

**Substitutable Protections:
Credible Commitment Devices and Socioeconomic Insulation**

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Abstract

Scholars argue that credible commitment institutions affect political outcomes as diverse as economic growth and social order. If commitment institutions function as theorized, then their effects should vary across individuals or states, based upon their respective vulnerability to promise breaking. Yet existing empirical studies never pursue this implication. The failure to do so risks a number of inferential errors and can lead to suboptimal policy prescriptions for institutional reform. In this paper, we develop these claims within the context of a commitment problem that scholars believe undermines social order and discuss implications for legal reform.

The enforceability of promises is central to explanations of many political, social and economic phenomena. When individuals cannot trust others to follow through with their commitments, social behavior becomes mildly inefficient at best, nasty and cruel at worst. A core tenant of neo-institutional social science is that institutions designed to enforce promises, commitment institutions, are crucial tools for inducing efficient behavior. In particular, scholars commonly argue that constitutional promises to respect individual rights enforced by effective judiciaries are critical for establishing social order and generating economic growth (Barro 1997; Frye 2004; North, Summerhill and Weingast 2000; Stasavage 2002).¹ However, we know that courts cannot always constrain the state from violating constitutional terms (Vanberg 2001; Helmke 2005), and that individual confidence in legal institutions varies considerably, even within states whose legal systems are thought to perform very well (Yates n.d).² For these reasons, states have strong incentives to develop trust in their legal institutions. As a result, legal reform efforts are commonly designed to construct general societal confidence in legal institutions via comprehensive, system-wide changes (e.g. Prillaman 2000).³ Yet, an implication of the general commitment argument, one that has yet to be explored empirically, suggests that targeting such reforms to the general population may be suboptimal.

What is this implicit implication? Put simply: If the commitment argument is correct, institutional effects should be strongest among individuals most vulnerable to the consequences

¹ While not the only example of credible commitment devices, courts are salient examples and our argument holds for all other commitment devices.

² The variance in legal system confidence in the data we analyze is substantial in all countries.

³ Also see World Bank indicators for reform success and justice institutions to evaluate reform success (<http://go.worldbank.org/3HH5NP2JM0>).

of breaking a promise and weakest among those least vulnerable. Indeed, for individuals perfectly insulated from promise-breaking, we should observe no effect at all, precisely because commitment institutions substitute for characteristics that insulate a person from the negative consequences of non-compliance. While this is implied by the argument it goes untested in the empirical literature, where scholars commonly estimate additive models of various mutually beneficial outcomes or behavior as a function of some measure of institutional quality or belief in institutional quality (e.g. Barro 1997; Clague et al. 1996, 1999; Frye 2004; Knack and Keefer 1995, 1997). Such research designs suggest that commitment institutions have constant effects across individuals or states. Since the literature has not been explicit about this element of the argument, it is fair to wonder whether it matters that we do so here. What is the value of determining whether commitment institutions have varying effects across individuals?

We believe that this is important for several reasons. First and foremost, we will have clarified precisely how the mechanism of a commitment device operates across a range of states or individuals. Second, we will have gained valuable evidence in support of the general commitment story derived from a more discriminating test. Standard empirical tests, which rely on additive specifications, risk failing to find support for its implications in some cases, and more alarmingly, they may uncover support when the underlying data generating process is inconsistent with commitment theory. Last, we will have gained a prescription for institutional reforms aimed at constructing beliefs in the credibility of commitment institutions. For example, in a world of scarce resources and multiple policy problems, getting the correct answer to the question, ‘What precisely are the effects of a reforming the judiciary?’ is critical. If the implicit of commitment theory is correct, then a generalized reform effort may waste resources on individuals for whom increasing trust in the legal system is irrelevant and devote insignificant

resources on individuals for whom an increase in trust would have a substantial impact. In the absence of such information, reformers may unnecessarily expend scarce resources to supply or promote an institution to a subpopulation that has little need for it. Alternatively, reformers might fail to supply or promote an institution to a subpopulation for which the impact will be particularly strong. In short, we believe that testing the implicit element of the commitment argument has profound implications for scholars and policy makers alike.

We divide the remainder of the paper as follows. In the following section, we describe the commitment problem in a variety of contexts, and attempt to state precisely the mechanism through which institutions solve the problem. We then highlight how individual socioeconomic characteristics might insulate a person from the consequences of promise breaking, and identify why the failure to account for such vulnerabilities clouds a precise test of the commitment mechanism. We then turn to a particular commitment problem that is central to the construction and maintenance of a democracy, a problem for which effective legal institutions are a solution. How does the state ensure order in society when the state itself is a threat to its own citizenry? Subsequently, we present our empirical research design and present several tests. We conclude by considering implications of these results for commitment theory and legal reform.

Commitment Theory

Commitment problems infect social relations in a variety of contexts. Consider three parties: A, B and C. The first type of problem involves how party A might induce parties B and C to enter into efficient agreements with each other when party A is a threat to the assets of both. Empirically, this is a problem of contracting in the shadow of a potentially predatory state and is most commonly addressed in the literature on economic growth (Barro 1997; Frye 2004; Stasavage 2002). If the state cannot credibly commit to respecting the property rights of

investors within its jurisdiction, investment will be inefficient and growth will be retarded. A second type of problem involves how parties A and B might come to an agreement when neither A nor B can trust the other to comply with the agreement *ex post*. This is the commitment problem most commonly studied by scholars of international and domestic conflict (Fearon 1995; Powell 2006; Walter 1997, 2002; Walter and Snyder 1999). Here, the failure to solve the problem can result in new or continued warfare, as parties cannot trust each other to respect the terms of peace. A third problem concerns the ability of party A to induce party B to behave in some way when A is a threat to B but B is not a threat to A. This problem infects state-private contracts of many kinds, and has been analyzed in the context of state borrowing behavior (North and Weingast 1989; Stasavage 2007). Perhaps more significantly, the problem affects the state's ability to construct and maintain social order; the problem North, Summerhill and Weingast (2000) reference. How can the state construct and maintain societal order when it is also a possible threat to the rights of its citizenry? Although the particulars of these three scenarios are no doubt different, they share the central dynamic of the commitment problem: A failure of one party to credibly commit to some course of action makes it less likely that another party will act in reliance on that commitment, leading to an inefficient outcome. How is this problem solved?

