Dictators, Democrats, and Government Performance In an African Country

Chapter 7:

Sharing the National Cake: Dictators, Democrats, and Distributional Dilemmas

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INTRODUCTION

I argue throughout this dissertation that political institutions have consequences for policy outputs. Chapter 3 revealed how a dichotomous classification of regimes neither explains Nigeria's government performance nor captures the country's institutional variation. As an alternative, I introduce veto players in Chapter 4 as key constitutive units of Nigeria's policy process. I establish guidelines for identifying them in both democratic and authoritarian regimes. In Chapter 5 I describe regional vetoes, which arise when collective policy actors fail to cut across regional cleavages. Chapter 6 codes Nigeria's veto player regimes since 1960. I make three causal claims regarding the consequences of veto players on policy outputs: First, a policy process with many veto players increases the overall delivery level of particularistic goods or "pork." Second, additional veto players undermine the delivery of public goods by generating bargaining problems. Third, additional veto players increase per-unit costs, meaning that in the aggregate pork is more expensive.

This chapter re-states claims as three tests concerning the level of pork, the delivery of public goods, and the "efficiency" of policy, respectively. Utilizing data from Chapter 2, I find evidence to support the overall theory. Although my results are not entirely as expected, I do demonstrate that policy processes with more veto players have difficulty delivering both public goods and pork. The policy process therefore presents a dilemma between different types of policy outputs. This does not necessarily contradict the claim throughout the literature that policy is more stable in regimes with multiple veto players. But it does imply that policy making in these regimes is prone to bargaining problems. This has important theoretical implications because it means that political processes with many veto gates produce negative externalities for performance. Next, I find that particularistic goods in these regimes do tend to cost more, naira-for-naira. This challenges studies which anticipate that more inclusive policy making will reduce the opportunities for corruption or rents generally.

The first section of this chapter states my hypothesis and describes the measures of government performance I use to test it. I also identify controls for regime type and oil revenues. The second section specifies my models for three sets of tests. I begin by introducing variables to control for regime type and the impact of oil on federal revenues. My test results provide little evidence that veto players increase morselization through increased levels of pork. Then when we compare the impact of veto players on public goods compared to pork, additional veto players have an adverse impact on delivery. My tests for policy efficiency show that additional veto players increase wasteful spending. But the results are clearer with one variable more than the other.

The third and final section of this chapter reflects on the test results and explains their implications for the two theoretical traditions rooted in "commitment" and "distributive dilemmas." My findings provide a basis for a theory of performance that integrates the consequences of commitment. Multi-veto player regimes may include a broader range of interests. But by exacerbating bargaining problems they undermine the delivery of public goods. In a complex, heterogeneous polity such as Nigeria's, expanding political inclusion with more policy actors may be a necessary tradeoff. An appendix reports the results of tests of my independent variables for time series auto-correlation.

GOVERNMENT PERFORMANCE: PUBLIC GOODS AND PARTICULARISM

To build a model that can explain policy outputs rather than simply policy stability, Chapter 4 highlights a dispute implicit throughout the veto player literature. The "commitment" tradition emphasizes the importance of policy credibility and accountability (Henisz 2000, 1-31; Keefer and Stasavage 2003, 407-20). It argues that the number of veto players essentially generates a tradeoff between policy stability and instability; the policy process has to be flexible without being unpredictable. However, this framework does not adequately address the distributional consequences of multiple veto players. I contrast the commitment literature with the "distributional dilemma" tradition, which argues that more veto players leads to more pork (Cox and McCubbins 2001, 21-63; Alesina, Roubini and others 1997). This claim implies that regimes that produce policy stability also exacerbate bargaining problems. These problems undermine their ability to deliver nationally-oriented policies in the form of public goods.

