The Contribution of Veto Players to Economic Reform*

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Abstract

Contrary to the conventional understanding that reform is more difficult when veto players are numerous, we show formally that veto players may encourage policy change by weakening the power of special interests that prefer inefficient reform outcomes. Using the same model, we demonstrate that reform reversals are less likely in the presence of multiple veto players, implying that a constitutional framework conducive to initial reforms may also lock in those achievements over time. We find support for our theoretical perspective in a study of the relationship between veto players and reform in Eastern Europe and the former Soviet Union.

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> The monocratic chief is more open to personal influence and is more easily swayed, thus making it more readily possible to influence the administration of justice and other governmental activity in favor of . . . powerful interests.
>
> Max Weber (1978, pp. 283-284)

What is the role of veto players in economic reform? The conventional understanding is that reform is more difficult when veto players are numerous. Each additional veto player—a political actor with the ability to block change—either shrinks the set of policies that can defeat the status quo or leaves it unchanged. Thus, policy stability is greater as veto players increase in number (Tsebelis, 1995, 2002). Such stability may be desirable when there is a need to commit to established policy (Keefer and Stasavage, 2003), but it can be detrimental when economic reform is necessary (Cox and McCubbins, 2001).

Viewed from this perspective, the recent experience of economic reform in Eastern Europe and the former Soviet Union is anomalous. The top panel in Figure 1, which plots economic reform as measured by the European Bank for Reconstruction and Development (EBRD) against a measure of veto players from the Database of Political Institutions (Beck et al., 2001), shows that reform has been greater, not less (or the same), in countries with more veto players.[1] Yet as the second panel in Figure 1 depicts, reform reversals—a measured decline in any of eight particular reform indexes—are less likely in countries where veto players are numerous, precisely as standard veto-player theory would predict.

Within-country variation demonstrates similar relationships. Croatian voucher privatization, for example, stalled prior to 1999 amid accusations of preferential treatment of insiders and new private owners, but accelerated immediately after Croatia transitioned from a semi-presidential system to a parliamentary system with closed-list proportional voting, changes that increased the number of constitutional veto players. Although the death of the nationalist leader Franjo Tudjman may have been a contributing factor, a similar outcome occurred in democratic Poland after the adoption of the 1997 constitution, which restricted

the power of the president to dissolve parliament and gave the parliament greater control over economic policy. Shortly thereafter, a long-stalled bankruptcy bill was passed and two important new regulatory institutions—an independent bank regulator and a monetary council—were created. Finally, in Russia, a reduction in the number of veto players presaged reform reversals. Upon taking power, President Vladimir Putin centralized decision making, limiting the power of parliament and Russia's governors (the latter not considered in most measures of veto players). Powerful clans within the Kremlin proceeded to seize control of previously privatized companies, including but not only the oil giant YUKOS.

What explains the differing impact of veto players on economic reform and reform reversals? What does the postcommunist anomaly tell us about the role of veto players in economic reform more generally? We answer these questions by highlighting an actor not emphasized in conventional veto-player theory: special interests who do not have formal veto power but who may lobby veto players to implement particular policies. We show formally that, under certain conditions, the power of special interests is less when veto players are numerous. In particular, if special interests must compensate each veto player to choose some policy over the status quo, then that policy becomes less attractive as veto players increase in number. This has the effect of making it less likely that the status quo will be overturned in favor of policies that benefit a narrow but organized constituency.

In certain contexts, this logic implies that economic reform will be greater when veto players are numerous. Incomplete, or partial, economic reforms often generate rents for a chosen few while leaving the majority worse off than before. In the transition context, this tendency was stressed most notably by Hellman (1998), who argued that the largest obstacles to full reform were the enterprise managers, bankers, and others who could exploit arbitrage opportunities in partially liberalized markets, even as such incomplete reforms reduced living standards for most citizens. More generally, the idea that partial reform may be suboptimal is present in the economic theory of the "second best" (Lipsey and Lancaster, 1956–1957): satisfying one optimality constraint while leaving others unsatisfied may be

worse than doing nothing at all. If we assume that veto players trade off welfare-enhancing policies and contributions from organized interests (e.g., Olson, 1965; Bates, 1981; Grossman and Helpman, 1994), then when partial reform is inefficient and veto players are numerous, special interests may find that the rents generated by partial reform are outweighed by the cost of compensating each veto player for choosing it. If, in turn, the political demand for full reform outweighs any preference for the status quo, then movement toward full reform may be greater when the number of veto players is large.

Our model thus predicts that veto players may facilitate economic reform when the status quo is no reform. At the same time, we show that an increase in the number of veto players has no impact on movement toward full reform when the status quo is partial reform, so long as partial reform is inefficient. The efficiency of full reform implies that veto players are otherwise inclined to move that direction and need not be compensated to do so; their number is therefore irrelevant to the decision to move toward full reform. We find support for this conditional effect of veto players on economic reform in an analysis of panel data from 25 postcommunist countries, where we take advantage of the similar starting points and reform objectives of countries in this region. Controlling for time-invariant country-level heterogeneity, shocks that are common to the countries in the sample, and various time-varying country characteristics, including representation of communists in the legislature, the number of veto players is positively correlated with movement toward full reform when the status quo is little or no reform but uncorrelated at higher levels of reform.

In addition to generating predictions about when veto players might promote further reform, our model illuminates the role of veto players in preventing reform reversals. We show that reform reversals are never more likely, and may be less likely, when veto players are numerous. Intuitively, overturning an efficient status quo requires that special interests compensate each veto player, which is clearly more attractive when veto players are few in number. We find support for this prediction in a cross-sectional analysis of the determinants of reform reversals in postcommunist countries. (The measure of economic reform that we use

records no reversals for many countries, so within-country analysis would be uninformative.) Controlling for various country characteristics, we find that reform reversals are less common in countries with more veto players.

Although the setting is different, our paper has obvious antecedents in the large literature on veto players and economic reform (e.g., Hallerberg and Basinger, 1998; Henisz and Mansfield, 2006; Mansfield, Milner and Pevehouse, 2007; Treisman, 2000; Franzese, 2002; Keefer and Stasavage, 2003; Tsebelis, 2000). Haggard and Kaufman (1995, esp. ch. 5) present the conventional perspective, arguing that economic reform is less likely when party systems are fragmented and polarized. Haggard and McCubbins (2001) offer a related argument, contrasting the "separation of powers" defined by constitutions with the "separation of purpose" that arises when veto players have diverse views. For our argument, it is the separation of powers that is important—in contrast to standard veto-players theory, we show that policy may be affected by the number of veto players even when those actors have identical preferences—though our most important results assume a separation of purpose between special interests (who do not have formal veto power) and constitutional actors whose approval is necessary to overturn the status quo.

The early experience of Poland in postsocialist economic reform reinforced the conventional understanding, with policy gridlock (especially in reform and privatization of state-owned enterprises) seemingly related to political fragmentation produced by Poland's constitutional separation of powers and electoral system (Balcerowicz, 1994; Keefer and Shirley, 2001). Over time, however, it appeared that postcommunist countries with more veto players had generally progressed further from the communist status quo. Hellman (1998) explains this pattern by suggesting that a "partial-reform equilibrium"—an inefficient state where further reform is blocked by special interests—is less likely when veto players are numerous. We explore this argument in our formal analysis. Similarly, Frye and Mansfield (2003) show that trade liberalization was more likely in postcommunist countries with fragmented political power, Andrews and Montinola (2004) link progress on institutionalization of the

rule of law to a large number of veto players, and Horowitz and Browne (2008) examine the interactive effect of party fragmentation and ideological consensus on economic reform. Our argument builds on this work by specifying the precise conditions under which the presence of multiple veto players encourages economic reform and by testing the predictions of this model using data from 25 postcommunist countries.

Beyond the postcommunist context, our paper joins a small but growing literature that expands upon the conventional understanding of the role of veto players in policy change. Franzese (2007) argues that the presence of multiple policymakers affects policy not only by privileging the status quo but also through common-pool and bargaining effects, and he shows how these various effects can be identified empirically. Murillo and Martínez-Gallardo (2007) find evidence that the number of veto players is positively associated with market reform in Latin America, a result consistent with the pattern that we document in Eastern Europe and the former Soviet Union, though veto players are not the primary focus of their investigation. Finally, Scartascini, Stein and Tommasi (2008) demonstrate that policy adjustment might be facilitated by increasing the number of veto players if intertemporal bargaining among veto players is allowed, a mechanism different from that we identify. Our analysis contributes to this literature by showing that the presence of multiple veto players may reduce the power of special interests and so increase the likelihood of reform.

