

## ATTRIBUTION ERROR IN ECONOMIC VOTING: EVIDENCE FROM TRADE SHOCKS

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*This article exploits the international transmission of business cycles to examine the prevalence of attribution error in economic voting in a large panel of countries from 1990 to 2009. We find that voters, on average, exhibit a strong tendency to oust the incumbent governments during an economic downturn, regardless of whether the recession is home-grown or merely imported from trading partners. However, we find important heterogeneity in the extent of attribution error. A split sample analysis shows that countries with more experienced voters, more educated voters, and possibly more informed voters—all conditions that have been shown to mitigate other voter agency problems—do better in distinguishing imported from domestic growth. (JEL E3, E6)*

### I. INTRODUCTION

The vast literature on economic voting finds that voters in many countries reward incumbents for presiding over strong economic growth, low unemployment, and low inflation and punish for the reverse (Duch and Stevenson 2008; Fair 1978; Frey and Schneider 1978; Hellwig 2001; Kramer 1971; Lewis-Beck 1988; Powell and Whitten 1993).<sup>1</sup> Other economic variables such as tax increases have received inconsistent support (Niemi, Stanley, and Vogel 1995; Kone and Winters 1993) leading reviews to conclude

that there are two main variables of interest: inflation and gross domestic product (GDP) growth or unemployment (Lewis-Beck and Paldam 2000; Nannestad and Paldam 1994).<sup>2</sup>

While it is clear that many voters remain ignorant even of these summary variables (Paldam and Nannestad 2000), voter ignorance is neither absolute nor randomly distributed. Studies have found age, education, and income to be significantly related to voters' knowledge (Blendon et al. 1997; Paldam and Nannestad 2000).<sup>3</sup> Some

*Disclaimer:* The views expressed in this article are those of the authors and are not necessarily reflective of views of the Federal Reserve Bank of New York or the Federal Reserve System.

\*We thank anonymous referees, Daniel Riera-Crichton, Tomoharu Mori, and participants at the Economics Seminar of Wesleyan University, University of Zurich, Liberal Arts Colleges Development Economics Conference (Amherst College) for valuable comments. We acknowledge the financial support from the Quantitative Analysis Center of Wesleyan University (Hayes and Imai).

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1. See Lewis-Beck and Stegmaier (2013) for a recent comprehensive review of economic voting literature.

2. There is also a large literature that shows the effect of noneconomic variables such as war casualties (Mueller 1973), fiscal cost of war (Geys 2010), and natural disasters (Cole, Healy, and Werker 2012; Healy and Malhotra 2009). We stay clear of these variables because of difficulties standardizing their selection and measurement across a large sample of countries.

3. There is a related literature, which shows that the quality of government and its economic policy in just about any measure is strongly correlated with the level of education (e.g., Barro 1999; Glaeser, Ponzetto, and Shleifer 2007). Although causation is difficult to establish (Acemoglu et al. 2005), these results suggest that educated citizens are better at disciplining their political leaders than uneducated ones.

#### ABBREVIATIONS

2SLS: Two-Stage Least Squares  
 CEO: Chief Executive Officer  
 DPI: Database of Political Institutions  
 EWGP: Export-Weighted Growth-Predictor  
 GDP: Gross Domestic Product  
 OLS: Ordinary Least Squares  
 PWT: Penn World Table  
 WDI: World Development Indicators

voters clearly have the incentive to acquire information, either because it has personal value in investment decisions or because they feel a social duty to be informed. Aidt (2000) optimistically cites the fact that unemployment and inflation can explain about one-third of the variation of votes in an average election (Nannestad and Paldam 1994) as evidence that the informed voters are sufficiently influential to provide at least some discipline for the broader electorate. However, this line of argument assumes that voters reward outcomes that are correlated with representatives' skill and effort. Informed voters may take the time and effort to learn the state of the economy (macroeconomic aggregates), but may be incapable of distinguishing between that which can be attributed to the government and that which is out of the government's control. The motives that lead a voter to acquire accurate facts rarely require accurate interpretation of the government's role in generating those facts.<sup>4</sup>

Some authors have argued that voters determine the threshold of what constitutes acceptable performance by "benchmarking" to neighboring polities (Besley and Case 1995; Kayser and Peress 2012; Leigh 2009; Leigh and McLeish 2009). Kayser and Peress (2012) decompose a country's growth rate into two components: that which is common amongst neighbors and that which is idiosyncratic to the country in question. They then show that voters respond only to the idiosyncratic component, increasing (reducing) support for the incumbent when domestic growth is higher (lower) than growth among neighbors. Leigh performs the same decomposition.

But benchmarking does not prevent attribution errors: they are separate phenomena that can occur independently. Suppose reduced growth in the United States reduces growth in Brazil owing to trade spillovers but has little effect on growth in Uruguay, which is less dependent on U.S. trade. Benchmarking voters would reward Uruguayan politicians for avoiding the growth slowdown that has afflicted Brazil but they would be making an attribution error when doing so because the Brazilian growth slowdown was not due to Brazilian policy and the lack of it was not due to better decisions by Uruguayan policymakers.

4. As Caplan (2007) has argued, essentially the entire cost of a voter's misperceptions falls on the remainder of the electorate, enabling the voter to indulge in whichever worldview maximizes personal (often social) benefits. If so, we should note little attempt by voters to correctly attribute credit and blame, even in situations where assigning credit is relatively simple.

To date, two studies document the presence of attribution errors in the United States and India. Wolfers (2006) shows that the incumbent governors of oil-producing U.S. states tend to enjoy a higher reelection probability when oil prices are rising. Leigh and McLeish (2009) show that Australian voters reward state governors for both competence (unemployment in their state relative to the rest of Australia) and luck (unemployment common to all states). Cole, Healy, and Werker (2012) show that weather events (e.g., drought) have important effects on voting outcomes in India even though vigorous disaster relief spending can mitigate these effects. Hence, voters seem to be erroneously rewarding their political representatives for shocks that are both similarly observable and clearly exogenous.

Adding to this literature, we examine the panel data on government turnovers of 72 democratic countries from 1990 to 2009 and estimate the extent to which voters erroneously reward or punish their representatives for economic growth that is driven purely by a change in the economic conditions of their major trading partners. By extracting this exogenous component of economic growth that is outside the incumbent governments' control and estimating its impact on the probability of government turnover, we attempt to measure the prevalence of attribution error in a large panel of democratic countries.

Our approach yields several distinctive benefits. First, GDP is arguably more central to voters' decisions than crisis response. Second, the aforementioned studies on attribution error focus on one country (India or the United States) while this article investigates whether a similar attribution error can be detected in a broad panel of countries, thereby checking the generalizability of within-country studies. This is especially important as economic voting is notoriously context-specific (Lewis-Beck and Paldam 2000). Third, extending to panel data for a large set of countries allows us to probe whether there are institutional features that make voter attribution error more or less severe. For instance, electoral budget cycles constitute an inefficiency that similarly springs from the agency relationship between voters and political representatives. It has been shown that countries with greater media freedom (Akhmedov and Zhuravskaya 2004), greater budget transparency (Alt and Lassen 2006), more stable parties and thus more informative party labels (Shelton 2013), and more experience as a democracy (Brender and Drazen 2005, 2008) are able to suppress these cycles. The prevailing

interpretation is that improving the information that reaches voters and/or their ability to effectively process that information enables voters to recognize and punish the inefficient behavior at the heart of the budget cycle. We test whether these institutional advantages similarly enable voters to distinguish between domestic and imported growth.

