	(0.005)	(0.011)	(0.010)
Democracy*Oil		-0.039	-0.009
		(0.012)	(0.042)
Ethnic Fractionalization			0.299
			(0.324)
Fractionalization*Segregation			-0.329
			(1.015)
Ethnic Segregation			-2.270
			(1.249)
Formalism*Segregation			0.776
			(0.248)
Democracy*Segregation			-0.091
			(0.330)
Religious Segregation			-2.390
			(1.399)
Rel. Segregation Squared			7.417
			(4.917)
Democracy*Rel. Segregation			1.267
			(0.519)
Average GDPpc 1975-1980 (log)	0.443	0.439	0.514
	(0.059)	(0.057)	(0.062)
Maximum Humidity	-0.013	-0.010	-0.001
	(0.003)	(0.003)	(0.004)
Pop. w/in 100km Coast or River	0.456	0.376	0.255
	(0.093)	(0.089)	(0.149)
Mean Temperature	-0.020	-0.020	0.004
	(0.010)	(0.011)	(0.007)
Household Controls	Included	Included	Included
Observations	1271649	1271649	838272
Country Years	130	130	92
Countries	40	40	28
R-Squared	0.6329	0.6381	0.6437

The previous results on institutions are robust in Table 6. The highly significant quadratic impact of Legal Formalism remains, as does the smaller, marginally significant impact of Executive Constraints. The impacts of ethnic segregation and religious segregation are also robust, along with the interaction of formalism with ethnic segregation. Finally, the household controls for urban residence and education (not shown) have robust significant positive impacts on wealth.

What is the impact of Formalism?

Table 7 summarizes the estimated impact of Legal Formalism based on results from Tables 4, 5 and 6. All regressions in those tables show a significant quadratic effect of Legal Formalism. The positive linear term indicates that there are benefits to formalism, while the negative squared term indicates that these benefits are diminishing as formalism increases and become negative near a level of four. All specifications presented earlier, even with varied interactions with formalism, suggest a similar optimal level of legal formalism around 4 (recall that formalism ranges from 1 to 7).

Table 7: The Impact of Legal Formalism

Results	**Table 4**			**Table 5**			**Table 6**		
Level of Formalism	Min. 1.68	Mean 3.97	Max. 5.75	Min. 1.68	Mean 3.97	Max. 5.75	Min. 1.68	Mean 3.97	Max. 5.75
Average Marginal Effect of Legal Formalism	**0.554**	**0.073**	**-0.302**	**0.732**	**-0.0099**	**-0.587**	**0.800**	**-0.023**	**-0.664**
Optimal Level of Formalism (at means of other variables)	**4.32**			**3.94**			**3.91**		

The indication of an optimal level of formalism raises some interesting questions, especially since it is near the mean. Are many countries near the optimum? Does the optimal level vary much by country, and if so, to what extent is there room to improve wealth by increasing or reducing formalism? Some examples are constructive to consider these questions. The country in the sample with the lowest level of Legal Formalism is South Africa at 1.68. Factoring in interactions with ethnic segregation, democracy and exports, the marginal impact of increasing formalism by one point is estimated to be 0.83 standard deviations of wealth, which is by far larger in magnitude than any other variable. The estimates here thus provide strong evidence that South Africa is not near its optimal level of formalism, which would be 3.99. Similar results can be seen with overly formal countries. The three countries with formalism greater than five are Bolivia, Guatemala and Peru. Again factoring in interactions, the marginal impact of decreasing formalism in those countries is estimated at 0.67, 0.38 and 0.58 standard

deviations of wealth respectively. There are some differences, but there also is clear room for increasing wealth by decreasing formalism in these countries. The estimated optimal level of formalism for these three countries is again close to 4, although it is higher for Guatemala because of a high degree of ethnic segregation there. Further work is needed to determine the mechanisms linking formalism to wealth and to inform policy, especially for countries that are far from the optimal level of formalism.

Conclusion

This paper presents new estimates of the impact of several types of institutions on household wealth. Using household wealth as the dependent variable is theoretically intuitive because the frameworks for how different institutions impact wealth all posit micro level effects: property rights encourage investment by individuals and firms; contracting institutions allow for more trades to take place among strangers and for complex contracts to be written. Estimating the impact of such institutions on a micro level outcome follows logically.

Econometrically, the argument for household level outcomes is clear: regressions on household wealth conditional on household controls and clearly exogenous country characteristics are a reasonable approximation of an experiment. With adequate controls, they provide believable estimates of how institutions impact household wealth. While the basic question is slightly different from what has been studied before, the impact of institutions on household wealth is worth understanding and can inform policy.

