

**Open for Politics?:
Economic Globalization and Political Survival**

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Abstract:

Economic openness both generates economic benefits to countries and consumers and creates concentrated costs to certain firms or workers. In this paper we address the choice of governments to open up their economy based on the political logic of economic attribution. We argue that politicians could use “globalization” as a means to deflect blame for poor economic performance. In an original survey experiment in the United States fielded in April of 2014 we find little impact of globalization on credit claiming or blame avoidance. In a cross-national study of economic voting, we find some evidence that politicians in more open economies have a *lower* propensity to survive in office and larger *reductions* in seat shares relative to more closed economics. We conclude with some conjectures on how globalization affects attribution through the volatility of economic growth.

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1. Introduction

Economic openness, such as liberalization of international trade, has the potential to generate economic benefits to an economy, while concentrating the costs in a set of sectors, factors (such as low skilled labor), or regions. Much of the political science scholarship on the topic has incorporated these distributional consequences of economic openness into account when explaining the political economy of trade protection.

While the literature on the politics of trade policy is massive, a number of central themes have emerged. First, the concentrated “losers” and dispersed “winners” provide mobilization advantages for protectionist coalitions over pro-free trade interests. Second, political institutions, domestic and international, that shape the policy making process are essential to explaining trade policy.

While the power of protectionist interests varies across countries and over time, what is puzzling is the massive economic liberalization that has taken place across countries. Traditional trade barriers such as tariffs have become less of an impediment to trade, while non-tariffs barriers, still formidable across industries, have steadily declined. What could possibly explain this pattern? In the next section we outline this decline and some of the existing explanations for it, but our focus in this paper is on how electoral motivations have incentivized economic liberalization.

Our question has broader implications than simply the study of trade policy. As noted by Kayser (2007, 341) in a review of the existing literature on politics and globalization, “Very little of it addresses the effects of globalization on actual politics.” In this paper we directly examine how globalization, measured by openness to international trade, shapes domestic politics.

We argue that an important mechanism is how economic openness shapes the evaluations that voters make of their elected leaders in light of positive or negative economic performance. Building on existing work in political psychology, we argue that globalization can have two impacts on responsibility attribution. First, it can reduce “clarity of responsibility”, limiting the ability of voters to provide credit or assign blame during periods of economic expansions or recession. Clarity of responsibility theories have long shaped political science research on responsibility attribution and, more recently, work such as Hellwig (2001) has identified globalization as a means of further muddling responsibility attributions. In short, globalization makes it difficult for voters to reward or punish politicians for economic outcomes.

Our own theoretical contribution deviates from a straightforward clarity of responsibility theory. We argue that globalization can have an asymmetric impact on credit and blame attributions. Economic openness, under certain circumstances, can lead individuals to continue to attribute credit for good economic performance, while reducing blame for poor economic performance. We believe this asymmetry could explain the puzzle of why, despite the potential political costs, politicians choose economic openness.

We believe our theory could also explain the partial economic openness of many economies. In many cases, countries sign trade agreements, open up their economies to the ups and downs of global market forces, while at the same time sheltering certain industries from economic competition. We argue that as long as voters have the perception that market forces shape economic outcomes, politicians can continue to selectively shield industries. Thus politicians can reap the rewards of blame avoidance while at the same time playing the game of special interest group politics.

Our empirical results do not support our theory. First, we consider a survey experiment conducted in the United States in April 2014 in which we vary frames about economic growth as “high” or “low” and the source of the growth as “domestic” or “global”. Our survey experiments indicate that while voters are much more likely to blame politicians for poor growth and offer little credit during periods of high growth, this asymmetric response is not affected by global frames. Globalization has little impact on mitigating the punishment from poor economic performance.

Our findings on the asymmetry with which voters evaluate politicians during good and bad economic times is both interesting and important. Consistent with existing work on the topic, we find that voters blame politicians for poor economic performance and give politicians very little credit for positive economic performance. Thus, while globalization does not impact responsibility attribution, the costs and benefits of globalization may have very different effects on politicians. In an observational analysis of 33 countries from 1960-2007 we find voters in open economies are more likely to punish incumbent politicians (and parties) by removing them from office and reducing their seat shares.

These findings are inconsistent with our original theory about the blame-reducing effect of globalization. Our conjecture is that economic openness, with all of its benefits, also can impose serious costs on an economy. These costs, and not the benefits, have important implications for political elites.

2. Economic Liberalization Despite Interest Group Contestation

While select sectors remain relatively closed to trade and investment, economic liberalization has dramatically reduced tariffs across countries, led to reduced restrictions on economic development, and started to chip away at many non-tariffs barriers. How open

most economies are to the rest of the world is an interesting and important debate, but the general trend towards liberalization is difficult to dispute. Politicians have chosen to open up their economies to global market forces. Why?

The different explanations for economic liberalization all add a piece of the puzzle. International institutions, such as the World Trade Organization, may have been essential in promoting trade liberalization across countries.¹ Domestic institutions, such as democracy, can enhance the ability of states to cooperate in the formation of mutually beneficial trade agreements.² Other domestic institutions, ranging from delegation of trade policy to the executive³ to the inclusion of reciprocity into trade agreements all have been linked to trade liberalization.⁴ Electoral institutions can also shape both the amount of type of trade protection.⁵ Finally, studies of globalization preferences include studies on how trade views are affected by occupation⁶, consumer prices⁷, exposure to economic ideas⁸, economic insecurity⁹, and views towards out groups.¹⁰

This is just a quick brush at the different theories explaining trade liberalization. What is missing from many of these accounts is a direct test on how economic openness

¹ The classic study on the effect of the WTO on trade is Rose (2004). See Goldstein, Rivers and Tomz (2007) for a reevaluation.