We find that voters are, on average, sensitive to prevailing economic conditions: incumbents are more likely to be ousted during a recession and more likely to remain in office during a boom. While magnitudes cannot be directly compared, the results are roughly in line with the existing literature on economic voting. However, we also find that, on average, voters do not distinguish between growth that is imported from trade partners and growth that is home-grown. That is, incumbent governments seem to be rewarded or punished for economic outcomes that arise from pure luck. The extent of such attribution error is quantitatively important as well: the estimates suggest that, when exogenous negative trade shocks push down domestic economic growth by 1 percentage point, the likelihood of an incumbent chief executive (either the prime minister or the president) being replaced, on average, increases by 8.2 percentage points, which is substantial, given the sample average likelihood of chief executive replacement is 58%. However, the split sample results show that media freedom, experience as a democracy, and a more educated populace each significantly reduces the electorate's response to imported (exogenous) growth, suggesting that institutional context is highly relevant. As a result, the phenomenon is largely absent from a privileged subsample of countries.

There is relatively little prior work investigating the role of institutional variables in economic voting and it is limited to studies of benchmarking rather than those that, such as the current study, directly measure attribution error. Kayser and Peress (2012) examine whether economic news is benchmarked but have only a short time series for a single country. In a full panel, Leigh (2009) observes that higher GDP per capita and a more educated populace, and perhaps greater media penetration help reduce attribution error. These measures are not identical but are similar in spirit to the institutional measures that have been shown to be significant predictors of voting agency and on which we focus. Thus, we consider this as evidence that benchmarking and attribution errors respond to similar sets

of characteristics of the voting population and environment.

The rest of this article is organized as: Sections II and III describe the methodology and data sources, Section IV reports the results, followed by concluding remarks in Section V.

## II. METHODOLOGY

We follow closely the methodology of Bertrand and Mullainathan (2001). They compare the impact on chief executive officer (CEO) compensation of overall change in firm performance with that driven entirely by "luck" (e.g., industry-wide growth or oil prices) that should be readily observable to shareholders.<sup>5</sup> Incumbent electoral success may be viewed as analogous to CEO compensation in that both jobs are contingent on performance and subject to review by a supervisory body (in the case of a president or prime minister, this is the electorate or its representatives). Furthermore, the performance metrics used to evaluate both CEOs and governments are affected not only by the quality of their policy decisions, but also by exogenous shocks outside their control.

Studies of economic voting commonly estimate an equation of the form:

$$(1) \quad T_{ie} = \beta_i + \beta_e + \beta * Y_{ie} + \delta X_{ie} + \epsilon_{ie}$$

where  $T_{ie}$  is a dichotomous variable representing *turnover* that takes the value 1 if there is a change in government in country  $i$  during year  $e$ ,  $Y_{ie}$  measures GDP growth,  $\beta_i$  are the country fixed-effects, which capture country-specific unobservables that are correlated with the electoral stability of the incumbent government,  $\beta_e$  are the election-year fixed effects included to capture global shocks affecting the probability of government turnover, and  $X_{ie}$  is a vector of country- and government-specific variables such as the inflation rate or length of time in office which we will discuss subsequently. The coefficient  $\beta$  captures the average effect of economic growth on government turnover. It is expected to be negative if incumbents are less likely to be ousted during economic expansion.

5. Bertrand and Mullainathan (2001) show that CEO compensations are just as sensitive to a lucky dollar as a general dollar, which they consider as evidence that managerial agency problems are severe. They also find that the sensitivity of CEO compensation to a lucky dollar is closely related to firm-specific measures of corporate governance. The finding that attribution errors are mitigated in countries where voters have greater information and experience mirrors their results.

The purpose of this study is to test whether voters make attribution errors by crediting or blaming incumbent governments for economic performance that is beyond their control. Conceptually, we may decompose election-year GDP growth,  $Y_{ie}$ , into two components: one,  $Y_{ie}^D$  for which the government in country  $i$  can reasonably be held accountable and another,  $Y_{ie}^F$ , that is owing to factors outside its control. Rational voters ought to base their decisions on the first component while filtering out the second. The aggregate outcome of voter decisions then determines whether the incumbent government is ousted.

$$(2) \quad T_{ie} = \beta_i + \beta_e + \beta_1 Y_{ie}^D + \beta_2 Y_{ie}^F + \delta X_{ie} + \epsilon_{ie}$$

The two hypotheses are thus:

**H1:**  $\beta_1 < 0$ . *Accountability. Voters reward (punish) governments for good (bad) economic performance.*

**H2:**  $\beta_2 = 0$ . *No attribution error. Voters do not hold governments accountable for an observable component of economic performance that is outside the government's control.*

As  $Y_{ie}^D$  and  $Y_{ie}^F$  are not directly observable, estimating Equation (2) requires that we find a proxy for  $Y_{ie}^F$  that is orthogonal to  $Y_{ie}^D$ . To find such a proxy, we first project GDP growth onto a weighted average of the growth of country  $i$ 's trade partners, which we term imported growth,  $Y_{ie}^I$ .

$$(3) \quad Y_{ie} = \gamma_i + \gamma_e + \gamma Y_{ie}^I + \theta X_{ie} + \mu_{ie}$$

The predicted value from this regression,  $\hat{Y}_{ie}$ , is a component of GDP growth that has been purged of domestic influences and is thus due to factors outside the government's control. We then use these predicted values as a proxy for  $Y_{ie}^F$  in a second-stage regression to estimate  $\beta_2$ .

$$(4) \quad T_{ie} = \beta_i + \beta_e + \beta_2 \hat{Y}_{ie} + \delta X_{ie} + \eta_{ie}$$

Note that to estimate  $\beta_2$  consistently via two-stage least squares (2SLS), imported growth must satisfy the standard exclusion restriction for a valid instrument. To test the hypothesis of accountability, we estimate Equation (1) using ordinary least squares (OLS) and test  $\beta < 0$ . To test the hypothesis of no-attribution error, we estimate Equations (3) and (4) using 2SLS (to ensure proper standard errors) and test  $\beta_2 = 0$ . In each case, standard errors are clustered by country.<sup>6</sup>

In measuring imported growth, we follow Brückner and Ciccone (2010) and Burke (2012)

6. One might consider an alternative and simpler approach in which we include imported growth,  $Y_{ie}^I$ , as well as  $Y_{ie}$ , GDP growth, which is observable to voters, to directly test

in calculating an “export-weighted growth-predictor index” (EWGP), based on bilateral trade data.<sup>7</sup> As argued by Brückner and Ciccone (2010) and Burke (2012), the effects of domestic policies are likely to have only second-order effects on foreign growth rates, thereby making the instrument virtually independent of changes in domestic political conditions or economic policies.<sup>8</sup>

To be more specific, we construct the indicator as follows:

$$(5) \quad \text{EWGP}_{it} = \sum_{j \neq i} \omega_{ij} \Delta \text{GDP}_{jt}$$

$$(6) \quad \omega_{ij} = \frac{1}{T} \sum_{t=1}^T \frac{\text{Export}_{ijt}}{\text{GDP}_{it}}$$

$\text{Export}_{ijt}$  is the volume of exports from country  $i$  to country  $j$  in year  $t$ , which is calculated in current (year  $t$ ) U.S. dollars.  $\text{GDP}_{it}$  is the level of GDP in country  $i$  in year  $t$ , and is also calculated in current U.S. dollars. The ratio therefore measures the contribution to country  $i$ 's GDP in year  $t$  from its exports to trading partner  $j$ . As in the study by Acemoglu et al. (2008), we average this ratio over the period 1990–2009 to find time-invariant  $\omega_{ij}$ , or a constant average ratio of exports from country  $i$  to country  $j$  to country  $i$ 's GDP in Equation (6). This insulates the instrument from changes in domestic economic policy (particularly trade policy) and ensures that the

whether imported growth is discounted in the voters' evaluation of governments. This equation is hard to interpret, however, because  $Y_{ie}^I$  and  $Y_{ie}$  are scaled differently and not directly comparable as the coefficient on  $Y_{ie}^I$  reflects the degree of pass-through from foreign growth to domestic growth as well as the extent of attribution error (Bertrand and Mullainathan 2001). Thus, even if one finds the coefficient on imported growth to be small, it is difficult to determine whether this is due to small attribution error or because foreign growth is not very important in determining domestic growth. The first-stage equation (Equation (3)) circumvents this issue by properly scaling the effects of imported growth on government turnover.