The challenge in each scenario is ensuring vulnerable parties that obligations will be met. The conventional solution involves constructing institutions that can detect and provide a remedy for non-compliance. Clearly defined civil and property rights enforced by effective, independent judiciaries are designed to ensure that state promises to forgo financial predation and to respect the physical integrity of its subjects are perceived credible. Likewise, power-sharing agreements enforced by powerful third party states or international actors do the same for the commitments of civil combatants. As long as parties believe that these institutional structures will operate as

designed (Denzau and North 1994; Frye 2004; Jacobs 2005; North 1990, North 2005), promises are rendered credible and mutually beneficial behavior ensues (Canon 2006; Nellis 2000; North and Weingast 1989; Milgrom, North and Weingast 1990).

To summarize, the mechanism that links commitment institutions to efficiency enhancing behavior involves the vulnerable parties' beliefs that the institution designed to detect and remedy non-compliance will function as designed. When parties believe this to be the case, they are relieved of their concerns over promise-breaking and develop a belief in the attractiveness of some efficiency-enhancing behavior, which in turn results in a greater propensity to engage in that behavior.⁴ In this sense, commitment institutions are political vaccines, which inoculate people against the apprehension that a promise will be violated. Exposed to a credible institution, individuals entertain the sort of beliefs (e.g. my assets, my rights, or my liberties are secure) about their interactions that induce socially efficient behavior (e.g. I sign the contract, I support democracy, I lay down my weapons). But are all individuals equally vulnerable to the problem that commitment institutions theoretically resolve?

Highlighting Commitment Institutions as a Substitute for Socioeconomic Insulation

Commitment arguments imply that institutions should have their largest impact on the most vulnerable to breach. It is among the vulnerable where the *credibility* of a promise matters. If a party will not be hurt too badly by the failure of another to follow through with a promise, we ought not to expect an institution that ensures compliance to have much of an effect on that

⁴While it is plausible to simply write that a belief in the institution induces a behavior, in our view our statement is a more precise description of the underlying choice-theoretic logic on which the argument is based. Beliefs about an institution induce a utility value associated with some action, which in turn informs an individual's choice of behavior.

party. On the other hand, if the party is quite vulnerable to non-compliance, then the institution should matter greatly. If commitment institutions attenuate or even eliminate non-compliance concerns, this much is a straightforward implication of the argument. Yet this idea is neither explicitly stated nor empirically tested. In our view, this treatment should be corrected.

We argue that socioeconomic factors that insulate a person from the consequences of breach condition the effectiveness of commitment institutions. This is because institutions *substitute for the protections that individuals might naturally enjoy*. We call those natural protections *socioeconomic insulation*. Recognizing that commitment institutions substitute for insulation implies that the institution's effect should be strongest among the least insulated and attenuate as insulation increases. Properly testing hypotheses derived from commitment theories in different contexts requires information on not only the commitment device in question but also the context-specific socioeconomic factor that insulates parties from breach.

What types of sociological features might insulate parties against broken promises? Depending upon the context, a host of factors (e.g. wealth, asset mobility, religious tolerance, embeddedness in dense social networks, financial independence etc.) may serve this purpose. For example, in the context of contracting under the shadow of a predatory state, firms whose assets are highly diverse or mobile should be better protected against state financial predation than firms whose assets are concentrated or immobile (see Boix 2003 for a related argument). In this respect, the investment choices of firms with low insulation should be more sensitive to the credibility of the state promises to respect their property than the investment choices of firms with high levels of insulation. In the context of a civil conflict, the choices of vulnerable combatants (e.g. those expected to be in the minority under a democratic regime) to lay down their arms should be far more sensitive to the credibility of peace arrangements than less

vulnerable parties. This is because group size better insulates the majority against a subsequent violation of the terms of the peace by the new government. In each of these settings, a commitment device serves as substitute for a specific form of socioeconomic insulation.

Consequences of Assuming Away Insulation

Typical tests of commitment models, which estimate additive specifications of commitment effects, however, do not account for such socioeconomic insulation. What are the consequences of doing so? Additive models produce estimates that reflect the *average* institutional effect across the varying degrees of insulation associated with observations in the sample. Yet, if the effects of commitment institutions are not constant, the argument implies an interactive specification, and if the underlying process is truly interactive, additive models can result in incorrect inferences on the effects on commitment institutions in three ways.

First, imagine that the results of an additive specification suggest that a particular commitment institution increases the propensity of a party to engage in some efficiency-enhancing behavior (or the behavior itself). An analyst would conclude that there is support for the standard argument, yet the precise inference would be wrong. Given that the additive finding represents the average effect of the institution across various parties, it is perfectly plausible that the precise effect of this institution is stronger for some parties and weaker for others. Indeed, the mechanism seems to directly imply that this would be true. If we care about constructing credible institutions yet face budget constraints, knowing where we are likely to have the strongest impact is useful information. Second, and of more serious concern, depending upon the distribution of insulation among parties in a given sample, there are a host of “true” underlying interactive relationships that may yield supportive evidence for the standard commitment argument in an additive specification, even when some of these interactive relationships are completely

inconsistent with the commitment argument.⁵ Thus, if the theory really suggests an interactive relationship, the additive specification may suggest supportive evidence when the underlying data generating process is entirely inconsistent with theory. Last, additive models of commitment institution effects may produce null results, masking potentially important findings. With such results, an analyst may infer incorrectly that the commitment institution in question is ineffective, when actually it is functioning precisely as predicted by the theory, but only for a smaller subset of the population under study (the most vulnerable parties in the data). What is important to note is that this effect could nevertheless appear to be null, *on average*, across all parties, when their respective vulnerabilities are ignored. As before, ignoring the differential effects of commitment institutions across parties' vulnerabilities could result in an analyst drawing an incorrect inference – that a given commitment device benefits no party.

A full test of the mechanism underlying commitment theory should parse out the vulnerable from the invulnerable and consider institutional effects across these categories. Such an interactive test would reveal precisely how and for whom commitment institutions resolve the

⁵ Several “alternate” realities could exist across parties with varying degrees of insulation and yet appear *on average* to support the general commitment story. For example, it could be that the effect among the least insulated is quite strong, but that the effect of the institution on the most insulated is actually negative, though weakly so. In this context, the additive model will underestimate the effect among the least insulated. What's worse, it will completely miss the negative relationship among the most insulated, a relationship that is entirely inconsistent with commitment theory. There are other examples, of course, but the general point is clear, ignoring a theoretically expected interactive relationship can have stark implications for the analyst.

fear of non-compliance they are designed to fix. To develop this argument further, we now turn to a particular commitment problem, its solution and implications that follow from it.