I am not the first to point out a tension between these two traditions (Treisman 2000, 837-56; Tsebelis 2002). But this project is among the first to actually conduct empirical tests of them. Doing so involves distinguish pork from public goods by describing a "range of publicness."[1] My basic hypothesis states that an increase in veto players leads to an increase in pork. In this section I outline three sets of tests of this hypothesis and I identify appropriate controls. The first tests measure the impact of veto players on *levels* of pork. This includes particularistic policies that are geographically targetable. The second set of tests measure the impact of veto players on public goods. The variables here include policies that benefit the country as a whole, and therefore more closely resemble the strict definition of public goods. Next, I outline how to test for the impact of veto players on the *efficiency* of pork spending. Do schools and teachers become more expensive, naira-for-naira, as the number of veto players increases? Or do multiple veto players inherently enhance accountability? After introducing controls for regime type and oil revenues, I conclude with two parsimonious predictions: First, regimes with multiple veto players deliver more pork but they also deliver fewer public goods. Second, pork is more expensive with too many veto players.

Public Goods *versus* Particularism

First, I test to see if the overall level of pork increases as each additional veto player demands benefits. Thus having many veto players should improve aggregate performance by increasing overall levels of pork. Unlike public goods whose benefits are nationally-oriented, these particularistic policies are geographically targetable and excludable. Increased pork will be observable in terms of more primary schools and lower student/teacher ratios.

> **(1) Level of pork:** More veto players lead to an increase in the delivery of pork

Next, I test to see if the number of veto players affects the delivery of public goods. Following the criteria established in Chapter 2, the benefits of public goods policies are non-divisible and non-excludable. Fiscal discipline, operationalized as budget surpluses, closely resembles this definition of public goods (Alesina and Perotti 1996, 1203-28; Persson and Tabellini 2003). This second test can thus be stated as:

> **(2) Public goods:** Increasing the number of veto players undermines the delivery of public goods.

[1] See Chapter 2, where I define "publicness" in terms of varying degrees of excludability in consumption.

Public goods will be observable in terms of budget surpluses and the efficient resolution of property rights disputes. If Cox and McCubbins (2001) along with Alesina et al. (1997) are correct, then additional veto players will exacerbate bargaining problems, leading to the morselization of policy. Taken together with the first set of tests, I expect to show that public goods will decline while education outputs suffer no adverse effects. This implies that it is difficult to deliver both pork and public goods; veto player regimes therefore impact the type of policy.

Policy Inefficiency: The Costliness of Sharing the "National Cake"

The third test of my hypothesis tests for the impact of veto players on the per-unit cost of particularistic policies. Chapter 2 defines *policy efficiency* as a ratio of government spending (in constant terms) relative to policy output in education. The credible commitment literature expects additional players to improve accountability and reduce opportunities for corruption. This implies that multi-veto player regimes should improve policy efficiency.

I argue, however, that no veto player sees the value in self-restraint while her peers are permitted to "eat from the national cake" unrestrained. The problem is not small increases in the number of payoffs since individually these are only marginally expensive. Rather, the trouble is that collectively these payments add up to fiscal irresponsibility which is observable in policy inefficiencies. Along the same lines, recent studies suggest that geographically targetable policies are prone to corruption when there are incentives to cultivate personal reputation rather than collective discipline (Chang and Golden 2006, 115-37). I take a similarly broad view of inefficiency. I anticipate that governments with more veto players accumulate deadweight loss: naira-for-naira, these governments spend more without necessarily delivering more. This third test can be stated as:

> **(3) Policy efficiency:** An increase in veto players makes each unit of pork more expensive.

This is observable in my policy efficiency variables measuring the per-unit cost of primary schools and teachers. Evidence to support this hypothesis would not contradict the commitment tradition's contention that extra checks make policy more stable (Henisz 2000, 1-31; Keefer and Stasavage 2003, 407-20). However it would mean that our theories of accountability must disentangle policy stability from accountability induced by checks and balances. I expect to find that additional veto players increase the costs of individual policy outputs. If so, then by implication the same institutional framework that provides credibility also requires numerous side payments which I claim add up to overall policy inefficiency.

