On the Tax Morale Effects of Irregular Presidential Transitions*

Pedro A. Cabra-Acela†
University of California, Los Angeles

Bruno Jiménez‡
New York University & CEDLAS

February 8, 2026

Abstract

We study how irregular presidential transitions affect tax morale. Our analysis leverages administrative tax records and a difference-in-differences design exploiting geographic variation in electoral support for ousted Peruvian President Pedro Castillo. We find that tax revenues in pro-Castillo districts fell by up to 16% in the quarter following the impeachment. This decline coincides with a persistent drop in presidential approval, short-run reductions in trust in the police, and increased reports of racial discrimination following the repression of protests. Effects are absent during a routine left-to-right handover and persist after excluding protest districts, highlighting the distinctive consequences of democratic disruptions.

Keywords: Tax Morale, Public Finance, Impeachments.
JEL Codes: H26 , J15, P49.

*We thank Thomas Fujiwara, Natalia Guerrero, Almudena Valle, Jill Conte, Daniel Vaquero, Ignacio Lunghi, Sebastian Sardón, Pablo Zárate, Nicolás Urdaneta, German Orbegozo, Arielle Bernhardt, Ellora Derenoncourt, Gianmarco León-Ciliotta, Alberto Bisin, Raquel Fernandez, Ricardo Perez-Truglia, Leopoldo Fergusson, Juliana Londoño-Vélez, Giuliana Silva, and seminar participants at Princeton University, New York University, Institute for Humane Studies, and LACEA for their valuable comments and suggestions. Brayan Condori provided outstanding research assistance. All opinions, errors, and omissions are our own. This research was supported by the Institute for Humane Studies (grant no. IHS019014) and the Graduate School of Arts and Sciences at New York University.
†Contact: pacabra@g.ucla.edu
‡Contact: btj228@nyu.edu

1 Introduction

Over the last two centuries, democratic disruptions often occurred through overtly violent episodes such as military coups or civilian-led self-coups. Since the 2000s, they increasingly unfold through constitutionally sanctioned channels, including amendments to electoral rules and, most prominently, presidential impeachments (Bermeo, 2016). Even when lawful, these episodes remain irregular because they disrupt the electoral cycle and generate uncertainty about institutional stability. Despite the well-documented benefits of a healthy democracy and inclusive representation for economic growth, public health, and tax revenues (Acemoglu et al., 2019; Fujiwara, 2015; Acemoglu et al., 2015), the economic consequences of these constitutional disruptions remain understudied in both economics and political science. In particular, impeachment episodes can reshape citizens' trust in and approval of the state, with potentially important downstream effects and economic costs.

The literature has long documented that citizens' trust in the state is central to welfare and to the functioning of fiscal systems. In particular, non-pecuniary determinants such as trust in government, perceived legitimacy, and political alignment shape tax morale and tax compliance (Giaccobasso et al., 2025; Cabra-Acela, 2023; Cullen et al., 2021; Torgler, 2003; Scholz and Lubell, 1998; Webley, 1991). What remains less understood is whether irregular presidential transitions, even when constitutional, shift these determinants in ways that translate into changes in tax morale and tax collection. Therefore, this paper asks whether irregular transitions shape citizens' willingness to pay taxes.

Peru provides a clear setting to study the consequences of irregular transitions: the impeachment of President Pedro Castillo. His 2021 presidential campaign mobilized rural, Indigenous, and working-class voters through a grassroots left-wing platform. Racial and ethnic tensions were present from the outset and remained a defining feature of his presidency. His rise was perceived by many elites as a threat to Peru's longstanding racial and class hierarchies. As a political outsider, he faced sustained media hostility, reinforcing existing class and ethnic cleavages. Despite this, he won a narrow victory, prompting the opposition to challenge the result with unsubstantiated claims of fraud. After these claims were dismissed, Congress initiated several impeachment attempts, invoking allegations of misconduct under the charge of "moral incapacity."[1] Hours before a third impeachment vote, he attempted to dissolve Congress in what many interpreted as a self-coup.[2] Congress rejected the move and impeached him, after which he was arrested.[3]

Vice President Dina Boluarte assumed office and quickly distanced her government from Castillo's platform. She replaced key officials and clashed with left-leaning governments abroad.[4] Many supporters interpreted this shift as an abrupt political realignment away from the coalition that brought Castillo to power, while others regarded it as the natural course of action given the circumstances. Protests followed and were met with violent repression, leaving approximately 70 dead, mostly from Indigenous communities, and hundreds injured (Amnesty International, 2023). This sequence of events made questions of legitimacy, representation, and state authority highly salient, creating a setting in which an impeachment could plausibly translate into shifts in trust and fiscal compliance.

We embed the impeachment episode within a difference-in-differences framework. Specifically, we compare changes in outcomes between districts where Castillo won the first round of the presidential election and districts where he did not, before and after the impeachment. Our primary outcomes capture local tax collection. We also examine mechanisms that may mediate these fiscal effects by testing whether the impeachment leads to changes in government approval, trust in institutions, and self-reported experiences of racial discrimination.

[1] Moral incapacity (*incapacidad moral*) is a constitutional ground in Peru permitting Congress to remove a sitting president deemed morally unfit for office. While originally intended for cases of mental incompetence, it has evolved ambiguously, allowing Congress substantial discretion to impeach presidents over broad ethical or political allegations.

[2] A *self-coup* occurs when a sitting executive seeks to remain in power by unlawfully dissolving or bypassing constitutional checks such as the legislature, the courts, or the constitution itself.

[3] Castillo was sentenced to 11 years in prison for conspiracy to commit rebellion and acquitted of other charges; the defense has appealed the ruling.

[4] Tensions escalated, resulting in Mexico imposing visa requirements on Peruvian travelers and vetoing Boluarte's assumption of the temporary presidency of the Alianza del Pacífico.

The Castillo–Boluarte transition, and thus the strategy described above, bundles at least three elements: a shift from a left-leaning to a right-leaning executive, a frictional transition shaped by widespread protests and repression, and a non-electoral (irregular) transition. Accordingly, we leverage variation in the data and complementary exercises to assess whether the estimated effects are driven primarily by ideological turnover and protest-related violence, or instead are most consistent with the irregular nature of the transition playing a central role.

Our main finding is that, following the impeachment, tax revenues in the treatment group fell by 16.3%, driven primarily by property taxes and concentrated in the first quarter after the transition. Implementing the same design for Peru's most recent regular left-to-right presidential transition yields no comparable effect. Moreover, re-estimating the main specification after excluding districts most exposed to protest-related violence (e.g., locations with killings or large crowds) produces estimates of similar magnitude. Together, these results suggest that the decline in tax revenues is not explained by ideological turnover or protest activity alone, and is instead most consistent with the irregular nature of the transition shaping the link between political attitudes and tax compliance.

We then investigate the mechanisms underlying this result. Motivated by the fact that Castillo's support base, and those most affected by the violent repression of protest activity, were disproportionately rural and Indigenous, we focus on trust in the armed forces and the police and on perceived racial discrimination as indicators of the strength of state–citizen ties. The evidence is consistent with erosion along both channels. Trust in the police and armed forces declines by 3.1 and 4.5 percentage points, respectively, representing 13.1% and 10.9% of the pre-impeachment control-group mean. At the same time, reported experiences of racial discrimination increase by 76.8% relative to baseline levels. These changes are concentrated in the first quarter after the impeachment, precisely at the time when police violence was at its peak, and are not observed in the benchmark analysis using a regular presidential transition.

Next, we turn to nationally representative household surveys on living standards and views about the government to further unpack the mechanisms behind our main result. We document a sharp and sustained drop in government approval: the impeachment reduced presidential approval by 12.2 percentage points, equivalent to 40.1% of the pre-impeachment mean. The decline is specific to the presidency and does not extend to subnational levels of government. Once again, this finding does not replicate in the setting of a routine presidential transition. Moreover, we show that respondents in the treatment group do not shift their preferences away from democracy, ruling out the possibility that the episode shifted respondents away from democratic preferences. Taken together, these results are consistent with citizens reducing tax contributions when they perceive the national executive as less representative of their interests.

We further assess the robustness of our interpretation in two ways. We show that trust in institutions unrelated to the impeachment (e.g., the Department of Education, the Catholic Church, among others), as well as discrimination based on non-politicized dimensions (e.g., gender, sexual orientation, age), remain unaffected, suggesting that our results are not driven by generalized discontent. We also document larger declines in approval among rural residents, agricultural workers, racial minorities, and those who report feeling closely tied to their Indigenous community. Notably, government disapproval is also statistically significant among non-minorities and urban residents. This pattern is consistent with the idea that individuals update their beliefs not only based on their own interactions with the government, but also on how they see the state treat others.[5]

Moreover, we provide evidence against an explanation based on sexist bias toward Peru's first female President. Presidential approval also falls in areas with low gender gaps in wages and employment, and in districts with low incidence of intimate partner violence. This pattern strengthens an interpretation in which the effects operate through the irregular nature of Castillo's removal rather than gender-based bias.

Finally, we further assess the role of the irregular nature of the transition by ruling out three po-

[5]This type of social multiplier effect has been documented in several contexts (Glaeser et al., 2003; Fujiwara et al., 2016; Jiménez, 2023).

tential confounders. First, satisfaction with government programs, both food and non-food assistance, remains unchanged, as does the reported incidence of corruption in interactions with government officials. Second, we find no declines in labor income or household expenditure in the treatment group relative to the control group. Third, we find no evidence of reduced tax enforcement capacity, as measured by municipal employment, tax-collection personnel, and the presence of delinquent-prosecution units. Therefore, alternative explanations based on reduced public service quality, depressed economic activity, or weakened enforcement capacity are inconsistent with the data.

All of our findings are robust to sensible changes in our identification strategy. For example, they retain their qualitative interpretation when we replace our treatment dummy with a continuous measure of first-round support for Pedro Castillo. The interpretation of our results also remains unchanged when we include district-months with zero tax collection, using a Poisson quasi–maximum likelihood estimator, following Chen and Roth (2024).

We also document that both groups followed similar trajectories prior to the impeachment across a wide range of outcomes. In particular, treatment and control groups not only evolved similarly in terms of tax contributions, but also in their views toward the government, institutions, and experiences of discrimination. These patterns strengthen the credibility of our identification strategy. Furthermore, our results remain qualitatively unchanged when we incorporate province-specific time trends, which account for unobserved province-specific dynamics.

We argue that the Peruvian case offers a valuable setting for studying the broader consequences of irregular democratic transitions. It represents an instance of a successful impeachment of a leader with substantial mass support. Similar episodes have occurred recently across a range of democracies. For instance, in the United States, Brazil, and South Korea, presidential impeachments or serious attempts were characterized by intense political polarization, unresolved questions about presidential wrongdoing, and strong reactions from segments of the population. Our study speaks directly to these and other similar experiences.

Moreover, our case study offers notable practical advantages. First, we assemble a dataset in which the outcome and treatment are measured at the same geographic level: district-level administrative records of local tax revenues and district-level presidential election results. Second, following (Ajzenman et al., 2023), we use district-level election returns as a proxy for individual political preferences. Districts are the smallest administrative unit in Peru [6] and the most granular level at which both official election results and tax collection data are consistently available. This feature allows us to proxy political preferences and measure fiscal behavior at the same level.

Throughout the paper, we argue that this is not only a reasonable proxy, but it is also a compelling source of identifying variation given the high degree of political polarization in Peru. In districts where Castillo won, he received an average of 36% of the vote, compared to only 12% in districts where he lost, an exceptionally sharp contrast.[7] This polarization allows us to construct a credible comparison between supporters and non-supporters before and after the impeachment, closely approximating an ideal quasi-experimental design.

Our findings bridge the literatures on tax morale and political disenfranchisement. The tax morale literature emphasizes that individuals' willingness to comply with taxes depends on reciprocity-based motivations toward the state (Luttmer and Singhal, 2014). Empirical evidence consistently supports this view: survey studies document that trust in government and legal institutions is positively associated with self-reported tax compliance (Scholz and Lubell, 1998; Torgler, 2003), while laboratory experiments find that individuals with negative perceptions of government are more likely to evade taxes (Webley, 1991). Further, Cabra-Acela (2023) integrates intrinsic and extrinsic incentives for tax compliance and highlights how the effectiveness of enforcement and reward policies depends on the interaction between these two dimensions. In particular, when the intrinsic cost of evasion is low—such as when citizens feel increasingly detached from the state—tax evasion is likely to rise even in the presence of pecuniary incentives.

[6] With an average population of about 18,000 (35 million Peruvians divided by 1,874)

[7] These are remarkably high vote shares given that the presidential race featured 18 candidates.

In short, tax morale captures intrinsic, non-pecuniary motivations to comply with the state, a point well established in the literature. A large body of evidence shows that taxpayers' contributions depend on perceptions of fairness, representation, and political attitudes toward authorities (Feld and Frey, 2002; Giaccobasso et al., 2022; Cullen et al., 2021; Ajzenman et al., 2024; Giaccobasso et al., 2025). The study most closely related to ours is Cullen et al. (2021), who find that partisan alignment during routine political cycles in the United States reduces tax evasion. We complement this work by focusing on an irregular rather than a routine presidential transition. We show that this distinction is crucial: the effects of irregular transitions are orders of magnitude larger. In our setting, routine presidential transitions have no discernible impact on tax collection, whereas an impeachment induces a large and immediate decline in property tax revenues.

Our focus on property taxes connects to a recent strand of the tax morale literature that emphasizes this type of contribution (Nathan et al., 2025; Del Carpio, 2013; Cabral and Hoxby, 2012). Property taxes are a particularly informative instrument to study tax morale. In Peru, compliance is neither universal nor negligible, leaving scope for behavioral responses (Del Carpio, 2013). Liabilities are based on property size and cadastral values, making them largely invariant to business cycle conditions. They are also salient: unlike income taxes, they are not withheld automatically but paid consciously. Indeed, this is one of the few tax categories that involve a direct transfer from individuals to the government. Finally, although municipalities can perfectly observe compliance, they prosecute only a small share of delinquents (Del Carpio, 2013). The room for noncompliance with minimal risk of punishment, coupled with their saliency, makes property taxes particularly suitable in our context.

Our finding of decreased property tax contributions directly complements Nathan et al. (2025), Del Carpio (2013), and Cabral and Hoxby (2012). While Del Carpio (2013) stress the role of social norms—how contributions depend on peers' compliance—we focus instead on citizen–government relationships. We also complement Nathan et al. (2025), who emphasize extrinsic motives such as savings and filing frictions.[8] We show that intrinsic motivation also matters. Finally, Cabral and Hoxby (2012) demonstrate the role of salience, another extrinsic factor: people dislike handing over cash to the state for no immediate return. By contrast, our results highlight a non-pecuniary channel, where irregular political transitions undermine reciprocity-based intrinsic motivation to comply.

Recent studies on the erosion of citizens' trust in the state examine how individuals respond to abhorrent acts of targeted government violence. For example, Ang et al. (2021) document declining willingness to cooperate with the police in the aftermath of the murder of George Floyd. Even after an observed increase in gunshots, 911 calls declined across several cities in the US. In another example, León-Ciliotta et al. (2025) show that state-mandated forced sterilizations in Peru had persistent negative effects on the use of government-provided medical services. Notably, there are serious allegations—supported by survey evidence—that these sterilizations were racially targeted, disproportionately affecting Indigenous women (Rendon, 2020). Our work provides evidence of yet another channel of civilian resistance: reduced willingness to finance the state.

Finally, we broaden our understanding of the consequences of removing government officials from office. To our knowledge, Artiles et al. (2021) is the only existing study in the Economics literature that focuses on impeachments. Our work differs primarily in scope: while they examine how the removal of sub-national authorities affects the selection of politicians, we study how presidential impeachments shape individual attitudes toward the government and influence market interactions between individuals. Still, our results share substantial common ground. Artiles et al. (2021) show that sub-national impeachments reduce the representation of Indigenous groups among local authorities. Similarly, we find that the events we study carry significant racial undertones: individuals report feeling more racially discriminated against following the removal of their leader and the violent repression they face when protesting the impeachment.

In sum, our findings link political disenfranchisement and public finance. An irregular presidential transition that some citizens perceive as the removal of a rightful representative disrupts their relationship with the state. This disruption weakens reciprocity-based intrinsic motives, erodes trust in the government, and heightens perceived discrimination. Together, these changes translate into lower

[8] Strictly speaking, they show that these factors shape households' choice to protest or accept their tax bill.

tax compliance.

The remainder of the manuscript is structured as follows. Section 2 provides background information. Sections 3 and 4 describe the data and our identification strategies. In Section 5, we present the main results and mechanisms. Section 6 tests the robustness of the results, and Section 7 concludes.

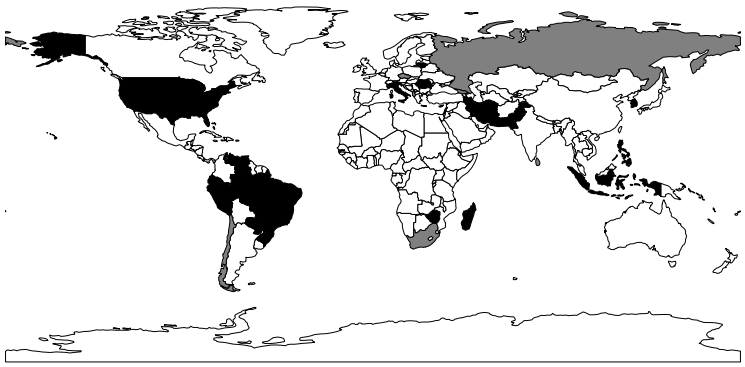
2 Background

2.1 Irregular Presidential Transitions and Tax Morale Across the World

Before turning to the Peruvian case study, we document a set of stylized facts on irregular presidential transitions and tax morale across countries. This global perspective serves two purposes. First, it establishes the broader relevance of the research question by showing that irregular transitions are neither rare nor idiosyncratic. Second, it motivates the Peruvian case as an informative benchmark: the patterns observed in Peru arise in a context that shares key institutional and political features with many other democracies, suggesting that the mechanisms we study are likely to generalize beyond a single country.

We begin by reporting the incidence of impeachments worldwide. In Figure 1, we present a global map of successful and attempted presidential impeachments from World War II through 2024. Over this period, a total of 16 countries experienced at least one successful impeachment, while 23 countries witnessed at least one attempt. That is, impeachments are a widespread and globally relevant phenomenon.

Figure 1: World Map of Impeachments and Attempted Impeachments Since World War II



Note: The figure displays the global incidence of impeachments and impeachment attempts, based on data from Wikipedia's list of impeachments of heads of state since 1945. Countries shaded in black experienced at least one event of the corresponding type. Grey-shaded countries had at least one attempt that did not succeed. White indicates no attempts or impeachment. A successful impeachment is defined as one in which the head of state is effectively removed from office (as in our case study), or one in which the head of state resigns in the course of an impeachment trial or attempt (e.g., Richard Nixon in the United States in 1974).

Our episode also bears some features of a (self) coup, an even more widespread mechanism for democratic erosion. Since the end of World War II, at least 31 countries have experienced a self-coup or an attempt, and 134 have witnessed at least one coup or attempted coup (see Figure B.2 in Appendix B). Taken together, these patterns suggest that irregular presidential transitions, broadly defined, are

a common feature of contemporary democracies.[9]

Episodes of irregular presidential removal, or attempted removal, have repeatedly triggered intense polarization and mass mobilization in both advanced and emerging democracies. In the United States, the impeachment proceedings against Donald Trump unfolded amid extreme partisan division, with limited cross-party consensus regarding the underlying allegations and sustained protests by both supporters and opponents. The second impeachment attempt, following the January 6, 2021, attack on the U.S. Capitol, illustrates how such contested processes can escalate into violent confrontation.

Comparable episodes occur elsewhere. In Brazil, the impeachment of Dilma Rousseff in 2016 was widely denounced by her supporters as a "parliamentary coup," a framing that closely parallels reactions in Peru following Pedro Castillo's removal. In both cases, the use of formal constitutional mechanisms was widely perceived as elite-driven and illegitimate, triggering large demonstrations for and against removal. In South Korea, the removal of Park Geun-hye, unanimously upheld by the Constitutional Court on clear corruption grounds, nonetheless generated large protests both demanding her ouster and defending her presidency. The Peruvian case thus shares core features with these episodes, including polarization, mass mobilization, and allegations of institutional abuse, placing it within a broader class of democratic crises rather than an isolated episode.

Next, we provide suggestive cross-country evidence linking tax morale to these types of institutional crises. Using cross-country survey data, we conduct before-and-after comparisons of attitudes toward tax evasion around episodes of presidential impeachment. We draw on the set of countries covered by the World Values Survey (WVS) and the Latinobarómetro (LB). We focus on countries with successful presidential impeachment episodes and available measures of tax morale both before the event and within one year afterward.[10]

Table 1: Changes in Tax Morale due to Presidential Impeachment.

Coefficient	(1)	(2)	(3)	(4)	(5)	(6)
After Impeachment	0.271***	0.130***	0.015	-0.100***	0.334***	0.124***
	(0.019)	(0.021)	(0.021)	(0.020)	(0.020)	(0.009)
Obs.	2,558	2,311	2,266	2,468	2,261	11,864
Country	Peru [Castillo]	Paraguay	Brasil	Peru [Fujimori]	Ecuador	All
Control Sample Mean	0.316	0.396	0.464	0.469	0.384	0.400
Control Sample SD	0.465	0.489	0.499	0.499	0.486	0.490

Note: This table reports before–after differences in the share of survey respondents who justify tax evasion to some extent following presidential impeachment episodes. Column (1) corresponds to Pedro Castillo's impeachment in Peru (2022), column (2) to Fernando Lugo in Paraguay (2012), column (3) to Dilma Rousseff in Brazil (2016), column (4) to Alberto Fujimori in Peru (2000), and column (5) to Lucio Gutiérrez in Ecuador (2005). Column (6) pools all episodes. Appendix A describes the data sources and the construction of the episode sample. Conventional standard errors are reported in parentheses. Significance:*** $p \leq 0.01$, ** $p \leq 0.05$ and * $p \leq 0.1$.

In some of the available waves, WVS and LB ask respondents how justifiable they consider tax evasion on a 1–10 scale, where 1 denotes "never justifiable" and 10 denotes "always justifiable." Table 1 reports the results and indicates a sizable deterioration in tax morale: the share of respondents who report that tax evasion is justified to some extent increases by 27 percentage points in Peru around the 2022 episode, with qualitatively similar patterns in Paraguay, Ecuador, and in the pooled sample.

These results suggest that the relationship between irregular presidential transitions and tax morale extends beyond the Peruvian case. However, drawing causal conclusions from cross-country evidence is not feasible due to key data limitations: tax morale questions are not consistently available across countries or time, and within-country geographic variation is often limited. For these reasons, we focus our main analysis on Peru, which offers rich identifying variation stemming from heterogeneous partisan views of the impeachment, as well as high-quality survey and administrative data. This setting allows us to study the effects of irregular presidential transitions on taxation, explore underlying mechanisms,

[9]Throughout the paper, we use the term irregular transitions to refer to any episodes that interrupt the expected electoral cycle, with a primary focus on presidential impeachments.

[10]Appendix A describes the data sources and the details on the events used.

and rule out alternative explanations.

2.2 Castillo's Presidency and Impeachment

In this section we outline the political events leading up to the impeachment and to explain how these may affect outcomes such as tax collection. While a full account of this complex sequence lies beyond the scope of the paper, we summarize the key elements most relevant for interpreting our identification strategy and results.

The 2021 Presidential Elections. Pedro Castillo emerged as a political outsider in the 2021 elections.[11] Mainstream news outlets largely ignored his candidacy, and most polling agencies predicted his vote share would remain below 5% just a few weeks before the ballot (Asensio, 2021). Some major media platforms did not even download his picture in preparation for the possibility of his election.[12] As a result, his campaign initially flew under the radar for many voters and analysts, particularly in urban areas that remained relatively isolated from the rest of the country due to the COVID-19 mobility restrictions that were still present in the first quarter of 2021.