Beyond the obvious connections to the veto-player literature, our modeling approach builds on Persson, Roland and Tabellini (1997), who model the Enlightenment argument that separation of powers reduces opportunistic behavior among elected officials. Although our environment is very different, rent seeking in our model is also reduced when formal checks and balances require that multiple actors approve any deviation from the status quo. Rather than the immediate consequence of actions by elected officials, however, this outcome results from the decision of special interests to forego influence activities when veto players are numerous. In essence, veto players act as toll takers (Shleifer and Vishny, 1993), though they collect tolls only when special interests ask them to do something different than they

otherwise would. The larger the number of toll takers (veto players), the less likely are special interests to lobby for policies that would pass only if tolls (e.g., bribes or campaign contributions) are paid.

Empirically, our paper contributes to a vast literature that attempts to explain variation in economic reform in postcommunist countries. Frye (2006) divides the literature into deep and middle-range causal mechanisms. Deep mechanisms include initial conditions for economic reform such as bureaucratic legacy and the relative distortions of central planning (de Melo et al., 2001; Kitschelt, 2001). Middle-range theories invoke instead the political and economic institutions that emerged in the early years of transition (Fish, 1998; Frye and Mansfield, 2003). Of course, institutions are themselves shaped by initial conditions, leading some to stress the tactics chosen by policy makers during the transition period to overcome historical constraints (Shleifer and Treisman, 2000). Recent work has shown that these institutional figurations take on additional importance over time as citizenries learn how to operate within them (Mishler and Rose, 2007).

Although insightful, a concentration on long-range determinants and the institutions that they shape provides less help in understanding the changing motivations of political and economic actors as reform progresses. A separate strand of the literature stresses instead the influence of reform sequencing on future policy choices (e.g., Dewatripont and Roland, 1992, 1995), as when privatization of state-owned enterprises to a narrow elite discourages the development of critical regulatory institutions (Sonin, 2003; Hoff and Stiglitz, 2004). Legacy and initial institutions help inform early choices, but they provide less insight thereafter. Our work advances this literature in two ways. First, it provides a theory that takes into account the contingent effect of political institutions at various levels of reform. Second, it tests this theory with an empirical strategy that addresses explicitly the potential endogeneity of political institutions to economic reform.

The paper proceeds as follows. In Section 1 we present our theoretical argument, demonstrating formally the contribution of veto players to furthering economic reform and prevent-

ing reform reversals when special interests are in a position to lobby for inefficient policies. In Section 2 we test our argument through an empirical analysis of economic reform in Eastern Europe and the former Soviet Union. We offer concluding thoughts in Section 3.

1 Model

1.1 Environment

In this section we show formally that the presence of multiple veto players may reduce the power of special interests to lobby for inefficient policies. Although our theoretical framework is general, we focus for simplicity on a stylized policy setting in which three reform outcomes are possible, with "full reform" efficient relative to "partial reform" and "no reform." We assume that the policy preferences of special interests are not necessarily aligned with those of veto players, who for unmodeled reasons are inclined to pursue policies that maximize social welfare but may be persuaded otherwise through the promise of contributions. Grossman and Helpman (1994) adopt the identical assumption in their canonical model of special-interest politics. In many settings, including the postcommunist context that is the focus of our empirical work, this arguably captures the tension between political demand for change and the desire of special interests to either block that change or redirect it toward inefficient policies. Nonetheless, any empirical strategy to test the model's predictions must take into account the preferences of veto players not captured by our baseline theoretical framework; we address this issue further below.

Formally, consider an environment with three sets of players: an organized group (denoted O), an unorganized group (denoted U), and one or more veto players. At stake is a policy $x \in \{0, 1, 2\}$, where $x = 0$ is no reform, $x = 1$ is partial reform, and $x = 2$ is full reform. Denote by $\bar{x}$ the status quo policy. Both the organized and unorganized groups have preferences over policy represented by ω_{Gx}, which is the payoff to group $G \in \{O, U\}$ when policy x is implemented. We normalize $\omega_{O0} = w_{U0} = 0$ and assume that $\omega_{O2} + \omega_{U2} > \max[0, \omega_{O1} + \omega_{U1}]$. Thus, full reform ($x = 2$) is efficient. In contrast, partial reform may be inefficient relative to

no reform; our conclusions about the role of veto players in economic reform depend critically on whether $\omega_{O1} + \omega_{U1}$ is greater than or less than zero.

Policy is chosen by one or more veto players $j = 1, \ldots, J$, where $j = 1$ is the agenda setter, and $j = 2, \ldots$ are "ratifiers." Assume for now that veto players have identical preferences represented by the utility function

$$U_j = \alpha \left(\omega_{Ox} + \omega_{Ux} \right) + C_j \left(x_j \right), \tag{1}$$

where $\alpha > 0$ is an exogenous parameter and $C_j \left(x_j \right) \geq 0$ is a contribution, defined below, promised by the organized group to veto player j in return for choosing policy x_j (which may or may not be the policy x that is ultimately implemented). The organized group incurs a cost from contributions equal to $\sum_j C_j \left(x_j \right)$. Equation 1 captures in a reduced-form way the assumption that veto players are motivated to increase social welfare, but that organized groups may influence policy by promising contributions that can be used to finance political campaigns or personal consumption. As in the Grossman-Helpman model, we assume that the organized group's promises of contributions are credible; this assumption can be motivated either by reputational concerns or by treating the exchange of money for policy as a more-or-less simultaneous transaction. For simplicity, we suppress j when considering the case of one veto player.

Assume the following timing of events. The organized group presents the contribution function $C_1 \left(x_1 \right)$ to the agenda setter, who then chooses $x_1 \in \{0, 1, 2\}$ to maximize utility as in Equation 1. Note that the policy payoff from x_1 depends on whether that policy is subsequently implemented or not. In particular, if the agenda setter is the only veto player or if $x_1 = \bar{x}$, then x_1 is implemented. Otherwise, the organized group presents $C_2 \left(x_2 \right)$ to the second veto player (the first ratifier), who then chooses $x_2 \in \{\bar{x}\} \cup \{x_1\}$ to maximize utility as in Equation 1. The process continues until $j = J$ or some veto player j chooses $x_j = \bar{x}$, whichever comes first. This process captures the idea that any departure from the status quo must be initiated by the agenda setter, that any such change must be approved by all ratifiers, and that the organized group can lobby each veto player.

1.2 One Veto Player

Given the assumption that contributions enter linearly into both the veto player's and organized group's utilities, the equilibrium policy x^* maximizes the joint payoff of the veto player and the organized group:

$$x^* = \arg \max_{x \in \{0,1,2\}} \alpha \left(\omega_{Ox} + \omega_{Ux}\right) + \omega_{Ox}. \tag{2}$$

By assumption, $\omega_{O2} + \omega_{U2} > \max\left[0, \omega_{O1} + \omega_{U1}\right]$, so that the equilibrium policy is full reform ($x^* = 2$) if the weight α that the politician places on social welfare is sufficiently large. In contrast, for α sufficiently small, x^* is skewed away from the social optimum if the organized group does not most prefer $x = 2$. This standard result follows from the assumption that bargaining between the organized group and the veto player is efficient.

1.3 Multiple Veto Players and Movement toward Full Reform

Our argument can be seen most clearly by comparing the equilibrium outcome with one veto player to that with two veto players. As will become clear shortly, the argument extends straightforwardly to the case of an arbitrary number of veto players. We examine first the case of a status quo of no reform ($\bar{x} = 0$).

To derive the equilibrium outcome when there are two veto players, we begin by considering the play of the organized group and the ratifier ($j = 2$) when the agenda setter ($j = 1$) has proposed x_1. By assumption, the only possible outcome when $x_1 = \bar{x} = 0$ is the status quo. Consider, then, $x_1 \neq 0$. Given the assumption of efficient bargaining between the organized group and the ratifier, the optimal policy $x_2^*\left(x_1\right)$ solves

$$\max_{x \in \{0, x_1\}} \alpha \left(\omega_{Ox} + \omega_{Ux}\right) + \omega_{Ox}, \tag{3}$$

which differs from Equation 2 in that the only possible policy choices are x_1 and the status quo $\bar{x} = 0$. To induce this outcome, the organized group must compensate the ratifier for any reduction in policy payoff from implementing $x_2^*\left(x_1\right)$ rather than choosing $x \in \{0, x_1\}$ to maximize $\alpha \left(\omega_{Ox} + \omega_{Ux}\right)$, i.e., rather than ignoring the promised contribution and maximizing

social welfare, given that the agenda setter has proposed x_1:

$$C_2^*\left(x_2^*\left(x_1\right);x_1\right)=\alpha\left(\omega_{O\hat{x}}+\omega_{U\hat{x}}\right)-\alpha\left(\omega_{Ox_2^*}+\omega_{Ux_2^*}\right) \tag{4}$$
$$\text{s.t. } \hat{x}=\arg\max_{x\in\{0,x_1\}}\alpha\left(\omega_{Ox}+\omega_{Ux}\right),$$

where x_2^* solves Equation 3.