As explained in Chapter 2, *Teachers1* and *Schools1* express the residuals from models where I compare the predicted outputs ($\hat{y}_i$) to the actual observed values ($\mu_i + \hat{y}_i$), given constant spending levels. I interpret good government performance as years when government inputs were equal to, or less than policy outputs. Sub-optimal government performance occurs when the predicted value exceeds the observed value. The residuals are expressed in a model where $\varepsilon = y - \hat{y}$. For purposes of hypothesis testing we must interpret the residuals differently though: For *Schools1*, large positive values suggest that the number of schools in a given year exceed the predicted value. However, for *Teachers1* we must reverse the logic because we interpret a lower

student/teacher ratio as "good" performance. Thus positive residuals in these tests are a sign of sub-optimal performance.

Controls

For all of these tests I use two control variables. *Oil* captures oil as a share of government revenue. It thereby controls for the years in my sample where government revenues were unusually high or low due to fluctuations in oil earnings.[2] By incorporating this control, I can claim that veto players, rather than oil revenues have a greater impact on policy outputs. This is useful because government revenue levels may in fact drive spending levels (Please 1967, 24-32). By such logic, bad performance is a consequence of scarcity since governments can spend when they have more revenue to do so (Przeworski, Alvarez and others 2000). Some scholars claim this is especially true in sub-Saharan Africa (Ekpo 1996, 219-242). Natural resource revenues also supposedly increase the value of being in power, leading politicians to spend more in order to preserve incumbency (Robinson et al. 2005).

Since the *oil* variable is not stationary I de-trend it with first difference tests, creating Δoil. The veto players variable, *vetoes*, is stationary at the least restrictive level. I utilize a lagged variable ($vetoes_{t-1}$) to help reduce autocorrelation. The appendix to this chapter shows the results of the unit root tests on which I based these decisions.

My other control is for regime type because democracy, rather than the number of veto players, might better account for variation in government performance. This control should also tell us whether democracies or veto players better pave the way to the accountability necessary to reduce corruption. Rose-Ackerman argues that "the case for autocracy as a technically efficient form of government is weak." But she also points out that democracy does not always succeed in checking corruption (pp. 113, 125-26, cf. 142). Finally, to the extent that one could claim that authoritarianism inherently undermines judicial independence, this dummy also serves as a proxy for independence. I utilize the coding of regimes established in Chapter 3. Between 1960 and 2003 there are 15 years where the value for this dummy variable, *Regime* is equal to 1, indicating democracy rather than dictatorship.

Summary and Predictions

I expect the "level of pork" tests to show that regimes with many veto players provide more pork overall. I predict that my tests for public goods will show that increasing the number of veto players reduces the delivery of public goods. For my policy efficiency tests, I anticipate that particularistic policies are more expensive when there are more veto players. Support for my overall hypothesis, that policy outputs increase with the number of veto players would yield two key conclusions: First, regimes with more veto players do suffer from bargaining problems and the morselization of policy. Second, additional veto players increase the number of salient policy makers, as the commitment tradition claims. But at the same time they do not necessarily produce accountability that precludes corruption or rents.

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[2] After comparing several sources reporting oil earnings in a time series, I discovered that no one seems to use the Nigerian National Petroleum Company's statistics in time series' including the 1960s. I calculate my *oil* variable using the following sources: Years 1961 – 1969 from Central Bank of Nigeria's *Annual Report* for years 1964, 1967, and 1970; data for 1970 – 2001 from CBN *Statistical Bulletin*, Vol. 13, December 31, 2003; years 2002 and 2003 are from the CBN *Annual Report* for 2003.

NIGERIA'S DISTRIBUTIVE DILEMMAS

In this section I offer empirical evidence that the number of policy actors, expressed as veto players, accounts for a range of government performance. The first set of tests, which measure the level of delivery, do not provide much evidence that multi-veto player regimes deliver more pork. However the results are only conclusive for the variable measuring student/teacher ratios. The second set of tests show that multi-veto player regimes are less likely to deliver public goods. The results are especially robust with regard to fiscal discipline. These findings hold even after controlling for regime type and the impact of oil revenues. Together, the results from the first and second tests only weakly support the overall theoretical claim about the expansion of the policy process: including more veto players induces a dilemma where governments must choose between different types of policy. In the third set of tests, I find evidence to support my predictions that more veto players do not improve policy efficiency. But the results are more conclusive with student/teacher ratios than with the variable measuring the number of primary schools.

