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Fiscal consolidations and electoral outcomes in emerging economies: Does the policy mix matter? Macro and micro level evidence from Latin America

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Does the Policy Mix Matter? Macro and Micro Level
Evidence from Latin America

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Abstract*

Do voters punish governments that introduce fiscal “austerity” measures? If so, does voter response vary according to the design (composition) of fiscal adjustments? What determines the timing of fiscal consolidations? The empirical literature on the political economy of fiscal adjustments, mostly OECD-based, argues that consolidations do not have significant electoral consequences. This paper re-examines these questions and finds that voters punish fiscal consolidations at the polls in Latin America and the Caribbean (LAC). To explain this result, we focus on the composition and timing of fiscal adjustments episodes. Such episodes rely fundamentally on increasing tax rates and bases of indirect taxes (such as the VAT) that hit broad segments of the population. Moreover, these policies are often implemented when politicians have no choice but to consolidate, that is, under severe economic circumstances. These macro results are corroborated with micro evidence from an original survey experiment that measures voter’s fiscal policy preferences over the business cycle in seven countries across Latin America. The experimental evidence shows that respondents prefer expenditure cuts to tax increases during recessions. This begs the question—if tax increases are more electorally costly, why do governments rely on them? It is argued the policy choice set available to pursue fiscal consolidation is relatively narrow in LAC, suggesting that investments in fiscal capacity are needed to expand the policy toolset of governments in the face of negative shocks.

JEL classifications: E62, H20, H50, H62

Keywords: Fiscal deficit, Taxes, Public expenditures, Business cycle, Elections

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“...We find no evidence that governments which quickly reduce budget deficits are systematically voted out of office in a sample of OECD countries.”

Alesina, Carloni and Lecce (2012)

“Argentina between this year and the next one is undertaking a fiscal adjustment of almost 3 percentage points of GDP... An adjustment of this magnitude was never done before without the Government falling.”

Nicolas Dujovne, Argentina’s Minister of Finance since 2017¹

1. Introduction

The literature on the economic vote assumes that voters reward incumbent governments when the economy is strong and punish them when it is weak (e.g., Tufte, 1978; Lewis-Beck, 1988). Early studies used aggregate level data, but survey evidence based on individual-level data confirmed results from cross-country regressions: when voter perceptions of the state of the economy improved, they were more likely to vote for the incumbent (e.g., Duch and Stevenson, 2008). While these studies focused on OECD countries, the results in Latin America appear to be no different. Macro evidence (Murillo, Oliveros and Vaishnav, 2010; Benton, 2005), survey-level evidence (Singer and Carlin, 2013) and recent studies that combine macro and micro-level data (Murillo and Visconti, 2017) find evidence on the importance of pocketbook economic voting in the region. In their review of this literature, Lewis-Beck and Stegmaier (2019: 258) conclude “Latin Americans, then, do appear to be economic voters.”²

This logic may be used to explain why voters supposedly oppose fiscal consolidations, that is, an increase in taxes and/or reduction in government expenditures. A fiscal shock of this nature presumably reduces economic growth to a lower level than before the consolidation,³ after which voters observe the slow-down and punish incumbents that initiate them. Yet, an influential literature mostly based on the experience of advanced democracies, contends that there is no

¹ “La Argentina entre este año y el próximo está haciendo un ajuste fiscal de casi 3 puntos del PBI...En la Argentina nunca se hizo un ajuste de esta magnitud sin que caiga el Gobierno (Source: *Infobae*, November 14, 2018; <https://www.infobae.com/economia/2018/11/14/nicolas-dujovne-nunca-se-hizo-un-ajuste-de-esta-magnitud-sin-que-caiga-el-gobierno/>)

² Building on this insight, the opportunistic political business cycle literature expects government to use the macro-economic policy tools at their disposal to strengthen the economy prior to an election. Clark and Hallerberg (2000) and Clark (2003) argue that governments use monetary policy to boost the economy prior to elections when the central bank is dependent and exchange rates are flexible while they use fiscal policy when the exchange rate is fixed.

³ According to recent estimates of the fiscal multiplier, a one percent of GDP fiscal consolidation leads output to contract by about 0.9 percent after two years (Guajardo, Leigh and Pescatori, 2014), with even larger impacts if the economy is growing below potential (Jorda and Taylor, 2015).

empirical support for this argument (Arias and Stasavage, 2018; Alesina, Carloni and Lecce, 2013). For example, using a dataset of 19 OECD countries, Alesina, Carloni and Lecce (2013) do not find a statistically significant effect of large budget adjustments on electoral outcomes. The policy implication is that fear of punishment at the polls should not be a reason for governments to avoid fiscal consolidations. However, OECD countries may only introduce consolidations when electoral risk is low, demonstrating strategic selection bias (Hübscher and Sattler, 2017), and hold elections when the timing is correct. Moreover, the fact that in many of these countries electoral timing is endogenous makes it difficult to uncover the electoral consequences of fiscal adjustments based on this particular sample.

In addition to understanding whether voters punish fiscal consolidations or not it is also relevant to ask *how* a government consolidates. On the one hand, the composition could have differential economic effects (Alesina, Favero and Giavazzi, 2019; Giavazzi and Pagano, 1990). On the other, the composition may fare differently with voters independently of its general economic consequences, by affecting broad or particular interests. In work that uses a survey experiment in five European countries, Hübscher, Sattler, and Wagner (2018) find that voters especially oppose spending cuts on broad public services, and to a lesser extent, object to tax increases.

In this paper, we first confirm the non-findings of Alesina, Carloni and Lecce (2013) by using alternative measures of fiscal actions for OECD countries. In Latin America, in contrast, we find that voters *do* replace government parties more frequently after the implementation of fiscal consolidations. Compared to a government that does not engage in fiscal adjustments, the probability of government turnover triples when countries implement ambitious adjustment programs (in terms of the size of GDP). To explain this result, we look at the mix (or composition) and timing of fiscal consolidation policies. Fiscal consolidations in Latin America are mostly tax-based and rely fundamentally on increasing tax rates and bases of indirect taxes (such as the VAT) that hit broad segments of the population. Moreover, our empirical analysis indicates that these policies are often implemented when politicians have no choice but to consolidate, that is, under severe economic circumstances, which amplifies the multiplier effects of fiscal contractions.

We additionally present original data from a survey experiment specifically designed to measure preferences for fiscal adjustment instruments (taxes, and different expenditure categories) in a cross-section of 6,000 Latin American citizens from seven different countries. Our analysis

suggests that voters in these countries prefer expenditure cuts to tax increases during fiscal consolidations. This provides micro evidence for the finding that governments are more likely to lose elections if they increase taxes in Latin America.

Our findings point to an important theoretical puzzle: why would governments ever choose a type of consolidation that has negative political consequences when they could choose another that does not? This question is not only theoretically relevant, but also methodologically challenging (in terms of identification). Our interpretation draws on the literature emphasizing large cross-country variation in fiscal capabilities, whereby countries with lower fiscal capacity have a more limited set of policy instruments to choose from when implementing a fiscal adjustment strategy (Besley and Persson, 2011; Gordon and Li, 2009; Franco Chuaire, Scartascini and Tommasi, 2017). Given the size and structure of taxation and public spending in Latin America, efficient policies to pursue successful fiscal adjustment may also be quite demanding in terms of the capabilities of governments to implement them. Countries with low fiscal capacity might not be able to deliver some of these more demanding policies and hence might be forced to deal with fiscal adjustment by blunt methods that quickly generate additional funds but are also politically unpopular. On the other hand, countries with high fiscal capabilities have a wider menu of policies at their disposal, allowing politicians to fine-tune the consolidation and mitigate its electoral repercussions.

Our paper contributes to the literature on the political economy of fiscal consolidations in three main ways. First, we use a measure of fiscal policy that attempts to isolate fiscal actions from output movements in ways that standard approaches do not. Past research typically calculates actual changes in the budget deficit to analyze the electoral consequences of fiscal adjustments in the OECD. It is possible, however, that other factors correlated with changes in primary budget deficits are important variables in their own right. To isolate the discretionary components of fiscal policy, we build on a fiscal action dataset produced at the IMF that relies on contemporaneous policy documents to identify changes in fiscal policy motivated by a desire to reduce the budget deficit and not by responding to current or prospective economic conditions (David and Leigh, 2018; Devries et al., 2011). Such data have been used to identify the causal effects of fiscal consolidations on economic growth both in advanced and emerging economies (Guajardo, Leigh and Pescatori, 2014; Carrière-Swallow, David and Leigh, 2018), but not to explore the electoral consequences of consolidations.

Second, in addition to presenting a new empirical pattern regarding the electoral effects of fiscal consolidations in emerging economies, we test a mechanism through which adjustments have political effects, mainly concentrating on the way the consolidation is executed both in terms of its specific design (composition) and the timing of the fiscal adjustment process, in particular with respect to the state of the business cycle (recessions, expansions) surrounding fiscal decisions. These features have usually been recognized as important determinants of the success of fiscal consolidations in the economic literature, but their political consequences remain understudied. Finally, we present data from an original survey experiment that provides a micro foundation for the finding that governments are more likely to lose elections if they increase taxes in Latin America. Our survey is devised to gauge voter preferences on specific policy instruments (taxes, spending) using a vignette design that randomly exposes respondents to different economic scenarios (recessions, expansions). The vignettes allow us to measure voter policy choices directly, allowing for a comparison between voter preferences and the actual policy choices observed at the macro level.

The remainder of the paper is organized as follows. Section 2 provides a brief review of the relevant literature and discusses the mechanisms linking fiscal actions to electoral results. Section 3 presents the data and describes our empirical strategy. Sections 4 and 5 present the main cross-country results on the electoral consequences of fiscal adjustments, while Section 6 considers the issue of when fiscal consolidations take place. Section 7 provides the results from our survey experiment. Section 8 concludes.

2. The Political Economy of Fiscal Consolidations

The literature on the economic (Alesina, Favaro and Giavazzi, 2019; Guajardo, Leigh and Pescatori, 2014) and political effects (Hübscher, Sattler, and Wagner, 2018; Pasarelli and Tabellini 2017; Alesina, Carloni, and Lecce 2013) of fiscal consolidations is mostly based on the experience of OECD countries. There are two contending views regarding the electoral consequences of fiscal adjustments in advanced democracies. According to an influential view, fiscal austerity is not punished by voters at the polls (Alesina, Carloni and Lecce, 2013; Alesina, Perotti and Tavares, 1998), possibly indicating that voters are fiscal conservatives (Peltzman, 1992). Others contend that the lack of correlation between fiscal austerity and political outcomes is due to a selection effect whereby governments implement austerity only when they can get away with it (Arias and

Stasavage, 2018; Hübscher, Sattler and Wagner, 2018). Electoral concerns are thus key to understand the incentives of policymakers to pursue fiscal adjustment in the first place (Hübscher and Sattler, 2017).

An often-overlooked feature in this debate are the policy tools governments use to implement fiscal adjustment. Given its exclusive focus on advanced economies, the literature on fiscal consolidations has taken for granted the broad set of tax and spending instruments at the disposal of politicians to pursue fiscal adjustments. For example, Alesina, Favero and Giavazzi (2019) code about 3,500 individual fiscal measures across the OECD, classified into up to 27 different tax and spending categories. Moving beyond the advanced world, it can be argued that the menu of policy options available in developing countries is quite narrower, both in terms of fiscal capacity and more general government capabilities to provide public goods and services (Besley and Persson, 2011; Gordon and Li 2009; Bird, 1992; Franco Chuaire, Scartascini and Tommasi, 2017). In particular, due to more diversified tax bases, OECD countries can raise more, and different, types of taxes on different classes of individuals and they can also curtail more kinds of spending, given the relative larger size of the public sector and more diversified set of entitlements that characterize well-developed welfare states. LAC countries are much more limited in choices, particularly during electoral years (Hallerberg and Scartascini, 2017).

In addition, the cyclical behavior of fiscal policy is quite different between advanced and developing countries (Kaminsky, Reinhart and Végh, 2004), with tax (expenditure) policy being acyclical (countercyclical) in the former group, and tax and spending policies strongly procyclical in the latter (Végh and Vuletin, 2015; Frankel et al., 2013). These timing features are important to assess the overall effects of tax increases and expenditure cuts on output growth (Jorda and Taylor 2015) and economic performance in general, which voters tend to evaluate at the ballot box. Thus, we should not expect similar electoral repercussions when broadening the comparative scope of analysis beyond the OECD.