Equation 4 says that the organized group provides a contribution to the ratifier to implement $x_2^*\in\{0,x_1\}$ if the ratifier is inclined to act otherwise (i.e., if $x_2^*\neq\hat{x}$). We can simplify the analysis by ignoring equilibria in which $x_2^*=0$: the organized group would never strictly prefer to induce the agenda setter to propose a policy that the ratifier would subsequently veto, as the same policy could be had more cheaply by inducing the agenda setter to propose $x_1=0$ (which, by assumption, is immediately implemented).[2] Thus, for observations of x_1 on the equilibrium path, we can write the equilibrium contribution as

$$\begin{aligned} C_2^*\left(x_1\right)&=\max\left[0,-\alpha\left(\omega_{O1}+\omega_{U1}\right)\right] \text{ if } x_1=1, \\ &=0 \text{ if } x_1=2. \end{aligned} \tag{5}$$

The second equality makes use of the assumption that full reform is efficient relative to partial reform (i.e., $\alpha\left(\omega_{O2}+\omega_{U2}\right)>0$).

Equation 5 defines the cost to the organized group of inducing ratification of some $x_1\neq 0$. Given that equilibrium policy maximizes the joint payoff of the agenda setter and the organized group, x^* is thus

$$x^*=\max_{x\in\{0,1,2\}}\alpha\left(\omega_{Ox}+\omega_{Ux}\right)+\left[\omega_{Ox}-C_2^*\left(x\right)\right], \tag{6}$$
$$\begin{aligned} \text{s.t. } C_2^*\left(x\right)&=\max\left[0,-\alpha\left(\omega_{O1}+\omega_{U1}\right)\right] \text{ if } x=1 \\ &=0 \text{ otherwise,} \end{aligned}$$

which incorporates the cost to the organized group of inducing ratification of any policy proposed by the agenda setter. (Equilibrium policy must also satisfy a "ratification constraint,"

such that the ratifier prefers not to veto the policy that solves Equation 6. However, this constraint does not bind when veto players have identical preferences.[3])

Comparing Equations 2 and 6, we see that when $\omega_{O1} + \omega_{U1} < 0$, partial reform is less likely—in the sense that an equilibrium with $x^* = 1$ exists for a smaller region of the parameter space—with two veto players than with one. Because partial reform is inefficient relative to the status quo, any veto player must be compensated for $x = 1$ to be implemented, making partial reform more costly to the organized group when there are multiple veto players. Whether this results in more or less reform depends on whether the organized group chooses to induce $x = 2$ or $x = 0$ as partial reform becomes prohibitively costly, i.e., on whether $\alpha(\omega_{O2} + \omega_{U2}) + \omega_{O2}$ is greater than or less than zero.

In particular, if $\omega_{O1} + \omega_{U1} < 0$ and $\alpha(\omega_{O2} + \omega_{U2}) + \omega_{O2} > 0$, then an increase in the number of veto players decreases the likelihood of partial reform and increases that of full reform. In contrast, if $\omega_{O1} + \omega_{U1} < 0$ and $\alpha(\omega_{O2} + \omega_{U2}) + \omega_{O2} < 0$, then an increase in the number of veto players decreases the likelihood of partial reform and increases the likelihood that policy remains at the status quo of no reform. Finally, if $\omega_{O1} + \omega_{U1} \geq 0$, then equilibrium policy is unaffected by the number of veto players.

We summarize these observations in the following proposition, which we present in general form: when $\omega_{O1} + \omega_{U1} < 0$, *any* ratifier must be provided with $-\alpha(\omega_{O1} + \omega_{U1})$ to implement the proposal $x_1 = 1$, so that partial reform is less likely, the larger the number of veto players. (We use the formulation "likelihood of a policy is smaller/larger" to mean that equilibria with that policy outcome exist for a smaller/larger region of the parameter space.)

Proposition 1. *Consider the case of a status quo characterized by no reform. If partial reform is inefficient relative to no reform, then the likelihood of a) partial reform is smaller, and b) either full reform or no reform is larger (depending on whether* $\alpha(\omega_{O2} + \omega_{U2}) + \omega_{O2}$ *is greater or less than zero, respectively) when veto players are numerous. In contrast, if partial reform is efficient relative to no reform, then equilibrium policy is unaffected by the number of veto players.*

Proposition 1 is the key theoretical contribution of this paper. Recall that $\omega_{O2} + \omega_{U2} > 0$ by assumption—that is, full reform is efficient relative to no reform. Then when partial reform is inefficient relative to no reform (i.e., when $\omega_{O1} + \omega_{U1} < 0$), the relationship between veto players and economic reform is positive, if any of three conditions hold:

1. The organized group prefers full reform to no reform ($\omega_{O2} > 0$).
2. The payoff to the unorganized group from full reform is large (ω_{U2} is large) .
3. Veto players sufficiently value social welfare relative to contributions (α is large).

Below we argue that both $\omega_{O1} + \omega_{U1} < 0$ and Condition 2 held in postcommunist countries during the transition period that is the focus of our empirical work.

1.4 Reform Reversals and "Partial-Reform Equilibrium"

An important question in the literature on the political economy of reform is how reforms can be made irreversible (e.g., Fernandez and Rodrik, 1991; Dewatripont and Roland, 1992, 1995). The conventional wisdom is that reform reversals are less likely in the presence of multiple veto players, as policy change becomes more difficult when veto players are numerous.

A related question is what institutional arrangements can prevent the emergence of a "partial-reform equilibrium," an inefficient state where further reform is blocked by special interests who benefit from incomplete reform. In the seminal contribution to the literature, Hellman (1998) argues that this outcome may be less likely when veto players are numerous, as "[b]roader coalition governments should have a lower risk of being captured" (p. 230).

How is the possibility of reform reversals affected by the number of veto players? Is a "partial-reform equilibrium" less likely when veto players are numerous? To answer these questions, we consider the equilibrium outcome when the status quo is partial and full reform, respectively. As before, we illustrate our results by comparing the cases of one and two veto players, though our arguments generalize to an arbitrary number of veto players.

With one veto player, the outcome is identical to that when the status quo is no reform:

equilibrium policy maximizes the joint utility of the veto player and the organized group. Consider, then, the case of two veto players and a status quo of partial reform ($\bar{x} = 1$). As before, we may derive the contribution to the ratifier for observations of x_1 on the equilibrium path, using the assumption that full reform is efficient and the fact that the organized group would never prefer to induce a proposal that would subsequently be vetoed:

$$\begin{aligned} C_2^*(x_1) &= \max\left[0, \alpha\left(\omega_{O1} + \omega_{U1}\right)\right] \text{ if } x_1 = 0 \\ &= 0 \text{ if } x_1 = 2. \end{aligned} \tag{7}$$

This in turn implies that equilibrium policy is

$$\begin{aligned} x^* = \max_{x \in \{0,1,2\}} & \alpha\left(\omega_{Ox} + \omega_{Ux}\right) + \left[\omega_{Ox} - C_2^*(x)\right], \\ \text{s.t. } C_2^*(x) &= \max\left[0, \alpha\left(\omega_{O1} + \omega_{U1}\right)\right] \text{ if } x = 0 \\ &= 0 \text{ otherwise.} \end{aligned} \tag{8}$$

Only the proposal $x_1 = 0$ requires a transfer from the organized group to the ratifier, and that only when $\omega_{O1} + \omega_{U1} > 0$, i.e., when no reform is inefficient relative to partial reform. Whether this in turn makes full reform more likely depends on whether the organized group chooses to induce $x = 1$ or $x = 2$ as a reform reversal becomes prohibitively costly. In contrast, when no reform is efficient relative to partial reform—the case examined by Hellman (1998)—the number of veto players is irrelevant to the equilibrium outcome.

