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Rosa C. Hayes, Masami Imai & Cameron A. Shelton

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Attribution Error in Economic Voting: Evidence From Trade Shocks[*]

Rosa C. Hayes (Federal Reserve Bank of NY)

Masami Imai (Wesleyan University)[**]

Cameron A. Shelton (Claremont McKenna College)

Abstract

This paper exploits the international transmission of business cycles to examine the prevalence of attribution error in economic voting in a large panel of countries from 1990-2009. We find that voters, on average, exhibit a strong tendency to oust 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 which have been shown to mitigate other voter agency problems—do better in distinguishing imported from domestic growth.

Keywords: Economic voting; Political agency problem
JEL Classification Codes: E3, E6

[**] The corresponding author. Economics Department, Wesleyan University, 238 Church Street, Middletown, CT 06459-0007, mimai@wesleyan.edu, TEL: 860-685-2155, FAX: 860-685-2301.

1. **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 (Kramer 1971, Fair 1978, Frey and Schneider 1978, Lewis-Beck 1988, Powell and Whitten 1993, Hellwig 2001, Duch and Stevenson 2008).[1] Other economic variables such as tax increases have received inconsistent support (Niemi, Stanley, and Vogel 1995 but Kone and Winters 1993) leading reviews to conclude that there are two main variables of interest: inflation and GDP growth or unemployment (Nannestad and Paldam 1994; Lewis-Beck and Paldam 2000).[2]

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).[3] Some 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

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[1] See Lewis-Beck and Stegmaier (forthcoming) for recent comprehensive review of economic voting literature.
[2] There is also a large literature which looks at the effect of non-economic variables such as war casualties (Mueller 1973); fiscal cost of war (Geys 2010); and natural disasters (Healy and Malhotra 2009; Cole, Healy, and Werker 2011). 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). Though causation is difficult to establish (Acemoglu, Johnson, Robinson, and Yared, 2005), these results suggest that educated citizens are better at disciplining their political leaders than uneducated ones.

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.[4]

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). 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.

But benchmarking does not prevent attribution errors: they are separate phenomena that can occur independently. Suppose reduced growth in the US reduces growth in Brazil due to trade spillovers but has little effect on growth in Uruguay, which is less dependent on US 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 Uruguan policymakers.

To date, a pair of studies document the presence of attribution errors in the US and India. Wolfers (2006) shows that the incumbent governors of oil-producing US states tend to enjoy a higher reelection probability when oil prices are rising. Cole, Healy, and Werker (2011) show

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[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 see little attempt by voters to correctly attribute credit and blame, even in situations where assigning credit is relatively simple.

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 1990-2009 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 of 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 paper 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

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(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, our 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: our 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, our 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 sub-sample of countries.

The rest of this paper is organized as follows. Sections 2 and 3 describe our methodology and data sources, section 4 reports the results, followed by concluding remarks in section 5.

2. Methodology

We follow closely the methodology of Bertrand and Mullainathan (2001). They compare the impact on 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.[5] 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:

$$T_{it} = \beta_i + \beta_e + \beta * Y_{ie} + \delta X_{ie} + \varepsilon_{ie} \tag{1}$$

Where T_{ie} is a dichotomous variable representing *turnover* that takes the value 1 if there is a change in government in country i in year e, Y_{ie} measures GDP Growth, the β_i are country fixed-effects, which capture country-specific un-observables that are correlated with the electoral stability of the incumbent government, the β_e are 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 below. The coefficient β captures the average effect of economic growth on

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[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. Our finding that attribution errors are mitigated in countries where voters have greater information and experience mirrors their results.

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government turnover. It is expected to be negative if incumbents are less likely to be ousted during economic expansion.

Our purpose 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 due 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.

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

Our 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.

Since 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.

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

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 β_2.