Constructing the Democratic Order

To demonstrate our approach, we turn to a specific commitment problem. Before a state can attend to how its predatory choices might influence growth, it must focus on how those choices might undermine political order itself (North, Summerhill and Weingast 2000).

According to North, Summerhill and Weingast, in order to avoid political disorder, a democratic state must, at a minimum, encourage citizen consensus that democracy is the legitimate governing institution best suited for their country. The failure to do so creates a pool of dissatisfied people that may be mobilized against the regime, heightening the risks of political disorder. At the individual level, this means that a democratic state must influence a citizen's propensity to support democracy as the system of government best suited for their country. As North, Summerhill and Weingast say directly "To maintain a stable democracy...citizens must believe that [democratic] institutions are appropriate for society" (2000, 25). In this argument, this is critical because citizens who hold these beliefs are less likely to engage in disorderly behavior, which, among other things, means offering support to a group mobilizing as an alternative to the state. Inducing supportive beliefs is non-trivial, however, precisely because without restraint a democratic state can threaten citizens' fundamental rights. Maintaining support for the democratic regime will be especially difficult when the alternative group has a legitimate chance of success and a citizen's support for that group look increasingly attractive.⁶ Therefore, while it is critical to induce regime support, the state's problem is far from trivial.

⁶ The marginal benefit of democratic support for a group mobilizing against the regime is larger when the regime is likely to fail. When a regime is stable, anti-system support risks having one's

Broadly conceived, the problem of maintaining the democratic order involves more than simply constraining the state from preying on financial assets, but rather involves the panoply of rights that states promise to respect and over which groups of individuals might challenge the regime. Of these many rights, perhaps none is more fundamental than a citizen's physical security. Unsurprisingly, democratic regimes routinely adopt constitutional provisions that explicitly recognize physical integrity rights. Yet there is mixed evidence that formal rules designed to protect physical integrity rights are associated with lower levels of violations (Keith 2002, 127). Indeed, the yearly average percentage of democracies between 1987 and 2004 that systematically engaged in state sponsored torture in violation of specific human rights commitments is 30% (Cingranelli and Richards 2004); and, 78% of democracies engaged in at least one act of torture in violation of their own commitments!⁷ Thus, we see the commitment problem here: How can the state ensure order in society by promoting democratic government when the democratic state itself is a potential threat to its own citizens' rights?

A standard commitment argument suggests that the solution lies in the state formally committing to protecting individual rights and then supplying a legal structure that can detect

assets extinguished via death or confiscated via imprisonment. This does not suggest that the way that vulnerabilities and institutions interact is influenced by the possibility of state failure, but it does suggest that one's support for the regime likely turns on whether the regime is strong or weak. And since it is likely that beliefs in institutional protections might vary with the likelihood of regime survival, it will be important to control for features of a regime that lead to breakdown in our empirical analysis.

⁷ A 'violation' is a state which contained at least one reported incident of state sponsored torture, while a 'systematic violator' is a state which contained at least 50 reported incidents.

violations provide a remedy when the state violates its commitment. Unfortunately, individuals do not perfectly observe whether the legal structure will genuinely constrain the state. It is certainly possible, even in a democracy, that the judiciary will merely defer to the decisions of the political branches (Helmke 2005; Vanberg 2001). As a result, individuals only observe that a judiciary exists, and have beliefs over whether it is genuine or not. Promoting beliefs in the credibility of legal institutions is thus crucial to solving the order problem. Under the standard commitment argument, we would expect that as citizens' beliefs in the credibility of the legal system increase so to should their propensity to support for democracy.

Of course, if commitment institutions substitute for socioeconomic factors that insulate individuals from breach, then different citizens within the state are differentially insulated from state incursions upon their rights. For example, mobile assets allow individuals to avoid financial predation (Boix 2003). Thus, we should observe the strongest effect of commitment institutions among individuals with immobile assets. In the context of physical security, we believe that the specific form of socioeconomic insulation that substitutes for citizen beliefs in legal institutions is a citizen's personal wealth. It is well known that wealth eases the process of vindicating rights in the legal system, if those rights happen to be violated. This is true for a variety of reasons, but most obviously, wealth provides access to quality legal counsel (Prillaman 2000, 29). It also offers the ability to buy justice if corruption is widespread. But more importantly, wealth has prophylactic effects, as well. In particular, wealth protects against physical security violations in the first place. Consider William Langewiesche's description of elite reaction to violence concerns in São Paulo, Brazil:

It was not only that the poor were being abandoned by the government but that the *very need* [emphasis added] for government was being questioned by the

elites. Armored cars, private guards, helicopters, and business jets. Walls and high-voltage fences. Cheap labor, filthy rivers, and private schools.

(Langewiesche 2007, 162)

Where physical security is at risk from the state or other citizens, walls, fences and private schools offer solutions, solutions that can only be purchased with money. Thus, our focus on physical integrity violations suggests the following two implications.

Implication: *Individuals should be more likely to support democracy as their confidence in legal institutions (wealth) increases; however, this relationship should be especially strong among the poor (individuals that do not trust the legal system).*

Prior to moving on, it is important to highlight that wealth and asset mobility are conceptually distinct forms of socioeconomic insulation. When the right at risk is financial, it is asset mobility that matters. A person may have mobile assets because her assets are easily liquidated and moved abroad, and this person may be wealthy. Yet, her assets might also be extremely mobile because they can all be carried on her person, and in this case she would be extremely poor. Either way, rich or poor, mobility protects financial assets. In contrast, now consider that the right at risk is physical security. Individuals with mobile assets are not necessarily better able to protect themselves against violations of their physical security. This is clear if we consider how well an individual with few, entirely liquid assets will fare in the legal system. What matters for the protection of physical security is wealth, and it is wealth that will condition the relationship between institutions and behavior aimed at ensuring physical security.