Proposition 2. *Consider the case of a status quo characterized by partial reform. If no reform is inefficient relative to partial reform, then the likelihood of a) a reversal to no reform is smaller, and b) either partial reform (no change) or full reform is larger (depending on whether* $\alpha\left(\omega_{O1} + \omega_{U1}\right) + \omega_{O1}$ *is larger or smaller than* $\alpha\left(\omega_{O2} + \omega_{U2}\right) + \omega_{O2}$*, respectively) when veto players are numerous. In contrast, if no reform is efficient relative to partial reform, then equilibrium policy is unaffected by the number of veto players.*

Finally, if the status quo is full reform ($\bar{x} = 2$), equilibrium policy is

$$x^* = \max_{x \in \{0,1,2\}} \alpha\left(\omega_{Ox} + \omega_{Ux}\right) + \left[\omega_{Ox} - C_2^*\left(x\right)\right], \tag{9}$$

$$\text{s.t. } C_2^*\left(x\right) = \alpha\left(\omega_{O2} + \omega_{U2}\right) \text{ if } x = 0$$

$$= \alpha\left(\omega_{O2} + \omega_{U2}\right) - \alpha\left(\omega_{O1} + \omega_{U1}\right) \text{ if } x = 1$$

$$= 0 \text{ if } x = 2.$$

Because full reform is efficient (i.e., $\omega_{O2} + \omega_{U2} > \max\left[0, \omega_{O1} + \omega_{U1}\right]$), any proposal $x_1 \neq 2$ that is ratified—*any* reversal from full reform—is more costly to the organized group when veto players are numerous, as each veto player must be compensated to implement a policy that is socially inefficient.

Proposition 3. *Consider the case of a status quo characterized by full reform. Any reform reversal is less likely when veto players are numerous.*

Together with Proposition 2, this suggests that an increase in the number of veto players works to prevent reform reversals when a reversal would result in a policy that is inefficient relative to the status quo, but contra Hellman (1998) does not discourage the emergence of a "partial-reform equilibrium" when no reform is efficient relative to partial reform.

1.5 Veto Players with Heterogeneous Preferences

Up to now, we have focused on the tension between political demand for change and the power of organized groups to divert policy to their own interests by assuming that veto players have identical preferences and an inclination to support policies that improve social welfare. The model, however, may be easily extended to incorporate more general preferences among veto players by assuming that veto players have preferences represented by the quasi-linear utility function

$$U_j = v_j(x) + C_j\left(x_j\right).$$

In the baseline model, $v_j(x) = \alpha\left(\omega_{Ox} + \omega_{Ux}\right)$ for all j.

How does this generalization affect the results of the model? As before, the cost to the organized group of inducing some policy is increasing in the number of veto players who must be compensated to ratify that policy. In particular, assume that $v_j(x) = \alpha\omega_{Ux}$ for all j, which represents a preference for policies that benefit the unorganized group (rather than a desire to maximize social welfare, as in the baseline model). Then if the status quo is no reform, full reform is more likely when veto players are numerous if a) the unorganized group prefers no reform to partial reform, and b) the payoff to the unorganized group from full reform is large.

On the other hand, the presence of *any* veto player whose preferences are aligned with those of the organized group may be sufficient to block implementation of some policy. To see this, assume that some veto player j has $v_j(x) = \alpha\omega_{Ox}$. Then the policy that maximizes the joint payoff of veto player j and the organized group is the policy most preferred by the organized group. This implies that the presence of a veto player with these preferences is sufficient to block policies that the organized group finds less preferable than the status quo.

In terms of empirical work, this implies that we must control for the presence of veto players whose preferences are aligned with those of special interests. We pick up this theme in the following section.

2 Evidence

2.1 Empirical Strategy

Our model generates distinct empirical predictions with respect to movement toward full reform and reform reversals. We therefore test these predictions separately, focusing first on movement toward full reform and then reform reversals, using data on veto players and economic reform in 25 postcommunist countries. The postcommunist context offers an attractive setting for this investigation, given the similar starting points and reform objectives of countries in the region.

With respect to movement toward full reform, Propositions 1 and 2 together suggest

a causal effect of veto players that may be conditional on the status quo. Identifying the particular effect requires some knowledge of the environment in which policy is made. In the postcommunist context, a reasonable assumption is that the benefit from full reform was large for most citizens, if not for certain organized groups. Decades of communism had failed to produce intensive (total factor productivity) growth, and with extensive growth exhausted, growth rates had slowed dramatically (Kornai, 1992). Popular desire to close the widening material gap with the West was arguably the primary motivation for citizens throughout Eastern Europe and the Soviet Union to call for change, and although full reform threatened some elites, it held the potential to improve the lives of millions.

At the same time, there is considerable evidence that partial reform may have left many citizens worse off than would have been the case had they engaged in no reform whatsoever, that is, that partial reform was inefficient relative to no reform. Average growth rates during the transition period were lowest for countries that implemented moderate reform; reform laggards such as Belarus and Turkmenistan performed approximately as well as high-reform countries such as Hungary and the Czech Republic (e.g., Hellman, 1998; Slantchev, 2005; Frye, 2008).[4]

Moreover, it appears that this possibility was understood by policy makers and their advisors at the beginning of transition. Several communist countries had already experimented with a type of partial reform during the 1970s and 1980s, decentralizing control over hiring and production decisions to state-enterprise managers. Without a concomitant hardening of enterprise budget constraints, these reforms produced massive macroeconomic imbalances but failed to increase firm efficiency. As a result, long-time observers maintained at the beginning of transition that there was no "third way," i.e., that only a full transition to capitalism could address the fundamental inefficiencies of socialism (see especially Kornai, 1990).

In terms of our theoretical model, partial reform was thus inefficient relative to no reform ($\omega_{O1} + \omega_{U1} < 0$) and the payoff to the unorganized group from full reform (ω_{U2}) was large.

These considerations suggest that movement toward full reform should be positively related to the number of veto players when the status quo is no or little reform (Proposition 1) but unrelated to the number of veto players at higher levels of reform (Proposition 2). To test this prediction of a conditional effect, we use annual data on reform from 25 postcommunist countries to estimate the following model:

$$r_{it}^{+} = \phi r_{it-1} + \psi v_{it} + \delta r_{it-1} v_{it} + \beta X_{it} + \gamma W_{it} + \alpha_i + \eta_t + u_{it}, \tag{10}$$

where r_{it} is the level of reform in country i in year t and $r_{it}^{+} = \max[r_{it}, r_{it-1}]$; v_{it} is a measure of veto players; X_{it} is a vector of exogenous time-varying country characteristics; W_{it} is a vector of endogenous time-varying country characteristics; α_i and η_t are country and year fixed effects, respectively; u_{it} is an idiosyncratic error; and ϕ, ψ, δ, β, and γ are (vectors of) parameters to be estimated. The fixed effects control for any time-invariant country-level heterogeneity and shocks that are common to the countries in the sample. The variable r_{it}^{+} implies that we investigate only movement toward full reform; further below we consider reform reversals. Acemoglu et al. (2008) use a similar specification when considering the possibility that economic crises affect transitions to and from democracy differently. Thus, we expect movement toward reform in country i at time t to depend on the status quo level of reform at the beginning of the period, r_{it-1}; on the number of veto players during that period, v_{it}; on their interaction; and on other variables captured in X_{it} and W_{it}. Our prediction is that the marginal effect of veto players on movement toward full reform, $\psi + \delta r_{it-1}$, should be positive when r_{it-1} is small and zero when r_{it-1} is large. As we discuss below, an important consideration is whether to treat v_{it}, and thus $r_{it-1}v_{it}$, as endogenous to reform.

Estimation of Equation 10 poses two econometric complications. First, because r_{it-1}^{+} is correlated with α_i by construction, both r_{it-1} and $r_{it-1}v_{it}$ are correlated with α_i. In the jargon of the literature, r_{it-1} is "predetermined"; by extension, so is $r_{it-1}v_{it}$ if v_{it} is exogenous. As is well known, estimation of dynamic panel data models such as this by ordinary least squares produces biased and inconsistent parameter estimates, where the bias is of order $\frac{1}{T}$, with T the number of periods (e.g., Wawro, 2002). Second, the endogenous variables in

W_{it} imply that estimation of Equation 10 by ordinary least squares would produce biased and inconsistent parameter estimates, even in the absence of the predetermined variables. Clearly, this problem would be compounded if v_{it}, and thus $r_{it-1}v_{it}$, were also endogenous

Thus, in addition to estimating Equation 10 by ordinary least squares, we use a "difference GMM" (generalized method of moments) estimator suggested by Arellano and Bond (1991). The logic of this estimator is to first difference the levels equation (Equation 10) to remove the unit fixed effects, and then to instrument the first-differenced predetermined and endogenous variables on lags of the levels variables sufficiently deep to be uncorrelated with the first-differenced error term $(u_{it} - u_{it-1})$. (We discuss the particular lag structure just below.) The key assumption of the model is that the u_{it} are serially uncorrelated, which can be tested by checking that the first-differenced residuals do not exhibit second-order serial correlation.