Meanwhile, Castillo ran a successful campaign centered on smaller cities and rural areas, appealing primarily to rural and Indigenous communities. He adopted an identity-based strategy, emphasizing his background as a schoolteacher, his rural origins, experience as a union leader, and his ties to the *rondas campesinas*,[13] presenting himself as an honest leader with deep connections to ordinary Peruvians (González and Moscoso, 2021; Asensio, 2021). His political platform reflected this orientation. He proposed a constitutional reform, which he framed as addressing the roots of systemic inequality, alongside a restructuring of the pension system, the nationalization of key industries, land reform, and partial restrictions on foreign trade (Grompone and Jimenez, 2021). He also pledged to live frugally, refusing the presidential salary and continuing to receive his elementary school teacher's wage (Swissinfo.ch, 2021). Other observers have argued that his success also stemmed from highlighting stark disparities in living standards between Lima and rural Peru, which generated strong support in the latter regions (Pajuelo, 2021). In addition, he aligned himself with popular, though controversial, political figures such as Vladimir Cerrón and Antauro Humala.

Regardless of the reasons, Castillo's strategy proved successful enough to secure a spot in the second round of the presidential election. In the first round, held on April 11, 2021, his ticket, comprising himself, Vladimir Cerrón, and Dina Boluarte, received 18.92% of the vote, while the runner-up, Keiko Fujimori, received 13.41%.[14] As a result, the two advanced to the runoff election.

The results took several observers by surprise. For most, Pedro Castillo was not even considered a relevant player in the electoral landscape. Only a few weeks before the election did he begin to register non-negligible, albeit still very small, predicted vote shares in surveys of voting preferences (Asensio, 2021). So much so that, just weeks before the election, an interviewer mocked Castillo for having only 0.001% of the voting intention (O Salto, 2021).

Naturally, Castillo's rise to prominence as a viable presidential candidate attracted significant public attention. The opposition responded on several fronts. First, a smear campaign sought to associate him with foreign authoritarian leaders and domestic terrorist groups, claims largely unsubstantiated by neutral sources (Camacho and Sosa-Villagarcia, 2021). Second, critics highlighted Castillo's weak performance in interviews and debates, reflecting both limited media experience and modest formal education. Third, Keiko Fujimori's campaign positioned itself as a fiscally responsible alternative to Castillo's more radical economic proposals, such as closing Peru to foreign trade. Fourth, several prominent opinion leaders, including Nobel Laureate Mario Vargas Llosa (Redacción La Mula, 2021)

[11] These elections were routine; up to that point, all elections in the 21^{st} century had followed a regular schedule. They occur every five years, with the previous one held in 2016.

[12] CNN, in fact, used a blank silhouette when announcing him as the winner (La República, 2021*a*).

[13] A grassroots civilian movement created to prevent petty crime in small communities, which also collaborated with state forces against terrorism in the 1980s and 1990s.

[14] The remaining votes were divided among 16 other candidates.

and former officials such as Carlos Bruce (El Popular, 2021), reinforced a narrative of the election as a contest between an educated, stable option in Fujimori and a less prepared, impulsive one in Castillo.[15] This discourse was laced with elitism and racial undertones, and at times included explicit calls for violence. For example, another presidential candidate, Rafael López Aliaga, who would later become mayor of Lima, closed a campaign rally with the words: "Death to communism! Death to Cerrón! Death to Castillo!"

Castillo's rival, Keiko Fujimori, is a controversial figure herself. She is not only the daughter of Peru's last dictator, Alberto Fujimori, but also a firm ideological supporter of his regime, which is widely criticized for rampant corruption and political violence. Nevertheless, the results of the election held on June 6, 2021, were extremely close, with Castillo winning by less than one percentage point. At the same time, Fujimori's party and its allies secured a notable majority in Congress.

The opposition viewed the election results as an alarming and unexpected outcome. In response, they swiftly shifted their strategy, focusing on annulling approximately 200,000 votes from rural and Indigenous areas where Castillo had strong support (BBC News Mundo, 2021). This effort reportedly cost the Fujimori campaign over 1 million PEN (around 300,000 USD) (La República, 2021*b*). Several Lima-based law firms offered their services to expedite the challenge and seek the cancellation of the election results (Ojo Público, 2021). Despite investigations, no credible evidence of systematic manipulation was found, and the review of these "suspicious" votes was broadcast repeatedly on over-the-air television for several days.

This process may have deepened political polarization, as politicians and non-state actors openly sought to annul rural votes in an effort to seize power through a seemingly legal strategy, despite the absence of any credible supporting evidence.

Rise to power and impeachment. After the opposition's challenges were resolved through legal channels, Castillo was officially declared President of Peru on July 19, 2021. Vladimir Cerrón and Dina Boluarte had initially been slated to serve as vice presidents alongside Castillo; however, legal proceedings, related to a corruption case, rendered Cerrón ineligible to hold public office. Hence, if Castillo were to leave office for any reason, Dina Boluarte would become the (first female) President of Peru.

Segments of the opposition maintained their efforts, shifting from contesting ballots to pursuing removal through institutional channels. Congress attempted to impeach Castillo three times. The first attempt, in November 2021, invoked the constitutional clause of "moral incapacity," citing corruption-related controversies (CNN Español, 2022). The motion did not secure the 52 votes required to advance the process and therefore failed. A second attempt followed in March 2022, again citing corruption allegations. In this case, the process advanced to a vote, but the motion garnered 55 votes in favor, 54 against, and 19 abstentions, falling short of the 87 votes needed to remove Castillo from office.

Congress initiated a third impeachment attempt against Castillo in December 2022. On December 7, facing corruption investigations and the prospect of removal, he declared the dissolution of Congress and announced the creation of an emergency government. He also imposed a curfew and called for both a constituent assembly and new presidential elections (Infobae, 2022). Authorities and most observers described these moves as an attempted self-coup, which triggered his immediate impeachment. Castillo was removed from office and later tried for rebellion, abuse of authority, and serious disturbance of the public peace. He was ultimately convicted of conspiracy to commit rebellion and sentenced to 11 years in prison, while acquitted of the remaining charges; his defense continues to seek annulment of the sentence.

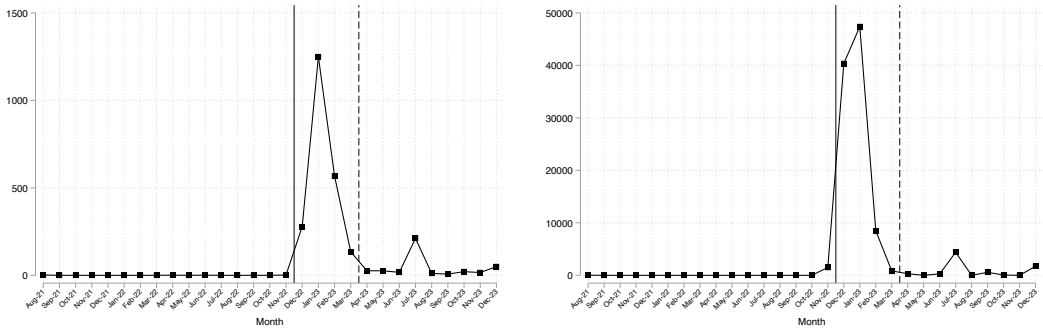
The Aftermath. After Castillo's removal, Vice President Dina Boluarte assumed the presidency.

[15] Some of the most striking remarks came from prominent figures such as Mario Vargas Llosa, who said of Castillo: "He seems profoundly unaware, consistent with his immense ignorance, that capital is necessary." He also questioned Castillo's fitness for office, stating: "That is something obvious to the vast majority of Peruvians, especially those in the cities, who are better informed than the rest." Carlos Bruce's comments were even more explicitly racialized. Referring to Castillo's health issues, he remarked: "It seems the abundant coastal oxygen has affected him." This comment alluded to Castillo's rural, highland origins and was widely interpreted as a classist and racist insult.

She immediately distanced herself from Castillo and initiated significant changes. Key officials were replaced, including the Prime Minister and the Minister of Economy and Finance. She also adopted a more confrontational foreign policy stance, particularly toward former allies such as Mexico, Argentina, and Colombia. Some observers suggest that Boluarte forged an informal alliance with Fujimorismo to remain in power until 2026,[16] a theory seemingly supported by the smooth approval of her first cabinet, which faced less opposition than any of Castillo's proposed cabinets.

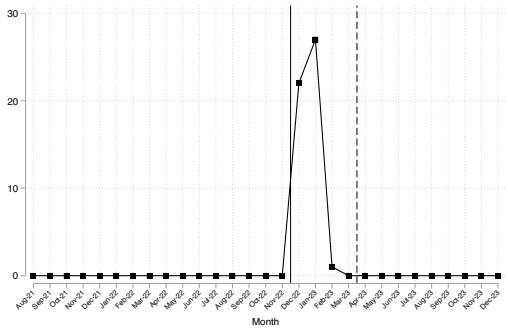
The backlash was immediate and widespread. Boluarte's rapid policy shifts signaled a stark ideological break from the elected administration of Pedro Castillo. Large-scale protests occurred across the country from December 7, 2022, through March 2023, with demonstrators blocking roads and occupying airports in several regions. Figure 2 plots time in protest intensity, measured through riots and demonstrations related to Castillo's removal. The data are drawn from the Armed Conflict Location and Event Data Project (ACLED) (Raleigh et al., 2023).

Figure 2: Time Trend in Event Counts, Crowd Size, and Casualties in Impeachment-Related Riots and Demonstrations



(a) Event Counts.

(b) Crowd Size.



(c) Number of Casualties.

Note: This figure displays the number of riots and demonstrations, crowd size, and number of casualties in impeachment-related events. We classify an event as impeachment-related if the event description includes the term "Dina Boluarte" or any of the following expressions: "removal of former president Castillo," "removal of Pedro Castillo," "pro-Castillo," "general elections," "new constitution," or "removal from office." The solid vertical line marks the timing of the impeachment, while the dashed line indicates the end of the associated protests. We elaborate on the treatment of these data in Appendix A. Source: Armed Conflict Location and Event Data (Raleigh et al., 2023).

Protest activity rose sharply following Castillo's removal. Both the number of events and the size of crowds increased markedly.[17] Security forces responded with violence: over 1,000 individuals were injured, and dozens of civilians and law enforcement officers were killed, including at least 10 minors. This pattern is visible in panel (c) of Figure 2. Indigenous communities accounted for roughly 80% of recorded civilian deaths (Amnesty International, 2023).

[16] See discussions on the notion of "Fujicerronismo."

[17] In Appendix A we describe these data in greater detail and show that the spike was large even relative to all other recorded events.

Summarizing, Pedro Castillo's rise and removal unfolded amid deep political polarization. The presidential race divided the country between a predominantly rural, left-leaning, and lower-income electorate and a more urban and affluent segment that tended to support conservative or authoritarian-leaning platforms.[18] Following the election, political power was contested in a protracted struggle: Castillo's supporters argued that they faced persistent efforts to undermine their electoral mandate, while the opposition framed its actions as a defense of democratic institutions. The repression of subsequent protests further intensified this volatile environment. As we show, the resulting irregular transition altered the relationship between citizens and the state, with tax compliance emerging as one of the domains where its effects are most evident.

2.3 Tax Collection in Peru

Our paper is focused on understanding the tax morale effects of irregular presidential transitions. Hence, an understanding of the relevant legislation is of paramount importance.

We focus on local tax revenues. Contributions to the central government through income or sales taxes are largely outside individual control, as these are typically withheld by employers or collected at the point of sale. This makes them less suitable for our research question.[19] Our analysis therefore concentrates on taxes governed by local legislation. Importantly, these are among the very few taxes that involve a direct and conscious transfer from individuals to the government. Aside from them, taxes on self-employment income are the only direct payments made by individuals; however, they are paid only by formal entrepreneurs. Given that formal employment represents roughly 28% of total employment and that only about 22% of formal workers are self-employed, these taxes are negligible and concern no more than about 6% of the workforce (Instituto Nacional de Estadística e Informática (INEI), 2025).

District-level taxes are governed mainly by *Decreto Legislativo Nº 776*, or the Law of Municipal Taxation, enacted in 2004. Article 6 establishes that property taxes are the most important source of municipal revenue, alongside the real estate transfer tax, the vehicle ownership tax, the betting tax, the gaming tax, and the tax on non-sporting public shows.

Article 8 of *Decreto Legislativo Nº 776* establishes that the property tax is levied on the value of both urban and rural properties. This provision makes this tax broad in scope, covering essentially all real estate holdings regardless of location.[20] Each real state unit has to pay taxes to the district where it is located. Article 10 clarifies that the amount due each year is determined as of January 1, according to the legal ownership of the property on that date.[21]

The same tax rates and schedules apply throughout the country, and, importantly, they did not change during our study period. Article 15 establishes the payment schedule. Taxpayers may either pay the full annual amount in a single installment, due by the last working day of February, or in up to four quarterly installments. In the latter case, the first quarter is due by the end of February and corresponds to one-fourth of the total tax liability, while the remaining installments are due by the last working day of May, August, and November. These are deadlines, however, as payments are made continuously throughout the year at dates chosen by taxpayers, so long as they occur before the corresponding deadline for each installment.

[18] The opposing candidate was the daughter of the former dictator who led the 1990s authoritarian regime. Her campaign invoked a promise to govern under a "demodura"—a Spanish wordplay combining *democracia* (democracy) and *mano dura* (iron fist) (BBC News, 2021). The term also echoes another expression blending *democracia* and *dictadura* (dictatorship).

[19] Although we expect no effect on these dimensions, we include them as a robustness check. The results and discussion are presented in Section 6.

[20] The law exempts, among others, government properties (except private concessions), properties of foreign governments and international organizations, and non-income-producing properties for purposes such as charities, hospitals, cultural heritage, religious entities, the Volunteer Fire Department, and some peasant or native communities.

[21] This means that the individual registered as the owner at the start of the year is liable for the entire year's property tax, regardless of whether the property is sold later in the year. In the case of a transfer, the new owner only assumes the status of taxpayer beginning on January 1 of the following year.

Under Decreto Legislativo Nº 776, the taxable base of the property tax is established by the taxpayer's sworn self-report, submitted each February (Art. 14). The applicable rates are progressive and fixed by law in Article 13: 0.2% on the portion of the value up to 15 tax units (UIT), 0.6% on the portion exceeding 15 UIT and up to 60 UIT, and 1.0% on the portion exceeding 60 UIT, brackets which municipalities are not authorized to modify. Importantly, municipalities typically maintain cadastral records of property area and value, which are updated through taxpayer self-reports and verified through municipal audits.[22]

Del Carpio (2013) provides a detailed account of how tax collection is operationalized. In the weeks preceding each deadline, municipalities post reminders and announcements on billboards and websites, and warn non-compliers of potential legal proceedings. By law, non-payment may result in the freezing of personal accounts or the repossession and auction of the debtor's property, depending on the outstanding balance. In practice, however, prosecutions are exceedingly rare: in Lima, Peru's largest city, with a population of roughly 11 million, only about 30 properties were auctioned following repossession in 2024 (Servicio de Administración Tributaria de Lima, 2024). More broadly, data on enforcement is scarce, but Del Carpio (2013) reports that even in high-capacity districts of Lima, only between 68 and 80 percent of debtors are ever prosecuted.

When a taxpayer falls behind on property tax payments and a formal collection process is initiated, the outstanding balance is typically increased by an additional charge—about 10% on average, though the exact rate varies across municipalities—to cover collection expenses. Some districts also grant tax amnesties that reduce accrued penalties to encourage taxpayers to settle overdue obligations. These amnesties may be introduced at any point in the fiscal year, which means that property tax payments can occur in virtually any month.

Notably, the Peruvian system for local taxation is particularly lax. While there are no official measure of the true extent of tax amnesties, they seem to be widespread and examples abound. For instance, in 2024 the wealthy district of La Molina introduced Ordenanza Nº 446/MDLM, granting up to 100% discounts on unsettled amounts of municipal taxes and penalties under its "beneficios de fiscalización tributaria" campaign. Similarly, in 2025 the rural district of Andabamba in Huancavelica launched its "Gran Campaña Amnistía Tributaria 2025", offering full condonation of interests and penalties to delinquent taxpayers. What is more, municipalities also resort to extensive debt rescheduling schemes. For example, Los Olivos has had an official system since 1997 that allows taxpayers to divide overdue municipal taxes into installments, setting the number of quotas and their amounts.

In sum, property taxes in Peru are only lightly enforced: a sizable share of debtors face no action at all, and even when fines are imposed, they are frequently pardoned or deferred through long-standing local amnesty policies.

2.4 Supporting Information.

Appendix B includes an overview of the presidential transition from Ollanta Humala to Pedro Pablo Kuczynski, which we use to benchmark the effects of *regular* presidential transitions on tax revenues. This episode represents the most comparable Peruvian case: a transfer of power from a left-leaning to a right-leaning administration carried out through a regular democratic process, without the political harassment, impeachment attempts, or violent repression seen in the Castillo–Boluarte episode. Since prior research shows that even routine presidential transitions can affect tax morale (Cullen et al., 2021), comparing our results to this episode highlights the distinct consequences of irregular ones. Moreover, Appendix B provides additional information on other types of municipal taxes, as well as business licenses.

[22] We do not examine evasion through fraudulent self-reports, as such information is difficult to obtain and, to our knowledge, is not centrally recorded.

3 Data

We use data from multiple sources covering August 2021–December 2023 for our main analyses, a window that begins with Castillo's inauguration and ends before subsequent political events could contaminate our estimates. These data provide both our outcome measures and auxiliary variables used to test mechanisms and rule out competing explanations.[23]

Our main data source are administrative records of local level tax collection. In particular, data come from Peru's Integrated System of Financial Administration (SIAF, from its Spanish acronym). This platform is managed by the Ministry of Economy and Finance and records the revenues and expenditures of all levels of government. For municipalities, it provides standardized information on property tax collections and other local sources of revenue, making it the most comprehensive administrative record of fiscal activity at the local level.[24]

We requested and obtained access to district-level monthly tax revenue data spanning January 2015 to December 2023. Our main analyses focus on the period from August 2021 to December 2023, while data from 2015–2016 are used to benchmark a routine presidential transition. Revenues are classified into five broad categories, each further disaggregated into subcategories that specify the exact tax type. For example, we can distinguish whether property tax revenues stem from transactions or from ownership.

We focus on a subset of the largest categories: property taxes, production and consumption taxes, and other tax revenues. We include taxes on international trade and mandatory contributions only when working with the total aggregate, but ignore them when performing disaggregate analysis. These are collected only in exceptionally rare instances and together represent around 0.2% of total revenues in the baseline (see Table 2), and less than 0.1% of of our observations have any information on these type of taxes. Estimating treatment effects for these categories is, therefore, either extremely imprecise or outright infeasible.

We aggregate our data so that each entry represents the amount of taxes of a particular type collected by a municipality in a given month.[25] Some district–month cells contain no information, and there is no documentation explaining why this occurs. We address this issue as follows. In our preferred approach, we assume that the missing districts collected zero taxes in those months. Since we apply a logarithmic transformation, these observations are dropped from the analysis. As a robustness check, we instead retain them using a poisson QMLE. Our results are not qualitatively sensitive to this choice.

Moving on, we begin by describing our data. Table 2 shows how tax revenues were distributed in 2022 before the impeachment.

Property taxes account for the vast majority of local tax revenues in Peru, representing over 90% of total collections. All other tax categories contribute only a negligible share. For this reason, the present work strongly focuses on property taxes.

We augment our data on local level tax collection with election outcome data, which we use to construct a measure of treatment exposure. Infogob, a website maintained by the Peruvian government, provides election results dating back to 1931. However, only the most recent election (2021) includes results disaggregated at the subnational level.

As we outlined in the previous sections, we are interested in benchmarking our results with respect to a routine presidential transition. That is, we aim to highlight the specific effects of presidential

[23] For example, Dina Boluarte's mid-2023 absence for cosmetic surgery only became public in 2024, when it emerged that she had taken two weeks off without formally delegating her duties.

[24] A small share of entries (0.31%) are negative values. Although no official documentation explains their meaning, the official who prepared the dataset at the Ministry of Economics indicated that these values correspond to tax returns, often arising from canceled transactions. We include these entries in our data as they are.

[25] If, after aggregation, the total tax collected remains negative, we mechanically drop the entry when applying the logarithmic transformation.

Table 2: Distribution of Tax Revenues by Type

Type of Tax	Share of Total Revenue
International Trade	0.08
Mandatory Contributions	0.13
Other Taxable Revenue	2.11
Production & Consumption	4.23
Property Tax	93.45

Note: The table reports the distribution of tax revenues across the five major tax categories for 2022, excluding the month of the event (December). Data are drawn from SIAF.

impeachments. To this end, we compare our main results to a regular transition that closely resembles our case study in all relevant dimensions—except for the fact that no impeachment occurred. This placebo is the presidential transition from Ollanta Humala to Pedro Pablo Kuczynski in 2016.[26] Because Infogob does not provide subnational election results prior to 2021, we rely on data from the National Office of Electoral Processes (ONPE), which reports results at the electoral table level. These can be aggregated to the district level using standardized district codes.

Next, our paper seeks to unpack the mechanisms behind the link between partisan alignment and tax morale. To that end, we complement our data with the 2021–2024 rounds of the Peruvian National Household Survey (henceforth ENAHO, by its Spanish acronym). These data are well suited for this purpose, as they contain detailed information on living standards, attitudes toward the government, views on democracy, and experiences of racial discrimination. We also observe the individual's district of residence and the month of the survey, which allows us to map each respondent into the treatment or control group. Thus, the unit of observation in these analyses is an individual living in a district during a given month. We also employ the 2015-2017 of these surveys for our benchmark analysis of a regular presidential transition.

These surveys have been conducted continuously by the National Institute of Statistics and Informatics since 1997. Each round consists of a nationally representative repeated cross-section of approximately 35 thousand different households per year. These data have been widely used in the economic literature to study a wide range of topics, from labor markets (Jiménez and Rendon, 2025, 2023; Sviatschi, 2022; Ñopo, 2008) to attitudes and interactions with the state (Jiménez, 2023; Hunt and Laszlo, 2012; Hunt, 2007). Only household heads respond to the module relating to attitudes and interactions with the state.

We are also interested in ruling out several alternative explanations, including one based on sexist bias toward Dina Boluarte, Peru's first female president. ENAHO does not contain the standard measures of gender attitudes used in the literature (e.g., Fortin, 2005). We therefore approximate local gender norms using complementary indicators, including the incidence of intimate partner violence from the 2022 round of the Peruvian Demographic and Health Survey (DHS). Additionally, we examine whether disruptions associated with the protests could account for our results. To this end, we draw on the Armed Conflict Location and Event Data (ACLED; Raleigh et al., 2023). Furthermore, to disentangle changes in tax morale from changes in tax enforcement capacity, we rely on a universal survey in which municipalities report both the number of workers they employ and the tasks those workers perform. We place particular emphasis on the number of workers exclusively dedicated to tax collection activities, as well as on the existence of units responsible for the prosecution of tax delinquents. These data come from the *Registro Nacional de Municipalidades* (RENAMU), Peru's national municipality registry. Finally, for the cross-country evidence on tax morale around impeachment episodes, we draw on the World Values Survey and Latinobarómetro. Further details on these auxiliary datasets are provided in Appendix A.

Table 3 provides a quantitative overview of our main data sources, SIAF and ENAHO. The table

[26] In Appendix B.1, we describe this transition in detail. To summarize briefly, it was the only recent transfer of power between a left-leaning and a right-leaning government.

reports means for several key variables, along with descriptive difference-in-differences estimates. These naïve estimates are constructed by first computing before-and-after differences within each group, and then taking the difference between treatment and control groups.

We carry out this exercise for a selected subset of variables—specifically, our main outcomes related to tax revenues, government approval, as well as selected measures of trust in institutions and experiences of discrimination.[27] We also report a set of demographic characteristics to describe our groups.

We highlight several features of Table 3. The naïve estimates show no significant effect of the impeachment on tax collection. However, as we later show, these estimates mask substantial temporal heterogeneity: the dynamics of the treatment effect closely track the sequence of political events described above. The entire relative drop in tax revenues occurs at the height of protest activity and dissipates once the protests subside. We return to this discussion in Section 5.