Our empirical findings point to a strong link between fiscal consolidations and electoral behavior in Latin America. To explain this result, we look at the composition and timing of fiscal adjustments. Both features have been well studied in the past to study the economic effects and determinants of fiscal consolidations, but less so in the analysis of their electoral consequences. In this latter respect, the focus on specific policies makes it possible to uncover the potential distributive effects of different fiscal consolidation strategies. For example, Fernández-Albertos

and Kuo (2016) find in Spain during the euro crisis that there is a distributional twist on opposition to expenditure cuts as part of a general consolidation package. The poor generally do not oppose such measures, while the “middle class” do try to block them. The logic is straightforward—most public spending is on programs like education and health that the middle class disproportionately use. Similarly, Hübscher, Sattler and Wagner (2018) find heterogeneous voter responses to fiscal adjustment strategies in five European countries using experimental data. In the following discussion, we describe in a stylized fashion two key features that may matter for understanding the electoral impact of fiscal adjustment strategies in Latin America: the design or composition of fiscal consolidation and its timing.

2.1 How to Adjust: Taxes and Public Spending in a Reduced Policy Choice Set

While general tax collection has been on the rise over the last decade in Latin America, it is still comparatively low, with most countries collecting less tax revenue than might be expected for their income levels (Corbacho, Fretes and Lora, 2013). In addition, tax structure depends excessively on consumption taxes: while only 30 percent of taxes collected in the OECD are indirect taxes, they explain the bulk of collection in almost all LAC countries. The Value-Added Tax (VAT) is the main source of revenue, with receipts above 6 percent of GDP for the typical country. In contrast, personal income taxes are among the lowest in the world, accounting for less than 1.5 percent of GDP, compared to an average of 8.4 percent of GDP in advanced economies, partly due to the generosity of tax regimes that allow deductions of various sorts (Barreix, Garcimartín and Velayos, 2013) and rampant tax evasion (Gómez-Sabaíni and Moran, 2016).

Such a composition implies that a fiscal consolidation driven by increases in general consumption taxes would directly affect a large share of voters, both from a pocketbook and sociotropic perspective. For example, given that the poor consume a larger share of their income than the wealthy, an increase in the VAT rate for consumer goods means the poor end up paying a higher proportion of their income in VAT than the rich. Thus, while voters would tend to oppose tax increases in general, opposition should be especially prevalent among lower income households when the composition of fiscal consolidation is skewed towards the use of instruments such as the VAT or other forms of indirect taxation.

Turning to the spending side of the budget, public expenditures in LAC tend to show limited redistributive capacity, given relatively high (low) spending on regressive (progressive)

programs (Izquierdo, Pessino and Vuletin, 2018; Lustig, Pessino and Scott, 2014; Goñi, López and Servén, 2011), calling into question the common assumption in the literature that the median voter is a net receiver of public transfers (Meltzer and Richards, 1981). For example, while in-kind spending such as health and education may seem progressive, an analysis by coverage and quality finds to be, in fact, *regressive* (Izquierdo, Pession and Vuletin, 2018). Moreover, social transfers have been historically biased toward social insurance programs that benefit workers in the formal sector, thus covering only a subset of the labor force and ultimately providing low levels of coverage, especially among the poor (Bosch, Melguizo and Pagés, 2013). While pro-poor expenditures such as conditional cash transfers or non-contributory pensions have been on the rise in recent decades, they are still relatively small in terms of budget size and are not exempt from targeting problems (Levy and Schady, 2013). According to recent estimates, the percentage of the extreme poor who are beneficiaries of cash transfers and non-contributory programs is only 46.9 percent and 12.8 percent, respectively (Robles, Rubio and Stampini, 2015).

Thus, unlike taxes that hit the general population, the reduced scope or “truncated nature” of the welfare state in LAC means expenditure cuts are likely to hit narrower constituencies, like public sector employees, or items from which the poor do not necessarily benefit in the first place. Compensation to employees is a major item in Latin American budgets, accounting for almost 30 percent of total outlays, but it is also a contentious one. The sector is often unionized, and cuts in pay or other compensation invites social protest. In terms of choice availability, while rationalizing the wage bill or current transfers may constitute an efficient choice for restoring fiscal sustainability in the medium term, cuts in public investments may prove more politically palatable. This logic may explain why fiscal consolidations are often associated with the over-compression of public investment in the region (Cavallo and Powell, 2019; Easterly and Servén, 2003).

2.2 When To Adjust: Adding Fuel to the Fire

In addition to the policy mix, the timing of fiscal consolidations is key to understanding the political strategies surrounding the implementation of fiscal austerity. In principle, politicians would prefer to avoid consolidations altogether, since such actions imply that some interest group is affected (Alesina, Ardagna and Trebbi, 2006). The delay of fiscal adjustments has been a central theme discussed in the political economy of reform literature (Alesina and Drazen, 1991). It implies that politicians would only implement these types of policies when they have no other

choice, such as a situation of binding economic constraints. This logic is consistent with the fact that fiscal policy in Latin America and the Caribbean has traditionally been procyclical on average (Klemm, 2014; Talvi and Végh, 2005; Gavin and Perotti, 1997). That is, governments have tended to increase public spending (or cut taxes) during periods of expansion and tended to cut expenditures (or raise taxes) during periods of recession. In this latter scenario, the procyclical bias implies that fiscal adjustments would tend to amplify the negative effects of fiscal austerity on economic activity (Riera-Crichton, Végh and Vuletin, 2015; Auerbach and Gorodnichenko, 2012), exacerbating negative voter reactions even further.

As the review above suggests, the literature on the political economy of fiscal consolidations concentrates almost exclusively on OECD countries. Yet, there are important reasons to think that the effects of government macro-economic policy decisions may be different in other settings, especially given variation in the specific policy choices and the timing of the fiscal measures being implemented. Thus, a focus on Latin America provides an opportunity to explore these issues empirically from both a macro (cross-country) and individual-level perspective.

3. Data and Empirical Specification

3.1 Fiscal Consolidations

Our independent variable of interest is a measure of fiscal consolidations. Early work (e.g., Alesina and Perotti, 1997; Alesina and Ardagna, 1998) looked at actual changes in budget balances. A problem with such data is that the government's effort to consolidate may not systemically translate into equivalent changes in the fiscal balance. A way to address this problem is to code directly the government's effort. Data on fiscal consolidation episodes come from Guajardo, Leigh and Pescatori (2014) and Alesina, Favero and Giavazzi (2019) for 17 OECD countries (1978-2014), and from David and Leigh (2018) for 14 Latin America and the Caribbean countries (1989-2016). This dataset, containing almost 300 fiscal consolidations episodes, is based on a careful reading of fiscal legislation and relevant historical records, and is known as the “narrative” approach to identifying fiscal shocks.⁴ The idea is to measure concrete actions taken to affect the budget balance, both on the spending and tax side.

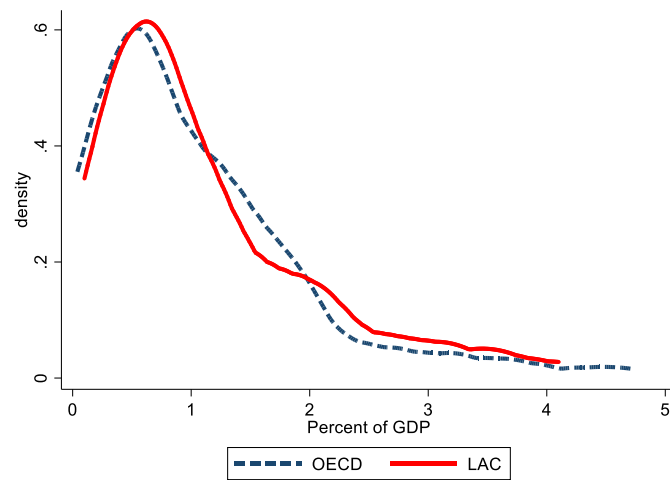
⁴ The narrative approach was pioneered by Romer and Romer (2010) to examine the effects on U.S. output of changes in tax rates identified from the historical record.

In comparison to measures of the budget balance, this dataset allows us to examine voters' reactions at the ballot box to actual policies rather than reactions to variables that may or may not be affected by government actions, such as changes in the fiscal balance over a given period of time. In our analysis, the variable for fiscal consolidation takes two forms: i) a dummy variable equal to 1 if a fiscal adjustment occurred during the term in office preceding the election, and ii) a continuous variable: the size of fiscal adjustment, captured by the accumulated budgetary impact of the fiscal actions implemented during the term (in percentage of GDP). We further code fiscal consolidations as tax (expenditure) based on whether tax (spending) measures represent more than 50 percent of total budgetary impact in a given year. Figures 1 and 2 show that, while the relative size of fiscal consolidations is similar across OECD and LAC, the composition is quite different: tax-based consolidations are more frequent in LAC, representing 72 percent of all such consolidations, while just 39 percent of consolidations are primarily tax-based in the OECD sample.

Moreover, we are interested in the specific spending and tax measures implemented. To approach this issue, we have decomposed each consolidation episode by classifying the policy instruments into different types of measures, on both the tax side and the spending side. On the former, income tax increases represent close to 42 percent of total tax measures in the OECD, whereas in LAC this share is less than 23 percent (Figure 3, Panel A). In stark contrast, increases in indirect taxes (including VAT) account for almost 60 percent of total tax measures in LAC, whereas in OECD that share is less than 41 percent.⁵ On the spending side, OECD countries rely on a variety of instruments, including especially cuts to social transfers, whereas in LAC more than half of total measures imply reduction in capital expenditures (Figure 3, Panel B) and cuts in social transfers are almost absent. Based on this decomposition, it seems plausible to think OECD and LAC politicians have different policy instruments available to them when undertaking a fiscal adjustment.

⁵ Based on a different dataset on tax reforms in 18 LAC countries during the 1990s and early 2000s (Focanti et al., 2013), Hallerberg and Scartascini (2017) identify 70 reforms aimed at increasing taxes, of which 68 percent are explained by VAT expansions.

Figure 1. Budgetary Impacts of Fiscal Consolidations (% GDP)



Note: Kernel density distribution of fiscal consolidation episodes.

Figure 2. Composition of Fiscal Consolidations (%)