Research Design and Variables

To test our argument, we require individual-level data that reveal citizens' support for their democratic regimes, their evaluations of the protections afforded by the legal system, the

level of their relevant socioeconomic insulation, and their perceptions of regime survival. We are unfamiliar with a data source that offers a perfect measure of each of these concepts; however, the 1995 World Values Survey (WVS) provides much of the information we require for the first three concepts (Inglehart et al. 2003), and we can augment these data with indicators derived from the democratic regime literature to measure the fourth.

The 1995 WVS includes 42 countries and 62,688 individual-level respondents. Given that we are testing an argument about citizens' support of a democratic regime, we limit our analysis to only those citizens living in a democratic system. To determine which countries were democracies, we use Bernhard, Nordstrom and Reenock's (2001) classification of democratic regimes.⁸ Applying this standard results in our dropping nine countries (Peru, The German Democratic Republic, Armenia, Georgia, Azerbaijan, Ghana, Nigeria, China and Pakistan) or 13,913 cases from the WVS dataset due to the fact that these countries are not classified as democracies. In addition, Bulgaria is dropped from our analysis given its missing data for all 1,072 respondents on the personal income variable. These data restrictions result in our having 32 democratic nations with 47,703 individual respondents in the final dataset. The unit of analysis is the individual respondent.

Unfortunately, missing data is a non-ignorable concern in this analysis. With respect to our measures of the dependent variable, most countries in the dataset have relatively small

⁸ The exclusion of these countries is not unique to the use of the Bernhard, Nordstrom and Reenock (2001) standard; these countries are also not classified as democracies by most other regime databases (i.e. the Political Regime Change Dataset (Gasiorowski 1996), Classifying Political Regimes (Alvarez et al. 1997), Polity III (Jagers and Gurr 1995) and Freedom in the World (Freedom House))

amounts of missing data (less than 5%). A few countries, however, have relatively high missing data rates (roughly 20%). Proceeding via listwise deletion can lead to selection bias and result in making incorrect inferences (King et al. 2001), and there is strong evidence that we would produce these biases in our study if we were to ignore the problem.⁹ To account for these missing data, we use the software package *Amelia* (King et al. 2001) to impute missing values over five different datasets, and then estimate one model from these imputed datasets using the *miest* routine in Stata 9.0 (King et al. 2001). All estimates reported below are multiple imputation estimates, derived from models run across the five imputed data sets.¹⁰

We use OLS regression to estimate all coefficients.¹¹ We weight the data by the *weight*

⁹ Difference of means tests conducted on both individual and state-level measures of wealth as well as our main variable, legal system confidence, indicate statistically significant differences ($p < .001$) across missingness on the dependent variables that we use to assess democratic support. This suggests that to proceed via listwise deletion would result in case selection bias.

¹⁰ King (Amelia listserve 2005) suggests that using *Amelia* to impute datasets that contain more than 40-50 variables will not allow the software to converge. As a result, we estimated the imputations on a reduced set of variables including 27 from the WVS as well as nine country-level control variables. Our selection of these 36 variables was driven by our theoretical expectations on factors that were likely to inform an individual's support for democracy, including individual-level demographics, attitudes and beliefs.

¹¹ *Amelia's* authors advise that whenever the analysis model permits, missing ordinal observations should be allowed to take on continuously valued imputations" (Honaker et al. 2003). Given that our key concepts of interests (support for democracy, wealth, and beliefs about legal institutions) can easily be conceived of as latent concepts that reflect underlying continuous

variable in the WVS.¹² Moreover, we estimated standard errors clustered within country to account for the non-independence of observations across nations.¹³

Assessing Democratic Regime Support

Given North, Summerhill and Weingast's theoretical claim, we require a measure of a citizen's support of his or her democratic regime. Specifically we require a measure that assesses whether a citizen believes that democratic institutions are 'appropriate and legitimate' for their society. Scholars have used a variety of individual-level evaluations of democratic regimes (e.g. Anderson and Guillory 1997, 70; Kornberg and Clarke 1992, 1994), noting the importance of distinguishing between measures that reflect satisfaction with democracy and support for the current government in office (Kornberg and Clarke 1992, 114-16).¹⁴ The World Values Survey

dimensions of support, several of our variables that were originally measured at the ordinal level in the WVS are now measured as continuous variables.

¹² We also re-imputed the data with the two dependent variables restricted to the original ordinal codings and re-estimated the models using MLE, Stata 9.0's *ologit* routine. The estimates reported here are robust to either estimation technique.

¹³ While we use multilevel data, given that we are only interested in micro-level interactions and we have a relatively large number of macro-level units in our study shrinkage estimators like Hierarchical Modeling are not required; pooled interaction with clustered-errors adequately estimates the desired effects (Franzese 2005).

¹⁴ While Anderson and Guillory (1997) and others commonly use single item measures of regime satisfaction, Canache, Mondak and Seligson (CMS) (2001) question the validity of such measures on the basis that the question appears to mean different things to different respondents. In particular, CMS show that the relationship between democratic satisfaction and political

offers two such items. The first (v164), *Democracy Better*, asked respondents to estimate their agreement with the following statement, “Democracy may have problems but it’s better than any other form of government.” Since our first measure may assess citizens’ generalized support for the ideal of democracy rather than support for their own democratic regime, as a robustness check, we also make use of an alternative item that directly targets the state in which the respondent lives.¹⁵ This item (v157), *Prefer Democratic System*, asked a respondent whether “having a democratic political system” was a good way of *governing their country*. These variables are coded such that support for the regime increases in both measures.¹⁶ After

system support, which includes a number of institutional confidence measures, varies across countries. However, this is only a problem in the absence of a theory that can account for variation in the relationship between democratic satisfaction and system support (e.g. support for the legal system). Importantly, we supply a theory that provides at least one answer to an empirical puzzle that these scholars had interpreted as a problem with the measure's validity. In addition, the alternative suggested by CMS, which makes use of scales of particular dimensions of political support (e.g. Muller, Jukam and Seligson, 1982), cannot be used if we wish to test the North, Summerhill and Weingast argument. The multiple item indices group together variables that, according to the theory tested here, belong on the right and left side of the equation, respectively (see Muller, Jukam and Seligson, 1982: 249).

¹⁵ This concern would be particularly troubling if we were attempting to measure regime support in an autocracy with our variables; however, since we have restricted our study to democracies, we are not prone to make this error.

¹⁶ We conducted two validity checks on our measures. First, if the previous literature is correct, our democratic support measures ought to be negatively associated with an individual’s

imputation, both of these variables are continuous variables with means of 3.20 and 3.23 and standard deviations of .724 and .754, respectively.