7. Brückner and Ciccone (2010) and Burke (2012) use this index, which fluctuates with the economic performance of close-trading partners, as an instrument to examine how an externally driven component of economic growth affects the likelihood of civil conflict risk and survival of national leader, respectively. Acemoglu et al. (2008) construct a similar instrument based on trade volume (rather than export volume) and the level of GDP rather than growth to capture the exogenous variation in income level that is orthogonal to domestic policy and institutions.

8. We later check the robustness of the results by removing large economies from the base sample to ensure that our results are not driven by these large economies whose domestic policies may have feedback effects.

measure depends only on differential effects of trading partners' economic conditions that are outside the domestic governments' control. These weights are then used to construct the export-weighted GDP growth of country  $i$ 's trading partners for year  $t$  in Equation (5).<sup>9</sup> The instrument has a single-peaked distribution with mean 0.33 and standard deviation 0.22, and is slightly left-skewed and fat-tailed relative to the normal. The histogram is included in Figure A2.

The literature on economic voting typically finds that voters respond to only a few macroeconomic variables. The "big two," as Lewis-Beck and Paldam (2000) put it, are unemployment or GDP growth and inflation. The literature has also consistently found a "cost of ruling"; support for the party in power declines even after controlling for economic performance. Thus, we include inflation and duration in power as control variables,  $X$ , when estimating Equations (2)–(4). We have also used unemployment data instead of GDP growth but these data present two major drawbacks. First, lower quality and coverage result in a smaller, noisier sample. Second, it has been shown that in some countries, unemployment is a partisan issue with high levels leading to greater support for left parties even if they are already in power (Carlsen 2000; Wright 2012). Thus, we focus on GDP growth and show the results with unemployment data in Table A1 as a robustness check. Summary statistics of the variables are presented in Table 1.

Studies of benchmarking, such as Kayser and Peress (2012), use the difference between a country's growth rate and the average growth rate of foreign countries to examine whether voters consider the performance of the domestic economy relative to the average performance of foreign economies. Unless growth in other countries passes through to the domestic economy one-for-one, this is not the same as the domestically generated component of growth that is relevant for proper attribution. Nonetheless, we do include time fixed effects that capture global economic conditions and thereby control for benchmarking.

9. We consider several variations of the instrumental variable constructing  $\omega_{ij}$  ratios from different time periods. One possibility is to use the lagged value of  $\omega$  as a weight. Another possibility is to construct  $\omega$  based on the pre-1990 data. All measures are highly correlated with one another (and also with GDP growth) and generate qualitatively similar results. We choose to construct  $\omega$  based on the 1990–2009 data as it maximizes the data coverage (bilateral trade data are spotty) and also to ensure that  $\omega$  does not reflect important shifts in the domestic environment, some of which might be anticipated.

Using country-level data to make inference on individual voting can potentially run into ecological fallacy. The ecological fallacy is essentially a problem of unobserved variation hindering the aggregation from the relationship in individual variables to the relationship between country-wide averages (Durlauf, Navarro, and Rivers 2010). We estimate the relationship between a country's voting behavior and a component of the country's GDP growth rate. It has been shown that sociotropic voting generally dominates egotropic voting (Lewis-Beck and Paldam 2000; Nannestad and Paldam 1994). Thus, we are not trying to infer the relationship between individual votes and individual income growth (egotropic voting), but trying to infer the relationship between individual votes and country-wide GDP growth (sociotropic voting). Arithmetically, if the aggregate vote total responds to aggregate imported growth, then there must have been many individual voters who responded to aggregate imported growth.

### III. DATA

The main source for political data is the World Bank Database of Political Institutions (DPI). Following Alesina et al. (1998, 2011), we construct two binary measures of government turnover to use as the dependent variables: EXECCH and IDEOCH. EXECCH indicates a change in the chief executive during an election year. A change in the chief executive usually results from the electoral loss of the incumbent ruling party in a parliamentary system or that of the incumbent president in a presidential system. IDEOCH indicates a change in the ideology of the cabinet as coded by the World Bank DPI. These measures are strongly correlated with a correlation coefficient of (.57).<sup>10</sup> Changes in the executive occur in 177 of the 306 elections (57.8%) whereas changes in the ideology of the government occur in only 111 of the 306 elections (36.3%). In practice, a change in ideology is almost always accompanied by a

10. Alesina, Carloni, and Lecce (2011) discuss the advantage and disadvantage of these two measures. On the one hand, they caution that EXECCH may falsely identify government turnover if it results from routine personnel replacement in a stable and reelected government. On the other hand, if a change in political conditions forces the incumbent coalition to run under different leadership, then the variable IDEOCH may underestimate political turnover since were it not for change in leadership, the incumbent might well have lost the majority.

**TABLE 1**  
Descriptive Statistics

Whole Sample		Observations	Mean	Standard Deviation	Min	Max
Inflation						
Overall	<i>N</i>	299	18.15	131.10	-1.17	2075.89
Between	<i>n</i>	71		81.22	0.54	522.93
Within	<i>T</i> -bar	4.21		110.70	-501.58	1571.11
Change in central government surplus						
Overall	<i>N</i>	236	-0.49	2.08	-9.72	5.65
Between	<i>n</i>	64		1.06	-2.95	2.02
Within	<i>T</i> -bar	3.69		1.82	-8.48	4.10
Duration of party in power						
Overall	<i>N</i>	301	8.04	8.61	1	71
Between	<i>n</i>	72		6.92	2	46.5
Within	<i>T</i> -bar	4.18		5.88	-24.46	43.87
Polity score						
Overall	<i>N</i>	267	8.91	1.54	0	10
Between	<i>n</i>	63		1.44	4	10
Within	<i>T</i> -bar	4.24		0.76	2.66	11.66
GDP growth						
Overall	<i>N</i>	306	3.10	3.71	-22.93	12.23
Between	<i>n</i>	72		2.55	-11.57	8.37
Within	<i>T</i> -bar	4.25		3.11	-8.27	14.46
EWGP						
Overall	<i>N</i>	306	0.635	0.606	-2.31	2.78
Between	<i>n</i>	72		0.420	0.0699	2.15
Within	<i>T</i> -bar	4.25		0.452	-1.86	2.19
Two-year growth						
Overall	<i>N</i>	305	3.24	3.27	-18.58	11.42
Between	<i>n</i>	72		2.36	-9.82	8.34
Within	<i>T</i> -bar	4.24		2.67	-16.83	12.76
Two-year EWGP						
Overall	<i>N</i>	306	0.664	0.521	-0.714	0.286
Between	<i>n</i>	72		0.402	0.752	0.226
Within	<i>T</i> -bar	4.2		0.347	-0.113	0.193
Ideological turnover (IDEOCH)						
Overall	<i>N</i>	306	0.36	0	1	306
Between	<i>n</i>	72		0	1	72
Within	<i>T</i> -bar	4.25		-0.44	1.20	4.25
Executive turnover (EXECCH)						
Overall	<i>N</i>	306	0.58	0	1	306
Between	<i>n</i>	72		0	1	72
Within	<i>T</i> -bar	4.25		-0.25	1.41	4.25
Unemployment						
Overall	<i>N</i>	204	8.32	4.91	1.9	35.99
Between	<i>n</i>	53		5.23	2.33	34.86
Within	<i>T</i> -bar	3.85		2.14	2.47	15.94

change in the executive. As a result, IDEOCH is virtually a subset of EXECCH.