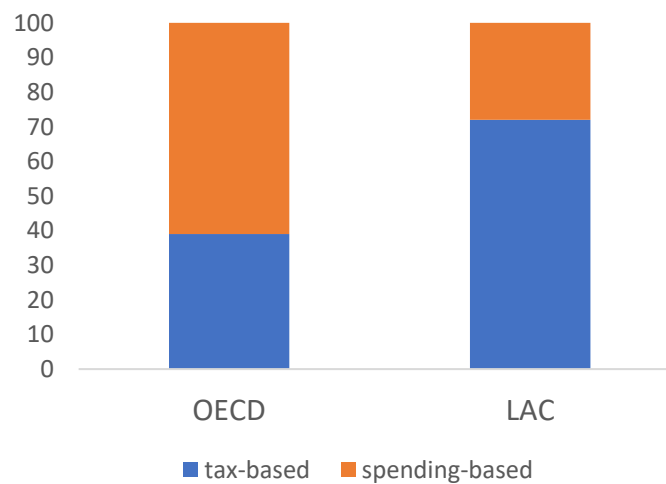
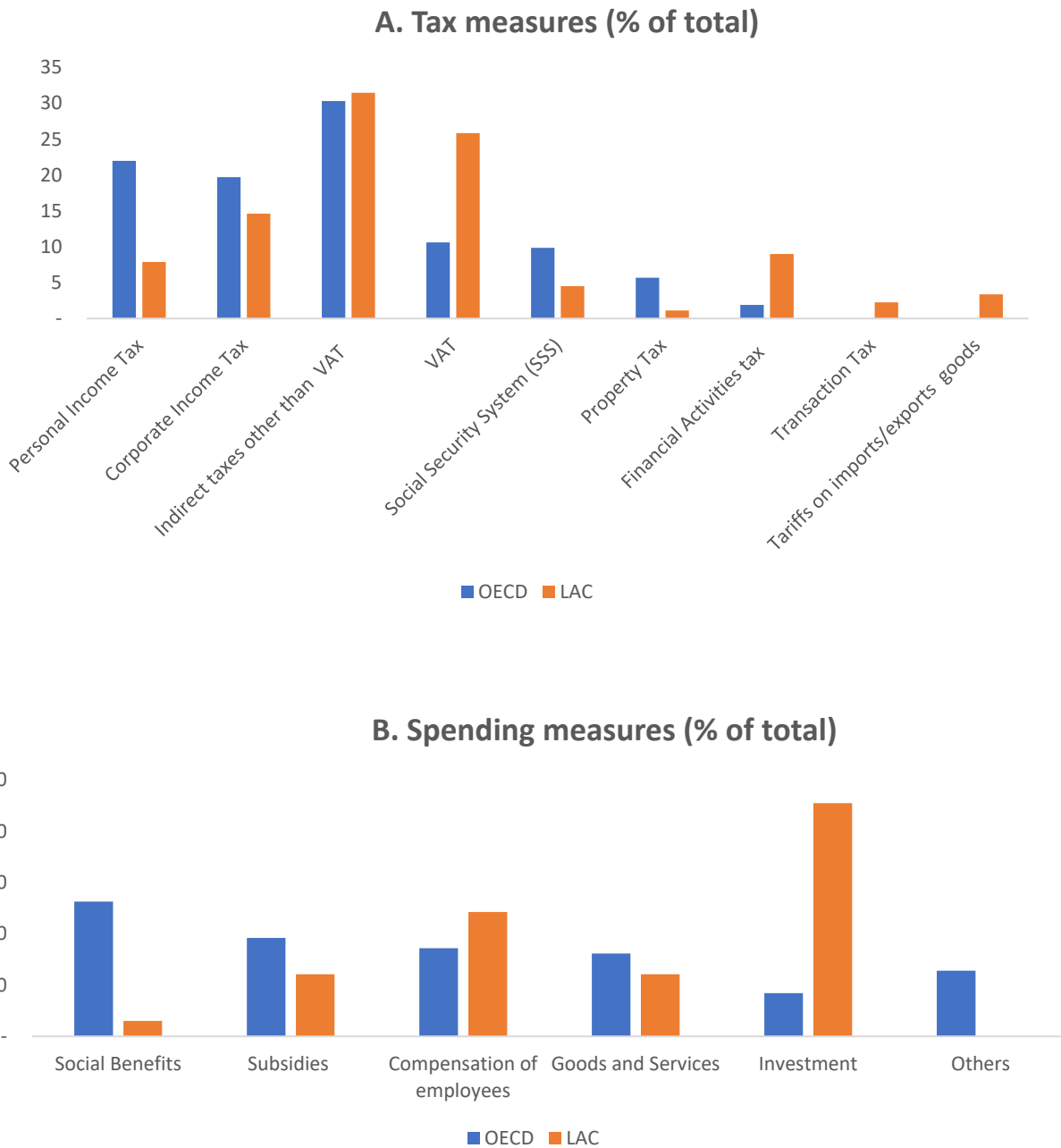


Figure 3. Frequency of Policy Instruments by Type and Region (%)



3.2 Outcome Variables: Election Results

For each election year in the dataset, we draw on the *Database of Political Institutions* (DPI) provided by Cruz, Keefer, and Scartascini (2018) to code whether there is a change in ideology of the executive party as a result of the election. In these data, ideological orientation takes three values: right, left, or center. The variable is the same dependent variable as in Alesina, Carloni and

Lecce (2013), and the coding of partisanship has been extensively used in comparative political economy literature to identify partisanship of governments (e.g., Hallerberg and Von Hagen, 2017; Cruz and Keefer, 2015). In robustness analysis, we also consider other ways of measuring electoral effects such as a change in party and the share of votes for the incumbent party.

3.3 Control Variables

To pick up the effects of the economy on electoral results, we include the change in GDP (or growth), change in prices (or inflation), and change in the unemployment rate in the election year.⁶ In terms of political variables, we begin again with the empirical model in Alesina, Carloni and Lecce (2013). They consider whether there is a majority government, whether there is a coalition government, and the duration of the government. Note that we are looking at many presidential systems in Latin America where it is possible that the party that has the presidency does not have a majority in Congress. Moreover, second houses of a legislature may also be important. For “majority government,” we therefore include a variable “*allhouse*” that is coded as a “1” if the same party controls all chambers of Congress and the executive; otherwise it is coded as “0.” We also include a dummy variable for coalition governments called “*coalition*.” We classify a coalition government as “1” if there is more than one party in government, identified through the variables that identify the 2nd, 3rd, and other parties in government in the DPI. To measure duration of government, a count of the number of years the current government is in office is included (“*years in office*”). Table A1 (Appendix) provides summary statistics and the sources for all relevant variables.

3.4 Estimation

The goal is to test whether fiscal consolidations affect electoral outcomes measured as government turnover. Thus, following the original Alesina, Carloni and Lecce (2013) specification, our baseline model specification states:

$$Y_{it} = \beta_0 + \beta_1 FC_{it} + \theta X_{it} + \gamma Z_{it} + \epsilon_i$$

where Y is a dummy variable for country i equal to 1 if there is a change in ideology of the executive party at election t ; X and Z are vectors of economic and political control variables in county i and

⁶ In unreported results (available upon request), we also use average values of our control variables over the term preceding the election. They are substantively the same.

election t . Our main coefficient of interest is β_1 , capturing the effect of fiscal consolidation (FC) on electoral outcomes. We use pooled logistic regressions to estimate the model for two reasons: first, we restrict ourselves to the Alesina, Carloni and Lecce (2013) baseline specification for comparison purposes. Second, the relative frequency of the data over time prevents us from fully exploiting within-country variation.

4. Results

4.1 Baseline Results

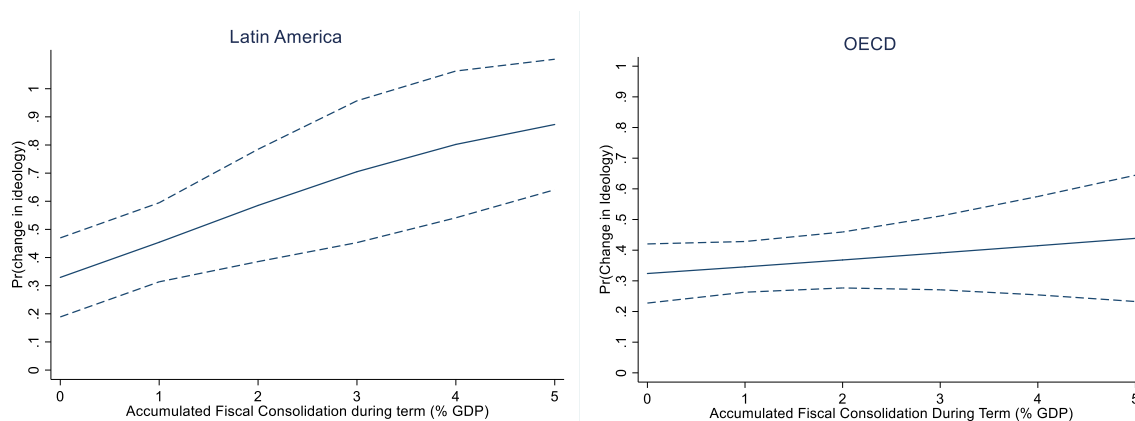
Table 1 presents our baseline results. Model 1 reports baseline economic controls while Model 2 adds the political variables as in Alesina et al. (2013). These models are run separately for OECD and Latin American countries. Results indicate that fiscal consolidation—whether coded as a dummy, or as the size of the consolidation effort—is not statistically significant in the OECD sample.

Table 1. Determinants of Political Turnover in LAC and OECD (pooled logistic regressions)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|----------------------|--------------------|--------------------|--------------------|---------------------|--------------------|--------------------|--------------------|---------------------|
| | OECD | OECD | LAC | LAC | OECD | OECD | LAC | LAC |
| | <i>dummy</i> | | | | <i>size</i> | | | |
| Fiscal Consolidation | -0.008 (0.344) | -0.019 (0.351) | 1.102** (0.508) | 1.491*** (0.573) | 0.130 (0.090) | 0.123 (0.093) | 0.412* (0.211) | 0.528** (0.239) |
| GDP growth | -8.025 (9.929) | -8.017 (10.655) | 3.450 (9.443) | -2.854 (11.462) | -6.753 (9.968) | -6.323 (10.676) | 5.922 (9.545) | 0.889 (11.271) |
| Unemployment | 14.051 (22.509) | 15.284 (23.517) | -9.477 (19.575) | -5.962 (20.556) | 17.351 (22.811) | 19.658 (23.860) | -2.397 (19.847) | 3.946 (20.900) |
| Inflation | 7.747 (9.997) | 8.335 (10.097) | 0.173 (0.128) | 0.159 (0.126) | 6.659 (10.321) | 7.411 (10.411) | 0.169 (0.123) | 0.158 (0.121) |
| Years in office | | -0.001 (0.026) | | -0.008 (0.020) | | -0.001 (0.026) | | -0.003 (0.020) |
| Allhouse | | 0.335 (0.500) | | -1.165 (0.713) | | 0.385 (0.503) | | -1.293* (0.732) |
| Coalition | | 0.074 (0.401) | | -1.432** (0.653) | | 0.111 (0.399) | | -1.456** (0.659) |
| Constant | -0.305 (0.363) | -0.395 (0.584) | -0.953* (0.507) | 0.099 (0.712) | -0.506 (0.321) | -0.627 (0.542) | -0.880* (0.492) | 0.205 (0.690) |
| Observations | 152 | 150 | 74 | 69 | 152 | 150 | 74 | 69 |

In fact, none of the variables in our specification are significant, though the sign on economic growth is in the expected direction, as is the sign for fiscal consolidation. The results are different for the Latin American sample. Regardless of the definition of the dependent variable we use, a fiscal consolidation makes it more likely that there is turnover in government. Moreover, adding the political variables seems to strengthen somewhat the result for these countries. Based on Models 6 and 8, which use the size of the fiscal adjustment as the independent variable for the OECD and Latin American samples, respectively, Figure 4 plots the predicted probability of political turnover as a function of the size of fiscal consolidation. One can see that the baseline figure for the OECD countries remains the same. For Latin America, however, there is a sizable change as the size of fiscal adjustment increases. In a case where there is no planned fiscal consolidation, the likelihood of a change in government ideology is essentially the same as OECD countries, or about 0.3. The greater the consolidation, the higher the likelihood that a government will not remain in office at the next election. Note that standard economic variables such as growth, unemployment, and inflation are already included in the analysis.

Figure 4. Predicted Probability of Ideology Change by Size of Fiscal Consolidation



4.2 Results from an Extended Empirical Model and Alternative Dependent Variables

The literature suggests that there may be other determinants of electoral results especially for Latin America. To rule out that it is our restricted model that is driving the results, we include additional variables that may be relevant for this particular sample. Moreover, there are multiple ways to measure electoral consequences. We thus consider a change in party (not just ideology) as well as the vote share of the incumbent government in an election following a consolidation.

With respect to our extended set of controls, given Latin American reliance on exports of commodities, previous research shows voters are sensitive to changes in the terms of trade, and we include this variable for the Latin American sample.⁷ In addition, the role of domestic price levels, external constraints, and income inequality in shaping electoral outcomes have also been typically discussed in the empirical literature.⁸ We thus include changes in inflation, the current account balance (percentage of GDP) and the Gini coefficient as controls. To control for the maturity of democratic institutions, we follow Brender and Drazen (2008) and include the number of years a country has been democratic. Finally, we add a measure of electoral volatility, a key feature of party systems in the region (Mainwaring, 2018; Roberts, 2015).

Table 2 presents results for an expanded set of independent variables. As shown by columns 1 and 2, the results for the effects of fiscal consolidation under both forms of the dependent variable remain with these additional variables included. Change in party ideology may underestimate political turnover, given the possibility of alternation between parties of similar ideological positions. Thus, a related version of the dependent variable consists in coding whether any party in government changed, regardless of their ideology. Columns 3 and 4 use this specification of the dependent variable, and the effects of fiscal consolidations are substantively the same. Finally, columns 5 and 6 of Table 2 present results from using incumbents' vote share as the dependent variable, conceptualized as the vote for the President's party in a presidential election. Once again, we get the same pattern as for the ideological complexion of government—a fiscal consolidation hurts an incumbent in Latin America (whether coded as a dummy variable or as a sum of the size of consolidations before the election). In the Appendix we report additional robustness tests: using an alternative measure of fiscal consolidation (Table A2), sensitivity of estimates to outliers (Table A3), and the inclusion of country fixed effects (Table A4).