Assessing Citizen Beliefs about Legal Institutions

To assess citizens' beliefs about legal institutions we use a survey instrument (v137), which asked each respondent to estimate the level of confidence that they had in their state's legal system. Higher values reflect stronger trust in the legal system. The mean and standard deviation of this variable, following imputation, are 2.52 and .876, respectively.

Assessing Socioeconomic Insulation

As we note above, there are a variety of socioeconomic characteristics that insulate citizens from violations of their rights; the relevant form of insulation depends on the right at stake. Data constraints limit our ability to test the effects of all forms of insulation (for example, individual level measures of asset mobility are non-existent in the data); however, one obvious

p propensity to engage in anti-system behavior (Anderson and Mendes 2006). Indeed, our two measures of democratic support, *Democracy Better* and *Prefer Democratic System*, were negatively correlated with a WVS variable that measures a citizen's beliefs about whether using violence to achieve political goals is ever justified (v164) at $-.19$ ($p < .01$) and $-.13$ ($p < .01$) respectively. Second, we conducted a predictive validity test in which we found that aggregated measures of our democratic support variables are correlated positively with estimated median democratic regime survival times and correlated negatively with estimated hazard rates of regime survival time derived from a standard event history model of democratic regime transition (Przeworski et al. 2000). So, as North, Summerhill and Weingast suggest, there is reason to believe that the beliefs the state is supposedly interested in constructing have important consequences for social order.

form of insulation, wealth, is easily obtained in the WVS. By using wealth as a measure of insulation, we are implicitly assuming that the right at stake is something resembling personal security, which is easier to protect as wealth increases. Since all members of a society are likely to be concerned with their physical integrity, we believe wealth offers an extremely general test of the argument. Of course, it might be argued that increasing wealth makes a person more vulnerable to financial asset predation. For the reasons we offered in our discussion of the socioeconomic insulation concept, we do not believe that wealth has this effect. Fortunately, our research design will allow us to test whether wealth actually increases an individual's vulnerability to predation. If it does, then we would expect our models to estimate effects that are precisely the opposite of the propositions that we advanced above.

To measure individual wealth we use a survey instrument that assesses personal income (v227). The original instrument assesses personal income by dividing income in each country into groupings by deciles. This variable is measured on a 10-point ordinal scale from the lowest income to the highest. Of course, following imputation, the variable is best described as continuous. The average income for all respondents was 4.65 with a standard deviation of 2.80. To test our hypotheses we interact this measure with the measure of legal system confidence.

Assessing the Likelihood of State Collapse

We include several country-level variables to control for the possibility of democratic regime collapse. These measures should reflect the costs that a citizen perceives in deciding to support democracy relative to some alternative and control for the viability of an alternate group's claim over the current regime. We include several country-level variables that have been linked to democratic regime instability (Gasiorowski 1995; Li and Reuveny 2003; Przeworski et al. 2000). $\ln(GDP)$ is the natural log of the Gross Domestic Product measured in real GDP per

capita measured in 1996 Constant Prices from the Penn World Table (Heston, Summers and Aten 2006). *Population* assesses the overall size of the nation and is measured in millions of people. *Count* measures the number of years that have passed since the democratic regime's inauguration, reflecting the time that has passed without authoritarian interruption. *Presidential Regime* is a dummy variable that indicates the presence of an executive, who was elected under a separate mandate to an office with fixed terms and who does not possess the ability to dismiss the legislature (Linz 1994). *Openness* measures openness of the economy with respect to trade, measured as the Percentage of GDP derived from both Exports and Imports (Penn World Tables 2005). *Growth* is the annual percentage change in GDP (Penn World Tables 2005). *Previous Experiences* is a variable that measures the number of times a democracy has previously experienced democratic failures. *Religious* and *Ethnic fractionalization* are controls for each democracy's cultural heterogeneity. We used Rae and Taylor's (1970) fractionalization index to capture this dimension. We calculated this index for religion and ethnicity in each country (Singer 1997, supplemented by national statistical annuals).

Controls

We also include several individual-level control variables including demographic controls as well as several attitudinal controls. We include measures of *Education* (v217) as a 12-point ordinal variable, *Female* (v214) and *Age* (v216), in years, as basic demographic controls. In addition to basic demographic controls, we also include several attitudinal measures to assess the general propensity for an individual to support democracy. We include variables to control for the respondent's evaluation of how widespread bribe taking and *Corruption* is in their country (v213) and the extent to which the respondent agrees that using *Violence* to achieve political goals is ever justified (v164). We also include a variable to control for the respondent's *Support*

for the Previous Regime (v151) and the extent to which the respondent agrees that the current government is doing enough to alleviate *Poverty* (v174). We include a variable to control for the respondent's *Political Interest* (v117). Last we note that all results that we report are robust to the inclusion of an objective measure of legal system performance, which examine whether the individual level belief effects are spurious.¹⁷

Results

To test our proposition that socioeconomic insulation and beliefs in the legal system substitute for each other in enhancing democratic support, we estimate multiplicative models in which citizens' democratic support is a function of the interaction between beliefs about the legal system and socioeconomic insulation. The first half of Table 1 presents results for this analysis; the second half of Table 1 shows the additive specifications, reflective of the typical analysis in the literature. In each model, all of the control variables that obtain significance are in the expected direction. With respect to testing the implications of our argument, the signs on the legal system confidence estimates are positive and significant. This is true for the income variable in the interactive models. Importantly, in the interactive specifications, the signs on the interaction terms are negative and significant, suggesting that the slope of the relationship between beliefs about the legal system and democratic support decreases for higher levels of income and that the slope between income and democratic support decreases for higher levels of legal system confidence. This is consistent with our expectations; however, the coefficients on the interaction terms are not the quantities in which we are interested. We want to know the

¹⁷ We included the level of *contract intensive money* that exists within a democracy (Clague et al. 1996, 1999) as well as a *law and order* measure (The International Country Risk Guide 1996), both of which have been used as valid indicators of the rule of law.

marginal effects of our key causal variables across the range of their conditioning variables.