² Mansfield, Milner, and Rosendorff. (2002). See also Milner and Kubota (2005) for a study of democracy and trade liberalization.

³ For example, see Lohmann and O'Halloran. (1994)

⁴ See Gilligan (1997) for an exploration of how reciprocity mobilized pro-trade export interests. Hiscox (1999) makes a strong case that reciprocity wasn't central to the land market Reciprocal Trade Agreements Act (RTAA).

⁵ See McGillivray (1997, 2004) for work on how electoral institutions shape the geographic targeting of trade protection. See Kono (2006) for how democratic institutions incentivize the use of more opaque forms of trade protection.

⁶ For work on trade policy preferences, see Scheve and Slaughter (2001); Mayda and Rodrik (2005). For foreign direct investment preferences, see Pandya (2010).

⁷ Baker (2003).

⁸ Hainmuller and Hiscox (2006).

⁹ Scheve and Slaughter (2004).

¹⁰ Mansfield and Mutz (2009, 2013).

shapes the outcomes of political leaders. Are politicians in open economies more or less likely to win reelection and live long careers as government incumbents?

We believe an exploration of how globalization affects political survival is an important empirical contribution, although it only further opens up interesting questions on the causal mechanism linking openness and survival. In the next section we develop a theory on how economic openness allows incumbents to avoid blame for bad economic outcomes and take credit for good ones.

3. Globalization and Responsibility Attribution

The ability of voters to sanction or reward politicians through elections is at the heart of democratic governance. Political scientists have documented that economic performance—either growth, inflation, or unemployment at the national level, or personal financial situation at the individual or household level—shape voting.¹¹ Existing work in political science has provided mixed evidence on how globalization affects responsibility attribution. Building on the influential work of Powell and Whitten (1993), scholars have argued that globalization can limit clarity of responsibility, reducing the ability of voters to assign credit or blame to elected officials for economic outcomes. Hellwig (2001) documented in a cross-national study of voting intentions that greater exposure to trade reduces the probability that economic factors shape an individual's vote choice, although Fernández-Albertós (2006) finds no such relationship. Hellwig and Samuels (2007) find that economic openness weakens the relationship between economic performance and vote

¹¹ See Lewis-Beck and Stegmaier (2000)

choice in 75 countries from 1975 to 2002.¹² Our first hypothesis is a straightforward application of the clarity of responsibility framework.

Hypothesis 1: Economic liberalization limits both the credit and blame for economic recovery and recessions.

This hypothesis, while grounded in the literature, cannot in itself explain why governments choose to liberalize, since doing so muddles both the punishment for poor performance and the benefits from good economic performance. There are a number of plausible explanations, ranging from governments being risk averse, some leaders or parties preferring to compete on non-economic issues, or international organizations pushing for economic liberalization.

While these extensions can examine some of the moves towards economic openness across the world, we believe a more satisfying theory would take into account the political incentives for liberalization across countries. Why do governments of many different stripes choose economic liberalization?

¹² Hellwig (2008) argues that this leads voters to increase their weight to non-economic factors when making voting decisions. In an original survey experiment in the United States, Hellwig et al (2008) find that the majority of Americans believe that the government still can affect economic policy outcomes, although this does vary by partisanship and level of knowledge. While these clarity of responsibility arguments are compelling, the empirical literature on the topic is mixed. Work such as Kayser (2009) documents how globalization, by affecting domestic business cycles, has led to co-variation in voting intentions across countries.

We argue that the way individuals view the relationship between economic performance, globalization, and government performance is asymmetrical. In short, we argue that governments can have the best of both worlds. They can take credit for strong economic performance and blame global market forces for poor economic performance.

Our theory has similarities to Carlin et al's (Forthcoming) contribution on political scandals, economic performance, and government approval in Latin America. In their contribution they argue that the effect of political scandals is conditional on economic performance, where voters are willing to trade poor political behavior off for strong economic performance. In periods of strong economic performance, voters largely ignore political scandals, while during periods of poor economic performance, they punish incumbent governments. They find support for their conditional theory on support for Latin American presidents in 18 Latin American countries.

Unlike the work on scandals, our theory does not focus on voters making an implicit tradeoff between scandals and economic performance.¹³ We argue that individuals also process information asymmetrically, blaming international markets for poor economic performance while attributing strong economic performance to incumbent politicians

The ability of politicians to avoid blame for poor performance follows from the clarity of responsibility hypothesis and has been empirically tested by Alcañiz and Hellwig (2011). More novel is our perspective on how strong economic performance is likely to be attributed to domestic governments, as opposed to market forces.

Hypothesis 2 (Blame Avoidance): Economic liberalization has no impact on credit during periods of economic recovery and reduces blame during recessions.

¹³ See Muñoz, Anduiza and Gallego (2012) for experimental work on this tradeoff.

4. Survey Experiment

To test our hypotheses, we designed an original survey experiment fielded in the United States. Our first step was to pre-register our hypotheses, research design and analysis plan at Experiments in Governance and Politics (EGAP).¹⁴ Design registration promotes both transparency in the research process and pre-commitment of theory and analysis, limiting the ability of research to “fish” or “mine” the data, or develop theory ex post.¹⁵

We fielded our online survey to 2,000 respondents in the April 2014 modules of the The American Panel Survey (TAPS) at Washington University in St. Louis. TAPS is a five-year panel of 2,000 respondents administered by KnowledgeNetworks which uses individual demographic data and residential addresses to build a nationally representative sample on observable characteristics.¹⁶ While the use of online, opt-in surveys can lead to concerns of unobserved factors leading to samples that are not representative of the national population, a recent comparison of online survey verses other forms of survey (mail-out, telephone, etc) finds little differences based on survey mode.¹⁷ More importantly for our study, we harness the power of randomization to achieve balance between treatment and control groups.