⁷ Campello and Zucco (2016a) argue that voters cannot tell whether economic success is due to domestic policies or the world economy. In related work, and using survey experiments, Campello and Zucco (2016b) find that voters misattribute economic performance to the policies of the president rather than to changes in commodity prices.

⁸ On the role of inflation, see Murillo and Visconti (2017) and Kaplan (2013). Murillo, Oliveros and Vaishnav (2010) discuss how domestic and external constraints shape electoral accountability.

Table 2. Extended Model Effects on Ideology Change, Party Change and Vote Shares of the Incumbent in Latin America

| | (1) | (2) | (3) | (4) | (5) | (6) |
|----------------------------|------------------------|----------------------|---------------------|----------------------|----------------------|----------------------|
| <i>Dependent variable:</i> | <i>Ideology change</i> | | <i>Party change</i> | | <i>Vote shares</i> | |
| | <i>dummy</i> | <i>size</i> | <i>dummy</i> | <i>size</i> | <i>dummy</i> | <i>size</i> |
| Fiscal consolidation | 2.219*** (0.808) | 1.150** (0.461) | 3.327*** (1.171) | 1.512** (0.661) | -0.106** (0.041) | -0.033** (0.016) |
| GDP growth | 3.623 (14.979) | 13.488 (17.119) | 12.289 (17.569) | 19.455 (18.612) | 0.848 (0.926) | 0.715 (0.950) |
| Unemployment | -2.502 (27.723) | 29.007 (31.362) | 38.565 (34.069) | 57.240 (35.403) | 2.879* (1.659) | 2.254 (1.742) |
| Inflation | 0.376 (0.712) | 0.366 (0.613) | 12.278 (9.554) | 7.593 (7.215) | -0.009 (0.006) | -0.008 (0.006) |
| Terms of Trade | -0.006 (0.048) | -0.009 (0.047) | -0.009 (0.053) | -0.011 (0.050) | -0.003 (0.003) | -0.003 (0.003) |
| Current account | 26.413* (14.335) | 22.715* (13.561) | 39.397* (21.097) | 36.528** (17.895) | -3.442*** (0.819) | -3.141*** (0.840) |
| Gini | 17.765 (11.114) | 22.155* (12.007) | 6.544 (11.071) | 9.974 (11.105) | -0.381 (0.620) | -0.387 (0.636) |
| Years in office | 0.012 (0.028) | 0.013 (0.027) | 0.039 (0.039) | 0.034 (0.029) | -0.002 (0.002) | -0.002 (0.002) |
| Allhouse | -1.621 (1.001) | -1.612 (1.044) | -3.231** (1.311) | -2.837** (1.190) | 0.197*** (0.057) | 0.188*** (0.058) |
| Coalition | -1.526* (0.846) | -1.830** (0.917) | -0.631 (0.918) | -0.925 (0.922) | 0.025 (0.048) | 0.020 (0.049) |
| System tenure | 0.019 (0.027) | 0.010 (0.027) | 0.014 (0.026) | 0.003 (0.025) | -0.002 (0.002) | -0.002 (0.002) |
| Volatility | 1.578 (2.221) | 2.210 (2.223) | 4.151 (3.058) | 4.018 (2.711) | -0.405*** (0.127) | -0.411*** (0.131) |
| Constant | -9.784* (5.240) | -11.923** (5.777) | -5.411 (4.935) | -6.559 (5.091) | 0.642** (0.281) | 0.636** (0.288) |
| Observations | 57 | 57 | 58 | 58 | 57 | 57 |

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

4.3 Composition of Fiscal Adjustment: Does the Type of Consolidation Matter?

Several studies based on OECD data suggest expenditure cuts are typically preferable to tax increases based on the macroeconomic effects of those choices. Tax increases are more likely to have larger contractionary effects over the short and medium term than expenditure cuts (Alesina, Favero and Giavazzi, 2019; Alesina, Favero and Giavazzi, 2018). Indeed, Alesina, Carloni and Lecce (2013) find in bivariate comparisons that tax increases are correlated with more government changes in OECD countries, although this finding does not hold up in multivariate regressions. Hübscher, Sattler, and Wagner (2018), however, find different results on preferability of fiscal consolidations based on electoral considerations. In their sample of five European countries, their survey experiment evidence indicates that voters are much more likely to punish spending cuts than tax increases. Yet, as our review of descriptive data indicated, OECD countries rely primarily on spending cuts 60 percent of the time.

We examine here whether the *type* of consolidation has electoral consequences for governments in Latin America. Following Guajardo, Leigh and Pescatori (2014), we classify consolidation packages into tax-based or spending-based adjustments, depending on whether tax hikes or expenditure cuts account for most of the budgetary impact of the consolidation. As we noted above, over 70 percent of consolidations in the region were primarily tax-based. As shown in Table 3, the probability of government turnover (measured as ideology and party change) is increasing with the occurrence and size of fiscal adjustment only when consolidations are based primarily on tax hikes. Based on Model 2, Figure 5 shows the differential impact of different types of fiscal consolidations. This result suggests that it is not the consolidation per se that voters punish, but the way the consolidation is executed.

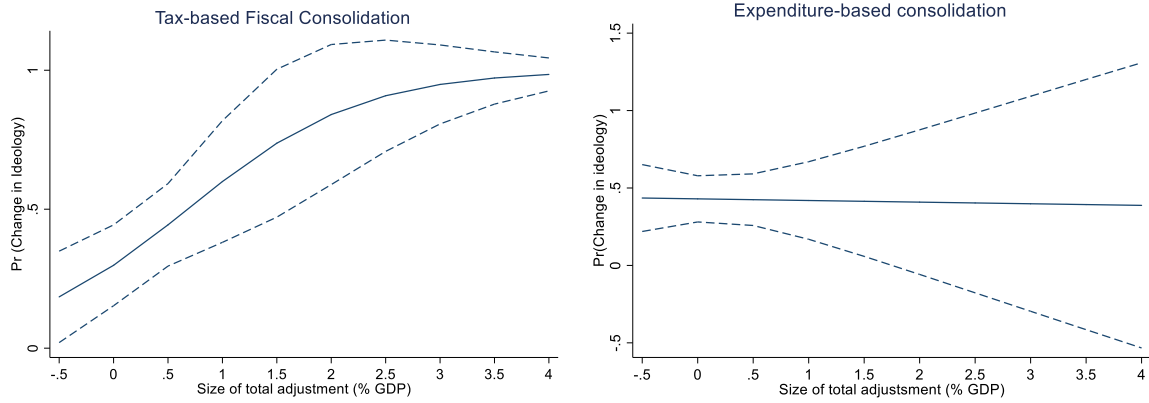
Table 3. Type of Fiscal Consolidation

| <i>Dependent variable</i> | (1) | (2) | (3) | (4) |
|---------------------------|------------------------|----------------------|----------------------|----------------------|
| | <i>Ideology change</i> | | <i>party change</i> | |
| | dummy | size | dummy | size |
| Tax-based | 2.033** (0.905) | 2.152** (0.913) | 3.670*** (1.364) | 3.590*** (1.327) |
| Spending-based | 0.142 (1.186) | 0.322 (0.655) | -0.598 (1.337) | -0.069 (0.879) |
| GDP growth | 4.085 (15.795) | 3.347 (17.563) | 9.324 (18.850) | -2.529 (18.860) |
| Unemployment | 3.095 (28.723) | 12.936 (32.999) | 38.546 (35.412) | 40.691 (38.727) |
| Inflation | 0.392 (0.833) | 0.350 (0.643) | 13.643 (9.508) | 8.224 (8.791) |
| Terms of Trade | -0.005 (0.048) | 0.006 (0.054) | -0.002 (0.057) | 0.035 (0.063) |
| Current account | 23.894* (13.673) | 27.337* (14.351) | 37.249* (20.011) | 43.974** (20.754) |
| Gini | 16.907 (10.805) | 23.667* (12.518) | 6.280 (10.889) | 13.363 (12.598) |
| Years in office | 0.022 (0.027) | 0.010 (0.028) | 0.049 (0.034) | 0.036 (0.038) |
| Allhouse | -1.894* (1.036) | -1.689 (1.049) | -3.622*** (1.364) | -3.814** (1.544) |
| Coalition | -1.554* (0.838) | -1.843** (0.936) | -0.699 (0.932) | -1.131 (0.979) |
| System tenure | 0.018 (0.026) | 0.010 (0.029) | 0.016 (0.026) | 0.008 (0.026) |
| Volatility | 2.039 (2.186) | 2.430 (2.288) | 4.726 (2.917) | 4.020 (2.966) |
| Constant | -9.478* (5.133) | -12.480** (6.007) | -5.362 (4.941) | -7.500 (5.706) |
| Observations | 57 | 57 | 58 | 58 |

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Figure 5. Probability of Political Turnover by Type of Fiscal Consolidation



5. Selection Bias and Identification Concerns

5.1 Selection Bias

Selection bias could arise from the failure to account for omitted variables that simultaneously drive the implementation of fiscal consolidation and correlate with electoral changes. For example, if only “weak” governments cannot avoid consolidation in election years (due to market pressure or other sources) and weak governments are more likely to lose elections in the first place, then government strength could explain our results. Alesina, Carloni and Lecce (2013) examine differences in one-party and multi-party government as proxies for strength in government on the probability of fiscal consolidation. We follow their lead and include in Table A5 (Appendix) a test of the equality of means to check whether government strength varies across two subpopulations: fiscal consolidation episodes and other observations. There are only minor expected differences, and in the “wrong” direction as one would expect if this alternative explanation was right. “Coalition” dynamics may be different under Latin American presidential systems. We use an alternative measure of strength from the ICRG (government stability) capturing “the government’s ability to carry out its declared program(s), and its ability to stay in office.”⁹ After separating countries scoring above/below the median in government stability, we again find no significant differences between fiscal consolidation episodes and the rest of country-year observations.¹⁰

⁹ The indicator ranges from a minimum of 0 to a maximum of 12 and it is itself composed of three subcomponents, namely i) government unity, ii) legislative strength and iii) popular support.

¹⁰ As also shown by Table A4, partisanship differences are also not significant across subpopulations.

5.2 Identification through Inverse Probability Weighted Estimators

We are using observational data, and we cannot randomly assign our cases. To address the problem of non-random treatment assignment, we draw on the augmented inverse probability weighted estimator (AIPW) discussed by Jorda and Taylor (2015), among others. The intuition behind the AIPW is to construct first a predictive model for the likelihood of fiscal consolidation based on a rich set of observable controls (as done in Section 6). This model then serves to reallocate probability mass from the regions of the distributions in the treatment/control units that are oversampled to those regions that are undersampled, thus enabling identification. In other words, less weight is given to fiscal consolidation episodes that are better predicted by the first stage model.¹¹ Figure A1 (Appendix) presents results from an AIPW estimation run on our original baseline model (Table 1, Models 3 and 4), with two alternative dependent variables (ideology change/party change). Average treatment effects are negative and statistically significant at conventional levels across all specifications. After correcting for the endogeneity of the fiscal treatment, we confirm our initial results: fiscal consolidations are politically costly.

In summary, the results from this empirical exercise indicate that in Latin America, fiscal consolidations, and especially consolidations that are tax-based, increase the probability of electoral turnover. We can rule out selection bias given that governments of various shape or “strength” tend to adjust their public finances, and endogeneity concerns were addressed by employing propensity score-based methods.

6. The Timing of Fiscal Adjustments

Previous sections have shown that fiscal consolidation increases the probability of a change in government in Latin America, and that tax increases are the type of consolidation that drives this result. This discussion begs the question, however, of why governments would consolidate in the first place? Politicians would prefer to avoid fiscal adjustments altogether, and they do so only when they have no other choice. We thus consider in this section the economic and political determinants of the decision to undertake such a consolidation.