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

Note that in order to estimate β_2 consistently via 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 last squares and test $\beta < 0$. To test the hypothesis of no-attribution error, we estimate equations (3) and (4) using two-stage least squares (to ensure proper standard errors) and test $\beta_2 = 0$. In each case, standard errors are clustered by country.[6]

In measuring imported growth, we follow Brückner and Ciccone (2010) and Burke (2012) in calculating an "export-weighted growth-predictor index" (EWGP), based on bilateral trade data.[7] As argued by Brückner and Ciccone (2010) and Burke (2012), the effects of

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[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, in order to directly test 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 since 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

domestic policies are likely to have only second order effects on foreign growth rates, thereby making our instrument virtually independent of changes in domestic political conditions or economic policies.[8]

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

$$EWGP_{it} = \sum_{j \neq i} \omega_{ij} \Delta GDP_{jt} \tag{6}$$

$$\omega_{ij} = \frac{1}{T} \sum_{t=1}^{T} \frac{Export_{ijt}}{GDP_{it}} \tag{7}$$

$Export_{ijt}$ is the volume of exports from country i to country j in year t, which is calculated in current (year t) U.S. dollars. 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 Acemoglu et al. (2008), we average this ratio over the period 1990-2009 to find time-invariant ω_{ij}, or a constant average ratio of exports from country i to country j to country i's GDP in equation (2). This insulates our instrument from changes in domestic economic policy (particularly trade policy) and ensures that our measure depends only on differential effects of trading partners' economic conditions that are outside domestic governments' control. These weights are then used to construct the trade-weighted change in the GDP of country i's trading partners for year t in equation (1).[9] The instrument has

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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 our 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.

[9] We consider several variations of our instrumental variable constructing ω_{ij} ratios from different time periods. One possibility is to use the lagged value of ω as a weight. Another possibility is to construct ω 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 ω based on the 1990-2009 data since it maximizes the data coverage

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 the appendix (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, 3, and 4. We have also used unemployment data instead of GDP growth but these data present two major drawbacks. First, lower quality and coverage results 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 the Appendix (Table A1) as a robustness check. Summary statistics of our 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 which is relevant for proper attribution. Nonetheless, we do include time fixed-effects which capture global economic conditions and thereby control for benchmarking.

(bilateral trade data are spotty) and also to ensure that ω 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 the 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 (Nannestad and Paldam 1994, Lewis-Beck and Paldam 2000). Thus we are not trying to infer the relationship between individual votes and individual income growth (egotropic voting). We are 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.

3. Data

Our 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 our 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 (0.57).[10] Changes in the executive occur in 177 of our 306 elections (57.8%)

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[10] Alesina et al. (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

whereas changes in the ideology of the government occur in only 111 of our 306 elections (36.3%). In practice, a change in ideology is almost always accompanied by a 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 our measures of electoral change in leadership, we use data from executive elections for presidential countries and data from legislative elections for parliamentary countries. Our preferred specification combines both systems, but we do check for robustness by limiting to only parliamentary countries[11]

Our main independent variable, economic growth, represents the percentage change in real gross domestic product, with data coming from the World Bank 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 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).

4. Results
A. Baseline Results

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and re-elected 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.

[11] We also examine a sub-sample of countries with presidential system, but find that the number of countries in this sample (36 countries) is not large enough to generate informative 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 while columns 9-16 are restricted to legislative elections (and thus parliamentary countries). Columns 1-4 and 9-12 use the measure of executive change for government turnover while columns 5-8 and 13-16 use the measure of ideological change for dependent variable. Finally, we vary the length over which we calculate economic growth. In the specifications marked "One Year", economic growth is calculated only for the year of the election. In the specification marked "Two 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 which are typically estimated at less than one 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. Our results are significant at the 5% level for all but one of the specifications (column 14). The inter-quartile 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 our 2SLS coefficients are larger than the OLS coefficients likely means our instrumental variable is mitigating the attenuation bias in the OLS coefficients that results from measurement error in our 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. [12] 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. [13]

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[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 change on average by 1.1% across revisions of the dataset. This observation has motivated many researchers, such

The large discrepancy between the 2SLS and the OLS results suggests that instrumentation—a step which 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.[14]

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.[15] There are three cases to consider. First, the incumbent might inflate growth figures during election years to increase re-election probabilities. In this case, the effects of manipulated data will be captured mostly by the intercept as it raises the re-election 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. Our two-stage estimation strategy helps to correct this bias. The first stage strips the deliberate political misreporting from the domestic GDP growth numbers (presuming multiple trading partners aren't doing the same political manipulation at the same time). The

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as Henderson, Storeygard, and Weil (2009) 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, p21) 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] We are indebted to an anonymous reviewer for this point.