In our setting, the Arellano-Bond model poses two additional complications. First, a large instrument set, i.e., the use of a large number of lags as instruments, may overfit the predetermined and endogenous variables and consequently bias parameter estimates toward their OLS counterparts. Equation 10 compounds this problem by including a large number of variables to be instrumented (the first differences of r_{it-1}, $r_{it-1}v_{it}$, W_{it}, and possibly v_{it}). We minimize this concern by using only the first available lag for any variable to be instrumented. Thus, we instrument the first difference of r_{it-1} on r_{it-2}, the first difference of W_{it} on W_{it-2}, and the first difference of v_{it} (when treated as endogenous) on v_{it-2}. Second, the interaction term $r_{it-1}v_{it}$ suggests a number of possible instruments, regardless of whether we treat v_{it} as exogenous or endogenous. We report results from specifications in which we instrument the first difference of $r_{it-1}v_{it}$ on $r_{it-2}v_{it-1}$ when treating v_{it} as exogenous and on $r_{it-3}v_{it-2}$ when treating v_{it} as endogenous (i.e., we instrument on the first exogenous lag of the levels variable). As we discuss below, our results are robust to instead instrumenting the first difference of $r_{it-1}v_{it}$ on $r_{it-2}v_{it}$ when treating v_{it} as exogenous and on $r_{it-2}v_{it-2}$ when treating v_{it} as endogenous (i.e., we construct interaction terms whose components are just as deep as necessary for exogeneity).

For our second empirical exercise, we examine the impact of veto players on movement away from full reform, i.e., on reform reversals. Propositions 2 and 3 predict the following:

1. If the status quo is partial reform and partial reform is inefficient relative to no reform, then the likelihood of a reform reversal is unaffected by the number of veto players.
2. If the status quo is partial reform and partial reform is efficient relative to no reform, then the likelihood of a reform reversal is smaller when the number of veto players is large.
3. If the status quo is full reform, then the likelihood of a reform reversal is smaller when the number of veto players is large.

In the postcommunist context, as discussed above, partial reform was arguably inefficient relative to the status quo. This suggests a conditional effect of veto players on reform reversals, with reversals negatively associated with the number of veto players only when the status quo is full reform. To test this prediction of a conditional effect, we could estimate a model like Equation 10, replacing r_{it}^{+} as a dependent variable with $\Pr\left(r_{it} < r_{it-1}\right)$ Unfortunately, the measure of economic reform that we introduce below records too few reversals for within-country analysis to be informative: roughly one third of the countries in the sample experience no reversals at any point during the thirteen-year period that we examine.

We therefore test the weaker prediction that reform reversals should be no more likely when veto players are numerous, i.e., that the relationship between veto players and reversals should be zero or negative. To do so, we estimate a cross-sectional model where the dependent variable R_i is the *total* number of reversals in country i between 1992 and 2004, and the key independent variable is the *average* number of veto players v_i over that time period. In particular, we assume that the generation of reform reversals in a country follows a Poisson process, with

$$E\left(R_i \mid v_i, X_i\right) = V\left(R_i \mid v_i, X_i\right) = \exp\left[\psi v_i + \beta X_i + u_i\right]. \tag{11}$$

In this equation, X_i is a vector of control variables; u_i is an idiosyncratic error; and ψ and β are (vectors of) parameters to be estimated. Our prediction is that $\psi \leq 0$.

One potential concern in estimating Equation 11 is that the average number of veto players v_i may be correlated with the error term u_i. Given that we sum over R_{it} to obtain R_i and average over v_{it} to obtain v_i, such a correlation could arise if a reversal decision R_{it} in country i at time t affected the number of veto players v_{is} at time $s \geq t$. For example, new veto players may form in response to policy debates about reform legislation, as when rapid reform empowered postcommunist successor and peasant parties (Grzymala-Busse, 2002). More drastically, constitutional reform inspired by disappointing reform outcomes could change the number of veto players, as happened following the Orange Revolution in Ukraine.

To address this possible reverse causality, we check the robustness of our results to replacing v_i with v_{i1}, the number of veto players at the beginning of transition. This identification strategy would fail if the number of veto players at $t = 1$ was affected by the process generating reversals at $t = 1$. In practice, however, reform reversals generally became a major issue only later in the economic transition.

One remaining concern that is inherent to this cross-section research design, where we cannot take advantage of within-country variation to control for time-invariant country characteristics, is the possibility of country heterogeneity not reflected in X_i. If correlated with both the number of veto players and the process by which reversals are generated, the omission of such variables would produce biased and inconsistent parameter estimates. In principle, we might address this concern by identifying a variable correlated with v_i but otherwise uncorrelated with the process by which reversals are generated at all t, and instrumenting v_i on that variable. In practice, this is difficult because we include in X_i a proxy for the preferences of veto players not captured by our model: representation of communists in the legislature. (We address potential simultaneity concerns with this variable in the manner discussed in the previous paragraph.) Just as unobserved variables may be correlated with the number of veto players, so may unobserved variables also be correlated with communists in the legislature. Any empirical strategy to address omitted-variable bias must therefore

identify *distinct* exogenous sources of variation in these two variables. Although such instrument sets may exist in principle, they are not currently known to scholars of transition. To the extent that unobserved country heterogeneity is a concern, the results of our analysis of reform reversals should thus be treated with somewhat greater caution than of our analysis of movement toward full reform.

2.2 Data

To operationalize our two dependent variables, and also to capture the conditional effect of veto players on movement toward reform, we follow numerous studies in measuring the extent of economic reform in postcommunist countries with yearly indexes provided by the European Bank for Reconstruction and Development (e.g., EBRD, 2005). We focus on 25 countries in Eastern Europe and the former Soviet Union for which the EBRD provides data and for which we also have data for other measures discussed below.[5] EBRD evaluates reform progress on a scale from 1 to 4.3 along eight policy dimensions: large-scale privatization, small-scale privatization, governance and enterprise restructuring, price liberalization, trade and foreign-exchange system, competition policy, banking reform and interest-rate liberalization, and securities markets and non-bank financial institutions. The average of these eight variables—the EBRD Average Transition Indicator—is a widely cited indicator of economic reform in postcommunist states. To facilitate interpretation of results, we rescale this variable to take values from 0 to 100.

We created two dependent variables to test the distinct empirical predictions of our theoretical model with respect to movement toward full reform and reform reversals. We define the first dependent variable as $\max[EBRD_{it}, EBRD_{it-1}]$, which considers only positive changes in the reform index from year $t-1$ to year t; we refer to this as *movement toward full reform*. The implicit assumption is that countries that are moving toward full reform have larger single-year reform movements. We provide support for this assumption in the online appendix, where we show that both the level and coherence of reform at the end of its trajectory is greater in countries with large single-year movements.

For the second analysis, we calculate *reform reversals* for each of the 25 countries in the data set, defining that variable as a negative change on any one of the eight individual EBRD policy dimensions in a given year.[6] We identify a total of thirty-one reform reversals in the data set. Uzbekistan has the maximum number, with five policy reversals from 1992 to 2004, whereas eight countries have no reversals at all. The majority of reversals take place in the late 1990s, and there is at least one negative change along every policy dimension but small privatization.

To examine the influence of veto players on economic reform, we adopt a widely employed measure of *veto players* (the CHECKS variable) from the Database of Political Institutions (Beck et al., 2001). As described there and in Keefer and Stasavage (2003), this measure is constructed from a count of distinct parties in government coalitions (for parliamentary systems) or in control of the executive and legislative chambers (for presidential systems). For our purposes, this measure is preferable to an alternative measure of veto players provided by Henisz (2000), as the spatial model on which the Henisz measure is based implicitly assumes that the marginal effect of veto players on policy change is decreasing in the number of veto players; our model implies no such effect. Nonetheless, we obtain similar qualitative results with the Henisz measure of political constraints, as well as with a measure of political fragmentation constructed by Frye, Hellman and Tucker (2000) that is available through 2000. To construct a balanced panel (especially important for the Arellano-Bond specification), we impute missing values for our measure of veto players from lagged values of the same measure and from contemporaneous values of the Henisz and Frye et al. measures.[7] For the panel analysis of movement toward full reform, we use the annual value of veto players for each country, whereas we use average veto players over the years 1992–2004 in the Poisson analysis of reversals (and in Figure 1 above). As discussed above, we treat veto players as endogenous to reform in some versions of the panel analysis.