Veto Players and Policy Morselization

The first test looks only at the relationship between the number of veto players and policy outputs, measured in terms of primary student/teacher ratios and the number of primary schools. If the predictions about delivery level are correct, then we would expect to see an increase in the number of veto players correspond with smaller student/teacher ratios and more primary schools. My tests use ordinary least squares regressions with and without control variables. The basic model for the level tests can be expressed as:

$$\text{Equation 1: } Pri.s.t._{t} = \beta 0 + \beta 1\ (vetoes_t) + \beta 2\ (vetoes_{t-1}) + \beta 3\ (regime_t) + \beta 4\ (\Delta oil_t) + \mu_t$$

Table 1: Education Performance, Student/Teacher Ratios

	Pri.s.t. Primary Student/Teacher Ratio			
	(1)	(2)	(3)	(4)
Vetoes	2.491***	2.210**	2.834***	2.883***
	(3.994)	(2.679)	(3.415)	(3.448)
$Vetoes_{t-1}$ Vetoes Lag -1		.244	.015	-.070
		(.290)	(.018)	(.086)
Regime Reg. dummy			-2.809**	-3.008**
			(2.274)	(2.375)
ΔOil Oil rev. share				-4.993
				(.794)
N	42	41	41	41
Adj-R^2	.267	.239	.314	.307
DW	1.054	1.093	1.229	1.315

Absolute value of *t*-stat in parentheses
*significant at the .1 level; **significant at the .05 level
***significant at the .01 level

Veto players are statistically significant predictors when we measure performance in terms of primary school student/teacher ratios. But the results are essentially the opposite of my predictions: an increase in veto players leads to higher ratios across the board, even after controlling for regime type and oil revenues. Moreover, the negative coefficients on the *Regime* variable indicate that democracy reduces student/teacher ratios. Both variables are statistically significant although veto players are more significant. The Durbin-Watson statistic improves as the model becomes more complex; with the controls there is virtually no sign of autocorrelation. While the tests show that veto players capture institutional variation, so far they do not support my prediction that additional veto players contribute to morselization.

Table 2 shows the results of tests where the dependent variable is the number of schools at time *t*+1, de-trended with first difference tests. Here we are able to introduce a separate control for enrollments since an increase in enrollments could impact school construction; this is not possible in the tests for student/teacher ratios since the ratios are calculated from gross enrollment figures. This is also a useful control because studies of education performance show that enrollments increase after transitions to democracy (Stasavage 2005, 53-73). As in Chapter 2, the variable measuring primary enrollment, *pri.enroll*, has been de-trended with first difference tests making it Δ*Pri.enroll*. This model can be expressed as:

Equation 2: $\Delta Schools_{t+1} = \beta 0 + \beta 1(veto_t) + \beta 2(veto_{t-1}) + \beta 3(regime_t) + \beta 4(\Delta oil_t) + \beta 5(\Delta pri.enroll_t) + \mu_t$

Table 2: Education Performance, Primary Schools

	ΔPri.sch$_{t+1}$ Number of Primary Schools (de-trended)				
	(1)	(2)	(3)	(4)	(5)
Vetoes	-111.451 (.317)	296.490 (.659)	555.563 (1.204)	570.965 (1.219)	267.691 (.566)
Vetoes$_{t-1}$ Vetoes Lag -1		-656.438 (1.426)	-777.840* (1.717)	-801.431* (1.736)	-698.867 (1.570)
Regime Reg. dummy			-1235.830* (1.762)	-1291.817* (1.790)	-1176.561* (1.696)
ΔOil Oil rev. share				-1503.662 (.426)	-792.465 (.233)
ΔPri.enroll Enrollment					.001** (2.029)
N	40	40	40	40	40
Adj-R^2	-.024	.003	.057	.035	.114
DW	.816	.826	.935	.945	1.106

Absolute value of *t*-stat in parentheses
*significant at the .1 level; **significant at the .05 level
***significant at the .01 level

Here the results show that an increase in veto players does not lead to more schools as my theory predicts until we introduce controls, and even so the tests are not significant. Oil as a share of revenues has no statistically significant impact. Regime type once again is significant. But this time the negative coefficient indicates that democracies consistently build *fewer* primary schools. Enrollment changes actually best predict the pace of school construction. This appears to

suggest that demand for new buildings predicts school construction better than any particular element of the policy process itself. The Durbin-Watson statistics improve as controls are introduced and the veto player variable shows some signs of autocorrelation even with the lagged variable included in the model.