Our research design involves fielding a small number of questions in a survey experiment that utilizes block randomization with two treatment conditions: the state of the economy and whether or not we prime respondents on domestic causes of economic performance or global causes.

Our first question randomizes the current status of the economy (high or low growth) and whether or not we highlight domestic or global market forces. This question

¹⁴ <http://egap.org/design-registration/registered-designs/>

¹⁵ See Humphreys et al (2013).

¹⁶ Technical details on the TAPS survey can be found online at <http://taps.wustl.edu/>.

¹⁷ See Ansolabehere and Schaffner (Forthcoming).

focuses on retrospective evaluations, asking respondents about credit or blame for previous economic growth. We have a total of four treatments and one control group. Our control question is as follows:

Question 1 (Control)

How much blame or credit do you place on policy makers for US economic growth in past decades?

1. A great deal of blame or credit
2. Some blame or credit
3. Very little blame or credit
4. No blame or credit
5. Don't know

For our experimental manipulations we varied whether or not we highlighted only domestic factors affecting growth or if we included global market factors. Our second manipulation is our variation on whether economic growth was “fast” or “slow” compared to previous growth. For example, we present questions 1a and 1b, where both groups are treated with “fast” growth yet we vary whether or not we frame global forces as affecting economic growth. Both questions are exactly the same with the exception of the treatments. We italicize the treatments here for the sake of illustration.

Question 1a: Fast Growth and Domestic Treatment

Economic growth can be affected by government policy and the decisions of companies. Some experts have noted that over the past decades US economic growth has been relatively *fast compared to the US historical average*. How much credit do you give government policy makers for this economic growth.

1. A great deal of credit
2. Some credit
3. Very little credit
4. No credit
5. Don't know

Question 1b: Fast Growth and International Treatment

Economic growth can be affected by government policy, the decisions of companies and *global market forces*. Some experts have noted that over the past decades US economic growth has been relatively *fast compared to the US historical average*. How much credit do you give policy makers for this economic growth.

1. A great deal of credit
2. Some credit
3. Very little credit
4. No credit
5. Don't know

We also have two additional treatments of “slow growth” and either domestic or international treatments. We provide text for the full questions in the appendix.

In Table 1 we present the survey weighted responses and the 90% confidence intervals by treatment. For the sake of presentation we coded responses where politicians were given “A great deal” of credit or blame as “High” blame and “Low” blame otherwise. Alternative coding, such as dropping “don't know” responses, yield the same results.

The interesting comparison for our purposes is between the “domestic” and “global” treatments. When economic growth is “fast” only 3% of respondents gave the government high levels of credit. This estimate was almost identical for the “domestic” and “global” treatment. We observe a similar pattern for slow growth. 48% of respondents indicated the highest levels of blame for slow growth in the “domestic” treatment, which is almost the same as the 50% of respondents in the “global” treatment.

Table 1: Retrospective Evaluations

	Domestic Fast	Domestic Slow	Global Fast	Global Slow	Control	Total
High credit	0.03 [0.01, 0.05]	0.48 [0.39, 0.57]	0.03 [0.01, 0.05]	0.50 [0.41, 0.60]	0.45 [0.36, 0.53]	0.28 [0.25, 0.32]
Low credit	0.97 [0.95, 0.99]	0.52 [0.43, 0.61]	0.97 [0.95, 0.99]	0.50 [0.41, 0.59]	0.55 [0.47, 0.64]	0.72 [0.68, 0.75]

Note: Survey weighted estimates with 90% confidence levels in parenthesis.

While our globalization treatments had no impact on blame or credit, the asymmetry between credit and blame is interesting. This blame could be an artifact of the recent financial crisis. Luckily we also included a question on prospective economic evaluations. Our block randomization assured us that respondents that were treated with a “high”

growth “domestic” treatment, for example, are exposed to the same treatment for all of our questions.

Our prospective evaluation question is almost identical to our first question with only the change in the focus on evaluation of growth in 2016. Our question for the fast growth, domestic treatment is as follows:

Question 2a: Fast Growth and Domestic Treatment

Economic growth can be affected by government policy and the decisions of companies. If the US economy is in recession and is growing at a slow rate in 2016, how much blame would you place on policy makers for this economic growth.

1. A great deal of blame
2. Some blame
3. Very little blame
4. No blame
5. Don’t know

In Table 2 we present the results for the prospective evaluations. Our results are unchanged. Contrary to our expectations, there is no meaningful difference between the domestic and global treatments, and we still observe the same asymmetry between substantial blame for poor economic performance and very minimal levels of credit for good economic performance.

Table 2: Prospective Evaluations

	Domestic High	Domestic Low	Global High	Global Low	Control	Total
High	0.03 [0.2, 0.05]	0.40 [0.32,0.49]	0.04 [0.02, 0.07]	0.48 [0.39, 0.58]	0.40 [0.30, 0.50]	0.23 [0.20, 0.28]
Low	0.97 [0.95, 0.98]	0.60 [0.51, 0.68]	0.96 [0.93, 0.98]	0.52 [0.42, 0.61]	0.60 [0.50, 0.40]	0.77 [0.72, 0.80]

Note: Survey weighted estimates with 90% confidence levels in parenthesis.

In our final test, we examined prospective voting intentions for the Democratic presidential candidate in 2016 using the same treatments. We present the survey weighted results and the 90% confidence intervals in Table 3. We find a pattern similar to the one in

Tables 1 and 2, although the confidence intervals between the “high growth” and “low growth” treatments overlap.