Unobserved heterogeneity in the panel analysis is primarily accounted for by country and year fixed effects. We supplement this two-way fixed effects specification with a vector of

controls for time-variant country characteristics. First, following the discussion in Section 1.5, we account for the presence of veto players whose preferences are aligned with those of organized interests. Because the strongest opposition to reform often came from communists aligned with the "red directors" lobby (e.g., McFaul, 1995), we include a measure from Armingeon and Careja (2004) of the proportion of seats in the legislature held by members of unreformed communist parties (*communists in legislature*). So that we have complete data for the 1992–2004 period, we supplement this measure with data from Darden and Grzymala-Busse (2006) for elections to republican legislatures prior to the collapse of the Soviet Union and Yugoslavia; such legislatures generally served until the first postcommunist elections could be held. Second, we use a dummy variable (*war*) to measure whether the country was at war during a particular year in the time series. Third, we control for *log GDP per capita* measured in U.S. dollars.

In the Poisson analysis of reform reversals, we control for average representation of communists in the legislature, as it is critical to account for the presence of veto players inherently opposed to reform. We drop other time-varying factors due to endogeneity concerns, replacing them with a vector of initial conditions, i.e., of variables exogenous to the transition that may have influenced support for economic reform and constitutional choices in transition states. First, we control for the impact of resource wealth on domestic politics by including a dummy variable equal to one if the country is endowed with *natural resources*, using the coding in de Melo et al. (2001). Second, we control for the level of economic development at the beginning of transition, using *1989 GNP per capita* at purchasing power parity in U.S. dollars, from EBRD (2000). Third, countries in physical proximity to the West may have been more likely to adapt their policies and institutions to those of the European Union. We follow Kopstein and Reilly (2000) in controlling for this possibility by including the distance of a country's capital to Vienna or Berlin (*distance from West*), whichever is closer. Finally, inherited industrial structure may have determined both the stakes from economic reform and the desirability of various institutional arrangements. We follow Pop-Eleches (2007)

in using energy efficiency as a proxy for inherited *industrial structure*: the assumption is that economies disproportionately populated by Stalinist-era industrial enterprises are less energy-efficient. In particular, we use GDP per unit of energy use (U.S. dollars per kilogram of oil equivalent) from the World Bank's 2005 World Development Indicators database.[8]

2.3 Analysis

We discuss the results of our empirical analysis separately for our two dependent variables: movement toward full reform and reform reversals.[9]

Movement Toward Full Reform

Table 1 displays the results of the panel analysis, where movement toward full reform is the dependent variable. The first four columns correspond to models estimated by ordinary least squares. Models 1 and 2 include only year fixed effects, so that estimates of the (conditional) effect of veto players on movement toward full reform are based on both cross-country and within-country variation. Models 3 and 4 include both country and year fixed effects. For all four models, as predicted, the number of veto players is positively and statistically significantly associated with movement toward full reform when status quo reform (measured by the lagged EBRD score) is zero. Further, as predicted, the marginal effect of veto players declines as the status quo level of reform increases, becoming statistically indistinguishable from zero for values of lagged reform close to the mean (48.86). Inconsistent with our model, the marginal effect is negative and statistically significant at the highest observed status-quo level of reform (86.74, in Hungary). The estimated effect of *War* is significant and in the hypothesized direction, whereas the estimated coefficients on other time-varying country characteristics are not significantly different from zero.

The inclusion of country fixed effects controls for time-invariant country characteristics that might be associated with both the number of veto players and movement toward full reform (e.g., distance from the West and the pull of the European Union). As we discuss above, however, the OLS results in Models 1–4 should be treated with caution, as endogeneity

is introduced mechanically into the model by the inclusion of lagged economic reform (alone and in interaction with veto players). In addition, GDP per capita and, perhaps, veto players and communists in the legislature may be endogenous to reform. We address these issues with the Arellano-Bond specifications that we describe above, instrumenting first-differenced variables on the first exogenous lag of the levels variables. Models 5 and 6 present results when the statistical model treats veto players and communists in the legislature as exogenous. Models 7 and 8 endogenize these variables. (With the exception of one value that is marginally significant, the AR(2) test statistics that we report support the key assumption of the model: no first-order serial correlation in levels, which is tested by checking for second-order correlation in first differences.) In both cases, the estimated effect of veto players on economic reform at the communist status quo (economic reform equal to zero) is positive and statistically significant. Further, we find strong empirical support for the prediction that the marginal effect of veto players on movement toward full reform declines as the status quo level of reform increases: in all six models, the estimated interactive effect of veto players and lagged economic reform is negative and statistically significant.

Figure 2 illustrates these results by plotting the marginal effect of veto players at various levels of lagged reform, using parameter estimates from Model 8. Confidence intervals are displayed to provide a sense of the statistical significance of the individual point predictions. When the status quo is no reform (Turkmenistan's score as late as 1993), a one standard deviation increase in veto players (1.67) yields an estimated increase in movement toward full reform of 4.09 points on a 100-point scale. The estimated effect of veto players continues to be positive until the status quo reaches a value of 46.1% reformed by EBRD standards (slightly less than Estonia's score in 1993 and about where Azerbaijan was in 2000), though as the confidence intervals indicate, the estimated effect is insignificant for a wide range of values either side of this point. Finally, at a status quo of 86.7% reformed (a level achieved only by Hungary in 2004), the effect of a one standard deviation increase in veto players on movement toward full reform is estimated to be -3.61 points on the EBRD scale, a

substantively large figure that is just shy of significance at the 0.01 level.

Figure 2 drives home our theoretical point. The presence of multiple veto players may be beneficial in the early stages of reform, but this becomes ineffective once reform is sufficiently advanced. The one inconsistency with our perspective—the negative estimated effect of veto players at very high levels of reform—suggests that the conventional view on veto players may be more informative once broad efficiency-enhancing reforms have been completed. O'Dwyer and Kovalĉík (2007) provide evidence consistent with this finding, showing that "second-generation" reforms (e.g., flat-tax implementation) have been more likely in East European countries with relatively few veto players. Critically, they observe that these reforms are more likely to be opposed by the general public than were first-generation reforms, suggesting a political context different from the primary focus of our paper.

As described in detail in Section 2.1, we employed a different instrumentation strategy in the Arellano-Bond models as a robustness check, constructing instruments for the interaction term by using the minimal lags of veto players and economic reform necessary to ensure exogeneity; our qualitative results are very similar. We also checked that our results are robust to the use of two alternative measures of veto players, as discussed above, finding no substantial differences. Additionally, we verified that our results are not driven by differences between democratic and authoritarian states. Most simply, we confirmed robustness to dropping observations for which Henisz's (2000) measure of political constraints is equal to zero, which corresponds to an authoritarian regime: although the CHECKS variable that we use as a measure of veto players does code authoritarian regimes (the variable takes the lowest possible value if legislative or executive elections are deemed not competitive), most observations of CHECKS with missing values are coded as zero by Henisz. In addition, we take advantage of the Polity IV data set (Marshall and Jaggers, 2006), using a measure of *political competition* (the PARCOMP variable), which captures the extent to which alternative preferences for policy and leadership can be pursued in the political arena. (The composite Polity measure includes constraints on executive decision making, which is analogous to veto

players.) In Models 9 and 10 of Table 1, we include this variable alone and in interaction with lagged economic reform, treating political competition as endogenous. The estimated relationship between veto players and economic reform is robust to this specification change.

Reform Reversals

In the second portion of our analysis, we study the probability of reform reversals. In line with the standard perspective on the role of veto players in policy change, our theoretical model predicts that reversals should be no more likely when veto players are numerous, i.e., that each additional veto player should either make reversals less likely or have no effect. Because the number of country-year reversals is a count variable, we employ a Poisson analysis, the results of which are summarized in Columns 1-4 of Table 1.[10]

Model 1 shows that our measures of initial conditions, although critically important in analyses of reform progress (de Melo et al., 2001; Horowitz, 2004), are statistically insignificant predictors of reversals. An F-test confirms that the four variables are not even jointly significant. As Models 2 and 3 demonstrate, the more important determinants of reform reversals are the number of veto players and the presence of veto players inherently opposed to reform, as proxied by the proportion of communist seats in the legislature. Both variables work in the predicted direction, with reversals less common when veto players are numerous and more common when communists are well represented in the legislature, and the effect of each is precisely estimated.