We estimate a panel logit model where the dependent variable is equal to one during years of fiscal adjustment and zero otherwise. We include a standard set of macroeconomic variables

¹¹ The first-stage model for predicting treatment probability is based on a logit specification as in Table 4, column 10.

such as lagged growth (t-1 and t-2) in one specification, and the lagged output gap (t-1 and t-2) in the second specification, terms of trade, inflation, and the current account balance. We also include fiscal determinants of a consolidation, such as the lagged debt level and the fiscal balance. We consider a dummy that picks up whether a country is under an IMF agreement, and to account for the implementation of past fiscal actions, we include a variable that counts the number of previous fiscal adjustments. Finally, among the political variables we include a dummy for the year with executive elections.

Table 4 presents the results from this exercise. To capture initial macroeconomic conditions, Panel (a) uses growth and panel (b) a measure of the state of the business cycle, namely the output gap. It is important to stress that, by construction, fiscal consolidation episodes in the narrative method are not correlated with *current* or *prospective* economic conditions, but of course may be influenced by *past* events.¹² Indeed, we find that lagged values of the output gap or GDP growth are significant determinants of fiscal consolidation. In particular, moving from a closed output gap to an extreme recession (top 1 percent) increases the likelihood of a fiscal consolidation by more than 40 percent. The probability reaches 1 in more extreme recessions, as governments have no choice but to consolidate (Figure 5). With respect to other determinants of fiscal consolidations, we observe that higher debt levels and deficits are positively correlated with the probability of observing a fiscal adjustment. Moreover, fiscal adjustments are less likely during executive election years.¹³ In particular, an election year reduces the likelihood of fiscal adjustment by 30 percent on average.

¹² As shown by the literature on narrative fiscal episodes (Guajardo et al., 2014), the data is consistent with this assumption. Contemporaneous values of growth/output are never significant. These results are available upon request.

¹³ This result for a longer time period, and with a different data source, fits Hallerberg and Scartascini (2017).

Table 4. Probability of a Fiscal Adjustment (fixed effects)

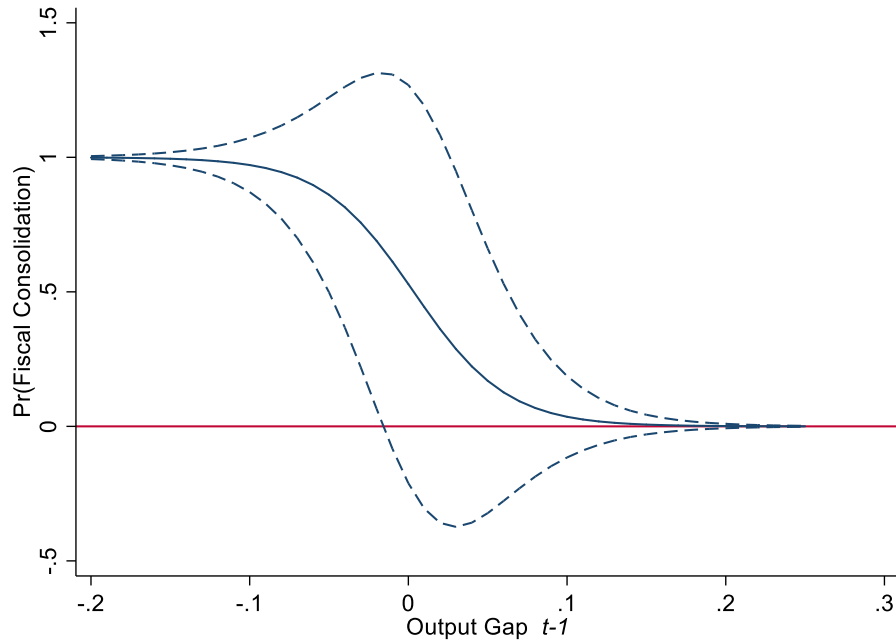
| Panel A | | | | | | | | | | |
|-----------------------------------|----------------------|----------------------|-----------------------|-----------------------|-----------------------|----------------------|----------------------|---------------------|---------------------|----------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| GDP Growth (t-1) | -11.088** (4.522) | -11.046** (4.700) | -14.323*** (5.018) | -15.392*** (5.137) | -15.392*** (5.137) | -14.944** (6.370) | -11.597* (6.832) | -12.398* (7.293) | -10.118 (7.679) | -12.682 (7.955) |
| GDP Growth (t-2) | -4.598 (4.500) | -4.271 (4.675) | -8.154 (5.181) | -9.089* (5.243) | -9.089* (5.243) | -9.582 (7.247) | -6.583 (7.652) | -8.700 (8.150) | -6.035 (8.430) | -4.945 (8.657) |
| Terms of Trade | | 0.006 (0.013) | 0.003 (0.013) | 0.005 (0.014) | 0.005 (0.014) | -0.002 (0.019) | -0.007 (0.020) | -0.010 (0.021) | -0.010 (0.020) | -0.006 (0.021) |
| Terms of Trade (t-1) | | -0.003 (0.018) | -0.003 (0.018) | 0.000 (0.018) | 0.000 (0.018) | 0.015 (0.029) | 0.016 (0.032) | 0.022 (0.034) | 0.027 (0.034) | 0.020 (0.038) |
| Terms of Trade (t-2) | | 0.003 (0.013) | 0.004 (0.013) | 0.000 (0.013) | 0.000 (0.013) | -0.007 (0.020) | -0.006 (0.022) | -0.007 (0.024) | -0.002 (0.025) | 0.005 (0.029) |
| Inflation (t-1) | | | -0.190 (0.168) | -0.192 (0.173) | -0.192 (0.173) | -13.489** (5.359) | -11.522** (5.762) | -8.101 (5.642) | -11.478* (6.218) | -13.251* (6.960) |
| Current account (t-1) | | | | -7.222 (5.703) | -7.222 (5.703) | -9.040 (8.251) | -3.408 (10.635) | -1.745 (10.475) | 1.865 (10.842) | 7.434 (11.939) |
| Debt level (t-1) | | | | | | 3.780** (1.765) | 5.260** (2.065) | 3.885* (2.106) | 4.547** (2.148) | 5.696** (2.224) |
| Structural primary balance (t-1) | | | | | | | -0.378*** (0.136) | -0.354** (0.138) | -0.336** (0.142) | -0.386** (0.165) |
| IMF program | | | | | | | | 0.266 (0.650) | 0.210 (0.676) | 0.533 (0.717) |
| Number of previous consolidations | | | | | | | | | -0.532 (0.347) | -0.570 (0.363) |
| Election | | | | | | | | | | -2.467*** (0.891) |
| Observations | 364 | 351 | 351 | 351 | 351 | 230 | 215 | 204 | 204 | 203 |
| Number of countries | 13 | 13 | 13 | 13 | 13 | 12 | 11 | 11 | 11 | 11 |

| Panel B | | | | | | | | | | |
|-----------------------------------|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| GDP Gap (t-1) | -16.217*** (5.663) | -16.544*** (5.939) | -16.544*** (5.939) | -19.658*** (6.331) | -24.495*** (6.947) | -16.887* (10.001) | -21.830** (11.101) | -26.469** (11.788) | -31.305** (13.183) | -34.124** (13.900) |
| GDP Gap (t-2) | 10.256* (5.358) | 10.709* (5.585) | 10.709* (5.585) | 13.013** (5.953) | 13.924** (6.028) | 17.299** (8.066) | 8.934 (9.231) | 7.827 (9.671) | 3.817 (9.883) | 7.346 (10.362) |
| Terms of Trade | | 0.003 (0.013) | 0.003 (0.013) | 0.000 (0.013) | 0.002 (0.013) | -0.002 (0.019) | -0.014 (0.022) | -0.020 (0.022) | -0.026 (0.023) | -0.021 (0.023) |
| Terms of Trade (t-1) | | -0.001 (0.018) | -0.001 (0.018) | -0.000 (0.018) | 0.005 (0.018) | 0.021 (0.028) | 0.021 (0.032) | 0.031 (0.035) | 0.041 (0.035) | 0.032 (0.039) |
| Terms of Trade (t-2) | | 0.004 (0.013) | 0.004 (0.013) | 0.005 (0.013) | 0.001 (0.013) | -0.012 (0.019) | -0.004 (0.022) | -0.006 (0.024) | 0.006 (0.025) | 0.012 (0.029) |
| Inflation (t-1) | | | | -0.137 (0.149) | -0.134 (0.148) | -14.173*** (5.310) | -10.013* (5.808) | -6.409 (5.472) | -9.896* (5.999) | -11.269 (6.867) |
| Current account (t-1) | | | | | -11.220* (6.253) | -9.540 (9.096) | -6.714 (11.350) | -6.543 (11.323) | -5.184 (11.773) | -0.018 (13.235) |
| Debt level (t-1) | | | | | | 4.933** (2.107) | 4.615** (2.345) | 2.867 (2.350) | 2.305 (2.386) | 3.306 (2.612) |
| Structural primary balance (t-1) | | | | | | | -0.480*** (0.147) | -0.492*** (0.156) | -0.484*** (0.159) | -0.527*** (0.174) |
| IMF program | | | | | | | | 0.336 (0.645) | 0.180 (0.699) | 0.563 (0.750) |
| Number of previous consolidations | | | | | | | | | -0.746** (0.347) | -0.756** (0.369) |
| Election | | | | | | | | | | -2.482*** (0.899) |
| Observations | 364 | 351 | 351 | 351 | 351 | 230 | 215 | 204 | 204 | 203 |
| Number of countries | 13 | 13 | 13 | 13 | 13 | 12 | 11 | 11 | 11 | 11 |

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

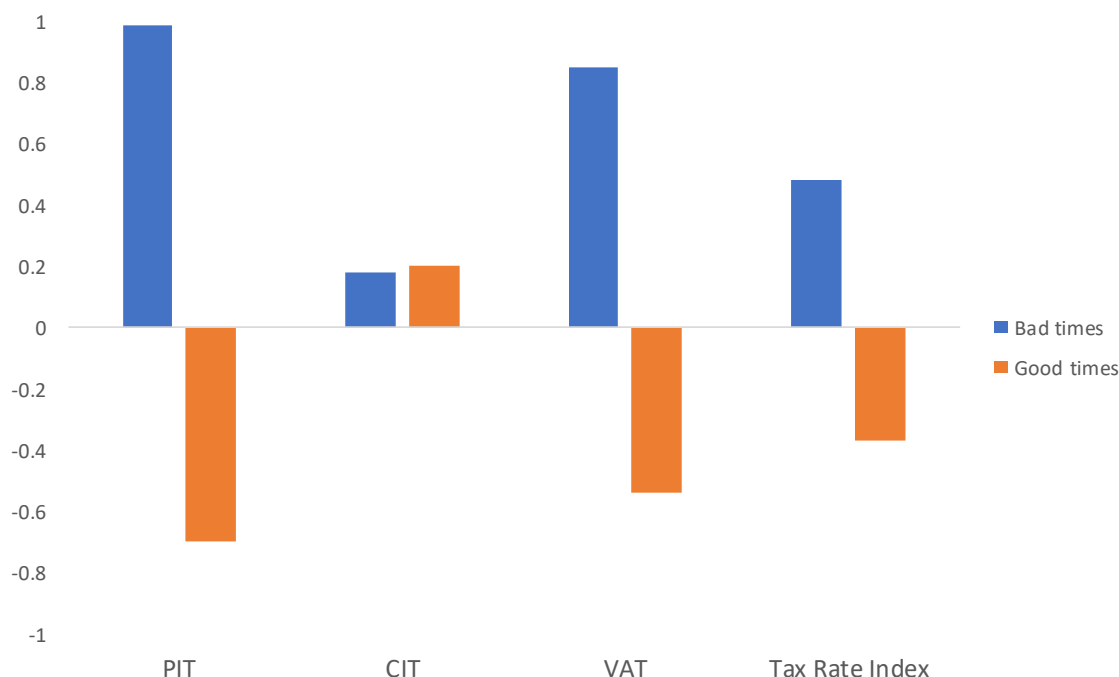
Figure 5. Probability of Fiscal Consolidation as Function of Output Gap



So far, we have shown that fiscal consolidations are introduced when past economic conditions have deteriorated sharply. As described in Section 2, tax-based consolidations are predominant in Latin America. To dig deeper into the policy choices under different economic scenarios, we now evaluate the behavior of tax rate changes at different points in the business cycle. Drawing on the Végh and Vuletin (2015) dataset on tax rates, Figure 6 shows the average percentage tax rate change for the personal income tax (PIT), corporate income tax (CIT), and the VAT in good times and bad times across 15 LAC countries.¹⁴ Changes are computed as the difference with respect to the overall mean, such that positive (negative) changes indicate tax rates changes above (below) the mean.