The results help to address a major puzzle in the literature: the weak empirical effects of contracting institutions. Contracting institutions are estimated to have a major quadratic impact on wealth. This result stems both from the household estimations and from the consideration of nonlinear effects of contracting institutions. Contracting institutions are measured by Legal Formalism, which was initially hypothesized to have a negative linear effect on wealth due to efficiency cost of excessive formalism. While there is a negative impact of high levels of formalism, the overall impact is quadratic and the linear term is large and positive. The net effect varies and the optimal level of formalism is estimated to be around four. The benefits of formalism likely stem from two sources. First, greater formalism allows for more trading among anonymous partners, thus increasing economic activity. Second, greater formalism enables the formation of long term complex contracts, which further increase economic activity.

The strong quadratic impact of Legal Formalism is robust to the inclusion of controls for other institutions, geography, economic indicators, historical factors and democracy. The estimated impacts of other institutions are small; property rights are marginally significant and corruption has no significant

effect once economic factors are included. Several geographic controls have fairly robust effects but they are relatively small in magnitude. Economic factors such as trade, aid per capita and inflation have little effect on household wealth, while oil revenues and the average prior GDP per capita have steady positive effects. Ethnic segregation has a negative estimated impact on wealth, which is mitigated both by greater ethnic fractionalization and greater formalism. Religious segregation also has a negative net impact on wealth, which is mitigated somewhat by democracy. Formalism has a slightly larger effect in democracies.

Household level outcomes provide cleaner identification and stronger results that differ dramatically from the cross country literature. The positive impact of contracting institutions should not come as a surprise to institutional economists who have long emphasized the need for anonymous trading and complex contracts (North 1994, Williamson 1975); rather, it should spur much further work measuring, modeling and testing how contracting institutions can encourage development. More work can also be done in gathering and analyzing household data across and within countries on institutions and growth and in investigating the mechanisms through which policy can improve the institutional environments for households.

Appendix 1: Summary of Country Variables and Sources

Variable	Source	Mean	Min	Max
Executive Constraints	POLITY IV	4.70	1.00	7.00
Expropriation Risk	Acemoglu and Johnson (2005)	6.31	3.64	8.41
Maximum Humidity	Acemoglu, Johnson and Robinson (2001)	68.40	35.00	87.00
Population w/n 100km of Coast/River	CID (Harvard)	0.44	0.00	1.00
Mean Temperature*	Acemoglu, Johnson and Robinson (2001)	24.09	14.70	29.43
Legal Formalism	Acemoglu and Johnson 2005, originally from Djankov et al. (2003)	3.97	1.68	5.75
Procedural Complexity	Acemoglu and Johnson 2005	5.98	2.90	9.03
Corruption Perceptions Index	Transparency International, QoG dataset	2.92	1.40	5.40
World Bank Control of Corr.	World Bank, QoG dataset	-0.56	-1.52	0.60
Aid per capita	World Development Indicators	34.98	0.45	403.46
Oil Exports (% of GDP)	World Development Indicators	3.77	0.00	70.13
Exports (% of GDP)	World Development Indicators	27.81	6.42	84.62
Inflation	World Development Indicators	11.65	-5.36	1096.68
Average GDP per capita 1975-1980 (log)	Alesina and Zhuravskaya (2011)	7.48	6.23	9.25
Ethnic Fractionalization	Fearon (2003), QoG dataset	0.60	0.10	0.95
Ethnic Segregation	Alesina and Zhuravskaya (2011)	0.15	0.00	0.39
Religious Segregation	Alesina and Zhuravskaya (2011)	0.08	0.00	0.27
Democracy Indicator	Cheibub, et al (2009) QoG dataset	0.53	0.00	1.00

Appendix 2: Minimum Distance Factor Analysis

The basic idea of factor analysis is that if one has many variables that are indicators of a single underlying unobserved variable, a structure is implied on the variance covariance matrix of the indicators. Suppose that the model is

$$y_i = \beta_i u + \varepsilon_i \qquad (A1)$$

where y is the indicator and u is the unobserved variable. This model implies a certain structure. For example, if i = 4 and we assume that $\sigma_{\varepsilon i \varepsilon j} = 0$ for all i,j, then the variance-covariance matrix V has the following structure:

$\beta_1^2\sigma^2_u+\sigma^2_{\varepsilon 1}$	$\beta_1\beta_2\sigma^2_u$	$\beta_1\beta_3\sigma^2_u$	$\beta_1\beta_4\sigma^2_u$
$\beta_2\beta_1\sigma^2_u$	$\beta_2^2\sigma^2_u+\sigma^2_{\varepsilon 2}$	$\beta_2\beta_3\sigma^2_u$	$\beta_2\beta_4\sigma^2_u$
$\beta_3\beta_1\sigma^2_u$	$\beta_3\beta_2\sigma^2_u$	$\beta_3^2\sigma^2_u+\sigma^2_{\varepsilon 3}$	$\beta_3\beta_4\sigma^2_u$
$\beta_4\beta_1\sigma^2_u$	$\beta_4\beta_2\sigma^2_u$	$\beta_4\beta_3\sigma^2_u$	$\beta_4^2\sigma^2_u+\sigma^2_{\varepsilon 4}$

Under the critical assumption that $\sigma_{\varepsilon i \varepsilon j} = 0$ for all i,j, there are in this case nine structural variables and ten unique entries in V, so the system appears to be identified. However, σ^2_u is never observed separately from one of the βs, so we cannot yet identify all the parameters. It is necessary to make some normalization, so we normalize $\beta_1=1$, and then we can identify all of the other parameters in the system.

The parameters are then estimated by minimum distance. We take the computed variance covariance matrix and stack the upper right triangle into the vector Π. We then stack the upper right triangle of V as well into the vector v and choose the parameter set Θ to minimize the distance between Π and v. The problem is:

$$\min_{\Theta} (\Pi - v)'\Omega(\Pi - v)$$

For efficient minimum distance, the matrix Ω should be the inverse variance of Π, that is the inverse variance of the sample variance. Solving this problem gives parameter estimates as well as a test of fit of the model. The parameter estimates are then used to create the factor scores. If we let G be the estimated matrix from the parameters, b be the vector of factor loadings for each indicator and X be the matrix of data, the factors scores are then given by:

$$\text{Scores} = X * G^{-1} * b * \sigma^2_u \qquad (A2)$$

In the analysis of this paper, I used the following indicators of wealth to create the wealth factor scores: radio, television, refrigerator, car/truck, and dirt floor. The coefficients for each of these indicators were estimated by a model exactly as presented above. The coefficient for the radio indicator

was normalized to one and the factor scores were then computed from the estimated parameters. The actual index weights used to create the variable used in the paper are given by:

$$\text{Weights} = G^{-1} * b * \sigma^2_u \qquad \text{(A3)}$$

Table A1 shows the factor weights and indicators used in the paper for the household wealth factor score.

Table A1: Wealth Factor Scores

Indicator	Factor Weight
Flush Toilet	0.1990
Electricity	0.2760
Radio	0.0331
Refrigerator	0.1953
Car	0.0843
Dirt Floor	-0.1328

Appendix 3: List of Countries and Survey Years in the full sample

Country	Survey Year	Observations
Albania	2008	7,999
Armenia	2000	5,980
Armenia	2005	6,706
Azerbaijan	2006	7,180
Bangladesh	1993	3,501
Bangladesh	1994	5,669
Bangladesh	1996	4,272
Bangladesh	1997	4,406
Bangladesh	1999	4,581
Bangladesh	2000	5,264
Bangladesh	2004	10,495
Bangladesh	2007	10,400
Benin	1996	4,427
Benin	2001	5,751
Benin	2006	17,511
Bolivia	1993	1,378
Bolivia	1994	7,703
Bolivia	1998	12,091
Bolivia	2003	16,796
Bolivia	2004	2,384
Bolivia	2008	19,564
Brazil	1996	13,231
Burkina Faso	1998	1,632
Burkina Faso	1999	3,159
Cambodia	2000	12,220
Cambodia	2005	9,330
Cambodia	2006	4,913
Cambodia	2010	15,667
Cameroon	1998	4,642
Cameroon	2004	10,386
Central African Republic	1994	3,176
Central African Republic	1995	2,342
Chad	1996	912
Chad	1997	5,887
Chad	2004	5,369
Colombia	1995	10,105
Colombia	2000	10,903
Colombia	2004	9,380
Colombia	2005	27,831
Colombia	2009	5,307