Table 3: Voting Intentions

	Domestic High	Domestic Low	Global High	Global Low	Control	Total
Very likely	0.24 [0.17,0.31]	0.20 [0.15,0.27]	0.20 [0.13,0.28]	0.13 [0.09,0.20]	0.25 [0.18,0.32]	0.20 [0.17,0.23]
Likely	0.14 [0.09,0.21]	0.08 [0.05, 0.13]	0.12 [0.07,0.18]	0.17 [0.11,0.24]	0.12 [0.08,0.17]	0.13 [0.10,0.15]
Undecided	0.19 [0.13,0.26]	0.21 [0.15,0.28]	0.38 [0.28,.49]	0.26 [0.18,0.36]	0.20 [0.14,0.29]	0.25 [0.22,0.29]
Unlikely	0.10 [0.06,0.16]	0.11 [0.06,0.20]	0.07 [0.04,0.11]	0.08 [0.04,0.14]	0.05 [0.03,0.10]	0.08 [0.06,0.10]
Very unlikely	0.33 [0.26,0.42]	0.40 [0.32,0.49]	0.25 [0.17,0.33]	0.36 [0.28,0.45]	0.38 [0.30,0.47]	0.34 [0.30,0.38]

Note: Survey weighted estimates with 90% confidence levels in parenthesis.

Our three survey experiments provide no evidence that globalization has any impact on evaluations or voting intentions, although we found an asymmetry between respondent evaluations of politicians in good and bad economic times.

We are careful in our interpretation of these results. In a manipulation check for our first two questions, we asked respondents to recount whether their treatment was “high growth” or “low growth”. While 78% of respondents correctly identified “low growth” when they were exposed to the “low growth” treatment, only 39% correctly identified “high growth” when they were exposed to the “high growth” treatment. Put another way, the majority of our respondents believed they were exposed to the low growth treatment.

We can only speculate why respondents were much more likely to believe they read “low growth” in our question, but one plausible explanation is that most respondents truly believed that the U.S. has experienced a period of low growth. Yet what is striking is that even given our weak “high growth” treatment, we find large differences in blame and credit.

5. Cross-national evidence

Our survey experiment highlights the limited ability of globalization to increase credit or reduce blame for economic performance. For the observational part of our analysis we gathered data on national elections in 33 European countries. The dataset covers 269 elections from roughly 1960 to 2007. Our key explanatory variable is *Trade Openness*, which is a proxy for the extent of exposure of a country to globalization. Trade openness is driven, among other factors, by policy decisions made by incumbents that may choose openness precisely because they expect that it will increase the length of their tenure in government. Alternatively, maybe trade openness and government survival are driven by unidentified factors; in this case, estimating the effect of trade openness on government survival in a regression would lead to a biased estimate of the effect of globalization on political fortunes. In short, we do not expect trade openness to be an exogenous predictor of political survival in an observational study.

Consequently, we have opted for an estimation strategy based on Frankel and Romer (1999), where we use the amount of trade that can be attributed to a country's geographical characteristics in a gravity model as an instrument for actual levels of trade. Put simply, the gravity model states that the level of trade between any two countries is *decreasing* in the distance between the two countries and *increasing* in the size of their economies. Thus countries that are closer to each other, such as the Netherlands and Belgium, should trade more, and countries that are economically large, such as Spain and Italy, should also trade more. Using a handful of geographic and demographic variables we constructed a dyadic-level instrumental variable based on the gravity model of trade. We then added up dyadic-

levels of predicted trade to obtain our instrument for total trade levels for each country-year.¹⁸ The appendix includes details on the construction of this particular variable.

Our key outcome variables are measures of incumbent electoral success. These include a dummy variable indicating whether an incumbent party is removed from the governing coalition following an election (*Government Death*), a dummy variable indicating whether the incumbent party loses control of the prime ministry (*PM Death*), and the incumbent's total vote share (*Vote Share*). The two dummy indicators come from Schleiter and Tavits (2014); we code these to indicate whether an incumbent party wins (=0) or loses (=1) the current election.¹⁹ Data on vote shares were taken from the Parties and Elections in Europe online dataset.²⁰ We determined the incumbent party to be the party of the prime minister at the start of each election. When the prime minister did not have a party affiliation, we chose the main party in government (taken to be the first party listed in the governing coalition) as the incumbent party.²¹ We recorded an incumbent's present and previous vote shares as a percentage of total votes cast.

Since we can make a strong theoretical argument for exogeneity of our instrument for trade, we estimate sparse models that only include controls for obvious confounders of the trade effect. Frankel and Romer (1999) convincingly argue that a country's population and area, which are variables used in the construction of the trade instrument, are

¹⁸ Both the dyadic and monadic estimates are scaled by a country's nominal GDP.

¹⁹ For example, in Austria 1966 the OVP won the election and became the only party in government. In 1970 they lost to the SPO. Tavits's variables, *surv_pmpty_pm* and *surv_pmpty_gov*, are coded as 0 for 1966 but 1 for 1970, indicating that the incumbent party that took office in 1966 did not win its next election.

²⁰ <http://www.parties-and-elections.eu/index.html>.