¹⁴Good (bad) times are defined as years for which the output gap is above (below) the 25th (75th) percentile for each country. The sample includes Argentina, Barbados, Bolivia, Brazil, Chile, Colombia, Costa Rica, Dominican Republic, El Salvador, Honduras, Jamaica, Mexico, Paraguay, Peru, and Uruguay

Figure 6. Percentage Tax Rate Changes across Different Stages of the Business Cycle in LAC



With the exception of the CIT, LAC countries tend to reduce taxes during good times on average. In contrast, policymakers increase all types of taxes during bad times. Indeed, the tax rate index¹⁵ falls during good times and increases during bad times, suggesting tax policy is procyclical and tends to reinforce the multiplier effect of fiscal contractions. This procyclical bias is contrary to the behavior observed across OECD countries, where tax policy is mostly acyclical on average (Végh and Vuletin, 2015). Evaluating voters' preferences on these policy choices at the micro level is the goal of our final empirical section.

7. Citizens' Preferences on Fiscal Consolidation Instruments

Our discussion so far has concentrated at the macro cross-country level. To complement our findings with individual-level data, we draw on the IDB-LAPOP 2017 survey to look at preferences regarding fiscal adjustment instruments. For this survey more than 6,000 people in seven Latin American capitals (Chile, Colombia, Honduras, Mexico Panama, Peru, and Uruguay)

¹⁵ This index is constructed as the average change in the different tax rates, weighted by the contribution of each tax to total tax revenues (Végh and Vuletin, 2015).

were interviewed. The purpose of the survey was to gather information regarding preferences on different public policies across the population, including fiscal policy. The survey was designed by the IDB and administered by LAPOP at the beginning of 2017 (for more details see Keefer, Scartascini, and Vlaicu, 2018).

Among the questions included in the survey, an experiment randomly divided the universe of respondents into two groups using a vignette design. One group was presented with a hypothetical case where the economy was in a recession and the government was running short on revenues (we will refer to them as treatment 1 group). The other group was presented with a scenario in which the economy was experiencing an expansion and the government was enjoying more revenues than expected (we will refer to them as treatment 2 group).¹⁶ In each case, respondents were asked to decide about their preferred fiscal response. In the economic crisis situation, respondents were asked to evaluate the following courses of action: mainly increase taxes, reduce public employment spending, reduce public investment, or divide the burden equally among them. In the economic expansion scenario, the options were parallel: rely mainly on tax cuts, increase public employment, or increase public investment. Box 1 (Appendix) presents the different scenarios that were provided to respondents. Given our interest in distinguishing between taxes and spending preferences in general, a choice to increase/reduce public employment or increase/reduce public investment will be treated equivalently as a preference to increase/reduce public expenditures.

To evaluate the effect of the scenarios we estimate the following model:

$$Y_{i,j} = \alpha + \beta T_i^1 + X_i \lambda + \eta_j + \varepsilon$$

where Y is the dependent variable or policy choice made by the respondent (taxes, spending). T is the treatment (recession), and X is a vector of controls. We include as control variables gender, age, education, employment status, type of occupation, neighborhood characteristics, income level (quintiles), health insurance, pension plan, being beneficiary of a government transfer, political knowledge, as well as country fixed effects. Table 5 presents results from the model.

¹⁶ As shown by Table A6 (Appendix) individual characteristics are balanced across both treatment groups.

Table 5. Treatment Effects on Probability of Support for Fiscal Policy Instruments (OLS)

| <i>Dependent Variable:</i> | <i>Tax measures</i> | | | <i>Spending measures</i> | | |
|------------------------------|----------------------|----------------------|----------------------|--------------------------|----------------------|----------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treatment 1 | -0.068*** (0.023) | -0.074*** (0.023) | -0.074*** (0.023) | 0.058** (0.022) | 0.070*** (0.023) | 0.071*** (0.023) |
| Female | | -0.024*** (0.008) | -0.025*** (0.008) | | -0.033* (0.016) | -0.035** (0.016) |
| Age | | 0.001** (0.000) | 0.001** (0.000) | | 0.002*** (0.000) | 0.002*** (0.000) |
| Education | | -0.002 (0.002) | -0.002 (0.002) | | 0.008*** (0.002) | 0.008*** (0.002) |
| Assets index | | -0.030 (0.030) | -0.077** (0.032) | | 0.016 (0.050) | -0.036 (0.047) |
| Salaried employee | | 0.010 (0.016) | 0.004 (0.017) | | 0.016 (0.028) | 0.008 (0.028) |
| Salaried employee (informal) | | -0.013 (0.022) | -0.015 (0.022) | | -0.055 (0.035) | -0.045 (0.035) |
| Public employee | | -0.003 (0.020) | -0.017 (0.019) | | -0.010 (0.026) | -0.017 (0.029) |
| Independent | | -0.006 (0.013) | -0.008 (0.014) | | -0.035 (0.023) | -0.037 (0.023) |
| Retired | | 0.007 (0.017) | -0.008 (0.017) | | -0.013 (0.041) | -0.023 (0.045) |
| Unemployed | | 0.011 (0.018) | 0.011 (0.017) | | -0.007 (0.031) | -0.005 (0.032) |
| Paved roads | | 0.015 (0.013) | 0.011 (0.014) | | -0.032 (0.022) | -0.025 (0.023) |
| Income Quintile | | 0.001 (0.005) | 0.006 (0.005) | | 0.008 (0.006) | 0.014** (0.005) |
| HH Head | | 0.007 (0.011) | 0.008 (0.011) | | 0.006 (0.012) | 0.008 (0.012) |
| Health Insurance | | 0.022 (0.014) | 0.024* (0.014) | | 0.002 (0.018) | -0.002 (0.018) |
| Pension plan | | -0.015 (0.013) | -0.019 (0.012) | | 0.053** (0.021) | 0.032 (0.020) |
| Govt. subsidy | | 0.020 (0.018) | 0.011 (0.019) | | 0.025 (0.026) | 0.011 (0.024) |
| Knowledge | | -0.003 (0.004) | -0.004 (0.004) | | -0.019*** (0.006) | -0.016*** (0.005) |
| Constant | 0.161*** (0.018) | 0.174*** (0.041) | 0.232*** (0.041) | 0.331*** (0.013) | 0.225*** (0.050) | 0.190*** (0.044) |
| Observations | 6,040 | 5,031 | 5,031 | 6,040 | 5,031 | 5,031 |
| Controls | N | Y | Y | N | Y | Y |
| Country Fixed effects | N | N | Y | N | N | Y |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

As shown in columns 1-3, support for taxation as a fiscal consolidation tool declines significantly during an economic crisis: citizens are about 7 percentage points less likely to prefer increasing taxes in recessions, which represents a 44 percent decrease with respect to the reference group.¹⁷ In contrast, columns 4-6 show that support for expenditure adjustment increases in the context of a recession: citizens are between 6 and 7 percentage points more likely to prefer cutting either public employment or delaying public investments, which represents an 18 percent increase relative to the reference group. Both results point to the same conclusion: individuals are highly reluctant to accept fiscal consolidations that require increases in taxes, and they seem more willing to accept fiscal consolidations that require adjustment of spending. In particular, given the choice of how to consolidate during crisis, voters are more than four times more likely to prefer an adjustment through a reduction in public expenditures than they are of solving the problem through a tax hike.

Average effects may hide important heterogeneity, such as the role of socio-economic status in shaping preferences for different consolidation instruments. We have a simple expectation in terms of the effects of income—voters would prefer that others pay the costs of the adjustment. In this logic, the key question is “who pays what” policy instrument, to then determine who would favor or oppose the policy. In the survey experiment, voters are confronted with three policy choices that mimic the typical instruments chosen by politicians during fiscal consolidations. The first is a tax on goods paid by the general population (such as the VAT), and the other two are different public spending categories: public employment and investment. Given the nature of the tax in question, a choice for a tax-led fiscal adjustment means lower income households would bear a disproportionate share of the cost of fiscal consolidation. In terms of the spending choices, government employment is a benefit that confers more to middle- and upper-income individuals, who are more likely to have the skill sets to be either potential or actual government employees.¹⁸ Investment spending is a type of public good that benefits voters across the income spectrum, and whose cuts tend to be less “visible” than cuts in current expenditures such as public wages or

¹⁷ See Figure A2 (Appendix) for evidence on cross-country heterogeneity regarding these results.

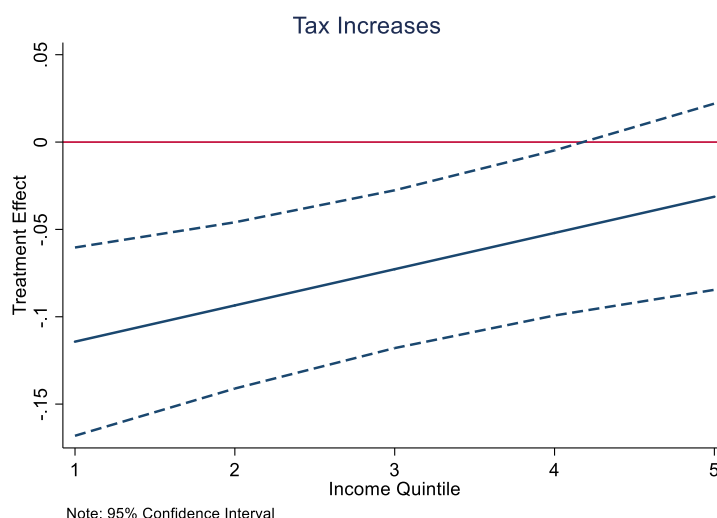
¹⁸ In our survey data, 80 percent of individuals who report being employed in the public sector do not belong to the bottom 40 percent of the income distribution.

transfers (Vergne, 2009; Katsimi and Sarantides, 2012; Gupta, Liu and Mulas Granados, 2016), so its distributive impact is not as clear cut.¹⁹

To test whether opposition to tax increases and support for expenditure cuts (either jobs in the public sector or public investments) varies by income level, we include an interaction term between treatment status and household income.²⁰ We find a clear distributional component to our results. As shown by Figure 7, treatment effects are stronger among lower-income individuals, who seem relatively more averse to using taxes as a policy measure for fiscal consolidation and more willing to accept spending cuts during recessions. This effect dissipates as income increases both in terms of tax increases and spending cuts. This fits the simple argument that voters prefer that others pay for the costs of fiscal consolidation.

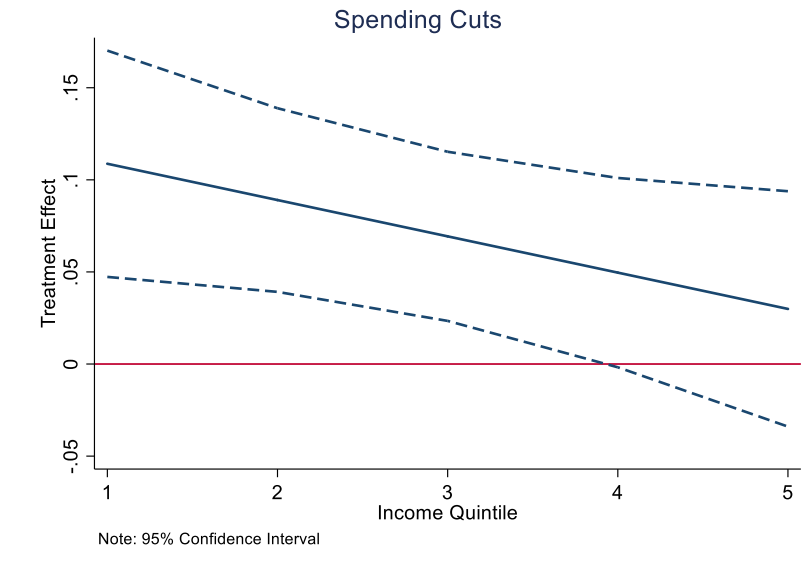
Taken together, the micro evidence suggests that individuals particularly dislike the use of taxes as a consolidation policy tool, especially during recessions. Yet, as we indicated earlier in the paper, fiscal consolidations in LAC are tax based and more likely to occur at the worst of times. This section provided micro-level evidence that complements the broader macro-level results on electoral turnover and the share of incumbent support—voters do not prefer the type of consolidation policies that governments are most likely to introduce.