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 which 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 non-election 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 our variables, the

remainder of the paper will use all elections to maximize the sample; the IDEOCH indicator

rather than PMCH because economic policies are more likely associated with a party than a

particular leader; and the one-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 et al (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 our coefficient

loses significance. At the same time, the point estimate declines slightly. To investigate further,

we restrict our sample to those countries with budget surplus data and use this smaller but

consistent sample to re-estimate 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 sub-sample.[16] Moreover, as the coefficient on the change in government surplus is never significant in our 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.

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 our instrument satisfies the exclusion restriction. If it is indeed violated for certain countries, then our 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 likely larger for the largest economies; e.g., recession in the US is likely to affect the entire global economy, which is likely to have the sizable effects on the US economy, while a similar recession in Mexico is unlikely to have such feedback effects. Comparing Tables 3 and 4, we can see that dropping the largest economies makes little difference (the magnitude actually increases slightly).

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[16] We investigate further with sample splits in the next section. See Table A2 in Appendix for the composition of countries in each sample.

Second, we explicitly calculate the feedback effects for each country based on the 1st stage regression results; i.e., 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 in Appendix. Note that in most cases, the feedback is less than 1/100th of the initial growth in the home country. We drop the nine countries with feedback effects > 0.05 and re-estimate the same regression equations (Table 5).[17] The point estimates 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 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.[18] Since 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 one month in advance of the constitutionally specified time. The results do in fact differ across these samples (compare Tables 3 and 6): economic voting is *stronger* in elections held at the constitutionally mandated date, so our results are not being driven by early elections. We believe the difference arises because snap elections

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[17] The excluded countries are Belgium, Czech Republic, France, Germany, Netherlands, Slovenia, Thailand, UK, and the US.

[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.

are frequently called in response to idiosyncratic political events that are largely orthogonal to the macroeconomic situation[19]. As a result, snap elections are more likely to be coincident with and focused on non-economic 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.

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[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. On the other hand, 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.

We test these hypotheses by splitting the sample at the median value for continuous variables (years of schooling, freedom of the press index) or between the categories for the dichotomous variable (new vs. established democracies).[20] 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 index. We measure the education level of citizens using the Barro-Lee Educational Attainment Dataset for average years of schooling in the adult population.[21] We adopt Brender and Drazen's (2005) definition of an established democracy as a country which has been through at least four consecutive democratic elections. We report the results in Table 7. We 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 our hypotheses that factors improving the transmission of information can reduce attribution error. In both tables, voters in new democracies respond strongly to imported growth while voters in established democracies do not seem 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 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 not strongly correlated and thus seem to measure distinct methods of improving voters performance.[22]

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[20] See Table A2 in Appendix for the composition of countries in each sample split.
[21] See http://www.barrolee.com/ for the Barro-Lee data.
[22] The bivariate correlations are 0.01, -0.15, and -0.36.

5. 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 put together a panel data set of 72 democracies from 1990-2009 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.

Our 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. Our results suggest that reducing such errors requires educated and experienced voters and a media able to set the information in its proper context. Our results 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 our paper. First, the literature on economic voting explores whether inflation affects election returns (Nannestad and Paldam 1994; Lewis-Beck and Paldam 2000) 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, our results show that experience, education, and access to information help reduce attribution error. Examination of attribution errors with voter-level micro data is a promising avenue that may further illuminate the mechanism by which attributor 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.

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Table 1: Descriptive Statistics