²¹ In a handful of cases neither of these criteria were available and so we simply recorded the data as missing. In other cases (such as the first post-communist elections in Eastern European countries) past election data was obviously untrustworthy (since the previous election was usually sometime in the 1940s). Here too we simply coded the past vote share as missing.

confounders of the trade effect on growth, and should therefore be included in the regression model to comply with the exclusion restriction assumption. We also include these variables in our models. Data on area comes from the *Centre d'Etudes Prospectives et d'Informations Internationales* (CEPII) and is measured in square kilometers. Population is measured in thousands and comes from the Correlates of War project.²² In models where the outcome variable is the incumbent's total present vote share we also control for the incumbent's vote share in the previous election. This is meant to account for the fact that some parties in certain countries might just enjoy higher vote shares than others, perhaps due to the presence of more parties. Since we are really interested in the electoral consequences of integration rather than in predicting each party's *actual* vote share, we omit indicators typically employed to predict vote shares. Summary statistics for all of these variables can be seen in Table 4.

Table 4. Summary statistics for observational data

Statistic	N	Mean	SD	Min	Max
Area	269	429,006.10	2,055,341	316	17,075,400
Population	269	19,153.21	26,090.45	185	148,146
Trade	171	65.109	31.487	16.767	5.313
Predicted Trade	269	4.009	0.469	2.593	24.7
Δ Vote Share	250	-3.272	9.114	-56.1	66.3
Vote Share _t	259	31.103	11.964	1.2	66.3
Vote Share _{t-1}	253	34.482	10.777	2.5	1
PM Death	204	0.475	0.501	0	1
Gov Death	204	0.402	0.492	0	1

Before looking at the results from our instrumental variable approach, we note that there is no statistical association between our measures of political survival and *actual* trade-to-GDP ratios. To measure actual trade we use *openk* from the Penn World Tables, which

²² For more details on the role these two variables played in the construction of our instrument, please see the appendix.

counts each country-year's total trade relative to GDP in base 1996 US dollars. We then estimated a logit model of *Government Death* using logged values of *openk* as a predictor. The results can be seen in Model (1) in Table 5; the estimated coefficient on *openk* is positive but statistically insignificant. We estimated two additional models to see if this null finding was dependent on our choice of outcome variable. As the rest of Table 5 shows, the effect parameter of *Trade Openness* remains statistically insignificant regardless of outcome variable.

Table 5. Estimates of the effect of trade openness on government survival, observational data (143 elections in 29 countries, 1975-2004)

Outcome:	Govt Death	PM Death	Vote Share _t
Specification:	logistic (1)	logistic (2)	linear (3)
Constant	-0.627 (1.491)	-0.811 (1.483)	12.917 (7.856)
log (Trade)	0.095 (0.364)	0.196 (0.362)	-1.094 (1.702)
Vote Share _{t-1}			0.623*** (0.075)
N	143	143	153
Adjusted R ²			0.325
log-Lik	-98.073	-98.97	
Residual SE			9.705
F Statistic			37.562***

Note: *p<0.1; **p<0.05; ***p<0.01

Contrast these null results with the findings in Table 6. Models (4) through (6) in this table are equivalent to models (1) through (3) in Table 5. The main difference is that we show 2SLS estimates of the effect of *Trade Openness* based on the instrumental variable for trade. As Model (4) makes clear, regressing *Government Death* on *Instrumented Trade Openness* and our control variables results in a positive coefficient for *Instrumented Trade Openness* that falls short of being statistically significant.

Table 6. 2SLS estimates of the effect of trade openness on government survival

	<i>Govt Death</i> (4)	<i>PM Death</i> (5)	<i>Vote Share</i> (6)
<i>Trade openness (instrumented)</i>	0.229 (0.201)	0.355* (0.198)	-18.16*** (4.916)
<i>Previous vote share</i>			0.498*** (0.091)
<i>Area (log)</i>	0.102* (0.061)	0.164*** (0.06)	-4.281*** (1.401)
<i>Population (log)</i>	-0.021 (0.055)	-0.090* (0.054)	-1.368 (1.184)
Constant	-1.497 (1.525)	-2.05 (1.499)	149.5*** (37.65)
log-Lik	-102.3	-99.86	
Adjusted R ²			0.127
Residual SE			11.03
First stage			
<i>Trade Instrument</i>	4.188*** (0.509)	4.188*** (0.509)	3.706*** (0.51)
<i>Previous vote share</i>			-0.003 (0.002)
<i>Area (log)</i>	-0.085*** (0.032)	-0.085*** (0.032)	-0.098*** (0.032)
<i>Population (log)</i>	-0.009 (0.033)	-0.009 (0.033)	-0.013 (0.033)
Constant	4.457*** (0.435)	4.457*** (0.435)	4.827*** (0.446)
N	143	143	153
Adjusted R ²	0.581	0.581	0.571
Residual SE	0.301	0.301	0.309
F Statistic	66.550***	66.550***	51.600***

Note: *p<0.1; **p<0.05; ***p<0.01

This is not the case in Models (5) and (6), where the estimated coefficient on *Trade Openness* suggests a negative effect of the level of integration in a country on the survival of incumbent politicians. This general effect obtains in the last two models: in each case, higher levels of integration appear to worsen an incumbent's electoral prospects. To gauge the magnitude of the effect of openness on *Vote Share* consider that the sample mean value of this variable is 31.1 (with standard deviation 11.96) and recall that we measure *Trade Openness*

in the log scale. When all other variables are held at their median sample values, a standard deviation increase in *Trade Openness* suggests a rather large drop of 8.78 percentage points in *Vote Share*, from 32.43 (95% CI: 30.24–34.42) to 23.65 (95% CI: 19.25–27.84).