Because the powers of the chief executive, and thus the public's perception of the chief executive's responsibility, may vary, we differentiate between countries with parliamentary systems or assembly-elected presidents and countries with presidential systems. When calculating the measures of electoral change in leadership, we use data from executive elections for presidential countries and legislative elections for parliamentary countries. The preferred specification combines both systems, but we do check for robustness by limiting to only parliamentary

countries.<sup>11</sup> The main independent variable, economic growth, represents the percentage change in real GDP, with data coming from the World Bank collection of World Development Indicators (WDI). To capture the exogenous component of GDP growth that is driven by external trade shocks, we make use of bilateral export data available from the International Monetary Fund Direction of Trade Statistics. To allow for the

11. We also examine a subsample of countries with a presidential system, but find that the number of countries in this sample (36 countries) is not large enough to generate informative results.

**TABLE 2**  
The Electoral Response to Imported Growth

Time Span for GDP Growth Dependent Variable	1 Year				2 Years			
	EXECCH		IDEOCH		EXECCH		IDEOCH	
	[1] OLS	[2] 2SLS	[3] OLS	[4] 2SLS	[5] OLS	[6] 2SLS	[7] OLS	[8] 2SLS
<b>All Elections</b>								
GDP growth	-0.0244*** (.00169)	-0.0494** (.0173)	-0.0219*** (.00919)	-0.0573** (.0304)	-0.0259*** (.00179)	-0.0629* (.0551)	-0.0278*** (.00464)	-0.0601*** (.00262)
Observations	306	306	306	306	306	306	306	306
Number of countries	72	72	72	72	72	72	72	72
$R^2$	0.102		0.069		0.094		0.074	
Kleibergen-Paap Wald $F$ statistics	10.25		10.25		21.01		21.01	
Stock-Yogo 15% critical value	8.96		8.96		8.96		8.96	
<b>Legislative Elections</b>	[9] OLS	[10] 2SLS	[11] OLS	[12] 2SLS	[13] OLS	[14] 2SLS	[15] OLS	[16] 2SLS
GDP growth	-0.0379*** (.000115)	-0.0704** (.0499)	-0.0367*** (.00142)	-0.105*** (.00114)	-0.0371*** (.000997)	-0.0863* (.0541)	-0.0331*** (.00840)	-0.116*** (.00378)
Observations	212	212	212	212	212	212	212	212
Number of countries	46	46	46	46	46	46	46	46
$R^2$	0.154		0.133		0.141		0.116	
Kleibergen-Paap Wald $F$ statistics	7.94		7.94		8.59		8.59	
Stock-Yogo 15% critical value	8.96		8.96		8.96		8.96	

*Notes:* This table establishes the basic results for the likelihood of change in government as a function of GDP growth. The relationship is estimated first using OLS and then via 2SLS using the trade-weighted GDP growth rate among trading partners to isolate the exogenous “imported” component of domestic GDP growth. We also vary three other specification choices to show the robustness of the results: GDP growth is calculated either for the year of the election only (1 year) or as the average of the election year and the preceding year (2 years); government turnover is either predicated on the identity of the chief executive (EXECCH) or on the ideology of the governing coalition (IDEOCH); and both legislative and executive elections are pooled (all elections) or the sample is constrained to only legislative elections (legislative elections).  $p$  values in parentheses. Kleibergen-Paap Wald  $F$  statistics show the instrument is borderline weak. Thus, we use robust standard errors for OLS and Anderson-Rubin standard errors, which are robust to weak instruments, for IV.

\*\*\* $p < .01$ , \*\* $p < .05$ , \* $p < .1$

broadest possible coverage and avoid an abrupt structural shift in the patterns of trade, we restrict our analysis to the years following the end of the Cold War (1990–2009).

#### IV. RESULTS

##### A. Baseline Results

Table 2 directly compares the OLS and 2SLS results from estimating Equations (3) and (4). To demonstrate that the results are fairly robust, we report the results for several minor variations on the specification. Columns (1)–(8) include both executive and legislative elections whereas columns (9)–(16) are restricted to legislative elections (and thus parliamentary countries). Columns marked “EXECCH” use the measure of executive change for government turnover

whereas columns labeled “IDEOCH” use the measure of ideological change for dependent variable. Finally, we vary the length over which we calculate economic growth. In the specifications marked “1 Year,” economic growth is calculated only for the year of the election, whereas in the other labeled “2 Years,” economic growth is the average of growth in the election year and the preceding year. A longer horizon has the advantage of smoothing out measurement errors but carries the potential disadvantage of overestimating voters’ attention spans that are typically estimated at less than 1 year (Nannestad and Paldam 1994).

The OLS coefficients are all negative, almost all are statistically significant, and they are all of similar magnitude, indicating that an additional point of GDP growth reduces the likelihood of replacing the government by somewhere between

2.2 and 3.8 percentage points. We thus fail to reject the accountability hypothesis. However, the 2SLS estimates show a much stronger effect, varying between 4.9 and 11.6 percentage points. To alleviate fears of weak instruments, we follow Brückner and Ciccone (2011) in calculating Anderson-Rubin  $p$  values that are robust to weak instruments. The obtained results are significant at the 5% level for all but one of the specifications (column (14)). The interquartile range of GDP growth is nearly 4 percentage points, suggesting that elections conducted in good growth years are roughly 20–45 percentage points more likely to return the government as elections conducted during bad years. Given the sample average likelihood of government replacement is either 36% (IDEOCH) or 58% (EXECCH) depending on the chosen measure of replacement, this is an extremely large effect. The hypothesis of no attribution error is clearly rejected.

In the following subsections, we discuss potential sources of measurement error, add the standard controls, and explore potential violations of the exclusion restriction. Finally, we add a split-sample analysis to explore whether certain factors mitigate the attribution error.

### B. Potential Measurement Issues

If voters were correctly ignoring growth that is plausibly exogenous, we would expect the 2SLS coefficients to be zero. At the least, we would expect them to be smaller than the OLS coefficients. The fact that the 2SLS coefficients are larger than the OLS coefficients likely means that the instrumental variable is mitigating the attenuation bias in the OLS coefficients that results from measurement error in the GDP growth data. So long as, after controlling for year and country fixed effects, measurement error in GDP data are not contemporaneously correlated between trade partners, estimation by 2SLS using trade-weighted GDP growth of trading partners serves to purge domestic GDP of idiosyncratic measurement error as well as the component of GDP that is due solely to domestic factors. The latter effect should push the 2SLS coefficient towards zero, presuming voters respond more strongly to domestic than imported growth. The former should correct the downward bias in the OLS coefficient. If voters do not strongly distinguish between imported and domestic growth, the latter could easily dominate, thereby making the 2SLS

coefficient larger than the OLS coefficient.<sup>12</sup> This may be especially so among less-developed countries for whom the GDP growth statistics of trade partners are of much higher quality than those for the domestic economy.<sup>13</sup>