Taken together, these tests for my two measures of education performance present an interesting contradiction: In the first instance, democracy improves student/teacher ratios while additional veto players make them worse. In the second instance, veto players have only a minor impact on the number of schools (if any). Yet we can state that democracies build fewer schools once we account for enrollments. Changes in gross enrollment have a large effect on the number of schools but democracy has a relatively small effect. Thus while the tests do not offer much support for the predictions about the level of pork, the implications for democracy are scarcely better.

Veto Players and Public Goods

My second set of tests determines how well the government delivered public goods. By comparing these results to the first set of tests, we can also determine whether the number of veto players therefore impacts the type of policy. I expect an increase in veto players to correspond with declining fiscal discipline and lower clearance rates in the courts. Table 3 shows the results of ordinary least squares regressions where the basic model for these tests can be stated as:

Equation 3: $\hat{Y}_t = \beta 0 + \beta 1\ (\text{veto}_t) + \beta 2\ (\text{veto}_{t-1}) + \beta 3\ (\text{regime}_t) + \beta 4\ (\Delta \text{oil}_t) + \mu_t$

Table 3: Court Performance

	Clearance Observations for 1960 – 1987 only				*Alt-clear* Including estimated values for 1988 - 2003			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Vetoes Veto Players	-.436*** (3.383)	-.309* (1.731)	-.307 (1.659)	-.309 (1.633)	-.195*** (2.919)	-.138 (1.537)	-.142 (1.486)	-.153 (1.542)
Vetoes$_{t-1}$ Veto lag -1		-.170 (.945)	-.170 (.927)	-.161 (.846)		-.074 (.811)	.-073 (.776)	-.058 (.561)
Regime Regime dummy			-.013 (.071)	-.001 (.008)			.018 (.134)	.027 (.180)
ΔOil De-trended oil				.242 (.255)				.430 (.540)
N	28	27	27	27	43	42	42	42
Adj-R^2	.279	.261	.229	.197	.152	.133	.111	.094
DW	1.834	1.616	1.613	1.611	1.396	1.259	1.263	1.296

Absolute value of *t*-stat in parentheses
*significant at the .1 level; **significant at the .05 level;
***significant at the .01 level

Veto players in the simplest models predict clearance rates at a statistically significant level. The columns on the left predict *clearance*, my variable based on the observed clearance rates from my case sample through 1987. The columns on the right show predictions for *alt-clear*, the clearance rate variable that incorporates estimated values for 1988 – 2002. In both models, veto players impact judicial efficiency but the results decline after introducing the controls. The

negative coefficient on *vetoes* indicates that an increase in the number of veto players reduces judicial efficiency. The large Adjusted R^2 indicates a rather well fit model overall. The Durbin-Watson statistic shows only very limited evidence of autocorrelation with *alt-clear*, while the observed clearance values pose no significant autocorrelation issues. Neither democracy nor oil revenues have a significant effect on judicial efficiency.

Table 4 shows the results of a model where veto players predict whether the federal government will run a budget surplus or not.

Table 4: Fiscal Discipline

	Discipline 1961 – 2003			
	(1)	(2)	(3)	(4)
Vetoes Veto players	-3.119*** (3.547)	-2.925** (2.383)	-3.356** (2.634)	-3.604*** (3.023)
$Vetoes_{t-1}$ Vetoes lag -1		-.257 (.206)	-.048 (.038)	.233 (.195)
Regime Regime dummy			2.141 (1.186)	2.820 (1.603)
ΔOil De-trended oil				23.903** (2.604)
N	43	42	42	42
Adj-R^2	.216	.191	.199	.319
DW	1.625	1.628	1.646	1.858

Absolute value of *t*-stat in parentheses
*significant at the .05 level; **significant at the .01 level

Veto players predict budget surplus at a statistically significant level, even after introducing the controls. The large negative coefficients offer strong support for my predictions. In all four models, they suggest that an increase in the number of veto players leads to larger deficits. Democracy has little measurable impact. Oil does have some effect but even in this model the number of veto players has a larger effect on the federal government's ability to maintain fiscal discipline. In every model, the Durbin-Watson statistics indicate virtually no positive or negative autocorrelation. In sum, we can claim that larger numbers of veto players impair the ability to deliver public goods.