Colombia	2010	46,140
Comoros	1996	2,227
Congo	2005	5,879
Congo	2009	7,096
Cote d'Ivoire	1994	5,905
Cote d'Ivoire	1998	1,090
Cote d'Ivoire	1999	1,013
Cote d'Ivoire	2005	4,287
Dominican Republic	1996	8,808
Dominican Republic	1999	1,373
Dominican Republic	2002	27,038
Dominican Republic	2007	32,431
Egypt	1995	15,058
Egypt	1996	509
Egypt	2000	16,956
Egypt	2003	10,089
Egypt	2005	21,951
Egypt	2008	18,968
Ethiopia	2000	14,058
Ethiopia	2005	13,721
Gabon	2000	6,151
Ghana	1993	5,822
Ghana	1998	3,285
Ghana	1999	2,685
Ghana	2003	6,251
Ghana	2008	11,778
Guatemala	1995	11,253
Guatemala	1998	1,174
Guatemala	1999	4,368
Guinea	1999	5,008
Guinea	2005	6,265
Guyana	2005	2,578
Guyana	2009	5,632
Haiti	1994	4,265
Haiti	1995	529
Haiti	2000	9,582
Haiti	2005	3,760
Haiti	2006	6,238
Honduras	2005	4,268
Honduras	2006	14,398
India	1998	1,878
India	1999	1,994
Indonesia	1994	33,696

Indonesia	1997	34,255
Indonesia	2002	19,286
Indonesia	2003	13,749
Indonesia	2007	40,701
Jordan	1997	7,335
Jordan	2007	14,564
Jordan	2009	13,577
Kazakhstan	1995	4,178
Kazakhstan	1999	5,840
Kenya	1993	7,840
Kenya	1998	8,294
Kenya	2003	8,561
Kenya	2008	4,159
Kenya	2009	4,898
Kyrgyzstan	1997	3,665
Lesotho	2004	7,887
Lesotho	2005	692
Lesotho	2009	8,040
Lesotho	2010	1,350
Liberia	2006	551
Liberia	2007	6,207
Madagascar	1997	7,148
Madagascar	2003	2,545
Madagascar	2004	5,849
Madagascar	2008	4,145
Madagascar	2009	13,712
Malawi	2000	14,185
Malawi	2004	10,281
Malawi	2005	3,382
Malawi	2010	24,825
Maldives	2009	6,443
Mali	1995	2,688
Mali	1996	5,963
Mali	2001	12,225
Mali	2006	12,998
Moldova	2005	11,093
Morocco	2003	8,173
Morocco	2004	3,339
Mozambique	1997	9,282
Mozambique	2003	12,257
Mozambique	2009	6,097
Namibia	2000	6,284
Namibia	2006	2,375

Namibia	2007	6,672
Nepal	1996	8,082
Nepal	2001	8,602
Nepal	2006	8,706
Nicaragua	1997	2,559
Nicaragua	1998	8,910
Nicaragua	2001	11,316
Niger	1998	5,857
Niger	2006	7,660
Nigeria	1999	7,178
Nigeria	2003	7,148
Nigeria	2008	34,069
Pakistan	2006	62,591
Pakistan	2007	23,595
Peru	1996	27,987
Peru	2000	28,840
Peru	2003	488
Peru	2004	5,889
Peru	2005	6,837
Peru	2006	7,226
Peru	2007	7,188
Peru	2008	18,445
Philippines	1993	12,976
Philippines	1998	12,392
Philippines	2008	12,469
Rwanda	2000	9,656
Rwanda	2005	10,272
Sao Tome and Principe	2008	3,536
Senegal	2005	7,327
Sierra Leone	2008	7,284
South Africa	1998	12,169
Swaziland	2006	3,638
Swaziland	2007	1,160
Tanzania	1996	7,862
Tanzania	1999	3,604
Tanzania	2004	6,692
Tanzania	2005	3,043
Tanzania	2007	4,983
Tanzania	2008	3,514
Tanzania	2009	908
Tanzania	2010	8,715
Timor-Leste	2009	9,676
Timor-Leste	2010	1,787

Togo	1998	7,455
Turkey	1993	8,615
Turkey	1998	8,053
Turkey	2003	4,376
Turkey	2004	6,431
Uganda	1995	7,501
Uganda	2000	3,868
Uganda	2001	3,984
Uganda	2006	8,870
Uzbekistan	1996	3,696
Vietnam	1997	7,001
Vietnam	2002	7,048
Vietnam	2005	6,336
Zambia	1996	7,265
Zambia	2001	1,503
Zambia	2002	5,598
Zambia	2007	7,164
Zimbabwe	1994	5,951
Zimbabwe	1999	6,369
Zimbabwe	2005	6,854
Zimbabwe	2006	2,431
Totals: 64 Countries	190 Country-Years	1,704,700

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