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

Table 2: The Electoral Response to Imported Growth

Time span for GDP growth	One Year					Two Year			
dependent variable	EXECCH		IDEOCH			EXECCH		IDEOCH	
	[1]	[2]	[3]	[4]		[5]	[6]	[7]	[8]
All Elections	OLS	2SLS	OLS	2SLS		OLS	2SLS	OLS	2SLS
GDP Growth	-0.0244***	-0.0494**	-0.0219***	-0.0573**		-0.0259***	-0.0629*	-0.0278***	-0.0601***
	(0.00169)	(0.0173)	(0.00919)	(0.0304)		(0.00179)	(0.0551)	(0.00464)	(0.00262)
Observations	306	306	306	306		306	306	306	306
Number of Countries	72	72	72	72		72	72	72	72
R-squared	0.102		0.069			0.094		0.074	
First Stage F Statistic		10.25		10.25			21.01		21.01
	[9]	[10]	[11]	[12]		[13]	[14]	[15]	[16]
Legislative Elections	OLS	2SLS	OLS	2SLS		OLS	2SLS	OLS	2SLS
GDP Growth	-0.0379***	-0.0704**	-0.0367***	-0.105***		-0.0371***	-0.0863*	-0.0331***	-0.116***
	(0.000115)	(0.0499)	(0.00142)	(0.00114)		(0.000997)	(0.0541)	(0.00840)	(0.00378)
Observations	212	212	212	212		212	212	212	212
Number of Countries	46	46	46	46		46	46	46	46
R-squared	0.154		0.133			0.141		0.116	
First Stage F Statistic		7.94		7.94			8.59		8.59

Table 2 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 (One Year) or as the average of the election year and the preceding year (Two Year); 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. We use robust standard errors for OLS; Anderson-Rubin standard errors, which are robust to weak instruments, for IV. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

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**	-0.0833**	-0.0816**	-0.0607	-0.0496	-0.0494	-0.0494
	(0.00304)	(0.00833)	(0.0268)	(0.233)	(0.259)	(0.233)	(0.235)
Length party has been in power		0.0177***	0.0219***	0.0291***		0.0289***	0.0289***
		(0.000571)	(4.22e-05)	(5.66e-06)		(4.30e-06)	(4.32e-06)
Inflation			-0.00503**	-0.000194			0.000401
			(0.0478)	(0.968)			(0.935)
Change in Government Surplus				0.0454			
Observations	306	301	294	229	229	229	229
Number of Countries	72	71	69	58	58	58	58
First Stage F Statistic	10.25	11.03	6.946	20.55	15.94	15.91	15.64

Table 3: column (1) is our preferred specification from Table 2: 2SLS estimator, all elections, IDEOCH indicator, one-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 is sufficiently rare that the standard errors become much larger and we lose significance when this control is included (compare columns 1 and 4). Reestimating 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. We use Anderson-Rubin standard errors, which are robust to weak instruments. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4: The Electoral Response to Imported Growth (no large economies)

Dependent Variable: IDEOCH	[1]	[2]	[3]
All Elections			
GDP Growth (imported)	-0.0624***	-0.0912***	-0.0868**
	(0.00158)	(0.00690)	(0.0258)
Length party has been in power		0.0177***	0.0234***
		(0.000556)	(7.88e-07)
Inflation			-0.00537*
			(0.0559)
Observations	260	256	249
Number of Countries	63	62	60
First Stage F Statistic	9.29	9.44	5.98

Table 4: repeats columns (1) – (3) from Table 3 while removing the largest economies (G-7 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. We use Anderson-Rubin standard errors, which are robust to weak instruments. *** p<0.01, ** p<0.05, * p<0.1

Table 5: The Electoral Response to Imported Growth (no high feedback economies)

Dependent Variable: IDEOCH	[1]	[2]	[3]
All Elections			
GDP Growth (imported)	-0.0500**	-0.0730**	-0.0753**
	(0.0187)	(0.0354)	(0.0307)
Length party has been in power		0.0161***	0.0196***
		(0.00187)	(0.000305)
Inflation			-0.00410*
			(0.0983)
Observations	266	260	254
Number of Countries	63	62	60
First Stage F Statistic	8.77	8.46	8.21

Table 5: 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 our 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, Netherlands, France, Germany, Slovenia, Thailand, UK, USA. Removing weakens the point estimates slightly compared to those of Table 3 but the Anderson-Rubin statistics indicate significance at the 5% level. P-values in parentheses. We use Anderson-Rubin standard errors, which are robust to weak instruments. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6: The Electoral Response to Imported Growth (no early elections)

Dependent Variable: IDEOCH	[1]	[2]	[3]
All Elections			
GDP Growth (imported)	-0.0949***	-0.103***	-0.103**
	(0.00207)	(0.00167)	(0.0150)
Length party has been in power		0.0250***	0.0256***
		(4.61e-06)	(3.13e-06)
Inflation			-0.00669
			(0.103)
Observations	222	219	211
Number of Countries	58	58	56
First Stage F Statistic	7.69	7.76	4.28