Policy Efficiency

My third set of tests expects additional veto players to increase the per-unit cost of particularistic policies. I measure these policies in terms of the efficiency of federal spending on primary school teachers, and then on primary schools. *Teachers1* expresses the residual from my model where federal spending predicts the student/teacher ratio. *Schools1* measures the efficiency of spending on school infrastructure, also through a residual. As with the "policy level" tests, I control for regime type and the impact of oil revenues. Using ordinary least squares, the basic model predicting the impact of veto players on policy outputs can be stated as:

$$\text{Equation 4: } \hat{u} = \beta 0 + \beta 1\ (\text{vetoes}) + \beta 2\ (\text{vetoes}_{t-1}) + \beta 3\ (\text{regime}) + \beta 4\ (\Delta \text{oil}) + \mu.$$

Table 5: Policy Efficiency

	Teachers1 1962 – 2002				*Schools1* 1962 – 2001			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Vetoes	1.591*** (2.746)	1.513* (1.946)	2.254*** (3.009)	2.209*** (2.926)	-526.344* (1.951)	-316.464 (.904)	-57.289 (.164)	-72.490 (.205)
$Vetoes_{t-1}$		.120 (.152)	-.152 (.209)	-.075 (.920)		-337.729 (.942)	-459.137 (1.340)	-435.718 (1.250)
Regime			-3.337*** (2.992)	-3.156*** (2.760)			-1235.887** (2.329)	-1180.310** (2.166)
ΔOil				4.529 (.797)				1492.632 (.560)
N	41	41	41	41	40	40	40	40
Adj-R^2	.141	.118	.271	.264	.067	.064	.164	.148
DW	1.249	1.242	1.577	1.566	1.447	1.439	1.720	1.698

t-stat in parentheses
*significant at .1 level; **significant at .05 level;
***significant at .01 level

Starting with the models on the right side of Table 5, veto players impact spending on primary school infrastructure only without the control variables ($p = .058$). However the low Adjusted R^2 indicates that the model explains very little without controls for regime type or oil revenues. The *regime* dummy is statistically significant. But the negative coefficients indicate that democracies actually waste ***more*** money on school construction than dictatorships.

Judging by the models on the left side, veto players have precisely the predicted effect on spending on teachers. Here we need to recall that positive residuals expressed by *Teachers1* indicate higher, and thus worse, student/teacher ratios. The models here therefore indicate that additional veto players reduce policy efficiency when supplying primary school teachers. The negative coefficients on *regime* mean that democracies actually do a better job lowering student/teacher ratios naira-for-naira. The Adjusted R^2 increases as we add the controls.

Oil revenues have no statistically significant effect in either model. Democracy has a statistically significant effect in every model. While the findings on democracy in these models seem to present a contradiction, I suggest a simple explanation: Democracies have more corruption and deadweight loss in school construction than they do with spending on education personnel. Veto players seem unable to account for the efficiency of spending on school construction. But they consistently capture spending inefficiencies on education personnel – under all types of regimes.

Summary

Multi-veto player regimes do not have a major effect on per unit costs of schools. Democracies do have a statistically significant effect on school construction, apparently by wasting a great deal of money. Veto players have the predicted effect on teachers, since additional veto players undermine the efficiency of spending on teachers. Commitment by itself is insufficient to induce accountability, and credibility can be costly – literally. My strongest results concern the relationship between veto players and bargaining problems, since find that multi-veto player regimes produce fewer public goods. These results are especially robust for my measure of fiscal discipline.