Next, we observe a large decrease in central government approval. Which amounts to roughly 50% of the baseline control mean. This results does not spillover to deeper layers of the government. We also see large decreases in trust in the police and the armed forces, which are not echoed in trust in other institutions that played no role in the events we study. The naïve diff-in-diff coefficients for trust in the National Identity Records or the Catholic Church are not statistically significant at conventional levels. A similar pattern emerges with reported experiences of discrimination. We find sizable increases in discrimination based on skin color and region of origin, while discrimination on the basis of gender and sexual orientation experience little to no change. This suggests that the results we may find later on are specific to a irregular presidential transition characterized by violent and targeted repression.

Lastly, we examine the demographic characteristics of respondents. These do not change in any quantitatively meaningful way, suggesting that differences between treatment and control groups remained stable over time. This supports the validity of our research design.

To close this section and complement the preceding analysis, Figure 3 plots raw trends in selected outcomes for the treatment and control groups. The solid vertical line marks the impeachment, and the dashed vertical line marks the end of the period of peak protest-related violence.

We find that the treatment and control groups followed similar trajectories before the impeachment, not only in their tax contributions but also in their views of government, trust in institutions, and experiences of discrimination. After the December 2022 impeachment, approval of the central government and self-reported discrimination diverge sharply across groups. These shifts are strongest during the protest period, and the decline in government approval persists thereafter. Trust in the police also diverges: it falls in the treatment group, while it increases slightly in the control group.

4 Empirical Strategy

4.1 Conceptual Framework

We study the political climate surrounding Castillo's election and subsequent removal. We refrain from making normative claims about these events, which involve unresolved legal disputes and inherently subjective moral or political judgments. This ambiguity is central to our identification strategy.

[27] The full set of outcomes includes more than 30 variables. Including all of them in a single table would significantly reduce readability. Appendix C presents the full set of estimates, including baseline means for the control group and adjustments for multiple hypothesis testing.

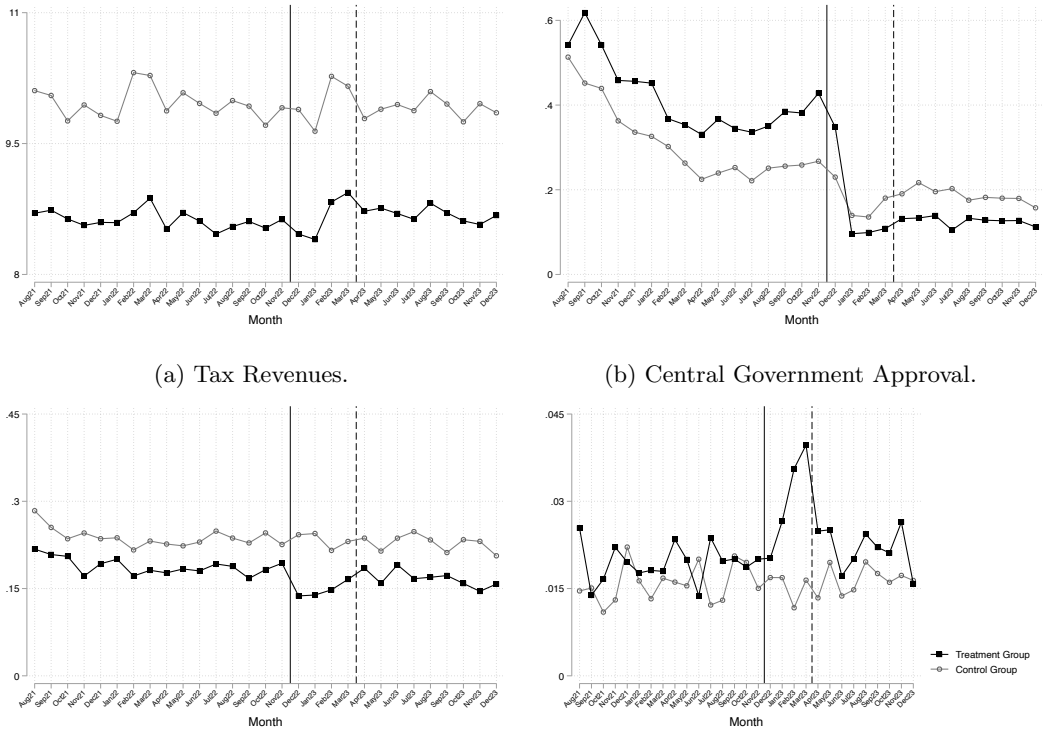
Table 3: Means of Selected Variables and descriptive Diff-in-Diff.

Variable	Castillo Supporters (Treatment Group)			Non-Supporters (Control Group)			Diff-in-Diff
	Before	After	Diff.	Before	After	Diff.	
a) Administrative Tax Data: SIAF							
Total Revenue	8.63	8.68	0.06	9.96	9.93	-0.03	0.08
	(2.46)	(2.50)	(0.04)	(2.80)	(2.89)	(0.05)	(0.06)
Property Taxes	8.60	8.66	0.06	9.93	9.90	-0.04	0.09
	(2.44)	(2.49)	(0.04)	(2.78)	(2.87)	(0.05)	(0.06)
Obs.	14,640	11,895		11,024	89,57		
b) Household Data: ENAHO							
Central Government Approval	41.22	13.60	-27.62	30.38	18.20	-12.18	-15.45
	(49.23)	(34.28)	(0.50)	(45.99)	(38.58)	(0.46)	(0.68)
Regional Government Approval	22.52	29.20	6.68	22.03	24.45	2.42	4.25
	(41.77)	(45.47)	(0.55)	(41.45)	(42.98)	(0.47)	(0.73)
Provincial Government Approval	25.16	34.36	9.20	23.59	28.74	5.14	4.05
	(43.40)	(47.49)	(0.56)	(42.46)	(45.25)	(0.48)	(0.74)
District Government Approval	29.06	41.25	12.19	25.80	34.36	8.56	3.63
	(45.41)	(49.23)	(0.56)	(43.75)	(47.49)	(0.49)	(0.74)
Trust in the Police	18.84	16.15	-2.70	23.78	22.99	-0.79	-1.91
	(39.11)	(36.80)	(0.42)	(42.57)	(42.08)	(0.44)	(0.61)
Trust in the Armed Forces	32.81	24.25	-8.56	41.21	37.25	-3.96	-4.60
	(46.95)	(42.86)	(0.51)	(49.22)	(48.35)	(0.51)	(0.73)
Trust in National Identity Records	49.16	46.74	-2.41	55.35	53.34	-2.01	-0.41
	(49.99)	(49.90)	(0.56)	(49.71)	(49.89)	(0.52)	(0.76)
Trust in the Catholic Church	45.51	41.62	-3.90	48.52	46.26	-2.26	-1.63
	(49.80)	(49.29)	(0.54)	(49.98)	(49.86)	(0.51)	(0.75)
Discrimination by Skin Color	1.94	2.45	0.52	1.58	1.62	0.04	0.48
	(13.78)	(15.47)	(0.16)	(12.48)	(12.62)	(0.13)	(0.20)
Discrimination by Origin	1.12	1.66	0.54	0.95	1.11	0.15	0.38
	(10.52)	(12.76)	(0.12)	(9.71)	(10.46)	(0.10)	(0.16)
Discrimination by Gender	0.61	0.40	-0.21	0.34	0.26	-0.08	-0.13
	(7.79)	(6.32)	(0.08)	(5.83)	(5.13)	(0.06)	(0.09)
Discrimination by Sexual Orientation	0.03	0.03	0.01	0.07	0.07	-0.00	0.01
	(1.59)	(1.78)	(0.02)	(2.73)	(2.70)	(0.03)	(0.03)
Mean Age	42.73	42.86	0.13	41.90	42.05	0.14	-0.02
	(19.63)	(19.68)	(0.13)	(19.26)	(19.28)	(0.11)	(0.17)
Males (%)	47.64	47.76	0.12	47.97	48.20	0.23	-0.11
	(49.94)	(49.95)	(0.33)	(49.96)	(49.97)	(0.29)	(0.44)
High School Graduates (%)	48.88	49.98	1.10	55.74	56.76	1.02	0.08
	(49.99)	(50.00)	(0.34)	(49.67)	(49.54)	(0.29)	(0.44)
Married (%)	27.77	26.82	-0.95	23.35	23.19	-0.16	-0.79
	(44.79)	(44.30)	(0.30)	(42.30)	(42.20)	(0.25)	(0.38)
Obs.	50,119	40,101		65,992	53,652		

Note: Observations collected before December 2022 are classified as belonging to the "Before" period. Standard deviations and standard errors are reported in parentheses. The standard errors for the first differences are obtained by regressing each variable on an indicator for the "After" period, separately for each group. The standard errors in the Difference-in-Differences (DiD) column come from a basic DiD regression without covariates—that is, they correspond to the coefficient on the interaction term $Post_t \times Castillo_d$ (see empirical strategy section). All standard errors in these naïve estimates are conventional. In panel (a), data on the log of total tax revenues and property taxes comes from administrative records of local level tax collection, SIAF. In panel (b) we use the full ENAHO sample.

The events can be interpreted through two opposing narratives. On one side, many perceive Castillo as a legitimate representative of rural and Indigenous Peru who was ousted after sustained political harassment by his opponents. This group also views the new president as a distant political figure who responded to protests with violent repression. On the other side are those who believe Castillo was an

Figure 3: Trends in Tax Revenues, Government Approval, Trust in the Police, and Discrimination by Skin Color by group: Castillo Supporters vs. Non-Supporters.



(a) Tax Revenues.

(b) Central Government Approval.

(c) Trust in the police.

(d) Discrimination by Skin Color.

Note: The figure plots trends in tax revenues, government approval, trust in the police, and experiences of skin-color discrimination by support for Castillo in the 2021 presidential election. Tax revenue data come from SIAF and are measured as the monthly average of log tax collections by group. Government approval, institutional trust, and discrimination measures are drawn from ENAHO. Government approval is defined as the monthly share of respondents rating the government's performance as good or very good; trust in institutions is the share reporting enough or a lot of trust in the police; and discrimination is the share reporting having experienced discrimination based on skin color in the past 12 months.

unqualified and authoritarian figure whose actions justified his removal; in this view, Congress acted to protect democracy, and the removal was considered justified and legal.

Under the first narrative, Castillo's election signaled the inclusion of groups long excluded from political power. His removal, followed by a violent crackdown that disproportionately affected rural and Indigenous protesters (Amnesty International, 2023), is interpreted as an institutional breach and an identity threat. Boluarte's subsequent policy reversal reinforced the perception that central institutions no longer represent minority interests. As a result, trust in the state declined, perceptions of discrimination increased, and discontent extended into other forms of political behavior.

These political shifts have fiscal consequences. The literature on tax morale highlights intrinsic motivations for compliance (Luttmer and Singhal, 2014; Cabra-Acela, 2023) and shows that trust in government and legal institutions is positively associated with taxpaying behavior (Scholz and Lubell, 1998; Torgler, 2003). It further suggests that negative perceptions of government increase evasion (Webley, 1991), while political attitudes and perceived representation shape compliance decisions (Feld and Frey, 2002; Giaccobasso et al., 2022; Cullen et al., 2021; Ajzenman et al., 2024). Property taxes offer a particularly suitable setting to study these mechanisms: they are locally administered, weakly enforced, flexible in timing, and largely under the taxpayer's control.

This framework yields testable implications that we bring to the data. We examine whether the impeachment is associated with lower approval of the central government; whether contemporaneous declines in trust in institutions and increases in perceived discrimination emerge at the protest peak;

whether fiscal reciprocity weakens, reflected in reduced property-tax compliance while more rigid obligations remain stable; and whether these effects are substantially larger than those observed around routine political transitions.

4.2 Impeachments as a treatment

In this section, we explain how impeachments can be studied within a treatment-effect framework and elaborate on how we operationalize this idea in our setting.

Our identification strategy leverages polarization to define a treatment group, consisting of units that supported Castillo, and a control group of units that did not, within a difference-in-differences framework.[28] Depending on the specification, these units correspond either to municipalities or to individuals. We favor this approach over alternatives such as a close-election regression discontinuity design for two main reasons. First, our interest is in how staunch Castillo supporters react to the impeachment relative to his detractors, rather than in comparing groups with nearly identical preferences. Second, because the impeachment is a national shock, a close-election design would compare units with nearly identical political views that experience essentially the same event, stripping away meaningful variation in exposure to the event.

Our setting approximates an ideal measurement environment because both outcomes and political preference measures are observed at the same geographic level. Specifically, we combine district-level data on tax collection with election returns at the district level, our most precise measure of local support for Castillo. Some analyses also use individual-level survey data. Because voting is anonymous, however, it is not feasible to classify survey respondents as supporters or non-supporters.[29] We therefore use district-level results from the first round of the Peruvian presidential election as a proxy for individual-level voting behavior, as it is the most granular measure of political preferences.

We consider the results of the first round of the presidential election a more accurate measure of candidate support than those of the second round. Second-round outcomes are influenced not only by voter preferences for a given candidate but also by aversion to the opponent. In other words, first-round results primarily capture support for Castillo, whereas second-round results reflect both support for Castillo and rejection of Fujimori.

With that, we assign districts to the treatment group if Pedro Castillo received the highest vote share among all candidates in the first round of the 2021 presidential election. Districts where another candidate obtained the highest vote share are assigned to the control group. Individuals are assigned to each group based on their district of residence. This binary classification allows us to compare outcomes between areas that supported Castillo most strongly and those that did not.[30]

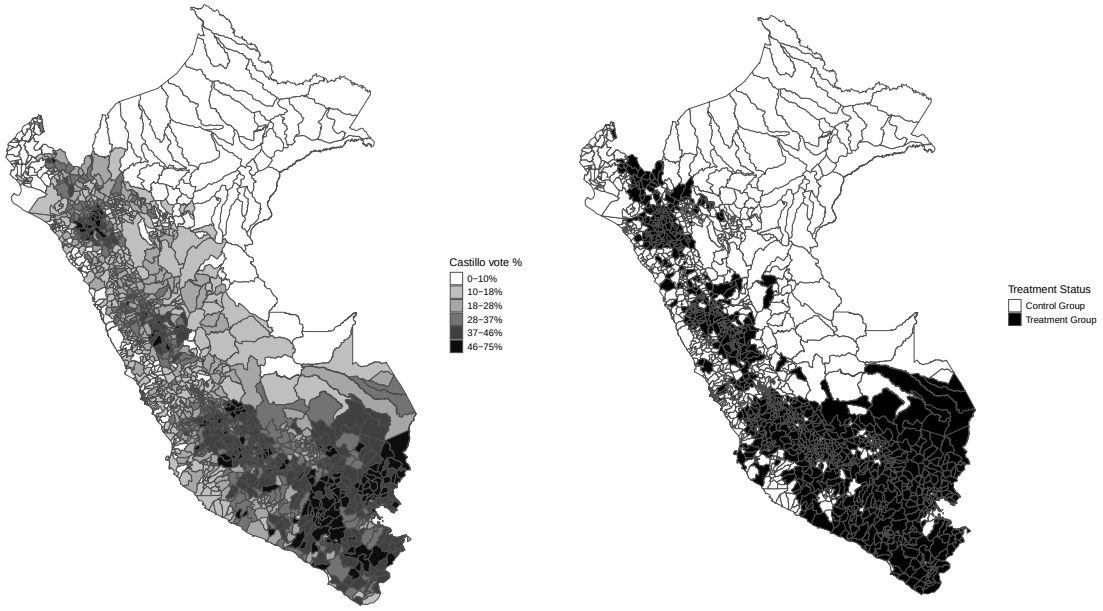
In Figure 4, we present district-level maps of Peru displaying the Pedro Castillo's performance in the 2021 presidential elections.

Both panels show that support for Castillo was concentrated in southern Peru, a region that has historically exhibited stronger support for left-of-center candidates. It was also prevalent along the Andes corridor, which runs through the center of the country. In contrast, support was much lower in the northern and central coastal regions, as well as in the Amazon, areas that tend to lean right and where Fujimori-aligned candidates have historically performed strongly.

Panel (a) quantifies the variation in Castillo's support that we exploit. Along the coast (western Peru), his vote share was low and rarely exceeded 10%. In the South it was seldom below 20% and surpassed 50% in a sizable number of districts. Given that the first round featured a large candidate field, these vote shares are unusually high. Panel (b) shows how we classify districts into treatment and control groups based on Castillo's vote share. This sharp geographic polarization generates substantial

[28] This rationale is rooted in the literature, see for example Ajzenman et al. (2023).
[29] To our knowledge, no alternative data source links individual vote choice to living standards and political attitudes.
[30] We will however, test the robustness of our results to this choice in several ways.

Figure 4: Spatial Distribution of Pedro Castillo's Vote Share and Discretization into Treatment Assignment



(a) Castillo's vote share

(b) Treatment assignment

Note: This map shows the results of the first round of the 2021 presidential election in Peru. Panel (a) shows Castillo's vote share across districts, binned according to the Jenks optimization method. In panel (b), districts where Pedro Castillo received the highest vote share are marked in black (treatment group), while districts where he did not win are marked in white (control group).

cross-district differences in exposure to the impeachment shock, strengthening the identifying variation in our difference-in-differences design.

Appendix Figure C.2 further illustrates this polarization by plotting vote-share distributions in Castillo-supporting and non-supporting districts. The distributions exhibit minimal overlap and have markedly different means. Castillo's average vote share is three times higher in the districts we label as supporters compared to non-supporting districts: 36% vs. 12%.

4.3 Identification Strategy

4.3.1 Dynamic Difference-in-Differences

Our research design exploits the geographic dispersion of Castillo's supporters in a difference-in-differences setting. More specifically, we estimate the following dynamic difference-in-differences equation via ordinary least squares:

$$y_{dt} = \sum_{j \neq \text{Nov 2022}}^{\text{Dec 2023}} \delta_j \, Castillo_d \times \theta_j + \gamma_d + \theta_t + \zeta_{dt}, \tag{1}$$

where y_{dt} denotes a tax-related outcome (e.g., log total taxes, log property taxes) for district d in month t. $Castillo_d$ is a dummy variable set to one in districts where Castillo received the highest vote

share in the first round of the 2021 presidential election. Finally, γ_d and θ_t are district and month-year fixed effects, respectively. For the analyses using individual-level data from ENAHO, each observation corresponds to an individual.

Because treatment is assigned at the district level, we cluster standard errors accordingly to account for within-district correlation in the error term over time. We also set November 2022 (the final pre-impeachment month) as the baseline event time. Consequently, each coefficient δ_j measures the treated–control outcome difference for that month relative to November 2022.

The δ_j coefficients for periods prior to December 2022 provide a test of pre-trends in the outcome variables. The absence of statistically significant pre-trends supports the parallel trends assumption underlying our identification strategy. A notable feature of our design is the strong parallel pre-treatment trends observed between treatment and control groups across a wide range of outcomes.

4.3.2 Canonical 2×2 Difference-in-Differences Estimates

Relying on the parallel-trends assumption, whose plausibility we assess using the dynamic specification, we also estimate a standard 2×2 difference-in-differences regression that summarizes the average post-impeachment effect with a single coefficient. Equation (2) presents the specification:

$$y_{dt} = \beta \, Castillo_d \times Post_t + \gamma_d + \theta_t + \upsilon_{dt}, \tag{2}$$

where $Post_t$ is a dummy variable equal to one for observations collected after Dina Boluarte assumed office (December 2022). In this specification, β captures the average treatment effect of Castillo's removal over the post-treatment period. As before, standard errors are clustered at the district level.

Among the outcomes we study, two families—institutional trust, and self-perceived discrimination—contain a relatively large number of variables. To account for the increased likelihood of false positives within these blocks, we apply the Holm (1979) step-down procedure to adjust p-values. Holm's method strongly controls the familywise error rate under arbitrary dependence across outcomes, making it appropriate given the likely correlation within each family. Hence, in Appendix C, we report both conventional p-values and the corresponding q-values obtained through Holm's adjustment. We apply this correction separately within each block.

5 Results

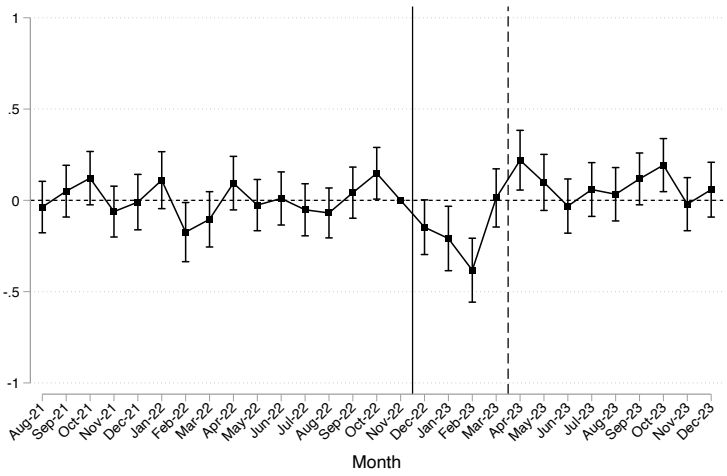
5.1 Main Results

We begin by reporting estimates of equation (1), using the log of district-level tax revenues as the outcome.

Figure 5 shows a large but temporary decline in tax collection in treatment areas relative to control areas. The timing of these results is also informative. Before the impeachment, there were no significant differences in tax revenues between groups, which reinforces the credibility of our quasi-experimental design by suggesting that both groups followed similar trends prior to the impeachment.

In the months following Castillo's removal, tax contributions in the treatment group drop sharply relative to the control group, and the gap persists through March 2023. As discussed above, January and February account for the bulk of property-tax payments, which provides part of the explanation for the concentration of the decline in those months. However, seasonal variation alone cannot account for the magnitude of the drop: while a modest seasonal uptick is visible in the same months of the

Figure 5: Effects of Irregular Presidential Transitions on Tax Revenues. Dynamic Specification.

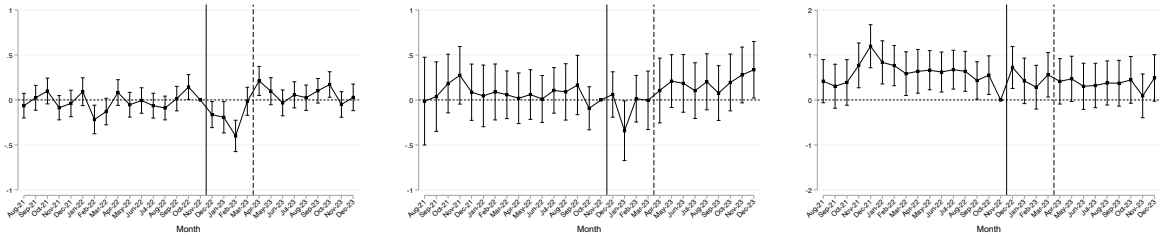


Note: The figure displays the δ coefficients from equation 1, along with their 90% confidence intervals. The coefficient for the month immediately preceding treatment is normalized to zero. The solid vertical line marks the timing of the impeachment, while the dashed line indicates the end of the associated protests. The outcome is the logarithm of monthly tax revenues. The data come from SIAF.

previous year, it is far smaller than the pronounced shortfall that aligns with the tail end of the protests demanding Castillo's reinstatement.

Consistent with the hypothesis that the aggregate decline reflects changes in property-tax payments, we use our data to decompose total tax revenues into property taxes, production and consumption taxes, and other taxable revenues. Figure 6 reports event-study estimates of equation (1) separately by tax type.

Figure 6: Effects of Irregular Presidential Transitions on Tax Revenues. Dynamic Specification By Type of Tax.



(a) Property. (b) Production and Consumption. (c) Other Taxable Revenue.