The large discrepancy between the 2SLS and the OLS results suggests that instrumentation—a step that almost none of the previous literature in economic voting adopts (Wolfers 2006 being a lone exception)—is of great importance in estimating the magnitude of economic voting. Prior insignificant results in this literature may simply be due to attenuation bias.<sup>14</sup>

Another potential measurement issue derives from the fact that voters form their opinions using real-time data. As a result, an econometrician who uses final revision data (as we do) is measuring the actual variable that went into the voter's decision with error. If governments systematically manipulate real-time data for electoral gain, this could introduce bias.<sup>15</sup> There are three cases to consider. First, the incumbent might inflate growth figures during election years to increase reelection probabilities. In this case, the effects of manipulated data will be captured mostly by the intercept as it raises the reelection probability in all years and countries, regardless of economic conditions. If certain cultures or institutions enable greater data manipulation, this will be captured in country fixed effects. Second, if the measurement error is white noise, then it is the classic measurement error problem that results in (downward) attenuation bias. Third, if the extent of data manipulation is systematically related to the electoral strength of the government, then using the final-revision data can result in serious omitted variable bias. The two-stage estimation strategy helps to correct this bias. The first stage strips the deliberate political misreporting from the domestic GDP growth numbers

12. See Ashenfelter and Krueger (1994) for detailed discussion of the property of IV estimates when measurement error and endogeneity problem are both present.

13. For example, Johnson et al. (2009) find that the Penn World Table (PWT), a widely used GDP growth estimate changes on average by 1.1% across revisions of the dataset. This observation has motivated many researchers, such as Henderson, Storeygard, and Weil (2012) to seek creative proxies and instruments to GDP growth rates in developing countries that limit the effect of measurement error.

14. In their review of single-country studies, Duch and Stevenson (2008, 21) state “in almost no country is there anywhere near the level of consensus of confidence that characterizes the American literature.” See the rest of this section of their book for a detailed review.

15. Jong-A-Pin, Sturm, and de Haan (2012) show some evidence of manipulation of budget projections rather than GDP.



**TABLE 3**  
The Electoral Response to Imported Growth (with controls)

Dependent Variable: IDEOCH	(1)	(2)	(3)	(4)	(5)	(6)	(7)
All Elections	Base	Allowing Sample to Vary		Consistent Sample			
GDP growth (imported)	-0.0573** (.00304)	-0.0833** (.00833)	-0.0816** (.0268)	-0.0607 (.233)	-0.0496 (.259)	-0.0494 (.233)	-0.0494 (.235)
Length party has been in power		0.0177** (.000571)	0.0219*** (4.22e-05)	0.0291*** (5.66e-06)		0.0289*** (4.30e-06)	0.0289*** (4.32e-06)
Inflation			-0.00503** (.0478)	-0.000194 (.968)			0.000401 (.935)
Change in government surplus				0.0454 (.0394)			
Observations	306	301	294	229	229	229	229
Number of countries	72	71	69	58	58	58	58
Kleibergen-Paap Wald $F$ statistics	10.25	11.03	6.946	20.55	15.94	15.91	15.64
Stock-Yogo 15% critical value	8.96	8.96	8.96	8.96	8.96	8.96	8.96

*Notes:* Column (1) is our preferred specification from Table 2: 2SLS estimator, all elections, IDEOCH indicator, 1-year GDP growth window. Columns (2) and (3) successively add the two standard controls in the literature: inflation and the duration the party has been in power. These strengthen the results on response to imported GDP growth. Column (4) adds the change in the government surplus, which has been used in recent studies. Data on government surplus are sufficiently rare that the standard errors become much larger and we lose significance when this control is included (compare columns (1) and (4)). Re-estimating with a consistent sample, columns (5)–(7), shows that it is the change in sample rather than correlation among independent variables that is causing the loss of significance.  $p$  values in parentheses. Kleibergen-Paap Wald  $F$  statistics show the instrument is in at least one instance borderline weak. Thus, we use Anderson-Rubin standard errors, which are robust to weak instruments. \*\*\* $p < .01$ , \*\* $p < .05$ , \* $p < .1$

(presuming multiple trading partners are not performing the same political manipulation at the same time). The GDP growth in the second stage that is predicted based on multiple trading partners' GDP growth rates is independent of the strength of incumbent governments that is captured by the error terms. Finally, we have also checked, using the OECD Economic Outlook, whether GDP data revisions are systematically different for data released during an election year and data released in nonelection years. We find no difference using either quantile-quantile plots or panel regressions.

### C. Adding Control Variables

Having established robustness to various methods of constructing the variables, the remainder of the article uses all elections to maximize the sample; the IDEOCH indicator rather than EXECCH because economic policies are more likely associated with a party than a particular leader; and the 1-year window because the evidence on voter myopia suggests this is a better fit of voters' time horizons. Next, we add the two explanatory variables that have consistently been found significant in the literature on economic voting: inflation and the length of time the governing party has been in power. The signs are as expected and significant at the

5% level (Table 3, columns (2) and (3)): higher inflation and longer time in power both increase the probability of turnover.

Finally, we add a third control that has recently been suggested by Alesina, Carloni, and Lecce (2011), and Brender and Drazen (2008): change in the government budget surplus. Including this variable reduces the sample, thereby increasing the standard errors enough that the coefficient loses significance. At the same time, the point estimate declines slightly. To investigate further, we restrict the sample to those countries with budget surplus data and use this smaller but consistent sample to reestimate the specifications from columns (1)–(3) to produce columns (5)–(7). The point estimates are smaller across these different specifications, suggesting that adding the change in government surplus does not reduce the magnitude of the coefficient on GDP growth, rather the effect is simply weaker in this subsample.<sup>16</sup> Moreover, as the coefficient on the change in government surplus is never significant in the regressions, we remove it from further specifications and revert to the classic specification for economic voting: the “big two” economic variables plus time in power.

16. We investigate further with sample splits in the following section. See Table A2 for the composition of countries in each sample.

TABLE 4

The Electoral Response to Imported Growth (no large economies)

Dependent Variable: IDEOCH All Elections	(1)	(2)	(3)
GDP growth (imported)	-0.0624*** (.00158)	-0.0912*** (.00690)	-0.0868** (.0258)
Length party has been in power		0.0177*** (.000556)	0.0234*** (7.88e-07)
Inflation			-0.00537* (.0559)
Observations	260	256	249
Number of countries	63	62	60
Kleibergen-Paap Wald $F$ statistics	9.29	9.44	5.98
Stock-Yogo 15% critical value	8.96	8.96	8.96

Notes: This table repeats columns (1)–(3) from Table 3 while removing the largest economies (G7 plus India and Brazil) from the sample to avoid feedback from a domestic shock reflected through trade links back to the domestic economy. The results are virtually identical to those of Table 3. If anything, the response to imported growth is a little stronger.  $p$  values in parentheses. Kleibergen-Paap Wald  $F$  statistics show the instrument is borderline weak. Thus, we use Anderson-Rubin standard errors, which are robust to weak instruments. \*\*\*  $p < .01$ , \*\*  $p < .05$ , \*  $p < .1$ .

#### D. Testing the Robustness of the Exclusion Restriction

We report three robustness checks in Tables 4–6. First, one might be concerned that a home-grown boom or recession reflects off a trade partner and back to the domestic country. Such an “echo” would not be exogenous, and it would likely result in an upward bias in the coefficient on GDP growth in the second-stage regression if voters react positively (negatively) to the initial home-grown boom (recession). This is essentially the question of whether the instrument satisfies the exclusion restriction. If it is indeed violated for certain countries, then the coefficients ought to decline significantly in magnitude when we remove the offending countries.