Table 6: repeats Table 3 while removing elections that were called early. Early elections are defined to include those at least one 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 to 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. We use Anderson-Rubin standard errors, which are robust to weak instruments. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 7: Mitigating the Response to Imported Electoral Growth

Dependent variable: IDEOCH	[1]	[2]	[3]	[4]	[5]	[6]
	New Democracies	Established Democracies	Low Schooling	High Schooling	Controlled Media	Free Media
All Elections						
GDP Growth (imported)	-0.0641****	0.00114	-0.242**	-0.0441	-0.0881***	-0.156
	(0.000400)	(0.987)	(0.0305)	(0.119)	(0.00137)	(0.330)
Observations	82	175	158	158	150	131
Number of countries	36	42	56	45	60	44
First Stage F Statistic	2.41	6.07	0.60	13.35	1.86	0.68

Table 7: Runs our preferred specification (2SLS estimator, all elections, IDEOCH indicator, one-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. We use Anderson-Rubin standard errors, which are robust to weak instruments. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 8: Mitigating the Response to Imported Electoral Growth (with controls)

Dependent variable: IDEOCH	[1]	[2]	[3]	[4]	[5]	[6]
	New Democracies	Established Democracies	Low Schooling	High Schooling	Controlled Media	Free Media
All Elections						
GDP Growth (imported)	-0.0779*	-0.0166	-0.238**	-0.0612	-0.348***	-0.172
	(0.0890)	(0.813)	(0.0193)	(0.328)	(0.000725)	(0.271)
Length of time in power	0.0199***	0.0228	0.0308**	0.0171	0.00911	0.0221**
	(6.29e-05)	(0.105)	(0.0221)	(0.157)	(0.687)	(0.0434)
Inflation, consumer prices (annual %)	-0.00279*	0.00500	0.00119	-0.0127	-0.00301	0.0459
	(0.0603)	(0.431)	(0.899)	(0.571)	(0.885)	(0.417)
Observations	82	175	158	158	150	131
Number of countries	36	42	56	45	60	44
First Stage F Statistic	1.60	6.27	0.60	4.06	0.33	0.59

Table 8: 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 seem more vulnerable to attribution error. P-values in parentheses. We use Anderson-Rubin standard errors, which are robust to weak instruments. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix
Figure A1: The Distribution of Feedback Effects of Domestic Growth
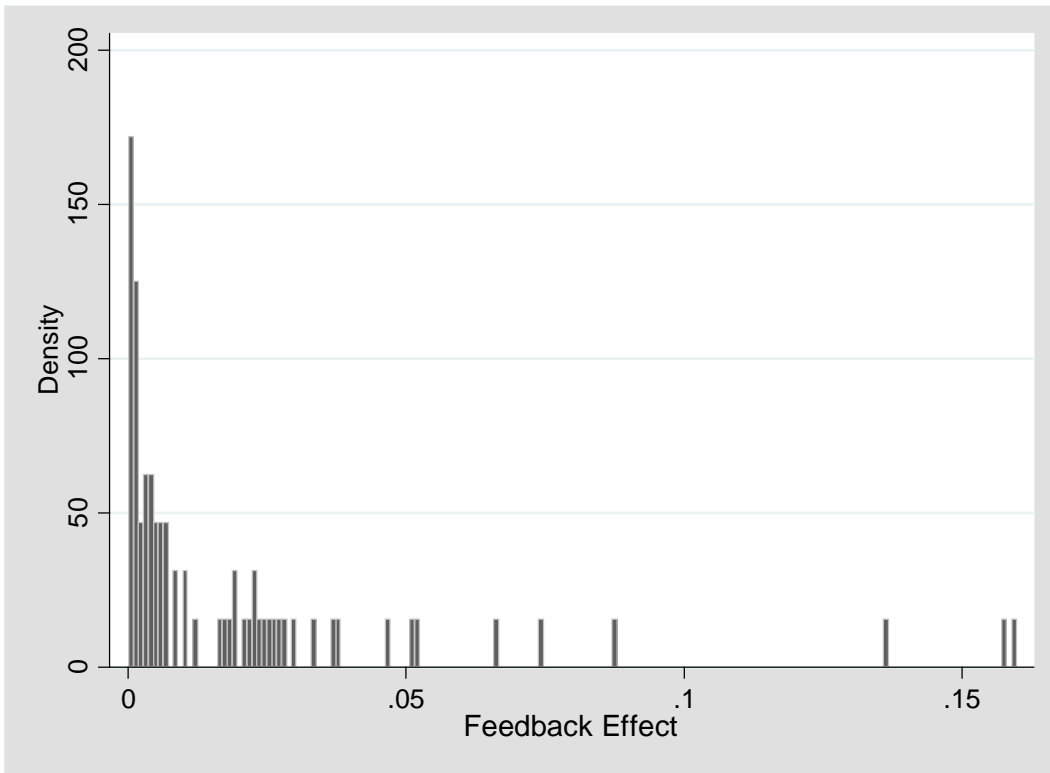
Figure A1: displays the histogram of the estimated feedback effect of a 1% increase in domestic GDP growth. A 1% shock to GDP in the domestic country will produce greater GDP in trade partners, which will then redound to further increases in domestic GDP. While most countries are too small or too closed to generate significant feedback from domestic shocks, nine countries in our 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