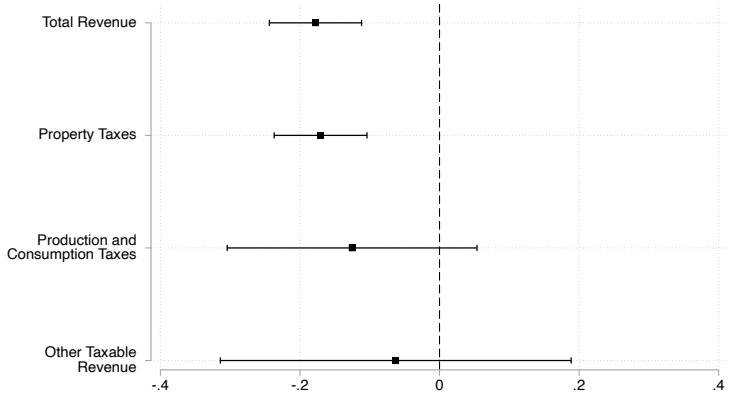
Note: The figure displays the δ coefficients from Equation 1, along with their 90% confidence intervals. The coefficient for the month immediately preceding treatment is normalized to zero. We also report the p-value from a joint significance test of the displayed pre-treatment δ coefficients. The solid vertical line marks the timing of the impeachment, while the dashed line indicates the end of the associated protests. Each panel shows the effects of the impeachment on the logarithm of district-level monthly tax revenues by type, as described in the corresponding caption. The data come from SIAF.

The decline in total revenues is driven almost entirely by property taxes, with little movement in other categories. As explained in Section 2, compliance with property taxes is generally low, and these taxes are notoriously easy to evade or defer. Accordingly, this pattern is consistent with a compliance response on a salient margin with limited short-run enforcement risk. Moreover, in Appendix C (Figure C.3), we use household survey data to show that other margins related to the tax base and formality—such as business registration and worker formality—remain unchanged.

To complement the dynamic estimates, we report canonical 2×2 difference-in-differences estimates

from equation (2). Motivated by the event-study evidence that the tax revenue effects are short-lived and concentrated during the protests, we focus on a short post-period window, such that we estimate equation (2) using the period from August 2021 to March 2023. We use the same windowing convention for subsequent outcomes summarized with the 2×2 specification.

Figure 7: Effects of Irregular Presidential Transitions on Tax Revenues. 2×2 Specification.



Note: The figure displays the β coefficients from Equation 2, along with their 90% confidence intervals. These are canonical difference-in-differences estimates of the treatment effects using data spanning August 2021 to March 2023. The outcomes are the logarithm of total tax revenue and its components. The data come from SIAF.

The results in Figure 7 are consistent with those presented in Figures 5 and 6. Tax collection in treatment areas, relative to control areas, falls by 16.3% (i.e., $100 \times (\exp(-0.178) - 1)$). This decline is entirely driven by property taxes.

To understand the importance of this effect, we provide a back-of-the-envelope calculation that scales the estimated short-run decline to annual revenues in Peru. A 16.3% decline in a single quarter translates into a 4.08% reduction in annual revenue for treated districts. Since these districts accounted for 17% all tax revenue in 2022, our estimates correspond to an aggregate yearly decline in tax revenue on the order of 0.69%.

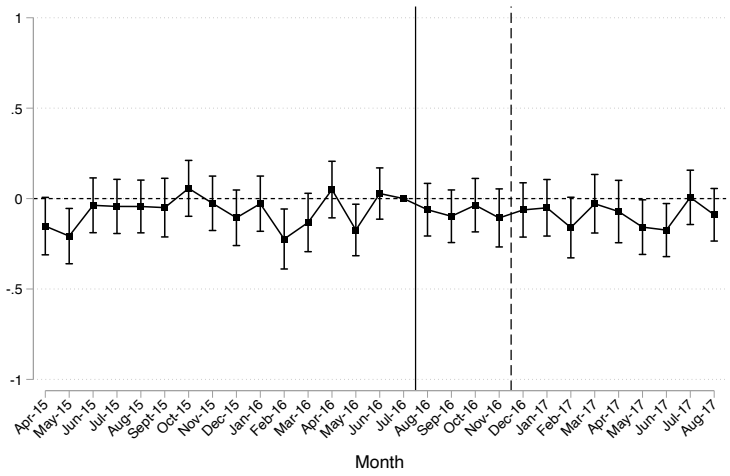
5.2 Comparison with a Regular Presidential Transition

A key concern is that the Castillo–Boluarte episode bundles an irregular interruption of the electoral cycle with other contemporaneous changes, including ideological turnover. To assess whether ideological turnover alone can account for the revenue decline, we benchmark our main design against a comparable left-to-right presidential transfer that occurred under routine electoral conditions. Our conceptual framework suggests that the irregular character of a transition can weaken tax morale by disrupting perceived representation and legitimacy; under this view, a regular transfer of power with a similar political orientation should generate smaller or no effects on tax collection.

We therefore replicate our quasi-experiment around Peru's most recent regular left-to-right handover, from Ollanta Humala to Pedro Pablo Kuczynski in 2016. As discussed in Appendix B, this episode features a standard electoral transition and a shift from a moderately left-leaning to a right-leaning executive, and the geography of Humala support overlaps substantially with that of Castillo. This benchmark provides a diagnostic test of whether the revenue responses we document arise primarily from the irregular mode of presidential replacement rather than ideological turnover per se. Figure 8 presents the results.

Around the Humala–Kuczynski handover, we find no differential change in tax revenues between pro-Humala districts and other districts. This null effect is consistent with ideological turnover under routine electoral conditions being insufficient to generate the revenue declines we document in 2022. We next test whether contemporaneous protest-related violence explains the remaining gap.

Figure 8: Effects of Regular Presidential Transitions on Tax Revenues. Dynamic Specification.



Note: The figure displays the δ coefficients from equation 1, along with their 90% confidence intervals estimated for the 2016 presidential transition. The coefficient for the month immediately preceding this regular transfer of power is normalized to zero. We also report the p-value from a joint significance test of the pre-treatment δ coefficients. The solid vertical line denotes the transition date; the dashed line indicates the first quarter afterward, included to mirror our main results. The outcome variable is the logarithm of monthly tax revenues. The data come from SIAF.

5.3 The Role of Protest Activity

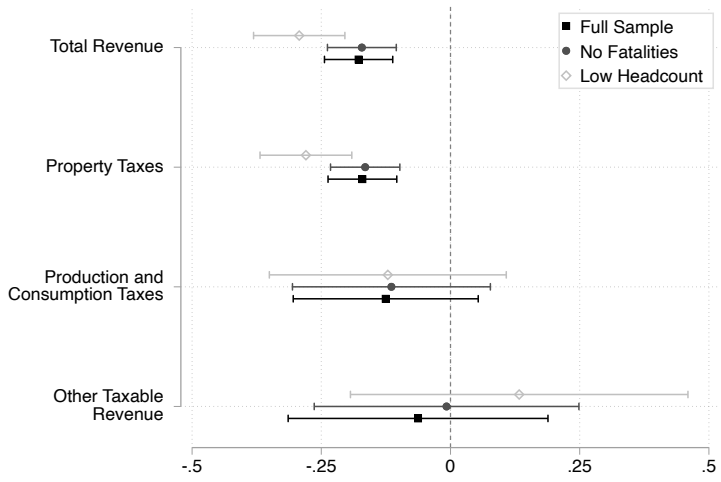
Having shown that routine ideological turnover is unlikely to fully account for the 2022 revenue decline, we next assess the role of protest activity and repression. The key question is whether protests are an amplifying component of the broader irregular-transition shock, deepening perceived illegitimacy and weakening reciprocity toward the state, or whether the revenue decline is instead explained entirely by a mechanical payment constraint. For instance, with unrest restricting mobility and access to municipal offices.

We assess these channels by examining whether the revenue response persists in areas with limited protest exposure. Specifically, we estimate equation 2, focusing on the protest window, and then sequentially exclude districts with high protest intensity. Using ACLED data, we identify (i) districts that experienced impeachment-related protests involving fatalities and (ii) districts that hosted particularly large demonstrations, and exclude each set in turn. If the estimated effects were driven primarily by protest-related disruptions, the revenue decline should attenuate when high-exposure districts are excluded. Figure 9 reports the corresponding results.[31]

These results suggest that protest disruptions are unlikely to be the sole driver of the revenue response. Under a purely mechanical mobility story, excluding districts with fatalities or especially large demonstrations should substantially reduce the estimated effect, as these are precisely the places where payment frictions should be most severe. Instead, the decline in revenues persists with a similar magnitude after these exclusions. This evidence points to protest repression as a component of the irregular-transition bundle, rather than as a purely mechanical constraint on tax payment. It also accords with a social-multiplier view in which tax compliance depends partly on how fairly citizens perceive the state to treat others, with related evidence in Peru in Del Carpio (2013) and Jiménez (2023).

[31] The procedure used to identify and exclude these districts is described in Appendix A.6.

Figure 9: Effects of Irregular Presidential Transitions on Tax Revenues. 2×2 Specifications Removing Areas with High Protest Activity.



Note: The figure displays the β coefficients from Equation 2, along with their 90% confidence intervals. These are canonical difference-in-differences estimates of the treatment effects using data spanning August 2021 to March 2023. The outcomes are the logarithm of total tax revenue and its components. Black squares report the baseline results using the full sample, gray circles exclude areas with casualties, and light gray diamonds restrict to areas with low headcount. Outcome data come from SIAF and protest activity from ACLED.

5.4 Mechanisms: Government Approval, Institutional Trust, and Discrimination

We now unpack the mechanism behind the tax collection effect. Our conceptual framework emphasizes that Castillo's removal may have created a breach between his followers and central government institutions. We examine this channel by estimating equation (1) for measures of approval across levels of government.[32]

Figure 10 displays the results. Pre-treatment trends were, on average, parallel not only for presidential approval but also for most measures of subnational approval. Although a few leads in the regional panel are statistically significant, the overall pattern is flat, which supports the credibility of our quasi-experimental design and suggests that both tax contributions and views toward multiple layers of government evolved similarly prior to the impeachment.

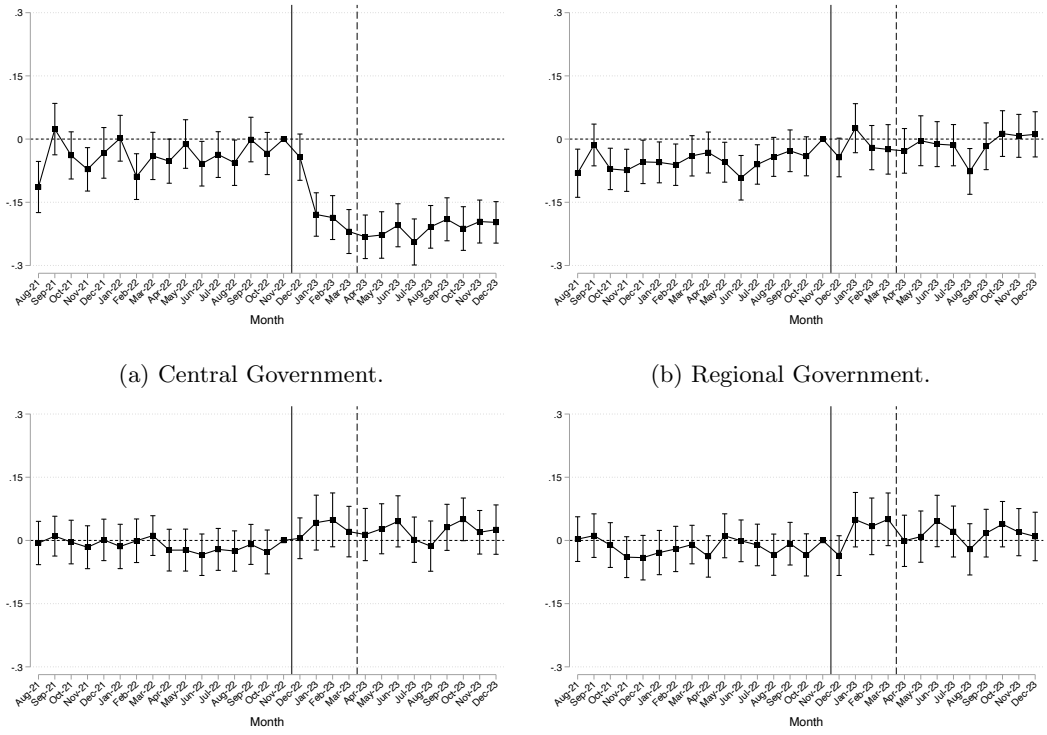
Furthermore, Figure 10 shows a large and persistent decline in presidential approval after the impeachment. This drop extends beyond the protest period and remains more than 15 percentage points below the pre-treatment level.

A second key result is that the impeachment does not generate statistically meaningful changes in approval of subnational governments. This pattern provides a useful falsification check: if respondents attribute responsibility for national events to the relevant political actors, evaluations of subnational officials—who had limited direct involvement in Castillo's removal—should remain stable. The fact that the effect is concentrated on the presidency strengthens an interpretation in which the impeachment primarily operated through perceptions of the national executive, rather than through a generalized shift in attitudes toward the state.

We now turn to perceptions of discrimination and institutional legitimacy. The irregular transition may have affected perceived legitimacy not only of the presidency but also of institutions implicated in the crisis and its enforcement. In addition, because repression disproportionately affected rural and Indigenous communities, protest-period violence may have intensified declines in institutional trust

[32]For clarity, "central government" refers to the presidency. Administratively, Peru is organized into regions, provinces, and districts, which are roughly analogous to U.S. states, counties, and municipalities.

Figure 10: Effects of Irregular Presidential Transitions on Government Approval. Dynamic Specification.



(a) Central Government.

(b) Regional Government.

(c) Provincial Government.

(d) District Government.

Note: The figure displays the δ coefficients from Equation 1, along with their 90% confidence intervals. The coefficient for the month immediately preceding treatment is normalized to zero. We also report the p-value from a joint significance test of the displayed pre-treatment δ coefficients. The solid vertical line marks the timing of the impeachment, while the dashed line indicates the end of the associated protests. The outcomes are dummy variables indicating approval of different government layers. Data comes from ENAHO and is at the individual level.

and heightened perceived discrimination, even if it does not mechanically account for the tax-payment response.

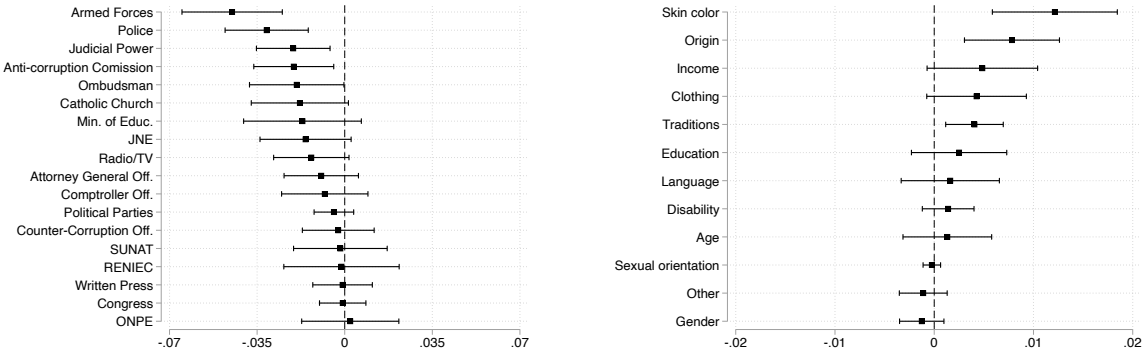
To examine these dynamics, we assess whether the impeachment eroded citizens' trust in institutions involved in the transition, such as the police and armed forces. In addition, given that protest repression disproportionately affected rural and Indigenous communities, we test whether it increased perceived discrimination across multiple dimensions. Figure 11 reports short-term estimates of equation 2 for the full set of outcomes related to institutional trust and experiences of discrimination. We focus on the short run to maintain comparability with the timing of the tax results.[33]

Figure 11 serves two purposes. First, it quantifies our short-run treatment effects on institutional trust and perceived discrimination. Trust in the police and the armed forces declines by 3.1 and 4.5 percentage points, respectively, which corresponds to 13.1 and 10.9% of the pre-impeachment control-group means. Reported discrimination based on origin and skin color increases by 0.8 and 1.2 percentage points, respectively, which corresponds to 82.2 and 76.8% of the corresponding pre-impeachment control-group means. Tables C.3 and C.4 report these estimates in tabular form and adjust inference for multiple hypotheses within each outcome family using the Holm (1979) step-down procedure. The key outcomes remain statistically significant under these adjustments.

Second, the same figure provides placebo checks that help assess the interpretation of these mechanism results. We find no meaningful changes in trust in institutions not implicated in the impeachment episode (e.g., the Department of Education and the National Registry of Identification), nor in reported

[33]Figures C.4 and C.5 in Appendix C show that these treatment effects closely track the evolution of protest activity.

Figure 11: Effects of Irregular Presidential Transitions on Institutional Trust and Discrimination. 2×2 Specification.



(a) Institutional Trust

(b) Discrimination

Note: The figures display the β coefficients from equation 2, along with their 90% confidence intervals. These are canonical difference-in-differences estimates of the treatment effects using data spanning using data spanning August 2021 to March 2023. The outcomes are dummy variables indicating, respectively, (a) trust in a large set of government and civil institutions and (b) reported experiences of discrimination based on various attributes. Data come from ENAHO.

discrimination along less politicized dimensions such as gender or sexual orientation. By contrast, the effects are concentrated in trust in the security forces and in perceived racial discrimination, consistent with a deterioration in state–citizen ties in the aftermath of the impeachment.

Two channels could link the irregular transition to the rise in reported discrimination. First, perceived discrimination may increase because protest activity heightened exposure to coercive state actors, particularly the security forces. Second, to the extent that Castillo was viewed as a representative of historically marginalized groups, his removal may have heightened perceived discrimination by signaling a loss of political representation and social standing. We provide suggestive evidence consistent with the first channel. Figure C.6 decomposes reported incidents by setting (e.g., schools, workplaces, and encounters with the police) and examines within-treated-district changes in the probability that discrimination is reported in each setting. The increase in race-based discrimination is largest in police encounters, consistent with protest-related interactions with security forces contributing to the observed rise.[34]

For the second channel, we study heterogeneous effects of the impeachment on presidential approval across identity dimensions. Specifically, Table 4 reports estimates of equation (2) allowing for heterogeneity by observed identity and reference group.

Panel (a) focuses on minority racial and socioeconomic groups, including individuals who identify as Quechua, Aymara, or other Indigenous backgrounds; residents of rural areas; and agricultural workers. As a placebo, we also report estimates for individuals who hold savings accounts, a group that is plausibly less connected to Castillo's core support base and unlikely to be differentially affected through the representation channel. The first three subgroups therefore provide a test of whether the impeachment generated larger declines in approval among groups for whom perceived representation is plausibly more salient.

Consistent with this interpretation, discontent is markedly higher among Indigenous individuals, rural residents, and agricultural workers, relative to non-Indigenous individuals, urban residents, and non-agricultural workers.

The survey also asks respondents to report the group with which they feel most closely connected, which we interpret as their reference group. Respondents choose among several options, including their local community (region, province, or district), their racial group, their Indigenous community,

[34]This exercise is descriptive and, given low baseline frequencies, the estimates should be interpreted with caution.

or their religious community.

We construct a dummy for each category and estimate equation (2), allowing treatment effects to vary along these margins. Individuals who report a strong attachment to their Indigenous community exhibit significantly higher disapproval of the incoming government following the impeachment. In contrast, effects are smaller and less precisely estimated for those who identify primarily with local, racial, or religious communities, categories that may pool heterogeneous constituencies and thereby attenuate detectable differences. The sharper pattern for Indigenous attachment is consistent with a representation-based mechanism in which the removal of a leader perceived as allied with marginalized groups disproportionately affects political evaluations of the national executive.

Table 4: Effects of Irregular Presidential Transitions on Government Approval. Heterogeneity by Observed Racial Identity and Reference Group

Heterogeneity/ Measure	(1)	(2)	(3)	(4)
a) Observed Identity	Minority Race	Rural	Agricultural Worker	Has a Savings Account
Treatment Effect	-0.058***	-0.090***	-0.094***	-0.136***
	(0.017)	(0.017)	(0.017)	(0.016)
Interaction with Identity Measure	-0.125***	-0.081***	-0.074***	0.029**
	(0.018)	(0.021)	(0.018)	(0.014)
Obs.	44,107	44,112	44,112	44,112
	(5)	(6)	(7)	(8)
b) Reference Group	Locality	Ethnicity or Race	Indigenous Community	Religious Community
Treatment Effect	-0.118***	-0.122***	-0.114***	-0.128***
	(0.016)	(0.014)	(0.015)	(0.014)
Interaction with Reference Group	-0.006	0.002	-0.035**	0.039**
	(0.013)	(0.035)	(0.017)	(0.019)
Obs.	44,101	44,101	44,101	44,101

Note: The table reports estimates from equation (2), including an interaction between the double-difference treatment variable and a dummy indicating an individual's racial identity (panel (a)) or reference group (panel b). We also include the un-interacted dummy variables as controls. These are canonical difference-in-differences estimates of the treatment effects using data spanning August 2021 to March 2023. The columns in Panel (a) examine heterogeneity by observed characteristics. In Column (1), individuals are classified as minorities if they self-identify as being of Quechua, Aymara, or other Indigenous descent. Column (2) focuses on residents of rural areas, and Column (3) on agricultural workers. Column (4) presents a placebo test, analyzing heterogeneous effects among individuals who hold savings accounts. Panel (b) explores heterogeneity by reference group. The ENAHO survey asks respondents to identify the group they feel most closely connected to. Accordingly, Columns (5) through (8) report heterogeneous effects among individuals who identify most strongly with their locality, race, Indigenous community, and religious community, respectively. Cluster-robust standard errors in parentheses. Significance:*** $p \leq 0.01$, ** $p \leq 0.05$ and * $p \leq 0.1$.

As a further diagnostic, Appendix C shows that none of these patterns emerge during a regular presidential transition. Figure C.7 indicates that routine transfers of power do not generate comparable changes in presidential disapproval, trust in the police, or reported racial discrimination. This contrast reinforces that the mechanism outcomes we document are specific to irregular episodes rather than routine alternation in office.

Taken together, the evidence suggests that the post-impeachment decline in tax contributions is driven neither by routine partisan turnover nor by payment frictions induced by protest-related disruptions alone. We find no comparable effects around the orderly Humala–Kuczynski handover, and the revenue decline persists even when restricting attention to districts with limited protest exposure. Instead, the mechanism evidence is most consistent with a tax-morale channel: the impeachment coincides with a sharp and persistent drop in presidential approval, temporary declines in trust in the police and armed forces, and an increase in reported racial discrimination, with larger responses among Indigenous and rural respondents who were disproportionately affected by protest-period violence.

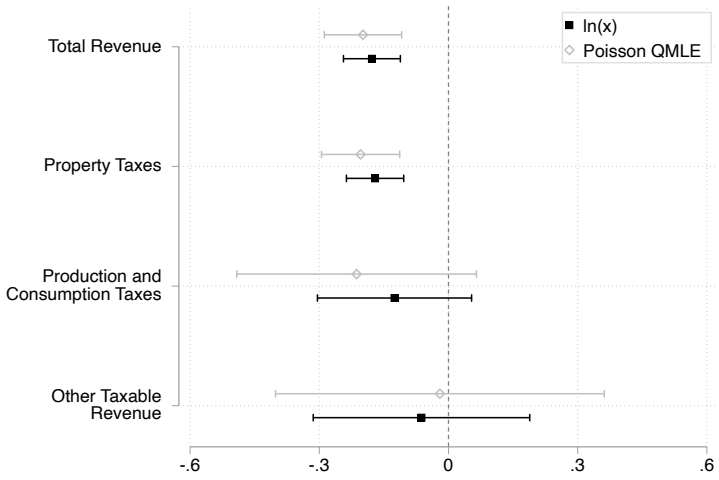
6 Robustness checks

We begin by assessing the robustness of our results to data cleaning assumptions. The raw dataset is an unbalanced panel of monthly district-level tax revenues, with two features that matter for log

specifications: (i) missing district-month cells when no taxes were collected, and (ii) negative values when tax returns exceeded collections. Because our main outcome is the log of raw revenues, both types of observations drop out of the analysis. Our first robustness check therefore examines whether our results are sensitive to including these data.

Figure 12 addresses this concern by estimating Poisson quasi-maximum likelihood (QMLE) specifications, following Chen and Roth (2024). Poisson QMLE accommodates zeros in the dependent variable while remaining consistent under weak distributional assumptions. For implementation, we recode negative net revenues to zero and estimate the model in levels with the same fixed effects structure as in the baseline. The resulting estimates are qualitatively similar to our main findings.