We identify those countries in two different ways. First, we drop the largest economies: the G7 plus Brazil and India. The echo is probably larger for the largest economies; for example, recession in the United States is likely to affect the entire global economy, which is likely to have sizable effects on the U.S. economy, whereas a similar recession in Mexico is unlikely to have such feedback effects. Comparing Tables 3 and 4, we can observe that dropping the largest

TABLE 5

The Electoral Response to Imported Growth (no high feedback economies)

Dependent Variable: IDEOCH All Elections	(1)	(2)	(3)
GDP growth (imported)	-0.0500** (.0187)	-0.0730** (.0354)	-0.0753** (.0307)
Length party has been in power		0.0161*** (.00187)	0.0196*** (.000305)
Inflation			-0.00410* (.0983)
Observations	266	260	254
Number of countries	63	62	60
Kleibergen-Paap Wald $F$ statistics	8.77	8.46	8.21
Stock-Yogo 15% critical value	8.96	8.96	8.96

Notes: This table repeats Table 3 while removing the economies with the largest estimated feedback from a domestic shock reflected through trade partners. We take a 1% exogenous shock to domestic GDP, use the first stage estimates of the strength of trade links to calculate the resulting effect on foreign GDP, and then repeat once more to calculate the reflected effect on domestic GDP. Countries with a coefficient of greater than 0.05 are removed from the sample. This list includes countries with large and/or trade-dependent economies: Belgium, Czech Republic, The Netherlands, France, Germany, Slovenia, Thailand, UK, and United States. Removing weakens the point estimates slightly compared with those of Table 3 but the Anderson-Rubin statistics indicate significance at the 5% level.  $p$  values in parentheses. Kleibergen-Paap Wald  $F$  statistics show the instrument is borderline weak. Thus, we use Anderson-Rubin standard errors, which are robust to weak instruments. \*\*\*  $p < .01$ , \*\*  $p < .05$ , \*  $p < .1$ .

economies makes little difference (the magnitude actually increases slightly).

Second, we explicitly calculate the feedback effects for each country based on the first-stage regression results; that is, we take a 1% impulse to country  $i$ 's GDP growth and feed it through the first-stage coefficient and export shares to calculate the predicted GDP growth in country  $i$ 's trading partners, then feed the predicted growth of country  $i$ 's trading partners through the first-stage coefficient and export shares to country  $i$  to calculate the total feedback effect. The distribution of feedback effects is displayed in Figure A1. Note that in most cases, the feedback is less than one-hundredth of the initial growth in the home country. We exclude the nine countries with feedback effects  $> .05$  and reestimate the same regression equations (Table 5).<sup>17</sup> The point estimates

17. The excluded countries are Belgium, Czech Republic, France, Germany, The Netherlands, Slovenia, Thailand, UK, and United States.

**TABLE 6**

The Electoral Response to Imported Growth (no early elections)

Dependent Variable: IDEOCH All Elections	[1]	[2]	[3]
GDP growth (imported)	-0.0949*** (.00207)	-0.103*** (.00167)	-0.103** (.0150)
Length party has been in power		0.0250*** (4.61e-06)	0.0256*** (3.13e-06)
Inflation			-0.00669 (.103)
Observations	222	219	211
Number of countries	58	58	56
Kleibergen-Paap	7.69	7.76	4.28
Wald <i>F</i> statistics			
Stock-Yogo 15% critical value	8.96	8.96	8.96

*Notes:* This table repeats Table 3 while removing elections that were called early. Early elections are defined to include those at least 1 month before the constitutionally required date excepting only the following. Early elections do not include those that are earlier than expected as the result of constitutional revisions. While this removes 25–30% of the data, the magnitude of the coefficient becomes larger (compare with Table 3). We suspect that elections called early are often due to political events which call voter attention from economic issues. *p* values in parentheses. Kleibergen-Paap Wald *F* statistics show the instrument is somewhat weak. Thus, we use Anderson-Rubin standard errors, which are robust to weak instruments.

\*\*\**p* < .01, \*\**p* < .05, \**p* < .1.

decline slightly in magnitude: between 8% and 12%. Meanwhile, the smaller sample increases the conventional standard errors: between 2% and 16%. Nonetheless, the Anderson-Rubin weak-instrument robust tests continue to reject the null hypothesis of no effect at the 5% level.

Finally, in many countries, the ruling party may call for new elections when electoral conditions are particularly favorable.<sup>18</sup> As we cannot observe all of the conditions that favor the ruling party and thus cannot control for them, we might have sample selectivity problems as elections called early and those allowed to occur at the mandated expiration of the term may constitute different samples. To address this issue, we have rerun the analysis having removed those elections that were actually held more than a month in advance of the constitutionally specified time. The results do in fact differ across these samples (cf. Tables 3 and 6): economic voting is *stronger* in elections held at the constitutionally mandated date, so the results are not being driven by early

18. We do have some instances of early presidential elections due to death in office but the lion's share is parliamentary elections in parliamentary systems.

elections. We believe that the difference arises because snap elections are frequently called in response to idiosyncratic political events that are largely orthogonal to the macroeconomic situation.<sup>19</sup> As a result, snap elections are more likely to be coincident with and focused on noneconomic issues than regularly scheduled elections.

### *E. Mitigating Factors*

Next, we address whether a free press, an educated citizenry, and a mature democracy mitigate the extent of this misattribution by voters. When voters choose whether or not to be informed, they are balancing the cost of acquiring information against its potential benefit. Importantly, the cost of acquisition includes both the direct cost (e.g., subscription price of a newspaper) and the consumption cost of reading, parsing, and filtering the raw and potentially biased information to achieve an informative signal. We would expect these costs to vary across voters. Better-educated voters are likely to have lower consumption costs and thus be better informed (Aidt 2000). On aggregate, we expect countries with a more educated population to respond less to the imported (irrelevant) component of growth as their more educated voters are more likely to realize that the government is not responsible for that component of growth.

Similarly, we expect that a free press will provide voters with higher quality information and thus an easier signal-extraction problem. Finally, we expect that the process of evaluating a government requires practice and the evolution of soft institutions dedicated to monitoring; that the media and the electorate both learn better what information is relevant and what is not and that as they do so the quality of information increases and the cost of consumption declines, both leading to an electorate that is less likely to make attribution errors.

We test these hypotheses by splitting the sample at the median value for continuous variables (years of schooling and freedom of the press index) or between the categories for the dichotomous variable (new vs. established

19. For example, the Japanese snap election of 2005 was fought over the issue of privatizing Japan Post. Incumbent Koizumi won big despite the weak economy. The New Zealand snap election of 2002 was precipitated by coalition struggles that were sufficiently arcane that the election took the opposition by surprise. However, the early dissolution of both the Israeli and Dutch governments in 2012 stemmed from failure to agree among the governing coalition to a budget in the face of economic downturn.

**TABLE 7**  
Mitigating the Response to Imported Electoral Growth

Dependent Variable: IDEOCH	[1] New Democracies	[2] Established Democracies	[3] Low Schooling	[4] High Schooling	[5] Controlled Media	[6] Free Media
All Elections						
GDP growth (imported)	-0.0641*** (.000400)	0.00114 (.987)	-0.242** (.0305)	-0.0441 (.119)	-0.0881*** (.00137)	-0.156 (.330)
Observations	82	175	158	158	150	131
Number of countries	36	42	56	45	60	44
Kleibergen-Paap Wald <i>F</i> statistics	2.41	6.07	0.60	13.35	1.86	0.68
Stock-Yogo 15% critical value	8.96	8.96	8.96	8.96	8.96	8.96

*Notes:* Runs our preferred specification (2SLS estimator, all elections, IDEOCH indicator, 1-year GDP growth window) while splitting the sample along four different dimensions. It is shown that voter response to imported growth is concentrated in new democracies, countries with low levels of education, and countries with a high level of trade. The role of a free press is less clear. *p* values in parentheses. Kleibergen-Paap Wald *F* statistics show the instrument is clearly weak. Thus, we use Anderson-Rubin standard errors, which are robust to weak instruments.