Histogram of Imported Growth
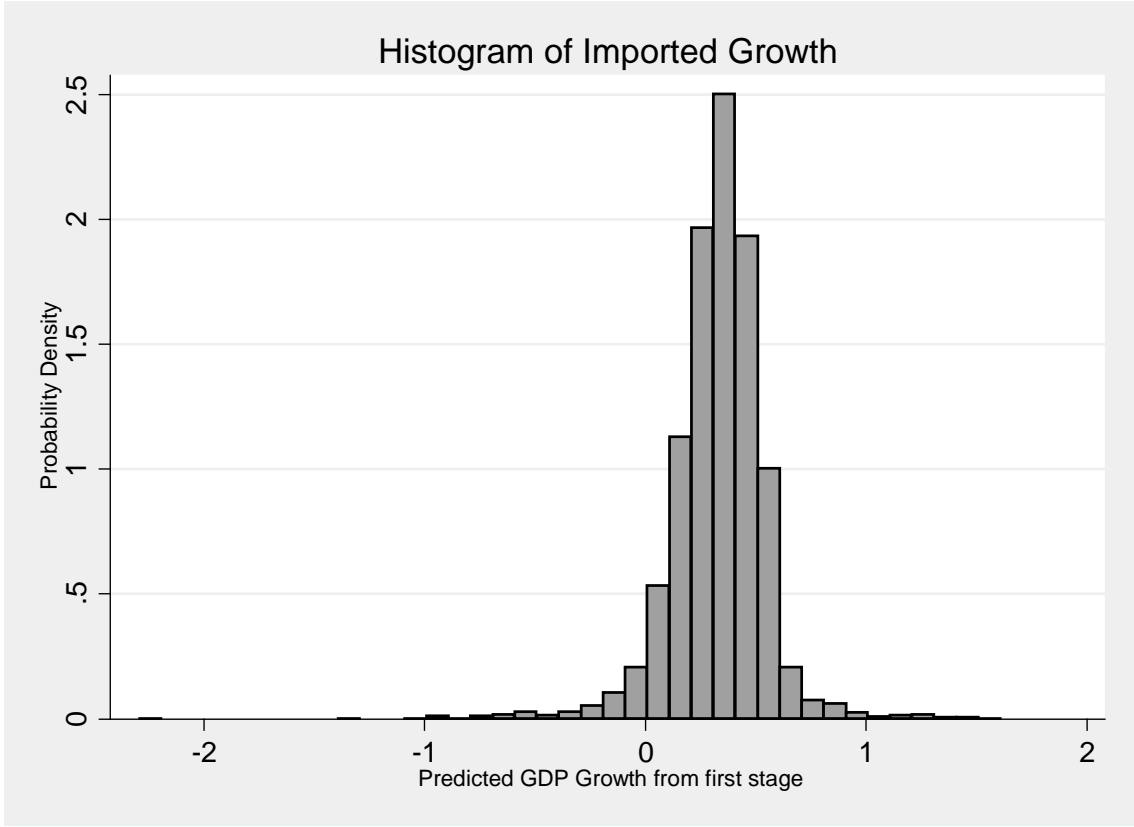
Table A1: The Electoral Response to Imported Growth (unemployment)