Figure 12: Effects of Irregular Presidential Transitions on Tax Revenues. Robustness to $ln(1+x)$ and Poisson QML Estimation.



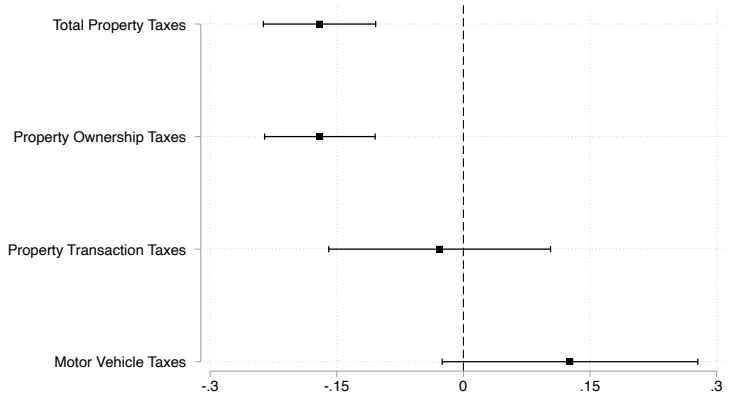
Note: The figure displays the β coefficients from Equation 2, along with their 90% confidence intervals. These are canonical difference-in-differences estimates of the treatment effects using data spanning August 2021 to March 2023. The outcomes are the logarithm of total tax revenue and its components. Black squares report the baseline, light gray diamonds include areas with zero tax revenues applying a Poisson quasi-maximum likelihood estimation. Outcome data come from SIAF.

As an additional robustness check, we exploit the fact that the estimated revenue response is concentrated in property taxes. Our data allow us to further decompose property-tax revenues into components linked to homeownership, property transactions, and motor vehicles. This breakdown helps distinguish a compliance response from changes in market activity: if the effect were concentrated in transaction-related components, it could reflect a contemporaneous decline in property transactions rather than a shift in tax morale. Figure 13 indicates that this is not the case. The decline is concentrated in the homeownership-linked component, with little movement in transaction-based revenues, making it unlikely that the main result is driven by coincident changes in housing-market activity.

We next assess robustness to alternative implementations of equation 2. In Figure 14, we avoid the discretization of treatment assignment by replacing our treatment dummy with a continuous measure of Pedro Castillo's vote share in the 2021 presidential elections at the district level.[35] We present results for tax revenues, government approval, institutional trust, and experiences of discrimination. As expected, the magnitude of estimated effects changes: here the coefficients capture the impact of a one–percentage-point increase in Castillo's vote share, whereas in our main analysis they represent the difference between districts with relatively low and high levels of support—implying a much larger treatment dosage. With that caveat in mind, the results from this exercise are in line with our main claims. The impeachment reduced tax revenues, lowered central government approval, and eroded trust in security forces, while simultaneously increasing race-based discrimination.

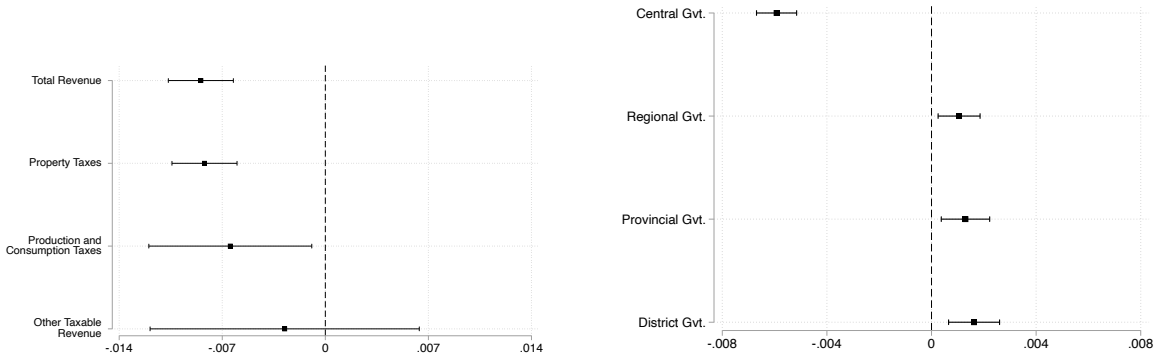
[35]The estimating equation takes the form $y_{dt} = \beta\, CastilloVS_d \times Post_t + \gamma_d + \theta_t + \upsilon_{dt}$. Where $CastilloVS_d$ indicates the voting share for Pedro Castillo in the 2021 presidential elections. All other variables inherit their meaning from equation 2.

Figure 13: Effects of Irregular Presidential Transitions on Tax Revenues. 2×2 Specification by Subtype of Property Tax.



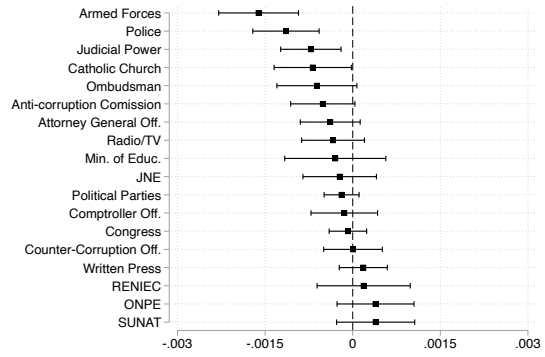
Note: The figure displays the β coefficients from Equation 2, along with their 90% confidence intervals. These are canonical difference-in-differences estimates of the treatment effects using data spanning August 2021 to March 2023. The outcomes are the logarithm of total property tax revenue and its components. The data come from SIAF.

Figure 14: Effects of Impeachments. 2×2 Specification with a Continuous Treatment.

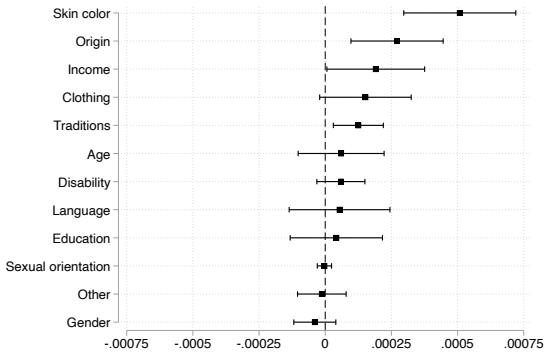


(a) Tax Revenues

(b) Government Approval



(c) Institutional Trust



(d) Discrimination

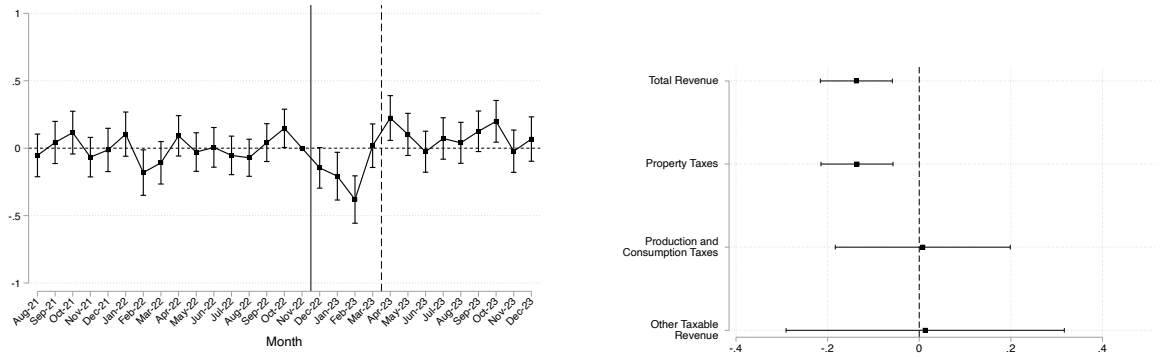
Note: The figures display the β coefficients from a continuous-treatment version of equation 2, along with their 90% confidence intervals. These are canonical difference-in-differences estimates of the treatment effects using data spanning August 2021 to March 2023. The outcomes are: (a) the log of tax revenues; (b) dummies for government approval; (c) indicators of trust in a broad set of government and civil institutions; and (d) reported experiences of discrimination by various attributes. Data for panel (a) come from SIAF; data for panels (b)–(d) come from ENAHO.

We extend the robustness analysis for equation 2 by allowing each province—the second-smallest administrative unit—to follow its own linear time trend (Figure 15). Although treatment and control

groups exhibit pre-treatment parallel trends across a wide range of outcomes, it remains good practice to account for potential heterogeneity at a higher administrative level.

The goal of this specification is to net out the potential influence of time-varying unobservable differences between treated and control groups from our estimated treatment effects. For instance, provinces with a higher share of rural households might have been experiencing a secular decline in institutional trust that would have persisted even in the absence of the impeachment. By allowing each province to follow its own linear trajectory, we absorb such underlying trends. The estimates are stable to this adjustment, with no meaningful change in the qualitative interpretation of the results.

Figure 15: Effects of Irregular Presidential Transitions on Tax Revenues with Heterogeneous Linear Trends by Province. Dynamic and 2×2 Specification.



(a) Dynamic Specification

(b) 2×2 Specification

Note: In panel (a) the figure displays the δ coefficients from equation 1, along with their 90% confidence intervals. The coefficient for the month immediately preceding treatment is normalized to zero. The solid vertical line marks the timing of the impeachment, while the dashed line indicates the end of the associated protests. Panel (b) reports their 2×2 counterpart, using data spanning August 2021 to March 2023. The outcome is the logarithm of monthly tax revenues. Both specifications allow each province to follow a heterogeneous linear trend in time. The data come from SIAF.

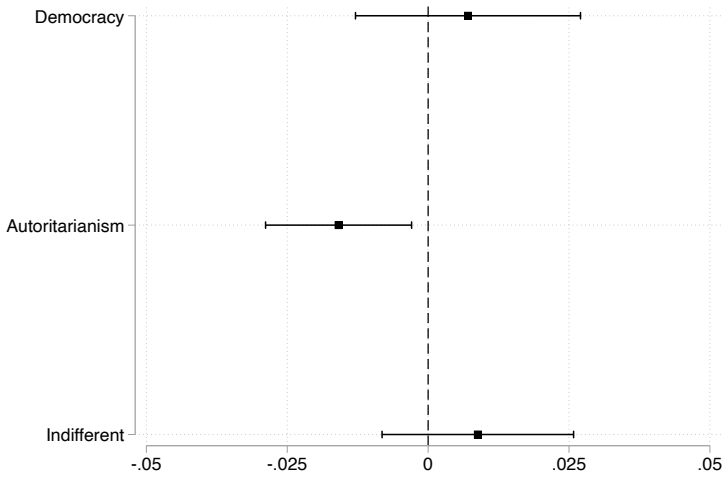
Our next set of robustness checks focuses on ruling out competing hypotheses. Throughout the paper, we have already taken steps in this direction by embedding placebo tests into our main analyses wherever possible. For example, we conduct placebo experiments on the approval of institutions unrelated to the impeachment and examine discrimination along dimensions unlikely to be affected by it. In all cases, the evidence supports an impeachment-based interpretation over alternative explanations rooted in generalized or unrelated discontent triggered by unobserved factors. We now turn to more concrete alternative channels that could mechanically generate similar patterns. Across these checks, the evidence continues to align with an impeachment-based interpretation rather than with generalized shifts in attitudes driven by unrelated shocks.

Still, it is crucial to examine this argument more closely and rule out alternative, more concrete explanations. We begin by addressing the possibility that our results reflect a shift in political preferences. Prior to his impeachment, Castillo attempted a self-coup. It is therefore reasonable to consider the possibility that his supporters came to view this attempt as a desirable course of action and, correspondingly, saw the incoming president as a return to a form of democratic governance they no longer supported. ENAHO elicits respondents' preferred form of government, distinguishing those who always prefer democracy from those who view authoritarian rule as sometimes necessary, those who are indifferent, and those who report not knowing.

In Figure 16, we estimate equation (2) using these regime-preference outcomes and find no evidence supporting this alternative mechanism. If anything, the treatment group becomes less likely to endorse authoritarian governments. This pattern is inconsistent with an interpretation in which Castillo's supporters see Dina Boluarte as an undesirable return to democratic rule.

We next consider whether gender-based backlash toward Peru's first female president could account for the observed declines in approval and tax payments. Because the transition placed Dina Boluarte

Figure 16: Effects of Irregular Presidential Transitions on Preferred Government System



Note: The figure displays the β coefficients from equation 2, along with their 90% confidence intervals. These are canonical difference-in-differences estimates of the treatment effects using data spanning August 2021 to March 2023. The outcomes are dummy variables indicating respondents' preferred government system. Data comes from ENAHO.

at the center of a highly salient political rupture, a concern is that gender-related attitudes, rather than the irregular nature of the transition, drive part of the response. We address this possibility directly using heterogeneity tests.

Table 5 presents a heterogeneity test of this channel. We estimate treatment effects separately by pre-impeachment proxies for local gender norms, assessing whether the overall response is concentrated in areas where gender-based backlash would be more likely. Because ENAHO does not measure individual attitudes toward gender roles, we rely on three district-level proxies: (i) above-median gender gaps in employment, (ii) above-median gender wage gaps (both from ENAHO), and (iii) above-median incidence of intimate partner violence (from the Demographic and Health Survey, DHS).[36] All proxies are constructed using baseline-period data from 2022.[37]

The interpretation of Table 5 is as follows. The first row reports the estimated impeachment effect for areas with below-median values of each proxy, while the second row reports the incremental differential effect for above-median areas. Across specifications, the estimated decline in presidential approval is substantial even in the below-median group, providing little support for a gender-backlash explanation. Moreover, the incremental effect in above-median areas is small and, if anything, positive, implying no amplification of disapproval where gender-based backlash would be expected to be stronger. One possible interpretation is that the transition also involved a salient rightward policy shift, and prior research has documented a positive association between sexist attitudes and support for far-right movements (Anduiza and Rico, 2024).

Next, we rule out the possibility that our results are driven by concomitant changes in satisfaction with government-provided services. A sudden change in the head of government could plausibly disrupt public service delivery, in which case lower tax payments might reflect short-run changes in state performance rather than a compliance response tied to perceived representation.

In Figure 17, we examine two related sets of outcomes. First, we estimate effects on reported bribery solicitations: respondents are asked whether they or an immediate family member were recently (within the past 12 months) forced, encouraged, or asked by a public official to provide a bribe, illegal tip, or other gift. Second, among beneficiaries, we estimate effects on satisfaction with food and non-food

[36] Although both ENAHO and DHS are nationally representative, neither covers all districts in every wave. Merging DHS-based measures at the district level therefore reduces the analysis sample.

[37] For ENAHO-based measures, we exclude December 2022 to avoid post-treatment contamination. DHS does not report month of interview, so an analogous restriction is not feasible.

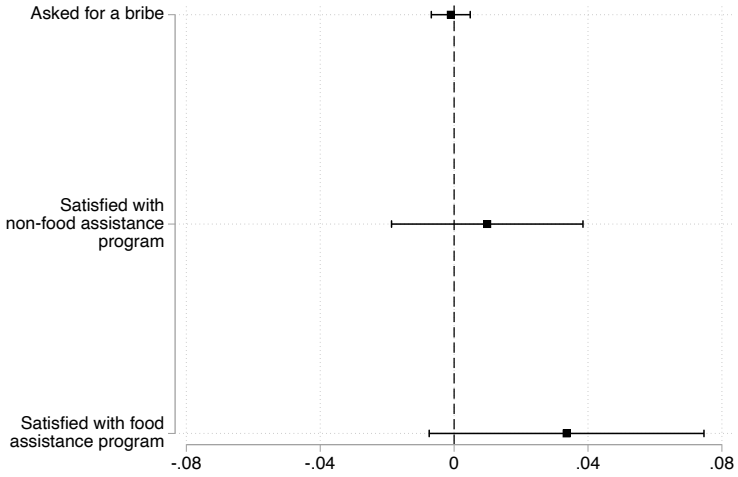
Table 5: Effects of Irregular Presidential Transitions on Central Government Approval. Heterogeneity by Potential Sexism.

Coefficient/Sexist Measure	(1)	(2)	(3)
Treatment Effect	-0.129***	-0.150***	-0.117***
	(0.019)	(0.019)	(0.020)
Interaction with Sexist Dummy	0.009	0.050**	0.004
	(0.024)	(0.023)	(0.026)
Obs.	43,454	43,454	37,456

Note: The table reports estimates from equation 2, and including an interaction between the double-difference treatment variable and a dummy indicating whether an individual belongs to a group characterized as potentially more sexist. We use data spanning August 2021 to March 2023. The definition of "more sexist" varies across columns. In column (1), we define residents of districts with above-median gender gaps in employment as the more sexist group. Column (2) uses districts with above-median gender wage gaps, and column (3) uses districts with above-median incidence of intimate partner violence. All data come from ENAHO, except for intimate partner violence, which is sourced from the DHS. Cluster-robust standard errors in parentheses. Significance:*** $p \leq 0.01$, ** $p \leq 0.05$ and * $p \leq 0.1$.

assistance programs. Across outcomes, we find no evidence of deterioration, which makes explanations based on changes in service delivery or public-sector performance less consistent with the data.

Figure 17: Effects of Irregular Presidential Transitions on Satisfaction with Public Services.

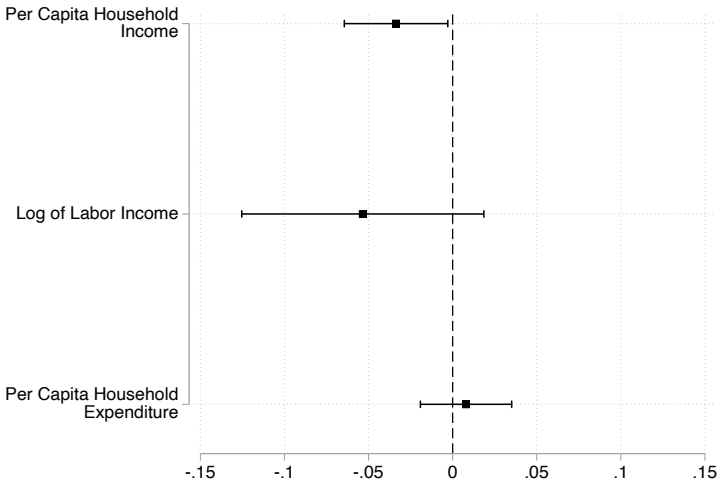


Note: The figure displays the β coefficients from equation 2, along with their 90% confidence intervals. These are canonical difference-in-differences estimates of the treatment effects using data spanning August 2021 to March 2023. The outcomes are binary indicators capturing respondents' experiences with public servants and their satisfaction with government programs. Specifically, they include dummies for whether respondents were victims of corruption solicitations, as well as their satisfaction with non-food and food assistance programs. Data comes from ENAHO.

We next assess whether differential local economic shocks could account for the estimated revenue and attitudinal responses. While time fixed effects absorb economy-wide fluctuations, the remaining concern is a localized decline in economic activity that disproportionately affected treated districts around the impeachment window. Such a shock could mechanically reduce tax payments and shift evaluations of the government independent of tax morale.

Figure 18 addresses this concern by reporting treatment effects on labor income and per capita household expenditure. We find little evidence of economically meaningful differential changes. Estimates for labor income and expenditure are small and statistically indistinguishable from zero. We

Figure 18: Effects of Irregular Presidential Transitions on Labor Income, Household Income and Expenditure



Note: The figure displays the β coefficients from equation 2, along with their 90% confidence intervals. These are canonical difference-in-differences estimates of the treatment effects using data spanning August 2021 to March 2023. The outcomes are the log of labor income, which is set to zero for the not employed; and per capita household expenditure. Data comes from ENAHO.

detect a modest decline in per capita household income; however, the absence of corresponding changes in labor income or consumption suggests that differential economic contraction is unlikely to be the primary driver of our main results.

Lastly, we assess whether the revenue decline could reflect changes in tax enforcement capacity rather than behavioral responses by taxpayers. We draw on universal municipal surveys from December 2021 through March 2023 from the *Registro Nacional de Municipalidades* (RENAMU), described in Appendix A. We construct three proxies for enforcement capacity: (i) total municipal employment, (ii) the number of workers dedicated exclusively to tax collection, and (iii) an indicator for the presence of an on-site unit responsible for prosecuting tax delinquents.

Table 6 shows no evidence of a relative decline in enforcement capacity in treated districts. If anything, these proxies increase slightly: estimates correspond to roughly seven additional municipal workers and a 2.9 percentage-point higher probability of having a delinquent-prosecution unit. These magnitudes are small relative to baseline levels and dispersion, and they are unlikely to account for the large revenue effects we document.

We therefore find no support for an alternative explanation based on declining enforcement capacity, as the available proxies show no evidence of deterioration.

Taken together, the robustness exercises reinforce the interpretation that the post-impeachment revenue decline reflects a behavioral response rather than artifacts of measurement, sample construction, or coincident shocks. Our main estimates are stable when we (i) incorporate district-months with zero or negative revenues using alternative estimators, (ii) vary treatment measurement and relax parallel-trends assumptions, and (iii) probe competing channels related to gender bias, service provision, local economic conditions, and municipal enforcement capacity. The remaining patterns are most consistent with a tax-morale channel in which the irregular mode of presidential replacement, and the associated legitimacy rupture, reduces willingness to comply with taxation.

Table 6: Effects of Irregular Presidential Transitions on Tax Enforcement Capacity.

Coefficient/Enforcement Measure	(1)	(2)	(3)
Treatment Effect	7.812***	0.152	0.029*
	(2.910)	(0.339)	(0.017)
Obs.	7,496	7,496	3,748
Control Sample Mean	132.6	8.582	0.288
Control Sample SD	401.1	31.77	0.453

Note: The table reports estimates of β_1 in equation $Y_{dt} = \beta_0 + \beta_1 Castillo_d \times Post_t + \beta_2 Post_t + \beta_3 Castillo_d + \upsilon_{dt}$. The sample spans December 2021 to March 2023 and uses data from the National Registry of Municipalities, a mandatory census-style survey with universal municipal coverage that collects detailed information on municipal characteristics (see Appendix A). Because the survey is conducted at a lower frequency than ENAHO, information is available only through December of each year, and for a subset of variables through March. Column (1) measures enforcement capacity as the total number of municipal workers. Column (2) restricts this measure to workers assigned to tax collection activities. Column (3) is an indicator for whether the municipality has a dedicated unit for the prosecution of tax delinquents. Cluster-robust standard errors in parentheses. Significance:*** $p \leq 0.01$, ** $p \leq 0.05$ and * $p \leq 0.1$.

7 Discussion

In this paper, we provide evidence on how irregular presidential transitions affect tax collection and related measures of tax morale. Using a difference-in-differences design that compares supporters of the ousted government to non-supporters before and after the event, we show that impeachments cause a sharp short-term decline in local tax revenues. In particular, total revenues in the treatment group fell by about 16% in the quarter following the impeachment. Back-of-the-envelope calculations suggest that this corresponds to roughly a 0.69% reduction in annual municipal tax collection nationwide.

The Castillo–Boluarte episode bundles an irregular interruption of the electoral cycle with contemporaneous ideological turnover and protest repression. Across a set of benchmarking and exclusion exercises, the revenue response is difficult to explain by ideological turnover alone or by protest-related payment frictions as the sole channel, and is instead most consistent with the irregular mode of presidential replacement playing a central role. In line with this interpretation, we document a large and persistent decline in presidential approval (about 40% of the pre-impeachment mean), alongside short-run declines in trust in the armed forces and police and a contemporaneous rise in reported racial discrimination. The latter effects track the protest period and revert toward baseline as unrest subsides, whereas presidential disapproval remains elevated.

We interpret these patterns through a framework in which an irregular presidential removal generates a shock to perceived representation and legitimacy among supporters, weakening reciprocity toward the state. In this setting, the sequence of removal and subsequent repression coincides with lower trust in central institutions, heightened perceptions of exclusion, and sustained political discontent. When compliance is partly discretionary, these belief shifts can translate into fiscal responses. Property taxes provide a natural margin because they are locally administered, weakly enforced, and allow substantial flexibility in timing.