\*\*\**p* < .01, \*\**p* < .05, \**p* < .1.

**TABLE 8**  
Mitigating the Response to Imported Electoral Growth (with controls)

Dependent Variable: IDEOCH	[1] New Democracies	[2] Established Democracies	[3] Low Schooling	[4] High Schooling	[5] Controlled Media	[6] Free Media
All Elections						
GDP growth (imported)	-0.0779* (.0890)	-0.0166 (.813)	-0.238** (.0193)	-0.0612 (.328)	-0.348*** (.000725)	-0.172 (.271)
Length of time in power	0.0199*** (6.29e-05)	0.0228 (.105)	0.0308** (.0221)	0.0171 (.157)	0.00911 (.687)	0.0221** (.0434)
Inflation, consumer prices (annual %)	-0.00279* (.0603)	0.00500 (.431)	0.00119 (.899)	-0.0127 (.571)	-0.00301 (.885)	0.0459 (.417)
Observations	82	175	158	158	150	131
Number of countries	36	42	56	45	60	44
Kleibergen-Paap Wald <i>F</i> statistics	1.60	6.27	0.60	4.06	0.33	0.59
Stock-Yogo 15% critical value	8.96	8.96	8.96	8.96	8.96	8.96

*Notes:* A repeat of Table 7 with the classic controls: inflation and duration in power. The results change a little. Voters in countries with controlled media appear more vulnerable to attribution error. *p* values in parentheses. Kleibergen-Paap Wald *F* statistics show the instrument is clearly weak. Thus, we use Anderson-Rubin standard errors, which are robust to weak instruments.

\*\*\**p* < .01, \*\**p* < .05, \**p* < .1.

democracies).<sup>20</sup> We run only our preferred specification: 2SLS using the IDEOCH measure of government turnover, the shorter 1-year window for economic growth, and pooling all elections. We measure freedom of the press using the Freedom House's Index. We measure the education level of citizens using the Barro-Lee Educational Attainment Dataset for average years of schooling in the adult population.<sup>21</sup> We adopt Brender and Drazen's (2005) definition of an established democracy as a country that has been through at least four consecutive democratic elections. We outline the results in Table 7, and report the same sample splits with controls for

inflation and duration in power in Table 8. In both tables, we continue to use the Anderson-Rubin statistic to test the null hypothesis of no effect in a manner that is robust to a potentially weak instrument.

The results support the hypotheses that factors improving the transmission of information can reduce attribution error. In both tables, voters in new democracies respond strongly to imported growth whereas those in established democracies do not appear to make such attribution errors. Likewise, the attribution error is characteristic of electorates with lower levels of education (the Anderson-Rubin test strongly rejects the null hypothesis of no effect in both specifications), but not those with higher levels of education. The evidence on media freedom is somewhat weaker but points in the expected direction. It is also worth noting that these three measures are

20. See Table A2 for the composition of countries in each sample split.

21. See <http://www.barrolee.com/> for the Barro-Lee data.

not strongly correlated and thus appear to measure distinct methods of improving voters' performance.<sup>22</sup>

## V. SUMMARY

How do voters treat their incumbent government in elections when their economies are in recession or boom? Does it matter to voters whether the state of the economy is home-grown or imported from trading partners? We present together a panel data set of 72 democracies from 1990 to 2009 and show that voters do reward incumbent government for good economic performance. However, they do so even when the economic boom results from their trading partners' economic boom. These results suggest that voters make systematic attribution errors by rewarding incumbents for growth that is plausibly exogenous. However, we have shown that the same factors which mitigate the electoral budget cycle also mitigate this form of voter misattribution, suggesting that voter attribution errors are less likely in countries with a long tradition of democracy, educated voters, and free media. These results are robust to exclusion of high-feedback economies, endogenous elections, and system of democratic government; the inclusion of standard controls; and choices of how to construct the instrument.

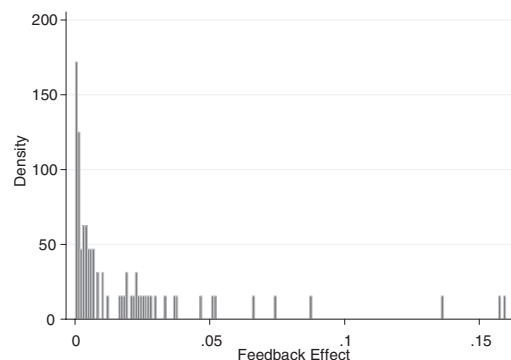
The results highlight an additional potential obstacle to democratic accountability. Voters may reduce the informational complexity by focusing on easily understood metrics such as economic growth and inflation. Voters may pay the costs to acquire such information out of civic duty or in service of social or personal financial gains. But even so, voters may not be capable of processing this information to correctly assign credit or blame to the incumbent government. The results suggest that reducing such errors requires educated and experienced voters and a media able to set the information in its proper context, and thus confirm the growing literature that touts the importance of the soft institutions of democracy. Improving the quality of information available to voters and improving, by practice and education, the ability of voters to process this information enables voters to better attribute economic performance to its proper source.

There are three natural extensions of this article. First, the literature on economic voting explores whether inflation affects election returns (Lewis-Beck and Paldam 2000; Nannestad and Paldam 1994) and also the turnover of central bank governors (Dreher, Sturm, and de Haan 2008). It would be of interest to explore whether imported inflation has similar effects based on the data on international monetary linkages as well as trade linkages. Second, the results show that experience, education, and access to information help reduce attribution error. Examination of attribution errors with voter-level microdata is a promising avenue that may further illuminate the mechanism by which attribution errors occur. Third, although the results that imported growth has important effects on government turnovers suggest that voters might not be attributing the source of economic fluctuation properly, an alternative interpretation is that voters punish governments for not responding to negative trade shocks aggressively enough to reduce their effects on the domestic economy. These are not necessarily competing explanations; they are likely to be taking place at the same time, potentially reinforcing each other (i.e., if voters are not attributing properly, then they might be more likely to demand explicit policy action to reduce the severity of a recession). Examination of whether government turnover varies with policy response is thus another potential avenue of future research.

## APPENDIX

**FIGURE A1**

The Distribution of Feedback Effects of Domestic Growth



22. The bivariate correlations are .01,  $-.15$ , and  $-.36$ .

Figure A1 displays the histogram of the estimated feedback effect of a 1% increase in domestic GDP growth. A 1% shock to GDP in the domestic country will produce greater GDP in trade partners, which will then redound to further increases in domestic GDP. While most countries are too small or too closed to generate significant feedback from domestic shocks, nine countries in the sample exhibit feedback in excess of 5% of the original shock. The calculation is based upon the first-stage regression coefficient and export-to-GDP shares of each country.