The results can be illustrated by comparing the predicted Poisson probability distributions for Model 3 for countries with low and high numbers of veto players, respectively, holding other covariates at their mean values. For countries with average veto players at the 25th percentile, multiple reversals are a strong possibility: there is nearly a 70% probability of one, two, or three reversals. Countries with unconstrained executives and zero reversals in the transition period are a rarity. In contrast, for countries with average veto players at the 75th percentile, there is a nearly 45% probability of zero reversals, followed by a 35% probability of a single reversal.[11]

Although suggestive, this analysis may suffer from reverse causality. To address these concerns, in Model 4 we use initial values of veto players and communists in the legislature rather than their averages. The results are qualitatively similar, with an estimated effect of veto players that is substantially larger and more precisely estimated than when average veto players is used instead. As a robustness check, as we discuss above, we substituted two alternative measures of veto players, with no qualitative change in results. We also tried various other covariates, including time spent in war and GDP growth. Although GDP growth especially suffers from obvious endogeneity concerns, inclusion of these variables leaves the estimated coefficient on veto players largely unchanged.

One possible concern with the results in Models 1-4 is that our dependent variable—the number of years in which there was any reform reversal—treats small and large reversals equally. In principle, some small reversals might not be "true" reversals in the sense of our model. Making case-by-case decisions about what reversals constitute real retrenchment could introduce subjectivity into our analysis, so as an alternative approach, we created a measure of "reversal magnitude," defined for each country as the total decline in the eight individual EBRD reform indexes, summed across all years in the sample.[12] We report estimates from OLS models with this dependent variable in Columns 5 and 6 of Table 2. In both cases, the estimated effect of veto players on reversal magnitude is negative, consistent with our theoretical model, which predicts a *weakly* negative effect. The estimated effect is large (one additional veto player reduces reform magnitude by approximately 15 percent) and significantly different from zero when using initial values to address possible endogeneity.

Overall, we find strong support for our second empirical prediction, which is that reform reversals should be no more likely when veto players are numerous, though as noted above the difficulty in accounting for unobserved country heterogeneity in these cross-section regressions means that the results in Table 2 should be treated with somewhat greater caution than those in Table 1.

3 Conclusion

In this paper we take issue with the conventional wisdom that economic reform is more difficult when veto players are numerous. We show formally that veto players may weaken the power of special interests who prefer partial to full reform, thus making movement toward full reform more likely when veto players are numerous. At the same time, we find support for the traditional perspective that reform reversals should be less likely when the approval of many actors is needed to overturn the status quo. A surprising implication of this analysis is that the institutions that make economic reform possible may be precisely those that assure its irreversibility.

We test this theory with data on veto players and economic reform from 25 postcommunist countries, addressing explicitly the potential endogeneity of political institutions to economic reform. Consistent with the predictions of our theoretical model, we find that veto players are positively associated with economic reform, but only when the status quo is associated with little or no reform. In addition, we find that reform reversals are less likely when veto players are numerous.

Further work on this topic might take two routes. First, the simple model in this paper could be generalized in a number of directions. We consider, for example, a stylized setting in which three reform outcomes (no, partial, and full reform) are possible. Other investigations of the relationship between veto players and organized interests may consider different and possibly richer environments. Second, future empirical work might follow our lead in examining the interactive effect of veto players and the status quo policy. Most analysis to date has simply considered the unconditional effect of veto players, generally finding that policy stability is greater when veto players are numerous. As we show, the role of veto players in economic reform may depend critically on the status quo, with the particular nature of that dependence related to the policy environment in which reform is negotiated.

Notes

[1]This relationship holds when controlling for the same country characteristics as in the cross-sectional analysis of Section 2.3 below, a fact that we document in the online appendix.

[2]When $\omega_{O1}+\omega_{U1}<0$, it is an equilibrium for the organized group to induce $x_1=0$ if and only if it is an equilibrium for the organized group to induce $x_1=1$ and for that proposal to be vetoed by the ratifier. The formal difference in these equilibrium outcomes is irrelevant for the discussion to follow.

[3]We examine the conditions under which the ratification constraint is binding further below when we consider veto players with heterogeneous preferences.

[4]We document this relationship in a figure in the online appendix.

[5]The data set includes all countries in Eastern Europe and the former Soviet Union but Bosnia-Herzegovina and Yugoslavia. Tables with summary statistics and correlations are available in the online appendix. Although the EBRD measure is based on expert opinions, it is strongly correlated with variables provided by Campos and Horvath (2006) that measure reform progress along three dimensions (privatization, external liberalization, and internal liberalization) using data on specific policy inputs.

[6]To avoid violating the Poisson assumption that events are independently generated, we choose not to count reversals on different reform dimensions in the same year as separate reversals. Indeed, it is impossible to tell from the data alone whether these policy changes represent distinct policy initiatives. Russia, for instance, recorded negative changes across five policy dimensions in the midst of its 1998 financial crisis. Because the motivation for all these changes was responding to potential financial meltdown, counting these as separate reversals would be inappropriate.

[7]We imputed values for 27 out of 300 country-years. Missing values for the CHECKS variable seem to be driven primarily by war and state breakdown. The following countries have more than two missing values: Azerbaijan, Georgia, Kyrgyzstan, Moldova, and Tajikistan.

[8]We use data from 1992, the first year generally available, for all countries but Azerbaijan,

for which energy efficiency is measured in 1993.

[9]As a robustness check, we also estimated a multinomial logit model where the dependent variable takes a value of 1 if there is any positive reform, 0 if no reform, and -1 if a reform reversal. Although the inclusion of country fixed effects introduces bias because some countries experience no reverals, the results are qualitatively similar. Details are available in the online appendix.

[10]Tests of overdispersion confirm that the Poisson specification is appropriate for this analysis. If overdispersion had been detected, i.e., if the mean and variance of total reversals had been significantly different, then a negative binomial specification would have been preferable.

[11]A figure illustrating these results is available in the online appendix.

[12]Let r_{ijt} be the EBRD index for country i on reform dimension j in year t, and define $d_{ijt} \equiv \max[r_{ijt-1} - r_{ijt}, 0]$. Then for each country i, we calculate $\sum_t \sum_j d_{ijt}$.

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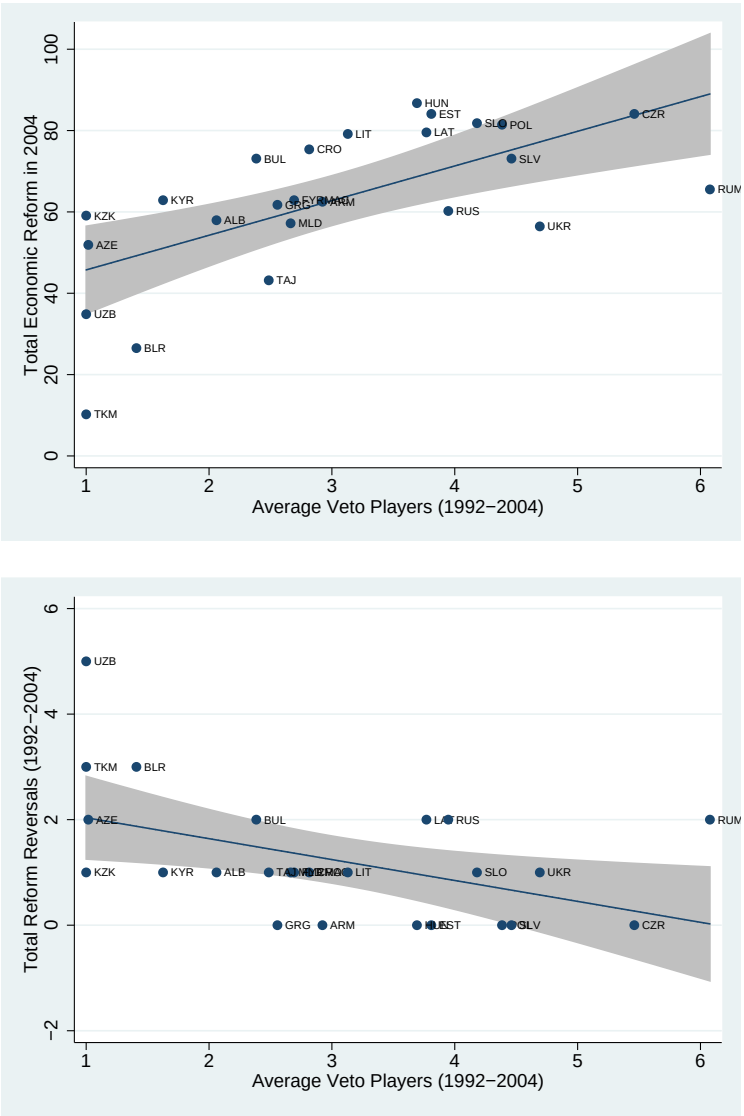

Figure 1: Bivariate relationship between average veto players and economic reform in 2004 (top panel), reform reversals (bottom panel). The figure depicts observed values, fitted values, and 95-percent confidence intervals. Veto players is the CHECKS measure from the Database of Political Institutions (Beck et al., 2001). Economic reform is the EBRD Average Transition Indicator, rescaled to take values from 0 to 100. Reform reversals are calculated as the number of years in which a country recorded a negative change in any of eight individual EBRD Transition Indicators.