CONCLUSION

James Madison may be credited with the underlying logic of veto player theory. In *The Federalist Papers*, he argues eloquently for dividing power in a republican system. The different branches of the American government allow "ambition to counter ambition," preventing any single sovereign interest from dominating distribution or debate. This view counters Hobbes, who thought a civil government where the power to levy money, to "conduct and command" an army, and making laws all lie in different bodies would "endangereth the commonwealth:"

> sometimes for want to consent to good laws, but more often for want of such nourishment as is necessary to life and motion. For although few perceive, that such government is not government, but division of the commonwealth into three factions…yet the truth is that it is not one independent commonwealth, but three independent factions, nor one representative person, but three (Hobbes 1994).

Hobbes saw too many sovereign voices as an invitation to chaos and the fragmentation of policy demands. By contrast, Madison feared that too few sovereign interests in government would pave the road to tyranny. The commitment tradition in the veto player literature implicitly recognizes the inherent tension between these intuitions, since too few or too many vetoes can both undermine performance. The implications for distribution of many sovereign voices in government are still emerging, and veto players offer a promising theoretical framework for exploring these issues in regimes old and new. However the distributional dilemma tradition requires much more testing.

In this chapter I found little support for Cox and McCubbins' suggestion that an increase in the number of policy actors contributes to larger overall levels of pork. Neither student/teacher ratios nor the number of primary schools improve based on the number of veto players. In the case of the former they decisively worsen. It is conceivable that this can be attributed to veto players stimulating enrollments in excess of government capacity. It is also possible that schools and teachers are not ideal operationalizations of pork, especially since Nigerian policy makers have sometimes explicitly recognized their role in human capital formation.

The consistently negative effects of democracy on school construction do offer potentially clearer implications: democracies improve accountability through more efficient spending on teachers but offer significantly sub-optimal outcomes in terms of school infrastructure. Not only do they build fewer schools but they waste more money doing so. This suggests a cautionary note for nascent democracies with ambitious school construction programs. Moreover, as the tests for *Teachers1* show, additional veto players actually reduce efficiency when it comes to spending on personnel. This points to a different kind of cautionary note for countries such Nigeria that encourage informal vetoes through political traditions that seek to share power. Finally, the most robust finding of this chapter concerns the types of policies: For both fiscal discipline and judicial efficiency, veto players consistently increase the risks of collective action problems. Not only does this mean that we need to re-think the relationship between courts and regimes, it means that governing arrangements that aim to ensure political inclusion by arbitrarily increasing the number of vetoes put fiscal discipline at risk. The distributional dilemma tradition is certainly correct, though, in suggesting a theoretical link between veto players and distributional outcomes, especially public goods. My concluding chapter will discuss the practical and theoretical implications of these findings.

APPENDIX
Tests for Stationarity of Independent Variables[3]

Augmented Dickey-Fuller (ADF) Unit Root Test, where:

Model 1: $\Delta x_t = \mu + \varphi t + \alpha X_{t-1} + \varepsilon_t$

Model 2: $\Delta x_t = \mu + \alpha X_{t-1} + \varepsilon_t$ (assuming: $t = 0$)

Model 3: $\Delta x_t = \alpha X_{t-1} + \varepsilon_t$ (assuming: $\mu = 0$, $t = 0$)

H_0: $\alpha = 1$
(Variable has a unit root.)

H_1: $\alpha \neq 1$
(Variable does not have a unit root.)

Reject H_0 if *t*-stat < critical test values
I = variable integration over time, and I(0) = stationarity

	LEVEL			**FIRST DIFFERENCE**			**DECISION**
	(1) Intercept test (trend, const)	(2) Trend & Intercept (const, no trend)	(3) None (no const, no trend)	(1) Intrcpt test (trend, const)	(2) Trend & Intrcpt (const, no trend)	(3) None (no const, no trend)	
Veto Players	-3.16*	-2.68*	-0.38	-5.97***	-6.04***	-6.33***	I(0)
Oil	-1.18	-1.79	0.2	-6.21**	-5.99**	-5.84**	I(1)

Reports ADF test-statistic
*significant at the .1 level
**significant at the .05 level
***significant at the .01 level

Note: No test for r*egime* variable is necessary because it is a dummy

[3] See Appendix in Chapter 2 for dependent variables' stationarity tests.

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