[Table 1]

Figure 1 presents the results of these tests for the marginal effect of a 1-unit increase in either confidence in the legal system or income conditioned on the other on each of our measures of democratic support. Panel A of Figure 1, on the top-left, shows the marginal effect of confidence in the legal system conditioned on income. The panel reveals that income does attenuate the positive relationship between beliefs in the legal system and democratic support. Indeed, the figure suggests that for individuals at lower levels of income there is a positive marginal effect of increasing confidence in the legal systems on their willingness to lend support to democracy. The size of this positive marginal effect, however, decreases as income rises and eventually becomes insignificant, with the confidence intervals straddling the x-axis, or has no effect on democratic support above approximately middle-high incomes ($Income \approx 7$). This finding suggests that at the highest levels of income, increasing beliefs in the legal system has no significant impact on the wealthiest citizens' willingness to support democracy, as predicted by the theoretical argument. A similar pattern holds for the other specification of support for democracy as measured by the "Prefer Democratic System" variable (shown in Panel C of Figure 1 at the bottom-left), with the only difference being a higher estimate of the income threshold beyond which beliefs about institutions have no effect ($Income \approx 9.5$). We should note that these results dismiss the possibility that wealth reflects a greater vulnerability to financial predation. If it had, we would have observed the opposite sign on the interaction term in the models.

Panels B and D in Figure 1 demonstrate the substitution effect. The marginal effect of income is significant in explaining democratic support but only for those citizens whose beliefs in the legal system are quite low, approximately below 2.0, for both measures of the dependent

variable. Above this level of confidence in the legal system, increasing citizens' socioeconomic insulation, or raising their income, has no effect on their democratic support. This evidence combined with that presented in the first panel of the figures suggests that our measures of beliefs and insulation do indeed serve as substitutes for each other.

[Figure 1]

The findings support our contention that beliefs in the credibility of legal institutions do not have constant effects across all individuals. The effects depend on an individual's wealth. The evidence we uncover is consistent with North, Summerhill and Weingast's (2000) theory of how a democratic political order is constructed and maintained. Importantly, our findings provide strong evidence for the commitment argument. It is not just that legal system confidence matters for regime support, but it matters in particular ways and under particular conditions. It is positive among the most poor, attenuates as wealth increases and has no effect among the very rich. This is entirely consistent with the notion that commitment institutions serve as political vaccines.

Discussion

We began this paper by suggesting that legal institutions can solve commitment problems; however these devices have differential effects across different populations. We argued that a complete test of the commitment argument would examine whether institutional effects have their strongest effect among the most vulnerable to promise-breaking and attenuate as vulnerability decreases. We suggested that the failure to do so might lead scholars to uncover both weaker and stronger commitment effects than the data really support. We also suggested that scholars might find null effects when they really should observe evidence in line with the theory. Finally, the most troubling possibility we raised was that scholars might find support for the argument when the underlying, interactive data generating process is entirely inconsistent

with commitment theory. Properly testing the interactive relationship was important because it would precisely examine the commitment mechanism. Additionally, with the mechanism more precisely identified, we would be better able to suggest policy implications for institutional reform. We are now in a position to examine those claims in light of the study we have just summarized.

Overstated and Understated Institutional Effects

What we want to know is how we would have misunderstood commitment effects if we had proceeded as if legal system confidence had a constant effect. To do so, we need to compare the results from the interactive and additive specifications. Table 2 displays the estimated marginal effects of beliefs in the legal system on democratic support for both the additive and interactive models across individual income. We also report the percent change in the estimate as you move from the conditional effect assumption to the constant effect assumption. Based upon the additive models, we would conclude that beliefs in legal institutions have the effect that the standard argument expects them to have. Namely, an increase in an individual's confidence in the legal system leads to greater support for democracy, or marginal effects of .0523 and .064 for each dependent variable, respectively. Moreover, we would conclude that these institutions operate on all individuals and with precisely the same magnitude across individuals. However, what we want to know is how different are the effects in the additive and interactive models.

[Table 2]

Note that under both the interactive (conditional) and additive (unconditioned) models, the effect of beliefs is estimated to be approximately equal for those individuals around middle income or at an income of approximately five on our 1-10 scale. However, as an individual's income moves away from this level the estimated effects diverge. First consider those citizens

who are most insulated from violations of their physical security (*Income* ≈ 10). Compared to the interactive models, the estimated effect from the additive model overestimates the impact of beliefs in the legal system on democratic support by 180.37% and 82.92% for each of the measures of democratic support, respectively. Moreover, the interactive models suggest that beliefs have no statistically significant impact on individuals' with incomes higher than 7 and 9.5, respectively, while the additive models predict a significant effect at that level. Next consider citizens least insulated from violations of their physical security (*Income* ≈ 1). Compared to the interactive models, the additive model underestimates the impact of beliefs in the legal system on democratic support by -25.39% and -19.22% for each of the measures of support, respectively. In sum, the comparison of effects in Table 2 suggest that treating beliefs in institutions as if they have constant effects across individuals leads to quite different inferences about commitment institutions. If the commitment institutions substitute for socioeconomic insulation, the additive model both overestimates and underestimates effects. In fact, in some cases, the additive model suggests an effect when there is none!

Substantively, these results suggest that commitment institutions have larger effects than the literature suggests among the least insulated (most vulnerable) in society, and that institutions likely have smaller effects than the literature suggests among the most insulated. They do not necessarily call into question the positive institutional results found in much of the commitment theory literature; however, they do reflect a more precise test of the mechanism. Moreover, our results suggest that null or mixed findings published in the literature (Boix 2003; Mauro 1995), or those that were never published due to their null results, may very well be masking commitment devices' beneficial effects for some subset of the population. In both of these cases, without considering how institutions differentially affect individuals with different

vulnerabilities, empirical tests cannot determine whether a commitment institution really inoculates a person from the fear of promise-breaking or is merely correlated on average with some efficiency-enhancing behavior.

In a broader sense, we should note that these findings sit easily with other results in the literature on political institutions. The effect of district magnitude on the number of parties, for example, depends on socio-cultural homogeneity. In relatively homogenous cultures with corresponding small numbers of issue cleavages, the equilibrium number of parties will be relatively small, no matter how permissive the electoral institution; however, as states become increasingly heterogeneous, the effects of the institution kick in (Cox 1997; Ordeshook and Shvetsova 1994). Like electoral institutions, commitment institutions do not have identical impacts under all conditions. Instead, they influence behavior only under particular conditions (e.g. Cox 1997, 19-20). This paper sheds some light on a behavior that should be affected by political institutions and some conditions under which we should observe those effects.