Table 7. 2SLS estimates of the effect of trade openness on government survival, including a real GDP growth as an additional predictor

	<i>Govt Death</i> (7)	<i>PM Death</i> (8)	<i>Vote Share</i> (9)
<i>Trade openness (instrumented)</i>	0.235	0.365*	-18.830***
	0.204	0.2	4.837
<i>Previous vote share</i>			0.521***
			0.088
<i>Real GDP growth</i>	-0.01	-0.017	1.068***
	0.016	0.016	0.316
<i>Area (log)</i>	0.105*	0.169***	-4.601***
	0.062	0.061	1.383
<i>Population (log)</i>	-0.026	-0.097*	-0.948
	0.055	0.054	1.149
Constant	-1.491	-2.04	149.000***
	1.528	1.499	36.64
log-Lik	-102.2	-99.58	
Adjusted R ²			0.172
Residual SE			10.75
First Stage			
<i>Trade instrument</i>	4.152***	4.152***	3.671***
	0.507	0.507	0.507
<i>Previous vote share</i>			-0.003
			0.002
<i>Real GDP growth</i>	0.014	0.014	0.015*
	0.009	0.009	0.009
<i>Area (log)</i>	-0.088***	-0.088***	-0.102***
	0.032	0.032	0.031
<i>Population (log)</i>	-0.003	-0.003	-0.007
	0.033	0.033	0.032
Constant	4.409***	4.409***	4.773***
	0.434	0.434	0.444
N	143	143	153
Adjusted R ²	0.585	0.585	0.577
Residual SE	0.299	0.299	0.307
F Statistic	50.940***	50.940***	42.450***

Our main finding from the survey experiment, which was not part of our original hypothesis, is the asymmetric impact of positive and negative growth on politicians. To speak to this effect, we summarize in Table 7 three additional models that gauge whether the rate of economic growth is associated with incumbent survival. In line with the exogenous character of *instrumented Trade Openness*, we verify that the addition of *real GDP growth* does not alter our estimates of the effects of openness; even if part of the causal effect of trade on incumbent survival were to occur through economic growth, our inclusion of *real GDP growth* in the first-stage regression model ensures that the exclusion restriction holds. Needless to say, we cannot make a strong causal interpretation of the coefficient for *real GDP growth*, since we lack an instrument for this variable that would allow us to defend an assumption of exogenous assignment.

Even then, we comment on the main result of this exercise. Though there is no statistically significant association between economic growth and the dummy indicators of government and prime minister survival (Models 7 and 8), Model 9 suggests that incumbent governments that manage to deliver higher rates of economic growth obtain a vote share bonus. The size of the estimated coefficient suggests that each additional point in a country's growth rate increases the estimated vote share of the incumbent government by about an extra percentage point. This estimate is not out of line with reasonable expectations of the effect of economic growth on electoral success.

Finally, we estimated a model (not shown) of *Vote Share* that included an interaction between economic growth and openness, under the expectation that rates of economic growth might increase a government's electoral returns only under conditions of relative autarky from international trade. We found that neither growth nor the interaction between growth and integration were statistically significant predictors of *Vote Share*.

6. Discussion

The results of our survey experiment are not consistent with the theory presented in Section 3. We find no direct impact of globalization on responsibility attribution. We can speculate on how these findings could be an artifact of a weak treatment or on how voters within the United States are already conditioned to think of economic performance as being driven by domestic factors. Our key finding is the asymmetric impact of economic growth on responsibility attribution and prospective voting. Politicians only receive a very small boost in credit with high levels of economic growth while low levels of growth are devastating for their future election prospects.

In our cross-national study of electoral outcomes we find some evidence that globalization *reduces* political survival for parties and executives. Our evidence is strongest for our measure of exogenous trade rather than actual trade flows.

Our conjecture is that these two findings are consistent with research arguing that globalization increases the volatility of economic performance (see Di Giovanni and Levchenko 2009). The major impact of globalization on politics is through the real economy. But voters respond to economic performance asymmetrically. Periods of high economic growth provide few benefits to politicians while low growth dramatically decreases incumbent survival. If globalization indeed increases the volatility of economic growth, then we could link globalization with a lower propensity of politicians to stay in power.

7. Conclusion

In this paper we outlined a theory on how globalization can affect the clarity of responsibility for economic performance. Using an original survey experiment we find no support for this theory. We find that globalization frames have no discernible impact on how voters attribute responsibility to politicians. We complement our survey experiment

with observational data on elections from 33 countries from 1960-2010. In contrast to our findings from our survey experiment, we find evidence that globalization *reduces* the changes of incumbents staying in office. These two findings, taken together, suggest that the primary mechanism linking globalization to elections is not through a straightforward link of responsibility attribution. As we noted in the discussion, the causal mechanism linking trade with a lower propensity to survive in office is through the real economy.

As we noted in section 4, we pre-registered our hypotheses and the design of our survey experiment. While our observational data analysis points to a negative effect of globalization on political survival, we are careful in our interpretation of these results since this finding does not correspond to the theory that we developed, and for which we designed and pre-registered our survey experiment and cross-national analysis. But we hope our commitment to research transparency helps give us some direction on where to go with this research agenda in the future.

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Appendix A: The American Panel Survey (TAPS)

Notes:

Randomize into five equally sized groups of respondents. For the treatment groups (Blocks 1-4), each respondent will receive five questions per block. For the control group (Block 5) respondents will receive 3 questions. Within each question, randomize the order of answers for each questions. “Don’t know” answers should be fixed as the last answer for each question.

BLOCK 1: DOMESTIC/High Growth

Question 1: Retrospective Credit and Blame

Economic growth can be affected by government policy and the decisions of companies. Some experts have noted that over the past decades US economic growth has been relatively fast compared to the US historical average. How much credit do you give government policy makers for this economic growth.