Our results are notably robust. We present a wide range of evidence supporting the validity of our identification strategy. Pre-treatment trends between groups are parallel across dozens of outcomes and remain stable across an equally large set of placebo outcomes, providing strong support for the parallel trends assumption. We also show that the estimates are not an artifact of treatment discretization by replacing the binary supporter measure with continuous variation in Castillo's first-round vote share, yielding consistent patterns. We also allow different geographic units to follow their own heterogeneous time trends, relaxing the parallel trends assumption, and our results remain robust.

Moreover, we rule out several plausible alternative hypotheses. The results are unlikely to reflect sexist bias toward the incoming female president, shifts in satisfaction with public service provision, changes in corruption, broader macroeconomic conditions, or tax enforcement capacity. Each hypothesis is empirically inconsistent with particular pieces of evidence, making it an implausible driver of our findings. Together, these tests strengthen the interpretation that the revenue response reflects a politically driven shift in compliance, rather than confounding changes in local conditions or enforcement.

While some of our findings are context-specific—such as those related to racial hostility or tax avoidance in a diverse country with limited state capacity—political partisanship in general aligns with demographic traits. Similar patterns may emerge elsewhere, though along different dimensions, such as region, religion, age, or income. An example of this is the clear age-based polarization observed in the Brexit vote in the UK. Similar dynamics have also been documented in the United States, where racial and educational divides strongly predict partisan alignment, and in the Indian subcontinent, where religious identity plays a central role in political polarization. In fact, we present suggestive evidence that our results do generalize across space and time in some instances.

Our findings are also relevant for policy audiences, given the recent prevalence of impeachments and serious attempts worldwide. The results imply that the details of constitutional crisis-management procedures can generate fiscal externalities when they trigger perceived losses of representation and legitimacy among sizable constituencies. In particular, when political conflict escalates into repression or sustained unrest, the resulting deterioration in trust and approval may translate into reduced tax compliance on margins where payment is discretionary.

We conclude by emphasizing what the evidence implies independent of one's view of Pedro Castillo's conduct or removal. Supporters may read the results as quantifying the costs of his ousting, but the findings are also relevant for detractors: they show that contested yet legal removals can have economically meaningful fiscal repercussions. In this sense, the results highlight a fiscal channel through which irregular transitions can impose costs, even when they occur within formal legal procedures. A broader implication is that crisis-management design and implementation can shape not only political stability but also fiscal performance through citizens' willingness to comply with taxation.

References

Acemoglu, D., Naidu, S., Restrepo, P. and Robinson, J. A. (2015), Democracy, redistribution, and inequality, *in* 'Handbook of income distribution', Vol. 2, Elsevier, pp. 1885–1966.

Acemoglu, D., Naidu, S., Restrepo, P. and Robinson, J. A. (2019), 'Democracy does cause growth', *Journal of political economy* **127**(1), 47–100.

Ajzenman, N., Cavalcanti, T. and Da Mata, D. (2023), 'More than words: Leaders' speech and risky behavior during a pandemic', *American Economic Journal: Economic Policy* **15**(3), 351–371.

Ajzenman, N., Cruces, G., Perez-Truglia, R., Tortarolo, D. and Vazquez-Bare, G. (2024), From flat to fair? the effects of a progressive tax reform, Technical report, National Bureau of Economic Research.

Amnesty International (2023), 'Lethal racism: Extrajudicial executions and unlawful use of force by peru's security forces', Report AMR46/6761/2023, Amnesty International. Accessed: 2024-04-10.
URL: *https://www.amnesty.org/en/documents/amr46/6761/2023/en/*

Anduiza, E. and Rico, G. (2024), 'Sexism and the far-right vote: The individual dynamics of gender backlash', *American Journal of Political Science* **68**(2), 478–493.

Ang, D., Bencsik, P., Bruhn, J. and Derenoncourt, E. (2021), 'Police violence reduces civilian cooperation and engagement with law enforcement'.

Artiles, M., Kleine-Rueschkamp, L. and León-Ciliotta, G. (2021), 'Accountability, political capture, and selection into politics: Evidence from peruvian municipalities', *Review of Economics and Statistics* **103**(2), 397–411.

Asensio, R. (2021), El provinciano redentor. crónica de una elección no anunciada, *in* 'El profe: Cómo Pedro Castillo se convirtió en presidente del Perú y qué pasará a continuación', Instituto de Estudios Peruanos.

BBC News (2021), 'Peru election: Keiko fujimori's double-edged legacy'. Accessed: 2025-05-12.
URL: *https://www.bbc.com/news/world-latin-america-57341541*

BBC News Mundo (2021), 'Pedro castillo y keiko fujimori: 3 claves que explican lo que está pasando en perú tras la segunda vuelta de las elecciones'. Accessed: 2025-04-10.
URL: *https://www.bbc.com/mundo/noticias-america-latina-57423108*

Bermeo, N. (2016), 'On democratic backsliding', *Journal of democracy* **27**(1), 5–19.

Cabra-Acela, P. A. (2023), 'Rewarding good taxpayers: an effective mechanism?', *International Tax and Public Finance* **30**(6), 1688–1717.

Cabral, M. and Hoxby, C. (2012), The hated property tax: Salience, tax rates, and tax revolts, Working Paper 18514, National Bureau of Economic Research.
URL: *http://www.nber.org/papers/w18514*

Camacho, G. and Sosa-Villagarcia, P. (2021), Una democracia asediada por su élite, *in* 'El profe: Cómo Pedro Castillo se convirtió en presidente del Perú y qué pasará a continuación', Instituto de Estudios Peruanos.

Chen, J. and Roth, J. (2024), 'Logs with zeros? some problems and solutions', *The Quarterly Journal of Economics* **139**(2), 891–936.

CNN Español (2022), 'Pedro castillo: cuántas veces enfrentó una moción de vacancia y por qué'. Accessed: 2025-04-10.
URL: *https://cnnespanol.cnn.com/2022/12/07/pedro-castillo-veces-enfrenta-mocion-vacancia-orix*

Cohen, M. J. (2018), 'Protesting via the null ballot: An assessment of the decision to cast an invalid vote in latin america', *Political Behavior* **40**, 395–414.

Cullen, J. B., Turner, N. and Washington, E. (2021), 'Political alignment, attitudes toward government, and tax evasion', *American Economic Journal: Economic Policy* **13**(3), 135–166.

Del Carpio, L. (2013), 'Are the neighbors cheating? evidence from a social norm experiment on property taxes in peru'.

Diario Correo (2021), 'Yonhy lescano: "me ratifico en el voto nulo como acto de protesta"'. Accessed: 2025-05-16.
URL: *https://diariocorreo.pe/politica/yonhy-lescano-me-ratifico-en-el-voto-nulo-como-acto-de-protesta-en-la-segunda-vuelta-elecciones-generales-de-peru-de-2021-nndc-noticia/*

El Popular (2021), 'Carlos bruce hizo comentario racista contra pedro castillo: "parece que la abundancia de oxígeno en la costa le afectó"'. Accessed: 2025-04-10.
URL: *https://elpopular.pe/actualidad/2021/04/29/carlos-bruce-hizo-comentario-racista-pedro-castillo-parece-abundante-oxigeno-costa-le-afecto-foto-61145*

Feld, L. P. and Frey, B. S. (2002), 'Trust breeds trust: How taxpayers are treated', *Economics of governance* **3**(2), 87–99.

Fortin, N. M. (2005), 'Gender role attitudes and the labour-market outcomes of women across oecd countries', *oxford review of Economic Policy* **21**(3), 416–438.

Fujiwara, T. (2015), 'Voting technology, political responsiveness, and infant health: Evidence from brazil', *Econometrica* **83**(2), 423–464.

Fujiwara, T., Meng, K. and Vogl, T. (2016), 'Habit formation in voting: Evidence from rainy elections', *American Economic Journal: Applied Economics* **8**(4), 160–188.

Giaccobasso, M., Nathan, B. C., Perez-Truglia, R. and Zentner, A. (2022), Where do my tax dollars go? tax morale effects of perceived government spending, Technical report, National Bureau of Economic Research.

Giaccobasso, M., Nathan, B., Perez-Truglia, R. and Zentner, A. (2025), 'Where do my tax dollars go? tax morale effects of perceived government spending', *American Economic Journal: Applied Economics* **17**(4), 223–259.

Glaeser, E. L., Sacerdote, B. I. and Scheinkman, J. A. (2003), 'The social multiplier', *Journal of the European Economic Association* **1**(2-3), 345–353.

González, N. and Moscoso, M. (2021), Al maestro con cariño (y cautela), *in* 'El profe: Cómo Pedro Castillo se convirtió en presidente del Perú y qué pasará a continuación', Instituto de Estudios Peruanos.

Grompone, R. and Jimenez, O. P. (2021), Pedro castillo, una identidad reconocida en múltiples trayectorias, *in* 'El profe: Cómo Pedro Castillo se convirtió en presidente del Perú y qué pasará a continuación', Instituto de Estudios Peruanos.

Holm, S. (1979), 'A simple sequentially rejective multiple test procedure', *Scandinavian journal of statistics* pp. 65–70.

Hunt, J. (2007), 'How corruption hits people when they are down', *Journal of Development Economics* **84**(2), 574–589.

Hunt, J. and Laszlo, S. (2012), 'Is bribery really regressive? bribery's costs, benefits, and mechanisms', *World Development* **40**(2), 355–372.

Infobae (2022), 'Pedro castillo anuncia toque de queda ante cierre del congreso de la república'. Accessed: 2024-04-10.
URL: *https://www.infobae.com/america/peru/2022/12/07/pedro-castillo-anuncia-toque-de-queda-ante-cierre-del-congreso-de-la-republica/*

Instituto Nacional de Estadística e Informática (INEI) (2025), 'Producción y empleo informal en el perú: Cuenta satélite de la economía informal 2022–2023'. Accessed: February 8, 2026.
URL: *https://cdn.www.gob.pe/uploads/document/file/7448702/6344108-produccion-y-empleo-informal-en-el-peru-cuenta-satelite-de-la-economia-informal-2022-2023.pdf?v=1735913240*

Jiménez, B. (2023), 'The political economy of the minimum wage', *Labour Economics* **85**, 102463.

Jiménez, B. and Rendon, S. (2023), 'Does employment protection unprotect workers? the labor market effects of job reinstatements in peru', *Labour Economics* **80**, 102286.

Jiménez, B. and Rendon, S. (2025), 'Labor market effects of bounds on domestic outsourcing', *Journal of Development Economics* **173**, 103406.

La República (2021*a*), 'Cnn presenta flash electoral sin la foto de pedro castillo'. Accessed: 2025-04-10.
URL: *https://larepublica.pe/elecciones/2021/04/12/elecciones-2021-cnn-presenta-flah-electoral-sin-la-foto-de-pedro-castillo-atmp*

La República (2021*b*), 'Keiko fujimori busca anular más de 200.000 votos a nivel nacional'. Accessed: 2025-04-10.
URL: *https://larepublica.pe/elecciones/2021/06/10/elecciones-2021-keiko-fujimori-busca-anular-mas-de-200000-votos-a-nivel-nacional-pltc*

León-Ciliotta, G., Zejcirovic, D. and Fernandez, F. (2025), 'Policymaking, trust, and the demand for public services: Evidence from a mass sterilization campaign', *American Economic Journal: Economic Policy* **17**(1), 181–215.

Luttmer, E. F. P. and Singhal, M. (2014), 'Tax morale', *Journal of Economic Perspectives* **28**(4), 149–68.
URL: *https://www.aeaweb.org/articles?id=10.1257/jep.28.4.149*

Nathan, B., Perez-Truglia, R. and Zentner, A. (2025), 'My taxes are too darn high: Why do households protest their taxes?', *American Economic Journal: Economic Policy* **17**(1), 273–310.
URL: *https://www.aeaweb.org/articles?id=10.1257/pol.20220768*

Ñopo, H. (2008), 'Matching as a tool to decompose wage gaps', *The review of economics and statistics* **90**(2), 290–299.

O Salto (2021), 'Castillo asume el poder en perú con promesas de cambio y escaso margen de actuación'.
URL: *https://osalto.gal/america-latina/castillo-asume-poder-peru-promesas-cambio-escaso-margen-actuacion*

Ojo Público (2021), 'Abogados de grandes estudios aportaron a la estrategia del fraude'. Accessed: 2025-04-10.
URL: *https://ojo-publico.com/politica/abogados-grandes-estudios-aportaron-la-estrategia-del-fraude*

Pajuelo, R. (2021), Nuevos demonios y viejos redentores: Una elección en tres vueltas, *in* 'El profe: Cómo Pedro Castillo se convirtió en presidente del Perú y qué pasará a continuación', Instituto de Estudios Peruanos.

Peyton, B., Bajjalieh, J., Martin, M., Alahi, S., Fadell, N. and Jeralds, M. (2025), 'Cline center coup d'État project dataset'.
URL: *https://doi.org/10.13012/B2IDB-9651987_V8*

Raleigh, C., Kishi, R. and Linke, A. (2023), 'Political instability patterns are obscured by conflict dataset scope conditions, sources, and coding choices', *Humanities and Social Sciences Communications* **10**(1), 74.

Redacción La Mula (2021), 'Vargas llosa: "los peruanos de las ciudades están más informados que el resto"'. Accessed: 2025-04-10.
URL: *https://redaccion.lamula.pe/2021/06/14/vargas-llosa-los-peruanos-de-las-ciudades-estan-mas-informados-que-el-resto-video/jorgepaucar*

Rendon, S. (2020), Sterilization policy with incomplete information: Peru 1995-2000, Technical report, IZA Discussion Papers.

Sangnier, M. and Zylberberg, Y. (2017), 'Protests and trust in the state: Evidence from african countries', *Journal of Public Economics* **152**, 55–67.

Scholz, J. T. and Lubell, M. (1998), 'Trust and taxpaying: Testing the heuristic approach to collective action', *American journal of political science* pp. 398–417.

Servicio de Administración Tributaria de Lima (2024), 'Sat de lima rematará 30 inmuebles por deudas tributarias'. Accessed: 2025-09-11.
URL: *https://www.sat.gob.pe/WebSiteV9/Noticias/aid/1193*

Silva Mercado, M. A. (2024), 'Factores psicosociales asociados al voto inválido de peruanos durante las elecciones presidenciales del 2021'.

Sviatschi, M. M. (2022), 'Making a narco: Childhood exposure to illegal labor markets and criminal life paths', *Econometrica* **90**(4), 1835–1878.

Swissinfo.ch (2021), 'En último debate, fujimori promete mucho gasto y castillo apela a honestidad'. Accessed: 2025-04-10.
URL: *https://www.swissinfo.ch/spa/en-%C3%BAltimo-debate-fujimori-promete-mucho-gasto-y-castillo-apela-a-honestidad/46663818*

Torgler, B. (2003), 'Tax morale, rule-governed behaviour and trust', *Constitutional Political Economy* **14**(2), 119–140.

Webley, P. (1991), *Tax evasion: An experimental approach*, Cambridge University Press.

Appendix

A Variable Definition

In this appendix, we provide additional details about our data sources. In particular, we describe how we construct all the variables used throughout the analysis.

A.1 SIAF

As previously mentioned, we submitted a request for access to sub-national tax collection data. Our request read as follows: "We kindly request the submission of tax information from local governments at the lowest possible level of spatial disaggregation, preferably at the district level, from the year 2015 onward. Please include, if possible, the following data: tax revenues broken down by type of tax, amounts collected, collection periods, as well as any other relevant variables that would allow for a detailed analysis of the information." As in the rest of the manuscript, we use information from 2015 to 2023.

The Peruvian government is obligated to fulfill such information requests, provided they comply with applicable laws on privacy and data availability. However, it is not required to offer any accompanying documentation or support for handling or interpreting the data.

Since our request met the necessary requirements—which is less straightforward than it may seem—we were granted access to a subset of the data we requested. Specifically, the data we received is an excerpt from a much larger dataset known as SIAF (*Sistema Integrado de Administración Financiera*, or Integrated System of Financial Administration). The broader dataset can be understood as an exceptionally detailed government balance sheet, containing information on a surprisingly wide range of public revenues and expenditures. The small excerpt we obtained reports month-by-month information on tax revenues for each district in Peru broken down by type of tax.

Each entry in the dataset corresponds to the amount of tax collected in a given month, in a specific district, and for a specific type of tax. While the tax types are reported in highly detailed categories, they can be readily aggregated into broader groups using government-provided classifications, as follows:

1. **Property taxes:** This category includes taxes on homeownership, real estate transactions, and vehicle ownership. A disproportionate majority of our entries fall under property taxes. These are further disaggregated as follows:
   (a) Ownership taxes: Taxes collected on ownership of any form of real estate.
   (b) Transaction taxes: Taxes collected on real estate transactions.
   (c) Motor vehicle taxes: Taxes collected on the ownership of motor vehicles such as cars or motorbikes.
2. **Production and consumption taxes:** This includes taxes on specific goods and services (not specified in the data), as well as taxes on income derived from owning slot machines.
3. **International trade:** These are customs-related taxes.
4. **Other taxable revenue:** This category primarily consists of fees and penalties for tax evasion or late payments. It also includes revenue from tax installment agreements (*fraccionamiento*).
5. **Mandatory contributions:** These refer to obligatory payments for infrastructure development.
6. **Total revenue:** Sum of all other categories.

Although these data are expected to be precise—originating from administrative records rather than survey responses—and are highly detailed, they present certain issues that cannot be addressed without relying on additional assumptions. In the absence of further documentation, our best course of action is to transparently outline these assumptions and assess the robustness of our choices. In this section, we focus on the first of these tasks.

In particular, there are two main issues we encountered. Here, we describe them and detail what assumptions we made for constructing our preferred measure.

1. **The data form an unbalanced panel:** Information is missing for some month-district cells.
    - *Assumed solution:* We take the data as literally as possible and do not impute values for missing month-district cells in our main analysis.
2. **Some entries take negative values:**
    - *Assumed solution:* Since we lack documentation explaining these negative entries, we assume they most likely correspond to tax returns resulting from overpayment or dispute resolutions. However, measurement error remains a possibility—especially given that only 0.2% of entries are negative. In our preferred specification, we interpret these negative values as tax returns and include them as reported.

In our main analysis, we use the logarithmic transformation of aggregate monthly tax revenues at the district level, applying the $\ln(x)$ transformation and treating negative values as they are. We also refrain from making any assumptions regarding missing month-district observations. Our analysis includes both the total tax revenue measure and its disaggregated components, grouped into the five broad tax categories described above. Crucially, we will later show that these assumptions are irrelevant to the qualitative interpretation of our results.

A.2 ENAHO

Our next data source is the ENAHO (*Encuesta Nacional de Hogares*), Peru's national household survey. It collects detailed information on living standards, as well as individuals' perceptions of and interactions with the government. In this study, we use the following information from the survey:

- Government approval: We capture this information using a series of dummy variables derived from categorical survey responses. Respondents are coded as 1 if they answer "Good" or "Very good" to the question: "IN YOUR OPINION, THE MANAGEMENT OF [BLANK] IS?", where the blank is filled with the name of a specific institution. Those who respond "Bad" or "Very bad" are coded as 0. No value is assigned for respondents who indicate they do not know or choose not to answer. The survey includes questions about the management of Central, Regional, Provincial, and District governments.
- Trust in institutions: We capture this information using a series of binary variables. Respondents are coded as 1 if they answer "Enough" or "A lot" to the question: "DO YOU CURRENTLY TRUST INSTITUTIONS SUCH AS:". Those who respond "Little" or "Not at all" are coded as 0. No value is assigned to respondents who indicate they do not know or choose not to answer. The survey includes a fairly extensive set of institutions; we use all of them in the present study.
- Discrimination experience: We construct a separate binary variable for each reported reason for discrimination. These are based on responses to the question: "In the last 12 months, have you felt discriminated against in the country because of..." followed by specific reasons (e.g., skin color, physical appearance, ethnic origin, etc.). Each dummy takes the value 1 if the respondent answers "Yes" for that particular reason, and 0 if they answer "No." No value is assigned for non-response or missing data.

- Racial minority: We use the question on racial self-identification, which asks respondents: "Because of your traditions and your ancestors, do you consider yourself to be..." Respondents may choose from a list of race categories, including Quechua, Aymara, Amazonian Indigenous peoples, members of other Indigenous or Native Peoples, Afro-Peruvian, White, Mestizo, Other, or indicate they do not know or prefer not to respond. For our analysis, we classify as minority population those who self-identify as Quechua, Aymara, Amazonian Indigenous, or as members of another Indigenous or Native People. All other responses are coded as non-minority. While Afro-Peruvians also face racial discrimination, public discourse surrounding the impeachment primarily targeted Indigenous populations. In this context, discriminatory remarks and attitudes were mostly based on whether individuals were perceived as Indigenous.

- Reference group: We define a respondent's reference group as the social group with which they identify most strongly. The available categories are locality, race, Indigenous community, and religious community. For each category, we construct a binary indicator equal to one if the respondent selects that group.

- Preferred government system: We use responses to the question: "Which of the following opinions do you most agree with?" Respondents are presented with four options: (1) a democratic government is always preferable, (2) in some circumstances, an authoritarian government is preferable to a democratic one, (3) it makes no difference whether the government is democratic or authoritarian, and (4) don't know. We construct a separate dummy variable for each response to capture the respondent's preferred type of government. Each dummy takes the value 1 if the respondent selects that specific option, and 0 otherwise.

- Gender gap in employment: We use aggregate data on employment rates by gender and district from January to December 2022 (i.e., the baseline period). We compute the between gender difference in employment rates in each district and create a dummy variable equal to 1 if the district's gender gap is at or above the median, and 0 otherwise. These district-level data are then merged with the individual-level dataset using unique district identifiers.

- Gender gap in wages: We use aggregate data on wages by gender and district from January to December 2022 (i.e., the baseline period). We compute the between gender difference in employment rates in each district and create a dummy variable equal to 1 if the district's gender gap is at or above the median, and 0 otherwise. These district-level data are then merged with the individual-level dataset using unique district identifiers. We include both employees and self-employed men and women when computing wages in this case.

- Experiences with bribery: We use responses to the question: "In the last 12 months, have you or any member of your household been asked, felt obligated to give, or voluntarily given gifts, tips, bribes, or other forms of payment to a public employee or government official?" Respondents may answer (1) Yes, (2) No, or (3) Did not use public services. We construct a binary variable coded as 1 if the respondent selects "Yes," and 0 if they select "No." Observations for which the respondent reports not using public services are coded as missing.

- Satisfaction with government programs: We use responses to questions 34A and 34C, which ask about household satisfaction with goods and services received through non-food and food government programs, respectively. Respondents are asked to rate their satisfaction as: (1) a lot, (2) sufficient, (3) little, (4) none, or (5) don't know. For each type of program, we construct a dummy variable equal to 1 if the household reports being at least sufficiently satisfied (i.e., answers 1 or 2), and 0 if they report little or no satisfaction (i.e., answers 3 or 4). Households that did not receive government assistance or responded "don't know" are coded as missing.

- Labor Income: We compute monthly labor incomes by summing the variables *i524a1*, *d529t*, *i530a*, and *d536*, which capture earnings received by independent (self-employed) and dependent workers in their main occupations. These variables are annualized and include official imputations for invalid or missing values, as provided directly by the survey agency. Since wages in Peru are typically paid on a monthly basis, we divide the total by 12. We use the term "labor income," rather than "wages," because our focus is on all the population, including the non-employed, which are assigned a labor income of 0.

- Household income: Household heads report income from a range of sources, including labor income, land rents, and government transfers. Based on these responses, INEI computes a measure of total household income. We compute per capita household income by dividing this aggregate measure by the number of individuals in a household.
- Household expenditure: Household heads report spending on a wide range of goods and services. Based on these responses, INEI computes a measure of total household expenditure. We use this aggregate variable and divide it by the number of household members.
- Demographic variables: Our survey also has information on respondents' sex and age, as well as a set of dummy variables indicating their highest level of educational attainment and marital status. For visualization purposes—such as descriptive statistics—we recode education as a binary indicator for high school graduates versus dropouts, and marital status as married versus not married. To clarify, we do not use these variables as controls in our individual-level analyses.
- Business registration: We assume a business is registered when a worker reports that their workplace is registered with the tax authorities. This variable covers both self-employed individuals and employees.
- Income reporting: We assume a business reports its income to the tax authorities when a worker states that the firm they work for keeps track of business income in accounting books.