Reconciling Targets of Reform with Varying Socioeconomic Insulation

We conclude by considering an implication of this research for legal reform. To clarify this implication, we make use of our concept of socioeconomic insulation and the corresponding point that different forms of insulation are relevant for different threats to citizen resources. Although the community of scholars and practitioners engaged in the debate around judicial reform recognize that similar judicial rules might be implemented differently by different judiciaries, or even by the same judiciary in different jurisdictions (e.g. Jappelli, Pagano and Bianco 2005), it is unclear that any scholar of reform seriously considers how individuals might

be differentially affected by identical reforms.¹⁸ Our paper suggests a reason for doing so.

States confront multiple policy problems, from how to best deliver educational resources to how to control pollution. Quite obviously, communicating a general respect for the physical security of its population is also an important state goal, and much theory suggests that constructing an independent, effective judiciary is a major solution to this problem. But in the context of multiple policy problems and significant budget constraints it is not clear that states will succeed in constructing quality judicial systems, especially in the developing world. Prillaman (2000) argues that the failure to properly reform Latin American judiciaries has resulted from the inability to pursue a comprehensive reform package. Unfortunately, it is immediately obvious that the scope of the Prillaman reform project is will be daunting (perhaps impossible) for relatively underdeveloped states, states that confront a wide array of problems. Perhaps more important, it is not at all clear how reforms aimed at making the judiciary more efficient, accessible and independent will be communicated to the population. Our study suggests that judicial reform efforts can have significant effects, on regime support at least; however, these reforms must be properly targeted. A generalize program of reform, communicated diffusely to the population at large, is not likely to be very effective. Our results suggest that a reform effort should be intense and targeted at the lower end of the income distribution. Importantly, it is not that the wealthy all support democracy, so that there is nothing to gain there. It is just that judicial reform will likely have little effect among those in the upper tail of the income distribution. Instead, the ‘bang’ for the reform ‘buck’ is to be found among the poor.

¹⁸ Channell’s (2006) point about taking cultural values into account when designing and evaluating reform projects is close.

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Table 1. Democratic Support as a Function of Confidence in the Legal System and Personal Income (Multiplicative and Additive Models)

| | "Democracy Better" Model 1 | | "Prefer Democracy" Model 2 | | "Democracy Better" Model 3 | | "Prefer Democracy" Model 4 | | | | | |
|---|-------------------------------|------|-------------------------------|---------|-------------------------------|--------|-------------------------------|--------|--------|---------|--------|--------|
| | B | S.E. | B | S.E. | B | S.E. | B | S.E. | | | | |
| Main Variables | | | | | | | | | | | | |
| Individual Income | 0.0186 | *** | 0.0068 | 0.0193 | *** | 0.0072 | 0.0043 | 0.0041 | 0.0067 | ~ | 0.0038 | |
| Confidence in Legal System | 0.0760 | *** | 0.0153 | 0.0873 | *** | 0.0199 | 0.0523 | *** | 0.0098 | 0.0664 | *** | 0.0114 |
| Income*Confidence | -0.0057 | * | 0.0030 | -0.0051 | * | 0.0030 | --- | --- | --- | --- | --- | |
| The Likelihood of State Collapse | | | | | | | | | | | | |
| ln(GDP) | -0.0570 | | 0.0919 | -0.0573 | | 0.0821 | -0.0594 | | 0.0930 | -0.0594 | | 0.0833 |
| Population | -0.0003 | | 0.0018 | -0.0001 | | 0.0018 | -0.0003 | | 0.0018 | -0.0002 | | 0.0018 |
| Years Democratic | 0.0041 | *** | 0.0015 | 0.0031 | ~ | 0.0019 | 0.0042 | *** | 0.0015 | 0.0031 | ~ | 0.0019 |
| Trade Openness | -0.0014 | | 0.0011 | -0.0014 | | 0.0011 | -0.0014 | | 0.0011 | -0.0014 | | 0.0011 |
| Presidential Regime | 0.0513 | | 0.0619 | 0.0403 | | 0.0646 | 0.0511 | | 0.0618 | 0.0402 | | 0.0645 |
| Economic Growth | 0.0083 | | 0.0074 | 0.0134 | ^ | 0.0081 | 0.0083 | | 0.0075 | 0.0134 | ^ | 0.0081 |
| Previous Democratic Experience | 0.0597 | | 0.0472 | 0.0191 | | 0.0479 | 0.0593 | | 0.0475 | 0.0188 | | 0.0481 |
| Religious Fractionalization | -0.2273 | * | 0.1374 | -0.1493 | | 0.1350 | -0.2242 | * | 0.1383 | -0.1466 | | 0.1359 |
| Ethnic Fractionalization | -0.0651 | | 0.1784 | -0.0903 | | 0.1685 | -0.0669 | | 0.1799 | -0.0919 | | 0.1697 |
| Individual-Level Controls | | | | | | | | | | | | |
| Education | 0.0240 | *** | 0.0046 | 0.0363 | *** | 0.0071 | 0.0239 | *** | 0.0046 | 0.0363 | *** | 0.0071 |
| Gender (Female) | -0.0098 | | 0.0082 | -0.0247 | ^^ | 0.0101 | -0.0102 | | 0.0082 | -0.0250 | ^^ | 0.0101 |
| Age | 0.0016 | *** | 0.0005 | 0.0014 | *** | 0.0005 | 0.0016 | *** | 0.0005 | 0.0013 | *** | 0.0005 |
| Corruption | -0.0412 | *** | 0.0118 | -0.0671 | *** | 0.0148 | -0.0405 | *** | 0.0118 | -0.0665 | *** | 0.0148 |
| Too Much on Poverty | 0.0029 | | 0.0176 | 0.0086 | | 0.0172 | 0.0023 | | 0.0177 | 0.0081 | | 0.0172 |
| Reject Violence | -0.1236 | *** | 0.0119 | -0.0707 | *** | 0.0118 | -0.1238 | *** | 0.0119 | -0.0709 | *** | 0.0118 |
| Support for Previous Regime | -0.0261 | *** | 0.0063 | -0.0265 | *** | 0.0064 | -0.0262 | *** | 0.0063 | -0.0266 | *** | 0.0064 |
| Political Interest | -0.0602 | *** | 0.0102 | -0.0926 | *** | 0.0113 | -0.0603 | *** | 0.0102 | -0.0927 | *** | 0.0113 |
| Constant | 4.0001 | *** | 0.8581 | 4.0397 | *** | 0.7457 | 4.0830 | *** | 0.8810 | 4.1130 | *** | 0.7729 |
| N | 47703 | | 47703 | | 47703 | | 47703 | | | | | |