FIGURE A2

The Distribution of the Fitted Explanatory Variable

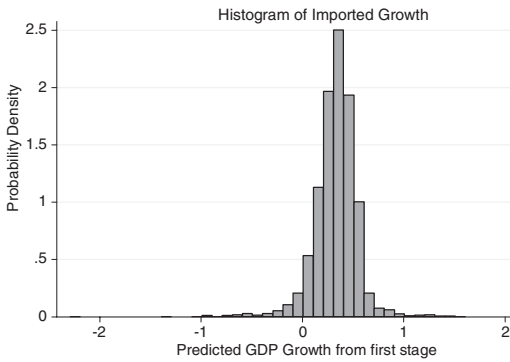


TABLE A1

The Electoral Response to Imported Growth (unemployment)

Dependent Variable: IDEOCH All Elections	(1)	(2)	(3)
Unemployment (imported)	0.0668 (.376)	0.0694 (.389)	0.0806 (.390)
Length party has been in power		0.0261***	0.0265***
Inflation		-0.00575	-0.00594 0.00655 -0.00963
Observations	204	204	204
Number of countries	53	53	53
Kleibergen-Paap Wald <i>F</i> statistics	2.52	2.48	2.15
Stock-Yogo 15% critical value	8.96	8.96	8.96

Notes: Table A1 repeats columns (1)–(3) from Table 3 using unemployment instead of GDP growth. The signs are as expected but the instrument is too weak to allow inference. *p* values in parentheses. We use Anderson-Rubin standard errors, which are robust to weak instruments.

\*\*\**p* < .01, \*\**p* < .05, \**p* < .1.

TABLE A2  
The Sample Under Different Conditions

Country	All Elections		Legislative Elections		All Elections with Government Surplus		Observations Lost Due to Government Surplus		Years in Sample		
	No	Controls	No	Controls	All Elections with Government Surplus	Government Surplus	New	Established	High	Low	Media Freedom
Albania	6		6		3	3	3	3	4	1	5
Argentina	4		—		3	1	2	2	1	3	4
Australia	7		7		6	1	7	7	7	1	1
Austria	7		7		6	1	7	7	2	4	3
Bahamas	4		4		2	2	4	4	4	4	3
Barbados	3		3		3	2	3	3	4	3	3
Belgium	5		5		3	2	5	5	5	3	5
Belize	3		3		1	2	3	3	1	2	1
Bolivia	5		—		4	1	1	4	1	4	3
Brazil	4		—		3	1	2	2	1	4	4
Canada	6		6		6	1	6	6	5	4	4
Cape Verde Islands	4		1		1	3	4	4	4	4	4

TABLE A2  
Continued

Country	Years in Sample													
	All Elections		Legislative Elections		All Elections with Government Surplus		Observations Lost Due to Government Surplus		Democratic Age		Years of Schooling		Media Freedom	
	No Controls	No Controls	All Elections with Government Surplus	Government Surplus	Government Surplus	New	Established	High	Low	High	Low	High	Low	
Chile	4	—	—	4	4	4	—	1	2	1	2	1	2	
Colombia	3	—	2	1	1	—	3	—	3	1	3	1	2	
Costa Rica	5	—	4	1	1	—	5	—	5	5	5	5	4	
Croatia	4	2	4	—	—	3	—	—	—	—	—	—	—	
Cyprus	3	—	2	1	1	3	—	2	2	2	4	2	4	
Czech Republic	3	3	3	—	—	3	—	3	3	3	—	2	3	
Denmark	6	6	5	1	1	6	—	6	6	6	—	6	3	
Dominican Republic	6	—	5	1	1	5	—	5	5	1	5	1	4	
Ecuador	3	—	2	1	1	2	—	2	2	2	2	1	1	
El Salvador	4	—	4	—	—	2	—	2	2	2	3	—	3	
Estonia	3	3	3	—	—	3	—	—	—	—	—	—	—	
Finland	5	5	5	—	—	5	—	5	5	5	—	5	3	
France	4	4	4	—	—	4	—	4	4	4	—	4	2	
Germany	6	6	5	—	—	5	—	5	5	2	2	5	2	
Ghana	2	—	—	1	1	—	—	—	—	—	—	—	—	
Greece	6	6	5	—	—	6	—	6	6	1	4	1	1	
Grenada	5	5	—	—	—	5	—	5	5	4	4	2	4	
Guatemala	4	—	2	—	—	2	—	—	—	—	—	—	—	
Honduras	5	—	5	—	—	5	—	—	—	—	—	—	—	
Hungary	5	5	4	—	—	4	—	—	—	—	—	—	—	
Iceland	5	5	5	—	—	5	—	—	—	—	—	—	—	
India	5	5	5	—	—	5	—	—	—	—	—	—	—	
Ireland	4	4	4	—	—	4	—	—	—	—	—	—	—	
Israel	7	5	7	—	—	7	—	—	—	—	—	—	—	
Italy	6	6	5	—	—	6	—	—	—	—	—	—	—	
Jamaica	4	4	2	—	—	4	—	—	—	—	—	—	—	
Japan	6	6	1	—	—	6	—	—	—	—	—	—	—	
Latvia	4	4	3	—	—	3	—	—	—	—	—	—	—	
Luxembourg	4	4	3	—	—	3	—	—	—	—	—	—	—	
Macedonia	3	3	2	—	—	2	—	—	—	—	—	—	—	
Malawi	3	—	—	—	—	—	—	—	—	—	—	—	—	
Malta	5	5	5	—	—	5	—	—	—	—	—	—	—	

TABLE A2  
Continued

Country	Years in Sample											
	All Elections No Controls	Legislative Elections No Controls	All Elections with Government Surplus	Observations Lost Due to Government Surplus	Democratic Age			Years of Schooling		Media Freedom		
					New	Established	High	Low	High	Low		
Mexico	2	—	2	2	2	2	2	2	2	2	2	
Moldova	4	3	3	1	4	4	2	1	3	3	3	
Nepal	3	3	3	3	3	3	6	2	2	2	2	
The Netherlands	6	6	6	2	6	6	6	6	6	6	6	
New Zealand	7	7	5	2	2	2	7	2	2	2	2	
Nigeria	2	—	5	2	2	2	5	4	4	4	4	
Norway	5	5	5	1	2	2	2	2	2	2	2	
Pakistan	2	2	1	1	2	2	2	2	2	2	2	
Paraguay	4	—	4	4	4	4	2	3	3	3	3	
Peru	2	—	2	2	2	2	2	2	2	2	2	
Poland	2	—	2	2	2	2	2	2	2	2	2	
Portugal	6	6	6	1	2	2	6	5	5	5	5	
Romania	2	—	1	1	2	2	2	2	2	2	2	
Senegal	2	—	2	2	2	2	2	2	2	2	2	
Slovenia	5	5	4	1	5	5	4	4	4	3	3	
South Korea	4	—	4	4	4	4	4	4	4	3	3	
Spain	5	5	5	5	5	5	5	2	2	1	1	
Sri Lanka	2	—	2	4	2	2	2	1	1	3	3	
St. Lucia	4	4	4	4	4	4	4	4	4	4	4	
Sweden	5	5	4	1	5	5	5	5	5	5	5	
Thailand	2	2	2	3	2	2	2	2	2	2	2	
Thailand	6	6	3	3	6	6	6	6	6	1	1	
Trinidad-Tobago	3	3	3	2	2	2	3	3	3	3	3	
Turkey	2	—	2	2	2	2	4	2	2	2	2	
Ukraine	4	4	4	4	4	4	4	1	1	3	3	
UK	5	—	5	4	5	5	5	4	4	4	4	
USA	4	—	4	6	4	4	6	3	3	1	1	
Uruguay	5	—	5	4	5	5	5	4	4	4	4	
Uruguay	4	—	4	6	4	4	6	3	3	3	3	
Vanuatu	6	6	6	6	6	6	6	5	5	1	1	



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