A.3 2021 Election Results

We construct our treatment indicator using the results from the 2021 presidential elections, as provided by the Peruvian government agency *Infogob*, which hosts electoral data on its online platform. The dataset includes the total number of votes for each presidential candidate, as well as pre-computed voting shares by district.

However, we identified a minor issue in these data: the reported voting shares by district do not sum to one. This occurs because blank and void votes are excluded when calculating candidates' vote shares.

Since voting is mandatory in Peru, it is common for individuals to show up at the polls and either cast a blank vote, unintentionally spoil their ballot (resulting in a void vote), or deliberately submit ballots containing profane, sarcastic, or highly politicized messages as a form of protest against the entire slate of candidates. Some presidential candidates who did not make it into the second round even encourage this practice. For example, Yonhy Lescano publicly declared (Diario Correo, 2021): "I reaffirm my null vote as an act of protest." While seemingly anecdotal, this phenomenon has been documented in the Peruvian and Latin American social science literature (Silva Mercado, 2024; Cohen, 2018). Nevertheless, blank and null votes represent a small minority. They can, however, be interpreted as informative, reflecting the share of voters who reject all available options.

Given this, we recalculate each option's vote share—including all political parties as well as blank and void votes—by dividing the number of votes received by the total number of votes (i.e., total turnout) in each district. A candidate's vote share is therefore computed as their raw vote count divided by total turnout.

We then classify a district as having supported Pedro Castillo if he received the largest vote share among all candidates.

A.4 2011 Election Results

Since we are interested in ruling out the possibility that our results are driven merely by political turnover, we conduct a placebo experiment designed to address this concern. Specifically, we examine

how government approval, trust in institutions, and experiences of discrimination evolved following a peaceful and routine presidential transition.

To ensure the placebo closely mirrors our actual setting, we focus on the most recent presidential transition that was routine rather than triggered by impeachment. In particular, we study how supporters of Ollanta Humala—elected in 2011—responded to his transfer of power to Pedro Pablo Kuczynski in 2016. Notably, this transition also involved a shift from a left-leaning to a right-leaning administration.

We measure support for the outgoing president as in the main text, using his vote share in the first round of the election that he eventually won.

Infogob, our main source of district-level election results for the main experiment, does not provide such data for earlier electoral cycles. Therefore, we rely on an alternative database provided by ONPE (the National Office of Electoral Processes). These data are reported at the voting table level.

Using these data, we construct a district-level dummy variable indicating support for Ollanta Humala by dividing votes for Ollanta by total turnout. We exclude two voting tables that were never activated during the election and those located abroad.

We then matched the election results to the 2015-2017 rounds of ENAHO and SIAF using a publicly available crosswalk that harmonizes district codes between both. We loose some information due to changes in boundaries for a handful of districts. Additionally, we omit one district, Mazamari-Pangoa, whose administrative boundaries are unclear and thus not coded by ENAHO's provider.

Further details on this transition, along with a visual comparison of the distribution of voting preferences in the placebo and main experiments, are provided in Appendix B.1.

A.5 DHS

Throughout the text, we explore and seek to rule out the alternative hypothesis that our main results are driven by sexist bias toward Dina Boluarte, Peru's first female president. To do so, we estimate heterogeneous effects across groups defined by their likely level of bias against women. Our primary proxies, measured from ENAHO, include male respondents and districts with larger gender gaps in employment and wages. Additionally, we use data from the 2022 round of the Demographic and Health Survey (DHS) to construct a potentially more direct measure: the incidence of domestic violence.

The *Encuesta Demográfica y de Salud Familiar* (ENDES) is Peru's version of the DHS, conducted by INEI. It provides nationally representative data on fertility, maternal and child health, family planning, nutrition, and domestic violence, with a focus on women aged 15–49. ENDES follows DHS methodology, enabling international comparability. The survey is widely used in academic research and includes a sample of approximately 35,000 women.

We compute the incidence of domestic violence using data from DHS as follows. For each district, we calculate the share of women who report having experienced mild (e.g., being pushed, slapped, or punched), severe (e.g., being strangled, burned, threatened with a weapon, or attacked with a weapon), or sexual violence (e.g., being physically forced to have sex or coerced into unwanted sexual acts). In all cases, the reported violence refers to acts perpetrated by the respondent's current domestic partner at any point in time. Hence, throughout the document we make use of the terms "domestic violence" and "intimate partner violence" interchangeably. Importantly, the classification of violence into mild, severe, and sexual categories is defined by the survey itself and not by the authors (who would not use the label "mild" to refer to any form of domestic violence).

Based on these district-level shares, we construct a binary variable equal to 1 if the incidence of domestic violence is at or above the median, and 0 otherwise.

A.6 Armed Conflict Location and Event Data

We rely on the Armed Conflict Location and Event Data (ACLED) to define what we refer to as the "protest window." Specifically, we identify protests related to Pedro Castillo's ousting and track their intensity over time.

Our data cover the period from August 1, 2021, to December 31, 2023, and were retrieved on April 26, 2025. Following Sangnier and Zylberberg (2017), we use the term "protest" as an umbrella concept encompassing both riots and demonstrations, as classified by ACLED.

With that in mind, we apply a set of transformations to the data, which we describe in this section. First, we are not necessarily interested in *all* protests, but rather those specifically related to the impeachment. Fortunately, ACLED provides a brief yet detailed description of each event, which allows us to classify them accordingly.

For instance, we aim to distinguish events such as: "On 28 December 2023, in Huancayo (Junín), neighbors of the Chilca river sector staged a demonstration at the SEDAM offices to demand the completion of sanitation works that had been paralyzed for two years"—which are largely irrelevant to our study—from events like: "On 27 December 2023, around Coronel Portillo province (coded to Pucallpa, Ucayali), residents staged a demonstration to demand the resignation of President Dina Boluarte, the closure of Congress, and the calling of early presidential elections"—which are clearly within the scope of our analysis.

To make this distinction, we search event descriptions for a set of regular expressions and classify an event as impeachment-related if the description includes any of the following terms: "dina boluarte," "removal of former president Castillo," "removal of Pedro Castillo," "pro-Castillo," "general elections," "new constitution," or "removal from office." To ensure robustness to case differences, all text was converted to lowercase prior to matching.

Second, since all of our analyses are conducted at the district level, we match the coordinates of each event to district boundaries using publicly available data on the administrative geography of Peru.

Lastly, we attempt to recover the size of the crowd participating in each event. This process is necessarily heuristic, as there is no official—or even informal—authority responsible for producing accurate headcounts of protest participants. As a result, this variable is naturally subject to missing information, measurement error, and qualitative descriptors such as "large," "medium," or "massive."

Still, we attempt to convert ACLED's data on protest headcounts into a quantitative variable by applying a set of admittedly heuristic and ad-hoc rules. We make no claim to be producing accurate estimates, nor do we intend to contribute to the methodological advancement of crowd size measurement. Rather, our goal is simply to apply a consistent set of rules across time and space to translate qualitative descriptors into quantitative values, allowing us to distinguish relatively large events from smaller ones. Table A.1 presents the full set of rules used in this process.

We conclude this subsection by placing the protests against Dina Boluarte's regime in relative perspective. While the main text focuses exclusively on this subset due to space constraints, we show here that these were, in fact, the most prominent protests during the period in question.

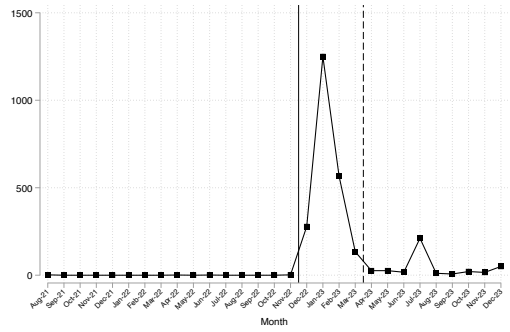
Figure A.1 shows time trends in event counts, crowd sizes, and the number of casualties, distinguishing between impeachment-related events (left-hand side) and all protest events (right-hand side). Clearly, events related to the impeachment account for the lion's share of riots and demonstrations observed during our study period.

Table A.1: Heuristic Rules for Converting Protest Size Descriptions to Headcounts

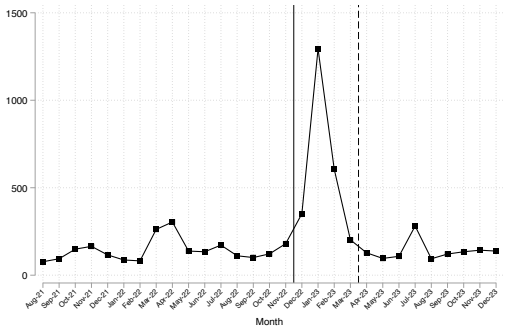
Description (matched text)	Assigned Headcount
"x"	x
"between x and y"	x
"three"	3
"six"	6
"small"	12
"a dozen", "dozen"	12
"dozens"	24
"a hundred", "hundred"	100
"hundreds"	250
"a great number", "a large number", "large group", "massive", "numerous", "significant", "several", "a numerous group"	250
"thousand"	1000
"between two hundred to two thousand"	1000
"thousands"	2500

Note: The table summarizes our recoding of qualitative protest headcount descriptions into scalar values. To clarify, the first two rows should be interpreted as follows: if the crowd size is explicitly stated as a number x, we assume x as the protest size. Similarly, when a range is described, we take the lower bound of the range as the assigned value.

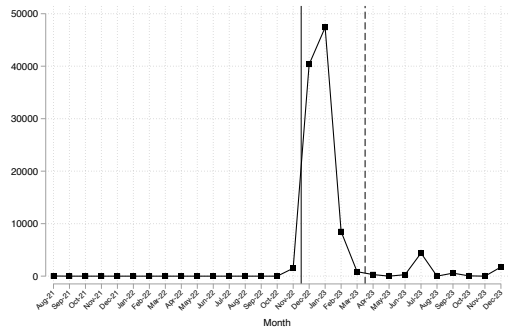
Figure A.1: Time Trend in Event Counts, Crowd Size, and Casualties in Riots and Demonstrations



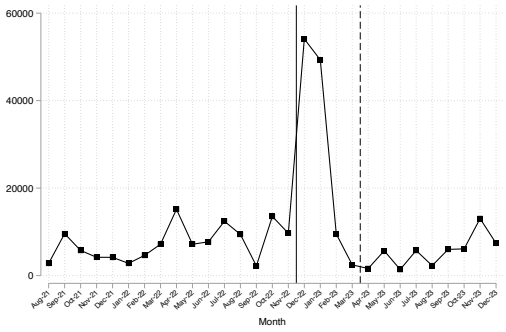
(a) Event Counts (Impeachment-Related)



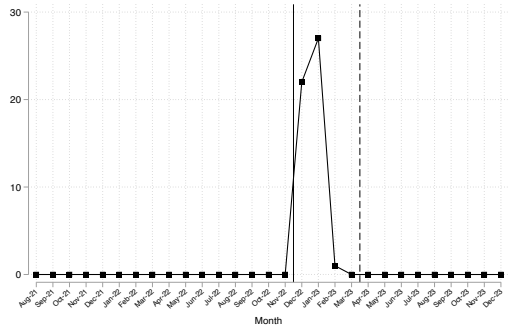
(b) Event Counts (Any Event)



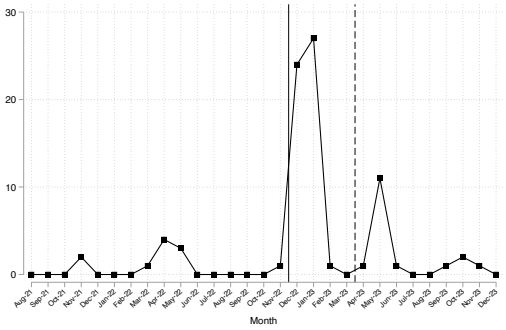
(c) Crowd-Size (Impeachment-Related)



(d) Crowd-Size (Any Event).



(e) Number of Casualties (Impeachment-Related)



(f) Number of Casualties (Any Event)

Note: This figure displays the time trends in event counts, estimated crowd sizes, and the number of casualties. The left-hand side focuses on events related to the impeachment, while the right-hand side includes all protest events. Protest intensity is clearly concentrated among impeachment-related events, which represent the bulk of mobilization during the period under study. Source: ACLED.

A.7 World Value Survey and Latinobarómetro

For the cross-country analysis of tax morale erosion, we draw on two complementary sources: the World Values Survey (WVS) and the Latinobarómetro (LB). The WVS is a large-scale, non-commercial academic survey program fielded in waves roughly every 5 years since 1981, harmonizing a common questionnaire across nearly 100 countries that together cover the vast majority of the world's population. Latinobarómetro is an annual public opinion survey that interviews about 20,000 respondents across 18 Latin American countries, representing more than 600 million inhabitants. Both surveys provide rich measures of political attitudes, institutional trust, democratic legitimacy, and, in selected waves, tax morale.

In the WVS, and in a subset of LB waves, the following question is available: "On a scale of 1 to 10, where '1' means 'not at all justifiable' and '10' means 'totally justifiable', how justifiable do you believe it is to evade paying taxes?" We match survey waves to a global list of presidential impeachment episodes and retain cases for which at least one wave is available before the episode and another wave is fielded within one year afterward. Table A.2 lists the episodes that satisfy this criterion.

We use this item to construct comparable measures of tax morale across surveys and time. In addition to the raw 1–10 score, we construct an indicator for whether a respondent views tax evasion as justified to any extent, defined as one if the reported score is strictly greater than 1 and zero otherwise. This binary measure captures movements away from the "never justifiable" moral benchmark and yields an interpretable metric for before-and-after comparisons around impeachment episodes.

Table A.2: Presidential Impeachment Episodes used in the Cross-Country Analysis

Country	Year	President
Peru	2000	Alberto Fujimori
Ecuador	2005	Lucio Gutiérrez
Paraguay	2012	Fernando Lugo
Brazil	2016	Dilma Rousseff
Peru	2022	Pedro Castillo

Note: This table lists the successful presidential impeachment episodes included in the cross-country analysis. Episodes are selected based on the availability of harmonized tax morale measures in the World Values Survey (WVS) or Latinobarómetro (LB) in at least one wave before the impeachment and another wave fielded within one year after the event.

A.8 RENAMU: Municipality Registry

One of our remaining concerns is separating changes in citizens' willingness to contribute—i.e., tax morale—from changes in local governments' enforcement capacity. With this goal in mind, we construct measures of municipal enforcement capacity.

The data come from the Registro Nacional de Municipalidades (RENAMU), an annual administrative census conducted by Peru's National Institute of Statistics (INEI) that collects detailed information on all provincial, district, and populated-center municipalities nationwide.

Information is gathered through self-administered questionnaires. The mayor may complete the form directly or appoint a designated official, who compiles information from the relevant municipal offices (e.g., administration, tax collection, public services, security, and environment). The questionnaires collect rich institution-level data on municipal characteristics and operations, including governance and planning instruments, staffing and human resources, tax administration capacity, licensing activity, public service provision (sanitation, security, health, and social programs), infrastructure and equipment, ICT adoption, environmental management, and disaster-risk preparedness.

We observe 1,678 districts. Most variables are reported for specific reference periods, particularly those related to personnel. For example, municipalities report the number of workers employed at specific points in time—typically March and December—rather than on a continuous monthly basis. As a result, it is not possible to recover month-to-month variation from this source. We therefore slightly modify our specification by replacing month fixed effects with a before–after indicator. This adjustment is necessary because, in some cases, we observe only two time periods per municipality, rendering fixed effects uninformative. Below, we define the variables used and explicitly describe the timing of their measurement.

- Total number of workers: We use responses to "Total personnel" reported as of December 2021,

March 2022, December 2022, and March 2023.

- Number of workers in tax collection activities: We use responses to "Total personnel exclusively dedicated to tax collection management" reported as of December 2021, March 2022, December 2022, and March 2023. For municipalities without a tax collection division, we assign a value of zero.
- Unit for prosecution of tax delinquents: We construct a dummy variable equal to one if the municipality reports having a unit dedicated to the prosecution of tax delinquents. This information is available for December 2022 and December 2023.

B Supporting Information

Throughout the paper we have referenced several institutions and events which carry significant, but secondary importance. For example, we have referenced irregular presidential transitions worldwide, previous regular presidential transitions in Peru, and we have produced results related to the tax system in Peru which we have not carefully described to keep the main manuscript resonably readable. In this appendix, we will provide that information.

B.1 The Ollanta-Kuczynski Transition

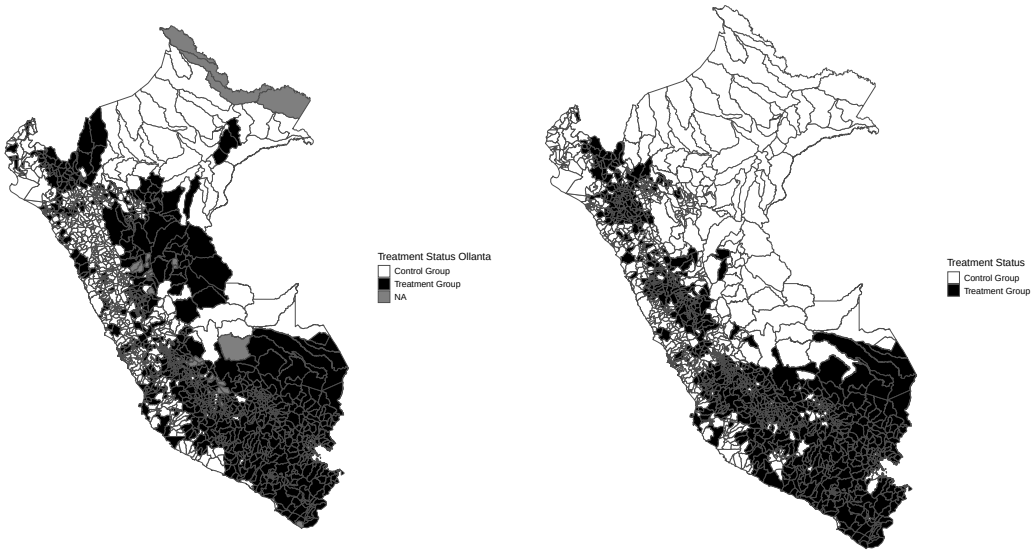
The transition from Ollanta Humala to Pedro Pablo Kuczynski on July 28, 2016, marked the second-to-last routine presidential handover in Peru as of the time of writing. Occurring through a peaceful and democratic electoral process, the transition represented a significant moment in Peru's political development—demonstrating a smooth shift from a left-leaning administration to a centrist, pro-market government.

Ollanta Humala, a former military officer, served as president from 2011 to 2016. Although initially elected on a left-wing nationalist platform, his administration gradually moved toward more centrist and market-oriented policies. Nonetheless, social inclusion remained a central focus of his presidency, with sustained efforts to integrate historically marginalized populations into the broader economy. Humala frequently described this approach as a strategy to "include to grow," highlighting his commitment to equity-driven development even as his government moderated its ideological stance.

Pedro Pablo Kuczynski, commonly known as PPK, is an economist and former investment banker who held several key government positions prior to his presidency, including Minister of Economy and Finance. In the 2016 presidential election, he ran as the candidate of the center-right party Peruanos Por el Kambio and narrowly defeated Keiko Fujimori in the runoff. His platform centered on economic liberalization, infrastructure development, and anti-corruption reforms. Although his administration maintained a moderate tone, Kuczynski consistently aligned with the far-right. His covert negotiations with Kenji Fujimori—son of former president Alberto Fujimori and brother of Keiko Fujimori—ultimately led to political scandal and his resignation in 2018. Despite this fallout, Kuczynski later expressed support for Keiko Fujimori during her presidential campaign against Pedro Castillo.

We argue that using this transition as a placebo experiment is adequate because its the most recent transition from a left-leaning government to a right-leaning one. More importantly, as we show below, the distribution of votes for Castillo and Humala in the first round of the presidential elections in 2021 and 2011, respectively, has substantial spatial correlation. Therefore, our placebo test is as best as an approximation can get to studying how do our treatment units react to routine (i.e., non-impeachment) presidential transitions to candidates that they do not support.

Figure B.1: Spatial Distribution of Election Results. Side-by-Side Comparison Between 2011 and 2021 Presidential Elections.



(a) 2011 Presidential Elections.

(b) 2021 Presidential Elections.

Note: This map shows the results of the first round of the 2011 and 2021 presidential elections in Peru. In panel (a) ((b)), districts where Ollanta Humala (Pedro Castillo) received the highest vote share are marked in black (treatment group), while districts where he did not win are marked in white (control group).

B.2 More Details on Tax Collection in Peru

Peru's tax system is notoriously complex—some international assessments even rank it among the most complicated in the world.[38] Given this, providing a comprehensive overview would be overly ambitious. Instead, in this section, we focus on the main features of the taxes and licensing items for which we have data. Specifically, we refer readers to the relevant legal frameworks governing their collection and briefly summarize key aspects such as payment schedules, responsible authorities, enforcement mechanisms, and penalties for non-compliance. The concept of UIT will be mentioned frequently, it amounts to close to 5,000 PEN (local currency), which is around 2.5 times the mean wage.

Labor Formality. Labor obligations are primarily regulated under the *Texto Único Ordenado del Decreto Legislativo N.º 728 – Ley de Productividad y Competitividad Laboral* and the *Ley General de Inspección del Trabajo* (Ley N.º 28806). Employers must register their workers in the *Planilla Electrónica* and ensure contributions to a health insurance provider and pension systems are made. Payments are due monthly. The enforcement body is SUNAFIL (*Superintendencia Nacional de Fiscalización Laboral*), which conducts inspections and imposes sanctions. Fines can reach up to 114 UIT for serious violations such as unregistered employment, and may include temporary business suspension.

Workplace Formality. Businesses are required to obtain a *Licencia de Funcionamiento* prior to operating, as established by Ley N.º 28976 (*Ley Marco de Licencia de Funcionamiento*) and local ordinances. This process is overseen by municipal governments. Fees are paid once upon application, and the license remains valid unless revoked. Enforcement is carried out by municipalities, sometimes in coordination with SUNAFIL or the police. Operating without a license can result in fines and immediate closure of the establishment.

Revenue Registration. Businesses must report income and pay taxes through SUNAT, under the *Código Tributario* (Decreto Supremo N.º 133-2013-EF) and the *Ley del Impuesto a la Renta* (D.S. N.º 179-2004-EF). Income taxes and VAT (IGV) are reported monthly via the *PDT* or *Mis Declaraciones y Pagos* systems. Annual declarations are required as well. SUNAT uses third-party data to cross-check filings and conducts electronic audits. Penalties for underreporting or non-compliance include interest, fines, or temporary suspension. Persistent offenders may be flagged as high-risk taxpayers.

Municipal Tax Collection. Local governments collect taxes under the *Ley de Tributación Municipal* (Ley N.º 27972), including the *Impuesto Predial* (property tax), *Impuesto de Alcabala* (real estate transfer tax), *Impuesto al Patrimonio Vehicular* (vehicle tax), and *Arbitrios Municipales* (public service fees). Property taxes are paid in four installments (March, May, August, November); arbitrios are often billed quarterly. Municipalities enforce payment through administrative processes, including fines and interest. However, enforcement is comparatively weak. Importantly, municipalities offer *fraccionamiento tributario* (installment agreements) that allow taxpayers to settle debts over time. These agreements are regulated locally but are common and often generous, reflecting the limited enforcement capacity at the municipal level.

Our brief summary leads to a stylized but useful conclusion: regulations governing labor formality, workplace registration, and income reporting are considerably stricter than those related to municipal tax collection. The latter are relatively lax and often allow for legal deferrals or negotiated payment agreements. In contrast, violations of labor or workplace formality regulations can result in significant penalties, including substantial fines or even business closure.