Note. Results generated using OLS with robust standard errors corrected for clustering on countries

* $p < .10$, ** $p < .05$, *** $p < .01$, two-tailed tests

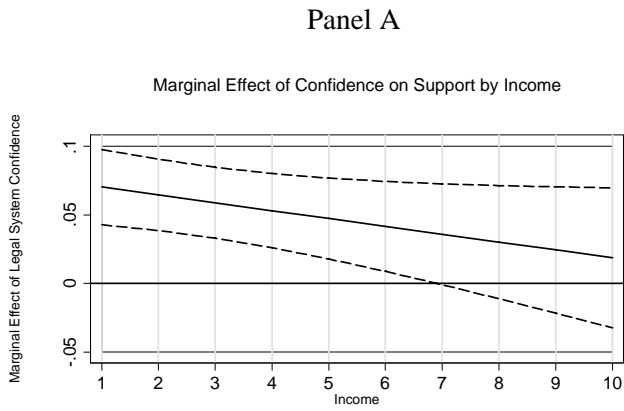
Table 2. Comparison of Additive and Interactive Model Effects of Legal System Confidence on Democratic Support (Two Measures) across Income

| Income | Support for Democracy Estimated Marginal Effect of Confidence in Legal System | | | Support for Democratic System Estimated Marginal Effect of Confidence in Legal System | | |
|--------|--|-------------------|----------|--|-------------------|----------|
| | Interactive Model | Additive Model | % Change | Interactive Model | Additive Model | % Change |
| 1 | 0.070* | 0.052** | -25.39 | 0.082* | 0.066** | -19.22 |
| 2 | 0.065* | 0.052** | -18.76 | 0.077* | 0.066** | -13.88 |
| 3 | 0.059* | 0.052** | -10.85 | 0.072* | 0.066** | -7.78 |
| 4 | 0.053* | 0.052** | -1.22 | 0.067* | 0.066** | -0.75 |
| 5 | 0.047* | 0.052** | 10.73 | 0.062* | 0.066** | 7.44 |
| 6 | 0.042* | 0.052** | 25.97 | 0.057* | 0.066** | 17.11 |
| 7 | 0.036* | 0.052** | 46.09 | 0.052* | 0.066** | 28.68 |
| 8 | 0.030 | 0.052** | 73.84 | 0.047* | 0.066** | 42.80 |
| 9 | 0.024 | 0.052** | 114.61 | 0.041* | 0.066** | 60.39 |
| 10 | 0.019 | 0.052** | 180.37 | 0.036 | 0.066** | 82.92 |

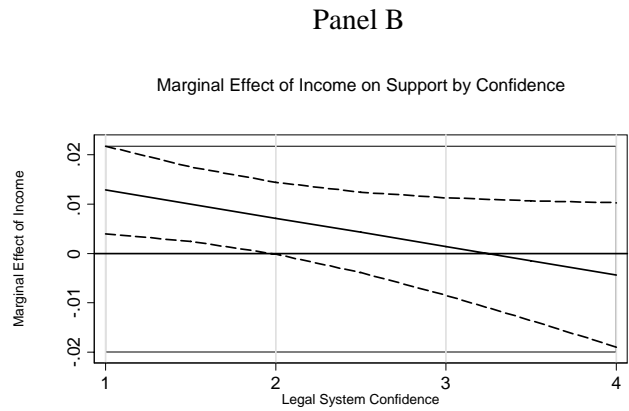
Note: The percent change in the estimated marginal effect of confidence in the legal system is calculated as the change in the additive model effect away from the interactive model effect for each level of income.

* $p < .05$, ** $p < .01$, two-tailed tests

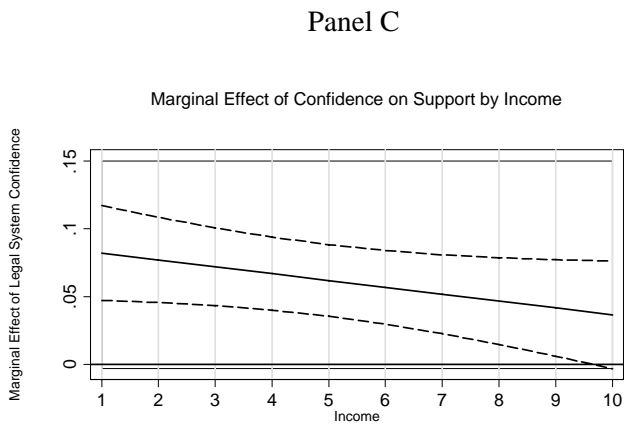
Figure 1. Substitution Effects of Legal System Confidence and Socioeconomic Insulation (Income) on Democratic Support (DemSupport and DemSystem)



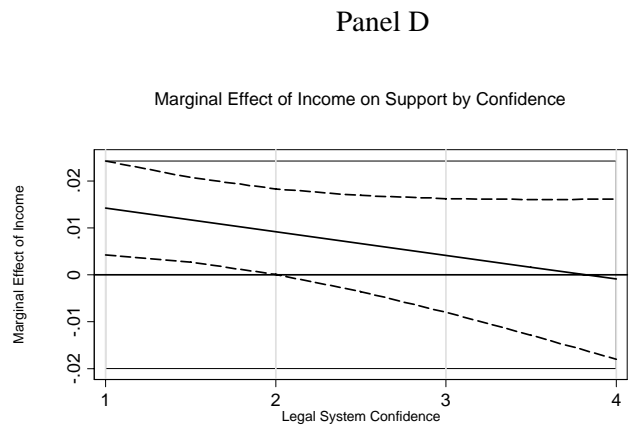
Note: Dashed lines represent 95% Confidence intervals around estimate



Note: Dashed lines represent 95% Confidence intervals around estimate



Note: Dashed lines represent 95% Confidence intervals around estimate



Note: Dashed lines represent 95% Confidence intervals around estimate

Note: Dashed lines represent 95% confidence intervals around the expected marginal effect.