Dependent Variable: IDEOCH	[1]	[2]	[3]
All Elections			
Unemployment (imported)	0.0668	0.0694	0.0806
	(0.376)	(0.389)	(0.390)
Length party has been in power		0.0261***	0.0265***
		-0.00575	-0.00594
Inflation			0.00655
			-0.00963
Observations	204	204	204
Number of Countries	53	53	53
First Stage F Statistic	2.52	2.48	2.15

Table 9: 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<0.01, ** p<0.05, * p<0.1

Table A2: The Sample under Different Conditions

Country	Years in Sample				democratic age		years of schooling		media freedom	
	All Elections No Controls	Legislative Elections No Controls	All Elections w/ government surplus	Observations lost due to government surplus	new	established	high	low	high	low
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		6	1
Austria	7	7	6	1		7	2	4	3	3
Bahamas	4	4	2	2		4	4		4	
Barbados	3	3	3			3		3	3	
Belgium	5	5	5			5	5		5	
Belize	3	3	1	2		3		2	1	1
Bolivia	5	-	4	1	1	4		4	1	3
Brazil	4	-	3	1	2	2		4		4
C. Verde Is.	4	1	1	3		4		4		4
Canada	6	6	6			6	5		5	
Chile	4	-		4	4		1	2	1	2
Colombia	3	-	2	1		3		3	1	2
Costa Rica	5	-	4	1		5		5	5	
Croatia	4	2	4		3	1		4		4
Cyprus	3	-	2	1		3	2		2	
Czech Rep.	3	3	3		3		3			3
Denmark	6	6	5	1		6	6		6	
Dom. Rep.	6	-	5	1	1	5		5	1	4
Ecuador	3	-	2	1	1	2		2	1	1
El Salvador	4	-	4		2	2		3		3
Estonia	3	3	3		3		3			3
Germany	6	6	5	1		6	3	2	5	
Finland	5	5	5			5	3	2	5	
France	4	4	4			4	2	2	2	2
Ghana	2	-		2		2		1		1
Greece	6	6	5	1		6	1	4	1	4
Grenada	5	5		5		5	4		2	2
Guatemala	4	-	2	2	2	2		4		4
Honduras	5	-	5		1	4		4	1	3
Hungary	5	5	4	1	4	1	3	2		5
Iceland	5	5	5			5	2	3	5	
India	5	5	5			5		4	1	3
Ireland	4	4	4			4	4		3	1

Israel	7	5	7			7	6		1	5
Italy	6	6	5	1		6	1	4	1	4
Jamaica	4	4	2	2		4	1	3	4	
Japan	6	6	1	5		6	6		3	3
Latvia	4	4	3	1	3	1	4			4
Luxembourg	4	4	3	1		4	2	1	3	
Macedonia	3	3	2	1	3		2			1
Malawi	3	-		3		3		2		2
Malta	5	5	5			5	1	3	4	
Mexico	2	-	2		2			2		2
Moldova	4	3	3	1	4		2	1		3
Nepal	3	3	3		3			2		2
Netherlands	6	6	6			6	6		6	
New Zealand	7	7	5	2		7	6		6	
Nigeria	2	-		2	2			2		2
Norway	5	5	5			5	4		4	
Pakistan	2	2	1	1	2			2		2
Paraguay	4	-	4		4			3		3
Peru	2	-	2			2		2		2
Poland	2	-	2		2		2			2
Portugal	6	6	6			6		5	5	
South Korea	4	-	4		4		4		1	3
Romania	2	2	1	1	2		2			2
Senegal	2	-		2		2		2		2
Slovenia	5	5	4	1	5		4			3
Spain	5	5	5			5	2	2	3	1
Sri Lanka	2	-	2			2	1	1		2
St. Lucia	4	4		4		4	4		4	
Sweden	5	5	4	1		5	5		5	
Thailand	2	2	2			2		2		2
Trinidad-Tobago	6	6	3	3		6		6	1	5
Turkey	3	3	3			3		3		3
UK	4	4	4			4	1	3	3	1
USA	5	-	5			5	4		4	
Ukraine	2	-		2	2		2			2
Uruguay	4	-	4		4			3		3
Vanuatu	6	6		6		6		5	1	4