Figure 7. Treatment Effects by Income Quintile



¹⁹ Given our focus on tax-based versus spending-based fiscal consolidations, we do not explore variation within spending categories in this paper, although it would be worthwhile to do so in future studies.

²⁰ Household income is a categorical variable that we collapse into five groups of equal size, or quintiles.



8. Conclusion

Contrary to a large and influential literature based on advanced economies where fiscal consolidations do not seem to affect election results, we find fiscal consolidations to be politically costly in Latin America. To explain this result, we look at the way governments adjust, and we find that voters punish governments when fiscal consolidations are tax-based. Why, then, do governments engage in these types of reforms? The most important reason is that governments face constraints in terms of the instruments they have available. OECD economies cut spending in about a quarter of the cases in social benefits, about a fifth of the time they cut subsidies, and about the same proportion of the time they cut employee compensation. On the tax side, there are several options as well, including increases in the personal income tax, property taxes, the corporate income tax and indirect taxes. In Latin America, in contrast, the range of options is narrower. There are fewer cuts in public employment, and cuts in public investment do not appear to contribute enough to bring public finances back on a sustainable path. Instead, governments are most likely to increase the VAT and other indirect taxes, which can immediately generate significant revenue for the government but also affect broad segments of the population. As a result, they suffer electorally from this decision, and are more likely to lose the next election. Our survey experiment provides one mechanism for this result—voters are more likely to prefer expenditure-led than tax-

led fiscal consolidations, with opposition to tax increases being most prevalent among the most vulnerable.

Our results may have important policy implications. First, they suggest that efforts to invest in fiscal capacity could be useful to expand the policy toolkit available to LAC governments, allowing politicians to fine-tune fiscal consolidations and avoid large negative electoral repercussions. Moreover, to ameliorate procyclical fiscal biases that exacerbate the negative macroeconomic effects of fiscal consolidations, countries could consider strengthening their rules-based fiscal frameworks, thus allowing automatic stabilizers to work during good times and provide room to maneuver when it is most needed—i.e., during recessions.

Secondly, our findings speak to current debates about the design of fiscal adjustments in emerging economies. Many LAC governments increased public expenditures during the years of the commodity boom and now face less favorable external conditions, resulting in sizable fiscal adjustment needs to stabilize debt-to-GDP ratios (Celasun et al., 2015; Powell, 2015). Is it possible to develop a fiscal adjustment strategy without affecting the welfare of the poor? Our results point to a clear “yes” because there are various sources of expenditure inefficiency that can be tackled for substantial savings (Izquierdo, Pessino and Vuletin, 2018). Greater efficiency in three key components of public expenditure—namely, the wage bill, transfers, and public procurement—could save as much as 4.4 percent of GDP (Izquierdo, Pessino and Vuletin, 2018).²¹ In this context, it is no surprise that in our survey results, among those citizens that chose to concentrate the consolidation effort in either one of two policy instruments (taxes, spending) during recessions, 80 percent chose to reduce government expenditures. Thus, seeking greater efficiency in public spending could be one way of making fiscal consolidations more consistent with the expectations and preferences of the majority of voters across the region.

Beyond these policy implications, our results also speak to current debates on the workings of democracy in Latin America. There are justified concerns about the chain of accountability that runs from voters to governments, and governments to policies in Latin America. Kaplan (2013) observes that fiscal austerity seems to be imposed in Latin American countries in similar ways

²¹ As shown by Izquierdo et al. (2018), wage differentials between the public and private sector—particularly for low-skilled workers—are on average close to 25 percent, meaning that a worker in the public sector earns one quarter more than his or her counterpart in the private sector. Moreover, several transfers, including cash transfers and non-contributory pensions—which in principle should go to the poor—end up favoring the non-poor. Finally, as much as 17 percent of acquisitions in public purchases could reflect either improper pricing or outright corruption.

regardless of which political party is in power. Lupu (2016) takes this argument a step further and contends that this behavior undermines the brand of parties because parties do not do what they promised in the campaign once they enter office. Our survey experiment indicates that voters know what they want when a consolidation is decided, and they punish governments that do not deliver what they expect. This dynamic could have far-reaching consequences. Implementing fiscal consolidations with unpopular policy tools could further reduce already low levels of trust in government in the region (Keefer, Scartascini and Vlaicu, 2018), providing a breeding ground for populist electoral shifts (Keefer, Scartascini and Vlaicu, 2019). Further research is needed to understand the effects of austerity policies on broader political outcomes of interest, such as electoral volatility, fragmentation, and the emergence of new parties.

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Appendix

Table A1. Summary Statistics Baseline Model

| Variable | Obs | Mean | Std. Dev. | Min | Max | Source |
|----------------------------|-----|------|-----------|-------|--------|--|
| ideology change | 273 | 0.42 | 0.49 | 0.00 | 1.00 | Database of Political Institutions (DPI) |
| Fiscal Consolidation dummy | 224 | 0.48 | 0.50 | 0.00 | 1.00 | Devries et al. 2011; Alesina et al. 2017; David and Leigh 2018 |
| Fiscal Consolidation size | 224 | 0.99 | 1.65 | -0.90 | 9.73 | Devries et al. 2011; Alesina et al. 2017; David and Leigh 2018 |
| GDP growth | 265 | 0.03 | 0.03 | -0.07 | 0.14 | IMF WEO 2018 |
| Unemployment | 262 | 0.00 | 0.02 | -0.06 | 0.18 | IMF WEO 2018 |
| Inflation | 260 | 0.64 | 7.09 | -0.52 | 108.95 | IMF WEO 2018 |
| Years in office | 263 | 9.33 | 10.96 | 1.00 | 71.00 | Database of Political Institutions (DPI) |
| Allhouse | 266 | 0.25 | 0.43 | 0.00 | 1.00 | Database of Political Institutions (DPI) |
| Coalition | 273 | 0.54 | 0.50 | 0.00 | 1.00 | Database of Political Institutions (DPI) |

Alternative Measure of Fiscal Consolidation

To check whether our findings are driven by choice of the independent variable, we introduce an alternative measure of fiscal consolidation based on the estimation of structural (or cyclically adjusted) primary balances, a typical measure of the fiscal stance in policy discussions.²² These data remove cyclical fluctuations from two main sources: output and commodity prices, and allows us to expand somewhat the sample size in our analysis by introducing countries for which data on “narrative” fiscal consolidations is not available.²³ Table 4 introduces the accumulated change in the structural primary balance over the term preceding the election, as percent of potential GDP, with positive (negative) values indicating fiscal consolidations (expansions). With both the ideological change and party change specifications of the dependent variable, we obtain similar results. Based on coefficients from Model 2, an improvement in the structural primary balance equivalent to moving from the 25th percentile to the 75th percentile in the distribution of fiscal adjustments increases the likelihood of a government change by more than 16 percent.

²² For example, see IMF Fiscal Monitor (various editions) and IMF WEO Reports (various editions) for discussion of country’s fiscal policy developments and outlook based on such a measure.

²³ For details regarding the methodology to estimate structural primary balances in 20 Latin America and Caribbean countries, see Ardanaz et al. (2016).

Table A2. Alternative Measure of Fiscal Consolidation

| | (1) | (2) | (3) | (4) | (5) | (6) |
|----------------------------|------------------------|---------------------|---------------------|-----------------------|-----------------------------|----------------------|
| <i>Dependent variable:</i> | <i>Ideology change</i> | | <i>Party change</i> | | <i>Incumbent vote share</i> | |
| Structural fiscal balance | 0.208** (0.098) | 0.264** (0.126) | 0.152* (0.092) | 0.232** (0.115) | -0.015** (0.007) | -0.011 (0.007) |
| GDP growth | 7.776 (11.819) | 15.269 (15.536) | 14.410 (12.838) | 27.267 (21.272) | -1.690 (1.023) | -1.126 (0.980) |
| Unemployment | 32.648 (24.953) | 48.435 (30.509) | 37.866 (25.667) | 67.079* (34.360) | -1.692 (2.039) | -1.314 (1.893) |
| Inflation | -7.888 (5.579) | -3.726 (6.959) | -2.901 (5.524) | -0.814 (7.346) | 0.255 (0.409) | -0.171 (0.388) |
| Terms of Trade | -0.011 (0.028) | -0.023 (0.038) | -0.017 (0.028) | -0.010 (0.039) | 0.001 (0.002) | 0.000 (0.002) |
| Current account | 6.033 (9.295) | 11.513 (11.581) | 20.276* (10.730) | 32.545* (16.626) | -1.338* (0.767) | -1.034 (0.709) |
| Gini | 11.719 (7.203) | 11.180 (10.379) | 17.735** (7.152) | 29.320** (11.557) | -1.262** (0.541) | -0.995* (0.529) |
| Years in office | | 0.044 (0.029) | | 0.031 (0.025) | | -0.001 (0.002) |
| Allhouse | | -2.125** (1.060) | | -1.874** (0.883) | | 0.091* (0.054) |
| Coalition | | -0.399 (0.729) | | | | 0.056 (0.045) |
| System tenure | | -0.018 (0.028) | | 0.024 (0.028) | | -0.002 (0.002) |
| Volatility | | 3.543* (2.002) | | 1.837 (1.892) | | -0.326*** (0.115) |
| Constant | -6.411* (3.550) | -6.975 (5.206) | -8.877** (3.490) | -15.741*** (5.751) | 1.010*** (0.259) | 0.963*** (0.260) |
| Observations | 75 | 67 | 75 | 68 | 70 | 67 |

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Sensitivity to Outliers

To test whether our findings are sensitive to outliers, Table A3 presents estimates from (a) a trimmed sample drops the largest and smallest 1 percent of the fiscal policy changes (columns 1 and 2), and (b) a winsorized sample in which values smaller (larger) than the 1st (99th) are replaced by the bottom and top 1 percent of the distribution, respectively (columns 3 and 4). We conclude that our results are not driven by influential outliers.

Table A3. Sensitivity to Outliers

| | (1) | (2) | (3) | (4) |
|----------------------|---------------------|----------------------|---------------------|----------------------|
| | Trimmed sample | Winsorized sample | | |
| Fiscal consolidation | 0.634** (0.316) | 1.150** (0.461) | 0.646** (0.309) | 1.150** (0.461) |
| GDP growth | 14.737 (12.319) | 13.488 (17.119) | 14.849 (12.320) | 13.490 (17.119) |
| Unemployment | 10.387 (26.741) | 29.005 (31.362) | 10.525 (26.830) | 29.011 (31.361) |
| Inflation | 0.203* (0.113) | 0.366 (0.613) | 0.203* (0.113) | 0.366 (0.613) |
| Terms of Trade | 0.005 (0.031) | -0.009 (0.047) | 0.005 (0.031) | -0.009 (0.047) |
| Current account | 19.188 (12.831) | 22.714* (13.562) | 19.398 (12.798) | 22.717* (13.560) |
| Gini | 13.988* (7.793) | 22.154* (12.008) | 14.142* (7.752) | 22.157* (12.007) |
| Years in office | | 0.013 (0.027) | | 0.013 (0.027) |
| Allhouse | | -1.612 (1.044) | | -1.612 (1.044) |
| Coalition | | -1.830** (0.917) | | -1.830** (0.917) |
| System tenure | | 0.010 (0.027) | | 0.010 (0.027) |
| Volatility | | 2.210 (2.223) | | 2.211 (2.223) |
| Constant | -8.060** (3.918) | -11.923** (5.777) | -8.143** (3.895) | -11.924** (5.776) |
| Observations | 64 | 56 | 65 | 57 |

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table A4. Fixed Effects (baseline model)

| VARIABLES | (1) ideoch | (2) ideoch | (1) ideoch | (2) ideoch |
|----------------------|--------------------|--------------------|--------------------|--------------------|
| | dummy | | size | |
| Fiscal consolidation | 1.262** (0.641) | 1.909** (0.855) | 0.651* (0.370) | 1.451* (0.766) |
| GDP growth | 8.096 (12.724) | 8.695 (18.844) | 11.675 (13.510) | 13.465 (23.745) |
| Unemployment | -4.349 (27.895) | 12.101 (34.531) | 13.188 (29.131) | 49.029 (43.293) |
| Inflation | 0.228 (0.168) | 0.488 (3.542) | 0.223 (0.168) | 4.651 (13.042) |
| Terms of Trade | 0.016 (0.035) | 0.009 (0.055) | 0.020 (0.034) | 0.015 (0.060) |
| Current account | 18.824 (13.485) | 24.001 (17.252) | 14.744 (13.000) | 19.132 (16.754) |
| Gini | 26.941 (17.842) | 41.154 (28.880) | 28.241 (17.630) | 36.345 (26.348) |
| Years in office | | 0.012 (0.042) | | 0.035 (0.041) |
| Allhouse | | -2.011* (1.165) | | -2.675* (1.449) |
| Coalition | | -1.380 (1.002) | | -1.259 (0.964) |
| System tenure | | -0.078 (0.081) | | -0.131 (0.100) |
| Volatility | | 5.808 (5.101) | | 9.951 (7.403) |
| Constant | | | | |
| | 64 | 52 | 64 | 52 |
| Number of countries | 11 | 10 | 11 | 10 |

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

**Table A5. Government Forms and Fiscal Consolidations:
Equality of Means Test**

| | Difference* | Std. error | t-value |
|------------------------|-------------|------------|---------|
| Absolute Majority | 0.08 | 0.06 | 1.30 |
| Coalition | -0.05 | 0.07 | -0.69 |
| Govt. stability>Median | 0.08 | 0.07 | 1.18 |
| Right wing | 0.05 | 0.07 | 0.68 |

*** p<0.01, ** p<0.05, * p<0.1

*Difference computed between episodes of fiscal consolidation and rest of observations.