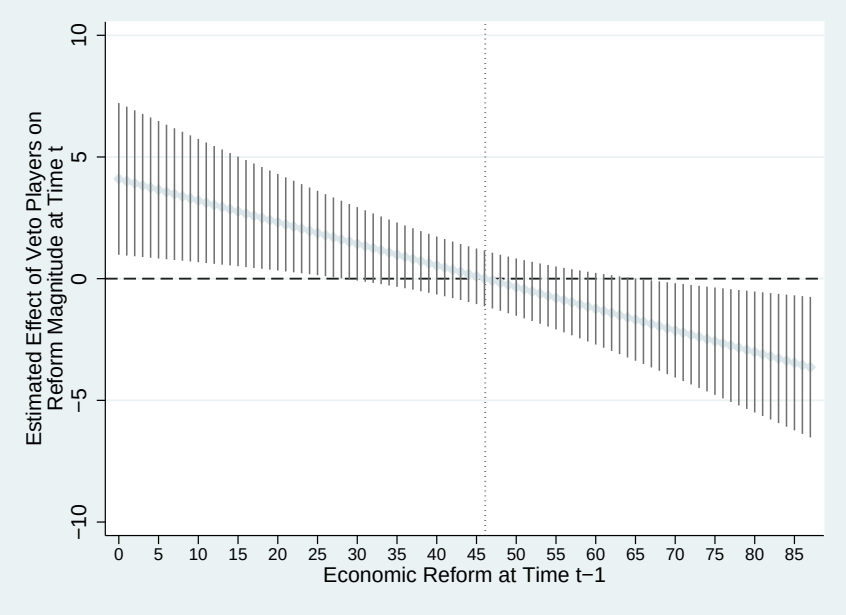

Figure 2: Predicted effect of veto players on movement toward full reform as a function of economic reform at time $t-1$. Bars depict 95-percent confidence intervals for a one-standard-deviation increase in veto players, using estimates from Model 8 of Table 1. Economic reform at time $t-1$ is restricted to the sample range of $[0, 87]$ rather than the theoretically possible range of $[0, 100]$.

Table 1: Veto Players and Movement Toward Full Reform

Dependent Variable: Movement toward Full Reform	**Ordinary Least Squares** Year FE Only		**Ordinary Least Squares** Two-Way FE		**Arellano-Bond 1** VP, Comm Exog		**Arellano-Bond 2** VP, Comm Endog		**Arellano-Bond 3** w/ Competition	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Veto Players	1.339**	1.287**	1.393*	1.400*	4.224***	3.827**	2.168*	2.456**	2.094	2.894***
	(0.541)	(0.573)	(0.796)	(0.805)	(1.335)	(1.567)	(1.285)	(0.954)	(1.383)	(0.967)
Veto Players × Lagged Economic Reform	−0.025***	−0.025**	−0.026*	−0.026*	−0.084***	−0.073**	−0.055**	−0.053***	−0.052*	−0.056***
	(0.009)	(0.010)	(0.013)	(0.013)	(0.028)	(0.030)	(0.028)	(0.020)	(0.027)	(0.019)
Lagged Economic Reform	1.048***	1.028***	0.763***	0.746***	0.348	0.286	0.636***	0.680***	0.540***	0.535***
	(0.024)	(0.026)	(0.056)	(0.061)	(0.230)	(0.209)	(0.164)	(0.095)	(0.171)	(0.116)
Communists in Legislature		−1.596		1.623		−2.050		5.877*		4.561
		(1.045)		(1.422)		(2.780)		(3.434)		(3.461)
War		−1.637**		−3.416***		−4.954***		−5.663***		−6.608***
		(0.779)		(1.079)		(1.715)		(1.370)		(1.652)
Log GDP per Capita		0.169		0.046		0.482		−1.209		−1.149
		(0.246)		(1.365)		(5.060)		(2.038)		(2.342)
Political Competition									−3.428	−2.286
									(3.327)	(2.241)
Political Competition × Lagged Economic Reform									0.012	0.030
									(0.043)	(0.034)
Country fixed effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	300	300	300	300	275	275	275	275	248	248
Panels	25	25	25	25	25	25	25	25	25	25
R-squared (within)	0.971	0.972	0.923	0.927						
Root mean squared error	3.554	3.515	3.112	3.032						
Chi-squared					454***	1340***	1210***	1449***	889***	1184***
Hansen J-statistic					11.03	7.94	11.83	8.10	5.59	9.82
p-value, Hansen J-statistic					0.946	1.000	0.998	1.000	1.000	1.000
Arellano-Bond AR(2) test					0.729	1.815	0.602	0.838	0.394	0.725
p-value, AR(2) test					0.466	0.070	0.547	0.402	0.693	0.468

Notes: Models 1 through 4 are estimated by ordinary least squares, with heteroskedasticity-robust standard errors in parentheses. Models 5 through 10 are Arellano-Bond models, with heteroskedasticity-robust standard errors corrected to allow for clustering at country level in parentheses. The following variables treated as endogenous: GDP per capita (Models 5–10); veto players and communists in legislature (Models 7–10); political competition (Models 9–10). Arellano-Bond models instrument on the first exogenous lag; see text for details. The Hansen J is a test statistic for over-identification; note that the Sargan test is invalid in the presence of heteroskedasticity. The Arellano-Bond AR(2) test is a test for second-order serial correlation in the first-differenced model, i.e., for first-order correlation in the levels model. All models implemented in *Stata*; Arellano-Bond models estimated with the *Stata* `xtabond2` package (Roodman, 2006). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 2: Veto Players and Reform Reversals

Specification:	**Poisson**				**OLS**	
Dependent Variable: Number of Reform Reversals (1–4), Reform Magnitude (5–6)	**Baseline**	**Average Values**		**Initial Values**	**Average Values**	**Initial Values**
	(1)	(2)	(3)	(4)	(5)	(6)
Average Veto Players		−0.314**	−0.273*		−9.897	
		(0.151)	(0.141)		(6.281)	
Average Communists in Legislature			0.662*		34.522	
			(0.382)		(31.775)	
Initial Veto Players				−0.708***		−15.439**
				(0.160)		(5.635)
Initial Communists in Legislature				0.677*		38.960*
				(0.359)		(21.901)
Natural Resources	0.300	0.450	0.418	0.503**	14.544	17.502
	(0.342)	(0.298)	(0.290)	(0.246)	(18.680)	(14.044)
1989 GNP per Capita	−0.162	−0.140	−0.134	−0.244***	2.585	0.307
	(0.111)	(0.134)	(0.136)	(0.076)	(3.830)	(3.118)
Distance from West	−0.048	−0.250	−0.297*	−0.532***	−6.786	−15.162**
	(0.150)	(0.160)	(0.168)	(0.106)	(8.530)	(6.428)
Industrial Structure	−0.228	−0.222	−0.243	−0.120	−6.610	−7.033
	(0.200)	(0.157)	(0.168)	(0.122)	(5.814)	(5.211)
Constant	1.530	2.564**	2.416*	3.799***	58.071	90.256*
	(1.152)	(1.223)	(1.249)	(0.856)	(51.788)	(45.495)
Observations	25	25	25	25	25	25
(Pseudo) R-squared	0.095	0.137	0.147	0.289	0.321	0.594
Chi-squared	6.20	11.67**	20.99***	45.75***		
Root mean squared error					32.20	24.91

Notes: Models 1–4 are Poisson models, models 5–6 OLS regressions, all implemented in *Stata*. Heteroskedasticity-robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.