1. A great deal of credit
2. Some credit
3. Very little credit
4. No credit
5. Don't know

[Reverse order answers 1-4]

Question 2: Credit and Blame Manipulation Check

In the previous question we asked you about US economic growth. According to this question, was growth relatively slow or relatively fast compared to the historical average?

1. Relatively fast
2. Relatively slow
3. Don't remember

[Reverse order 1-2]

Question 3: Prospective Credit and Blame

Economic growth can be affected by government policy and the decisions of companies. If the US economy has recovered and is growing at a fast rate in 2016, how much credit do you give policy makers for this economic growth.

1. A great deal of credit
2. Some credit
3. Very little credit
4. No credit
5. Don't know

[Randomly reverse order 1-4]

Question 4: Prospective Voting Intentions

Economic growth can be affected by government policy and the decisions of companies. If the US economy has recovered and is growing at a fast rate in 2016, how likely are you to vote for the Democratic Presidential candidate?

1. Very likely
2. Likely
3. Undecided
5. Unlikely
5. Very Unlikely

[Reverse order 1-5]

Question 5: Voting Intention Manipulation Check

In the previous two questions we asked you about future US economic growth. According to this question, was the economy in recovery (high growth) or in recession (low growth)?

1. Recovery (High Growth)
2. Recession (Low Growth)
3. Don't remember

[Reverse order 1-2]

BLOCK 2: DOMESTIC/Low Growth

Question 1: Retrospective Credit and Blame

Economic growth can be affected by government policy and the decisions of companies. Some experts have noted that over the past decades US economic growth has been relatively slow compared to the US historical average. How much blame do you place on policy makers for this economic growth.

1. A great deal of blame
2. Some blame
3. Very little blame
4. No blame
5. Don't know

[Reverse order 1-4]

Question 2: Credit and Blame Manipulation Check

In the previous question we asked you about US economic growth. According to this question, was growth relatively slow or relatively fast compared to the historical average?

1. Relatively fast
2. Relatively slow
3. Don't remember

[Reverse order 1-2]

Question 3: Prospective Credit and Blame

Economic growth can be affected by government policy and the decisions of companies. If the US economy is in recession and is growing at a slow rate in 2016, how much blame would you place on policy makers for this economic growth.

1. A great deal of blame
2. Some blame
3. Very little blame
4. No blame
5. Don't know

[Reverse order 1-4]

Question 4: Prospective Voting Intentions

Economic growth can be affected by government policy and the decisions of companies. If the US economy is in recession and is growing at a slow rate in 2016, how likely are you to vote for the Democratic Presidential candidate?

1. Very likely
2. Likely
3. Undecided
5. Unlikely
5. Very Unlikely

[Reverse order 1-5]

Question 5: Voting Intention Manipulation Check

In the previous two questions we asked you about future US economic growth. According to this question, was the economy in recovery (high growth) or in recession (low growth)?

1. Recovery (High Growth)
2. Recession (Low Growth)
3. Don't remember

[Reverse order 1-2]

BLOCK 3: International/High Growth

Question 1: Retrospective Credit and Blame

Economic growth can be affected by government policy, the decisions of companies and global market forces. Some experts have noted that over the past decades US economic growth has been relatively fast compared to the US historical average. How much credit do you give policy makers for this economic growth.

1. A great deal of credit
2. Some credit
3. Very little credit
4. No credit
5. Don't know

[Reverse order answers 1-4]

Question 2: Credit and Blame Manipulation Check

In the previous question we asked you about US economic growth. According to this question, was growth relatively slow or relatively fast compared to the historical average?

1. Relatively fast
2. Relatively slow
3. Don't remember

[Reverse order 1-2]

Question 3: Prospective Credit and Blame

Economic growth can be affected by government policy and the decisions of companies and global market forces. If the US economy has recovered and is growing at a fast rate in 2016, how much credit would you give to policy makers for this economic growth?

1. A great deal of credit
2. Some credit
3. Very little credit
4. No credit
5. Don't know

[Reverse order 1-4]

Question 4: Prospective Voting Intentions

Economic growth can be affected by government policy, the decisions of companies and global market forces. If the US economy has recovered and is growing at a fast rate in 2016, how likely are you to vote for the Democratic Presidential candidate?

1. Very likely
2. Likely
3. Undecided
5. Unlikely
5. Very Unlikely

[Reverse order 1-5]

Question 5: Voting Intention Manipulation Check

In the previous question we asked you about future US economic growth. According to this question, was the economy in recovery (high growth) or in recession (low growth)?

1. Recovery (High Growth)
2. Recession (Low Growth)
3. Don't remember

[Reverse order 1-2]

BLOCK 4: International/Low Growth

Question 1: Retrospective Credit and Blame

Economic growth can be affected by government policy, the decisions of companies and global market forces. Some experts have noted that over the past decades US economic growth has been relatively slow compared to the US historical average. How much blame do you place on policy makers for this economic growth.

1. A great deal of blame
2. Some blame
3. Very little blame
4. No blame
5. Don't know

[Reverse order 1-4]

Question 2: Credit and Blame Manipulation Check

In the previous question we asked you about US economic growth. According to this question, was growth relatively slow or relatively fast compared to the historical average?

1. Relatively fast
2. Relatively slow
3. Don't remember

[Reverse order 1-2]

Question 3: Prospective Credit and Blame

Economic growth can be affected by government policy and the decisions of companies and global market forces. If the US economy is in recession and is growing at a slow rate in 2016, how much blame would you place on policy makers for this economic growth?

1. A great deal of blame
2. Some blame
3. Very little blame
4. No blame
5. Don't know

[Reverse order 1-4]

Question 4: Prospective Voting Intentions

Economic growth can be affected by government policy, the decisions of companies and global market forces. If the US economy is in recession and is growing at a slow rate in 2016, how likely are you to vote for the Democratic Presidential candidate?