Table A6. Balance Test

| | N | Sample Mean | SD | Treatment 1 (T1) | Treatment 2 (T2) | Difference T1-T2 | t-value |
|------------------------------|------|-------------|--------|------------------|------------------|------------------|---------|
| Female | 6040 | 0.498 | 0.500 | 0.490 | 0.506 | -0.016 | -1.265 |
| Age | 6038 | 40.141 | 16.702 | 40.290 | 39.995 | 0.294 | 0.684 |
| Education | 5997 | 11.212 | 4.089 | 11.190 | 11.234 | -0.044 | -0.412 |
| Assets index | 6040 | 0.579 | 0.182 | 0.578 | 0.581 | -0.003 | -0.588 |
| Salaried employee | 6000 | 0.187 | 0.390 | 0.189 | 0.185 | 0.003 | 0.322 |
| Salaried employee (informal) | 6000 | 0.068 | 0.251 | 0.070 | 0.065 | 0.005 | 0.747 |
| Public employee | 6000 | 0.048 | 0.213 | 0.046 | 0.049 | -0.002 | -0.396 |
| Independent | 6000 | 0.213 | 0.409 | 0.220 | 0.206 | 0.014 | 1.327 |
| Retired | 6000 | 0.070 | 0.255 | 0.074 | 0.066 | 0.008 | 1.146 |
| Unemployed | 6000 | 0.116 | 0.320 | 0.114 | 0.118 | -0.004 | -0.522 |
| Paved roads | 6015 | 0.831 | 0.375 | 0.835 | 0.828 | 0.007 | 0.735 |
| Income Quintile | 5106 | 2.928 | 1.405 | 2.938 | 2.918 | 0.020 | 0.516 |
| Head of HH | 6040 | 0.500 | 0.500 | 0.511 | 0.489 | 0.022 | 1.699 |
| Health Insurance | 6040 | 0.828 | 0.377 | 0.830 | 0.827 | 0.002 | 0.216 |
| Pension plan | 6040 | 0.283 | 0.450 | 0.275 | 0.290 | -0.016 | -1.366 |
| Govt. subsidy | 6040 | 0.076 | 0.266 | 0.074 | 0.078 | -0.004 | -0.572 |
| Knowledge | 6040 | 2.522 | 0.964 | 2.525 | 2.519 | 0.006 | 0.234 |

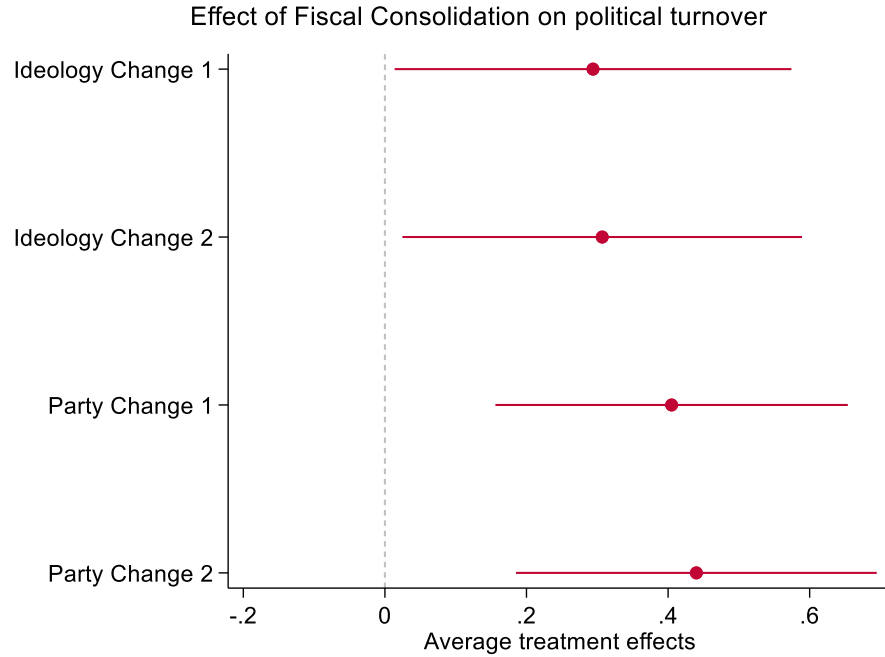
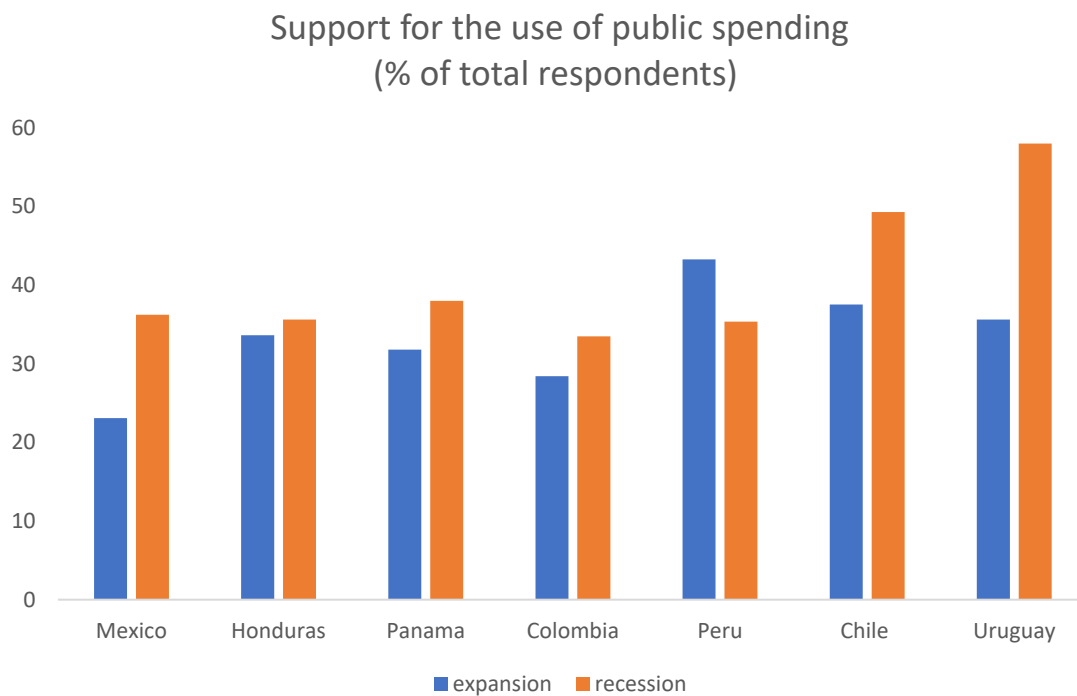
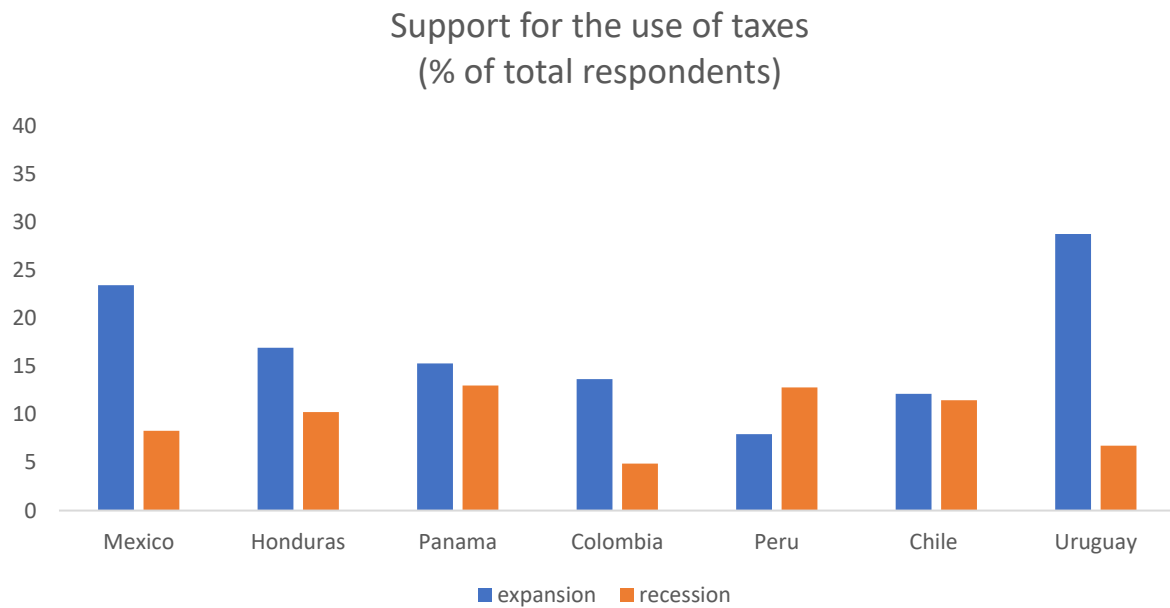
Figure A1. Average Treatment Effects from AIPW Estimator

Figure A2. Preferences on Taxes and Public Spending by Country



BOX: Randomized Vignettes

Vignette 1: Suponga que el país experimenta una crisis y el gobierno no tiene los ingresos que esperaba. Ante la crisis, el gobierno evalúa las siguientes opciones: despedir empleados públicos en educación, seguridad, y la administración pública; postergar inversiones en infraestructura (carreteras, agua y saneamiento); aumentar los impuestos en todos los productos que compra la gente. ¿Cuál de estas alternativas prefiere usted? **[Leer alternativas]**

- Cubrir la falta de recursos mayormente con el despido de empleados públicos
- Cubrir la falta de recursos mayormente postergando inversiones
- Cubrir la falta de recursos mayormente aumentando los impuestos
- Cubrir la falta de recursos con las tres opciones por igual

Vignette 2: Suponga que el país experimenta un boom económico y el gobierno tiene más ingresos de los que esperaba. Las cosas que puede hacer con estos ingresos son : contratar a más empleados públicos en educación, seguridad, y la administración pública, gastar ese dinero en inversiones en infraestructura (carreteras, agua y saneamiento), y bajar los impuestos en todos los productos que compra la gente. ¿Cuáles de estas alternativas prefiere usted que el gobierno haga con estos ingresos no esperados? **[Leer alternativas]**

- Gastar el dinero extra mayormente en la contratación de empleados públicos
- Gastar el dinero extra mayormente en inversión
- Usar el dinero extra mayormente bajando los impuestos que paga la gente
- Usar el dinero extra por igual en las tres opciones