1. Very likely
2. Likely
3. Undecided
5. Unlikely
5. Very Unlikely

[Reverse order 1-5]

Question 5: Voting Intention Manipulation Check

In the previous question we asked you about future US economic growth. According to this question, was the economy in recovery (high growth) or in recession (low growth)?

1. Recovery (High Growth)
2. Recession (Low Growth)
3. Don't remember

[Reverse order 1-2]

Block 5: Control

How much blame or credit do you place on policy makers for US economic growth in past decades?

1. A great deal of blame or credit
2. Some blame or credit
3. Very little blame or credit
4. No blame or credit
5. Don't know

[Reverse order 1-4]

How much blame or credit would you place on US policy makers for economic growth in 2016?

1. A great deal of blame or credit
2. Some blame or credit
3. Very little blame or credit
4. No blame or credit
5. Don't know

[Reverse order 1-4]

How likely are you to vote for the Democratic Presidential candidate in 2016?

1. Very likely
2. Likely
3. Undecided
5. Unlikely
5. Very Unlikely

[Reverse order 1-5]

Appendix B: Construction of the instrumental variable for trade

To construct our instrumental variable (IV) we followed Frankel and Romer's (1999) methodology. To begin, we first gathered data on international trade from the Correlates of War project, which has dyadic-level trade data for over 200 countries going back as far as 1870. The dataset includes two variables, *flow1* and *flow2*, which are the value of exports from country A to country B and from country B to country A, respectively. These values are expressed in millions of current (*i.e.* nominal) US dollars, and so are not adjusted for inflation. The total value of *trade* between country A and country B in each year was calculated simply by summing up these two variables. Next we added in the population (expressed in thousands) and nominal GDP (expressed in millions of US dollars) for both country A (*pop A*) and country B (*pop B*). The measures for population came from the Correlates of War project, while data on nominal GDP came from the World Bank. We then divided the total level of trade in each dyad by the GDP of country A and country B to create our *trade-to-GDP* ratio for both countries. Finally we merged this data with geographic variables taken from the Centre for Prospective Studies and International Information (CEPII). These variables included (1) the total land area for country A (*area A*) and country B (*area B*) measured in square kilometers; (2) the distance between country A and country B (*distance*) expressed in kilometers²³; (3) a dummy variable indicating whether either country A or country B is landlocked (*landlocked*); and finally (4) a dummy variable (*contiguous*) indicating whether country A was contiguous to country B.²⁴

Next we followed Frankel and Romer's approach in getting rid of any dyadic pairs that had trade-to-GDP ratios equal to 0, or in which the population of either country was less than 200,000. We then regress the trade-to-GDP ratio on all of the above variables, including interactions between contiguity and all of the population, area, distance, and landlocked variables. Note that we log all of the variables (except for the dummy variables) in order to normalize their distributions. More formally we estimate the following two models:

$$\begin{aligned} \log(\text{trade-to-GDP}_A) = & \log(\text{pop}_A) * \text{contiguity} + \log(\text{pop}_B) * \text{contiguity} + \\ & \log(\text{distance}) * \text{contiguity} + \log(\text{area}_A) * \text{contiguity} + \log(\text{area}_B) * \text{contiguity} + \\ & \text{landlocked} * \text{contiguity} + u_1 \end{aligned}$$

$$\begin{aligned} \log(\text{trade-to-GDP}_B) = & \log(\text{pop}_A) * \text{contiguity} + \log(\text{pop}_B) * \text{contiguity} + \\ & \log(\text{distance}) * \text{contiguity} + \log(\text{area}_A) * \text{contiguity} + \log(\text{area}_B) * \text{contiguity} + \\ & \text{landlocked} * \text{contiguity} + u_2 \end{aligned}$$

²³ Note that the CEPII dataset contains several different measures of distance. The one we employed is their standard *dist* variable, which uses the latitudinal and longitudinal coordinates of each countries' most important cities (in terms of population) as its basis for determining distance. See Mayer and Zignago 2011.

²⁴ Note that while the vast majority of the data was taken straight from these datasets, for some countries the data was simply unavailable. In these cases the authors used distance values for nearby countries as a proxy measure of distance for the other countries. We then manually corrected some of the area, contiguity, and landlocked variables as best we could, usually relying on quick google searches for measures such as the area of a particular country.

(The models include the main effects of each of the interaction terms.) These two models give us (logged) estimates of the trade-to-GDP ratios for each country in each dyadic pair in our dataset. Estimated coefficients from these two models can be seen in Table [1stIV]. It is worth noting that our predicted values of dyadic trade have a fairly decent correlation with actual levels of trade, ranging from 0.54 for model (1) to 0.62 for model (2).

[1stIV]

We then take these predicted values and sum them up for each country/year to convert the dyadic dataset into a monadic dataset. The predicted values thus become our instrument for trade, *Instrumented Trade*, as they measure the total amount of trade that we would expect a country to engage in based on its size and geographical location. We eliminate Liberia from the dataset because this country's actual trade-to-GDP ratios were clear outliers. Overall, the monadic level of trade is underpredicted by our models, but it correlates well with the actual level of trade (at least the logged values). Furthermore, the signs and magnitudes of our coefficients are similar to those of Frankel and Romer (1999). The final monadic dataset has data on roughly 165 countries during the period 1960-2007.²⁵

²⁵ Note however that to get the final instrumental variable that we actually use in this paper, we restricted our analysis in the next step to only those countries for which we had relevant electoral data (specifically, the 33 West and Eastern European countries taken from Schleiter and Tavits (2014)).