B.3 Irregular Presidential Transitions Around the World

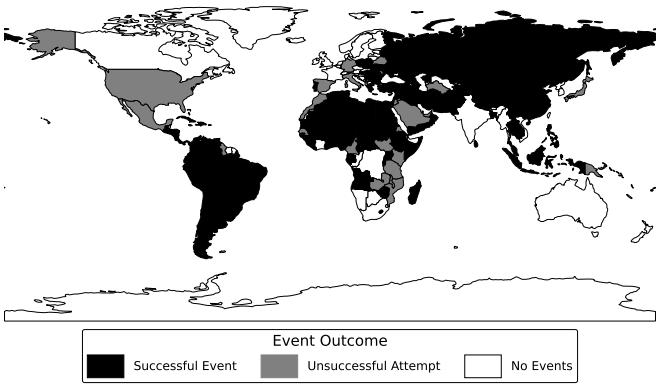
We present more facts about the prevalence of irregular presidential transitions throughout space and time. In this section, we focus on coups and self-coups.

This is because our episode also shares features with a self-coup—a particularly common mechanism

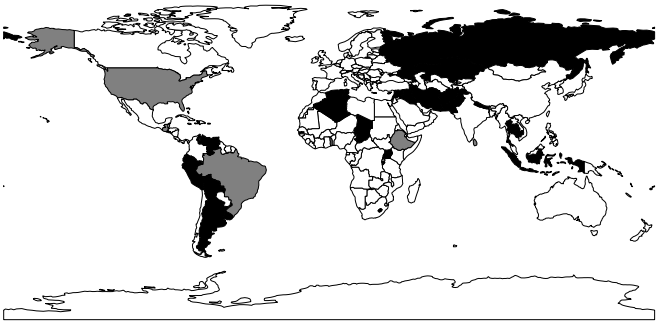
[38] see, for example, `https://www.taxcomplexity.org`

of democratic erosion in which an incumbent executive, originally elected through regular procedures, attempts to bypass or suspend constitutional constraints in order to retain or consolidate power. Since the end of World War II, at least 31 countries have experienced a self-coup, or an attempted self-coup, and 134 countries have witnessed at least one coup or attempted coup of any type (see Figure B.2). These figures underscore the widespread prevalence of irregular presidential transitions in the postwar period.

Figure B.2: World Map of Irregular Presidential Transitions Since World War II



(a) Self-Coups

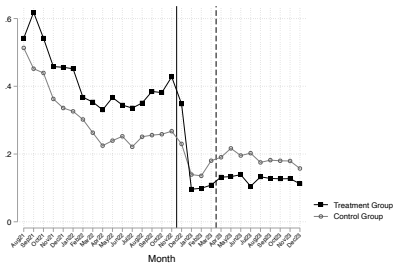


(b) Coups

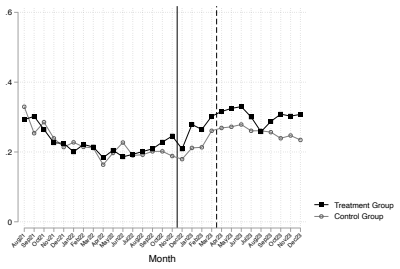
Note: The figure displays the global incidence of self-coups and coups since 1945. Panels (a) and (a) report the incidence of self-coups and coups, including attempts, extracted from Peyton et al. (2025). Countries shaded in black experienced at least one event of the corresponding type. Grey-shaded countries had at least one attempt that did not succeed. White indicates no attempts or no irregular transitions of that type.

C Further Robustness Checks

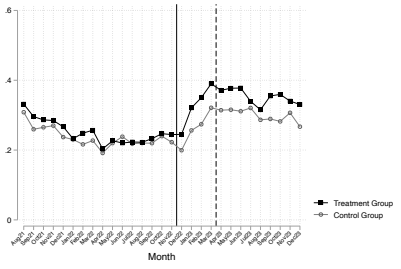
Figure C.1: Time Trends in Selected Outcomes by Group.



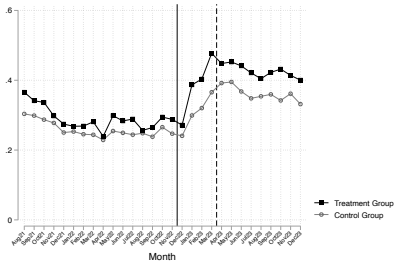
(a) Central Government.



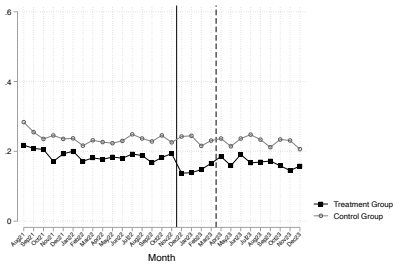
(b) Regional Government.



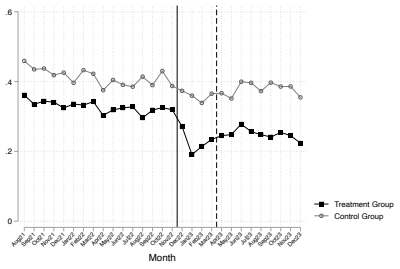
(c) Provincial Government.



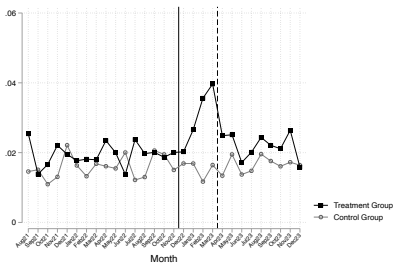
(d) District Government.



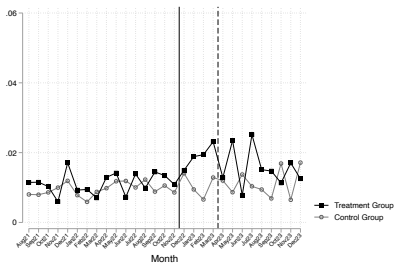
(e) Trust in the Armed Forces.



(f) Trust in the Police.



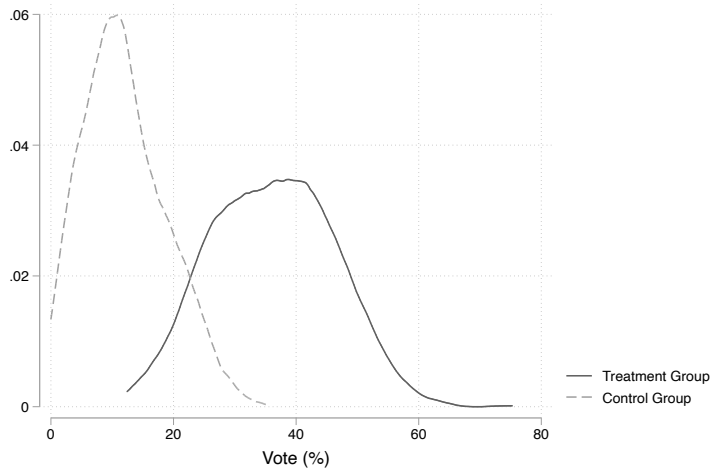
(g) Discrimination by Skin Color.



(h) Discrimination by Origin.

Note: The figure displays monthly averages of selected outcomes by group. The treatment group is represented by solid squares and a black line, while the control group is shown with hollow circles and a grey line. The solid vertical line marks the timing of the impeachment, while the dashed line indicates the end of the associated protests. Data comes from ENAHO.

Figure C.2: Distribution of First Round Voting Share for Castillo by Group. Supporters vs. Non-Supporters



Note: The figure displays the distribution of Pedro Castillo's vote share in the first round of the 2021 presidential election. The dashed grey line represents the control group (districts where Castillo did not receive the highest vote share), while the solid black line corresponds to the treatment group (districts where Castillo won). Data comes from Infogob.

Table C.1: Effects of Irregular Presidential Transitions on Tax Revenues. 2×2 Specification.

Coefficient/Log.Tax	(1) Total	(2) Property	(3) Prod. and Cons.	(4) Other
Treatment Effect	-0.178***	-0.170***	-0.125	-0.063
	(0.040)	(0.040)	(0.108)	(0.152)
Obs.	21,386	21,356	3,395	5,427
Control Sample Mean	9.958	9.931	9.212	7.934
Control Sample SD	2.803	2.784	2.532	2.593

Note: The table reports estimates from equation 2 along with their standard errors. Control sample mean and standard deviations at baseline are reported for reference. The specifications use data from August 2021 to March 2023. The outcomes are the logarithm of total tax revenue and its components. The data come from SIAF. Cluster-robust standard errors in parentheses.

Table C.2: Effects of Irregular Presidential Transitions on Government Approval. 2×2 Specification.

Outcome	Coefficient	p-value	q-value	Control Group Mean	Control Group SD	Obs
Central Government	-12.179	0.000	0.000	30.375	45.989	44,112
	(1.465)					
Regional Government	3.572	0.009	0.028	22.032	41.448	40,210
	(1.371)					
District Government	4.069	0.021	0.028	25.799	43.754	45,038
	(1.757)					
Provincial Government	4.254	0.011	0.028	23.592	42.459	42,081
	(1.665)					

Note: The table reports estimates from equation 2 along with their standard errors, p-values, and q-values computed following the step-down procedure in Holm (1979). The specifications use data from August 2021 to March 2023. The outcomes are dummy variables indicating approval of different government layers. All data come from ENAHO. Cluster-robust standard errors in parentheses. All values are reported in percentage points.

Table C.3: Effects of Irregular Presidential Transitions on Institutional Trust. 2×2 Specification.

Outcome	Coefficient	p-value	q-value	Control Group Mean	Control Group SD	Obs
Armed Forces	-4.498	0.000	0.004	41.209	49.222	47,307
	(1.218)					
Police	-3.118	0.002	0.035	23.776	42.572	49,364
	(1.009)					
Judicial Power	-2.057	0.021	0.344	16.509	37.127	46,395
	(0.893)					
Anti-corruption Comission	-2.036	0.036	0.541	14.753	35.464	37,777
	(0.970)					
Ombudsman	-1.918	0.094	1.000	25.100	43.360	43,341
	(1.146)					
Catholic Church	-1.793	0.129	1.000	48.522	49.979	49,971
	(1.179)					
Min. of Educ.	-1.688	0.238	1.000	42.117	49.376	48,333
	(1.428)					
JNE	-1.565	0.157	1.000	17.448	37.953	43,846
	(1.106)					
Radio/TV	-1.334	0.145	1.000	19.757	39.817	49,091
	(0.915)					
Attorney General Off.	-0.938	0.299	1.000	17.091	37.644	41,917
	(0.903)					
Comptroller Off.	-0.797	0.447	1.000	16.339	36.973	39,015
	(1.048)					
Political Parties	-0.433	0.368	1.000	4.318	20.326	48,293
	(0.481)					
Counter-Corruption Off.	-0.259	0.767	1.000	13.962	34.660	40,256
	(0.871)					
SUNAT	-0.174	0.879	1.000	23.017	42.095	40,837
	(1.136)					
RENIEC	-0.129	0.927	1.000	55.346	49.715	48,506
	(1.400)					
Written Press	-0.086	0.905	1.000	13.999	34.698	47,218
	(0.723)					
Congress	-0.080	0.886	1.000	7.091	25.668	47,810
	(0.563)					
ONPE	0.222	0.851	1.000	20.601	40.445	44,850
	(1.179)					

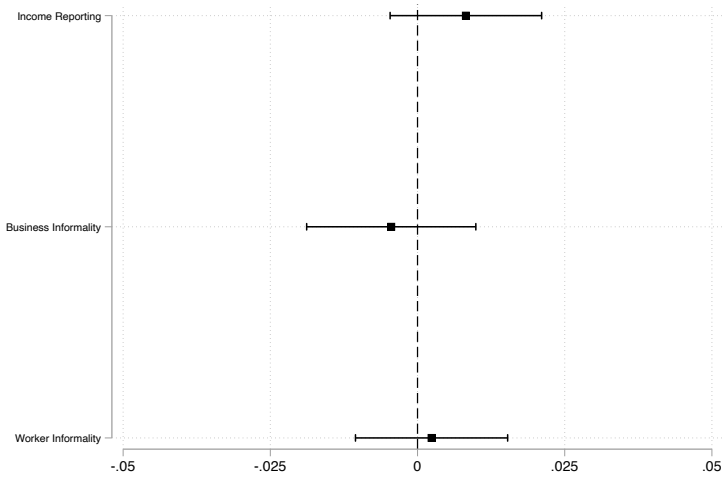
Note: The table reports estimates from equation 2 along with their standard errors, p-values, and q-values computed following the step-down procedure in Holm (1979). The specifications use data from August 2021 to March 2023. The outcomes are dummy variables indicating trust in a large set of government and civil institutions. All data come from ENAHO. Cluster-robust standard errors in parentheses. All values are reported in percentage points.

Table C.4: Effects of Irregular Presidential Transitions on Discrimination. 2×2 Specification.

Outcome	Coefficient	p-value	q-value	Control Group Mean	Control Group SD	Obs
Skin color	1.215	0.002	0.018	1.582	12.479	51,665
	(0.382)					
Origin	0.783	0.007	0.079	0.952	9.712	51,666
	(0.291)					
Income	0.485	0.152	1.000	1.242	11.073	51,666
	(0.338)					
Clothing	0.426	0.162	1.000	1.162	10.718	51,666
	(0.305)					
Traditions	0.405	0.021	0.215	0.467	6.816	51,666
	(0.176)					
Education	0.251	0.390	1.000	0.677	8.199	51,666
	(0.292)					
Language	0.162	0.591	1.000	0.971	9.805	51,666
	(0.301)					
Disability	0.140	0.375	1.000	0.369	6.061	51,666
	(0.158)					
Age	0.132	0.627	1.000	1.055	10.216	51,666
	(0.271)					
Sexual orientation	-0.024	0.652	1.000	0.075	2.732	51,666
	(0.054)					
Other	-0.111	0.450	1.000	0.546	7.370	51,666
	(0.146)					
Gender	-0.126	0.354	1.000	0.341	5.827	51,666
	(0.136)					

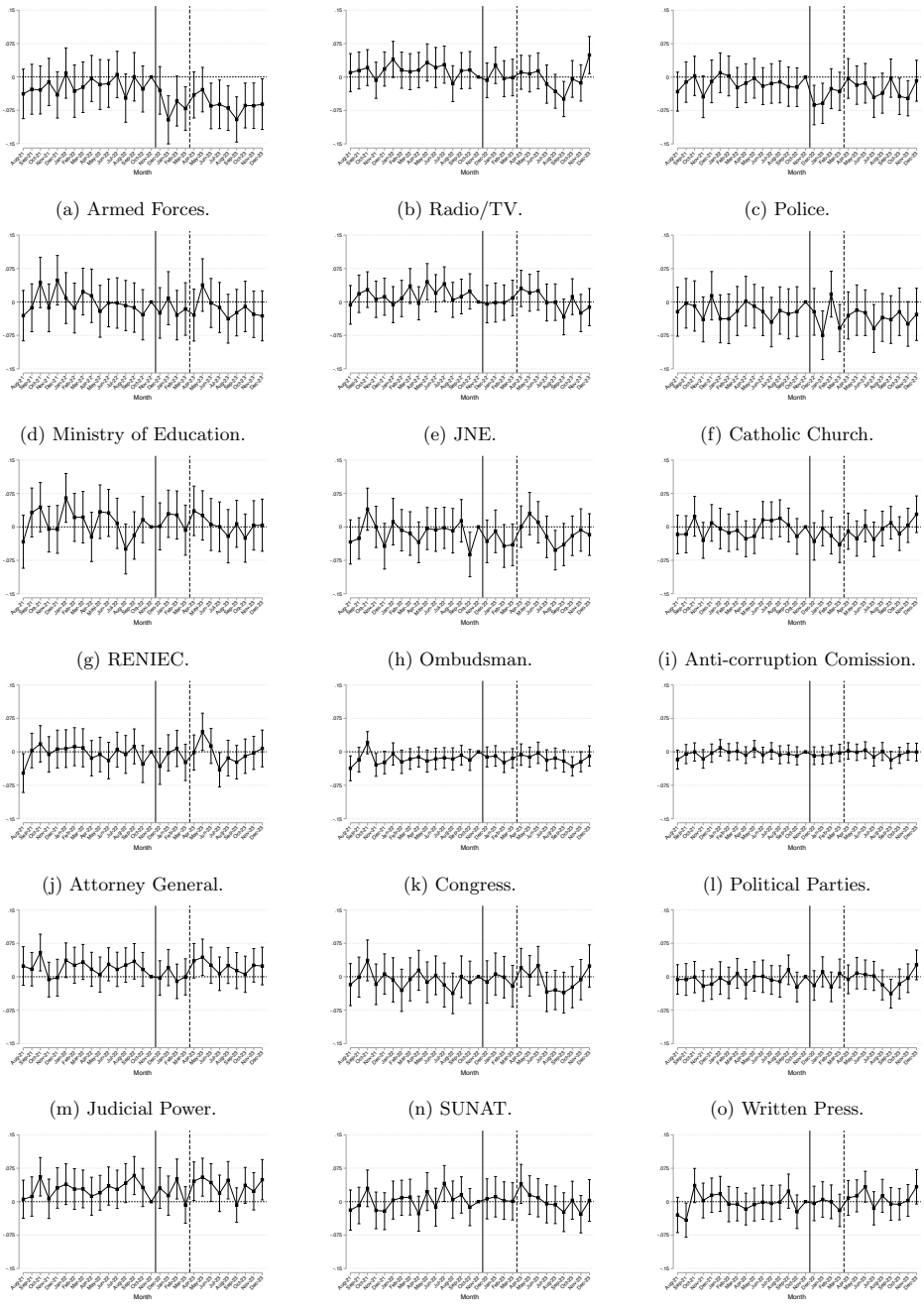
Note: The table reports estimates from equation 2 along with their standard errors, p-values, and q-values computed following the step-down procedure in Holm (1979). The specifications use data from August 2021 to March 2023. The outcomes are dummy variables indicating whether respondents report having experienced discrimination based on a range of attributes. All data come from ENAHO. Cluster-robust standard errors in parentheses. All values are reported in percentage points.

Figure C.3: Effects of Irregular Presidential Transitions on Worker Informality, Business Formality, and Income Reporting.



Note: The figure displays the β coefficients from equation 2, along with their 90% confidence intervals. These are canonical difference-in-differences estimates of the treatment effects using data spanning August 2021 to December 2023. The outcomes are dummy variables indicating whether a worker is informal, employed by an informal firm, or operates a business that does not record income for official reports. Data comes from ENAHO.

Figure C.4: Effects of Irregular Presidential Transitions on Institutional Trust. Dynamic Specification.



(a) Armed Forces. (b) Radio/TV. (c) Police.

(d) Ministry of Education. (e) JNE. (f) Catholic Church.

(g) RENIEC. (h) Ombudsman. (i) Anti-corruption Comission.

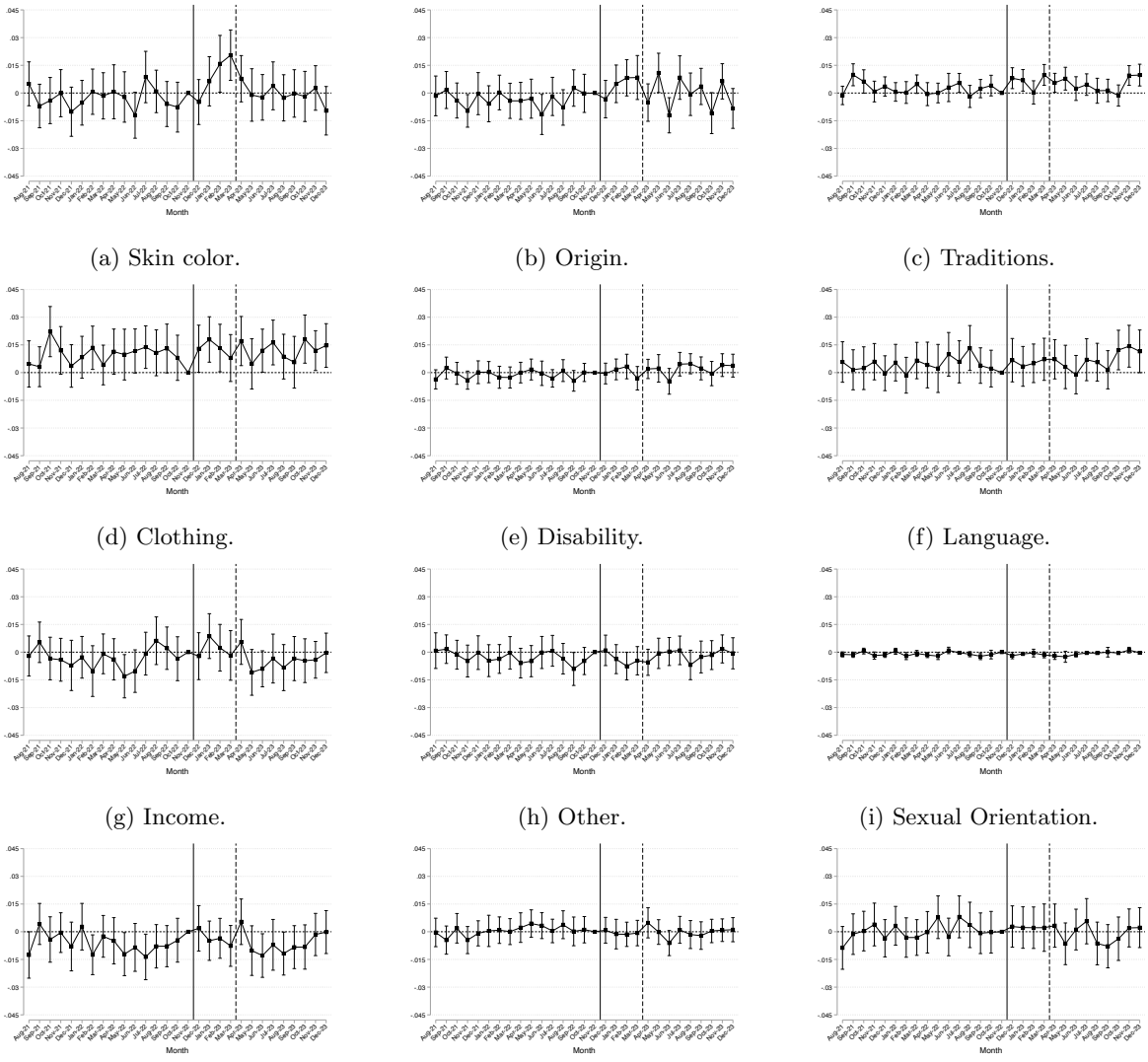
(j) Attorney General. (k) Congress. (l) Political Parties.

(m) Judicial Power. (n) SUNAT. (o) Written Press.

(p) Comptroller Office. (q) ONPE. (r) Counter-Corruption Office.

Note: The figure displays the δ coefficients from equation 1, along with their 90% confidence intervals. The coefficient for the month immediately preceding treatment is normalized to zero. The solid vertical line marks the timing of the impeachment, while the dashed line indicates the end of the associated protests. The outcomes are dummy variables indicating trust in a particular institution. Data comes from ENAHO.

Figure C.5: Effects of Irregular Presidential Transitions on Discrimination. Dynamic Specification.



(a) Skin color. (b) Origin. (c) Traditions.

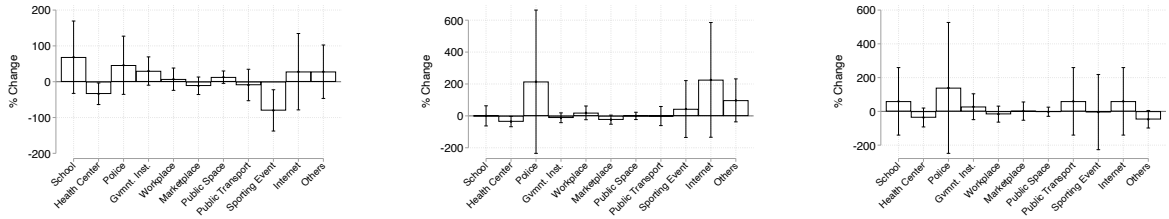
(d) Clothing. (e) Disability. (f) Language.

(g) Income. (h) Other. (i) Sexual Orientation.

(j) Age. (k) Gender. (l) Education.

Note: The figure displays the δ coefficients from equation 1, along with their 90% confidence intervals. The coefficient for the month immediately preceding treatment is normalized to zero. The solid vertical line marks the timing of the impeachment, while the dashed line indicates the end of the associated protests. The outcomes are dummy variables indicating being discriminated against based on a range of attributes. Data comes from ENAHO.

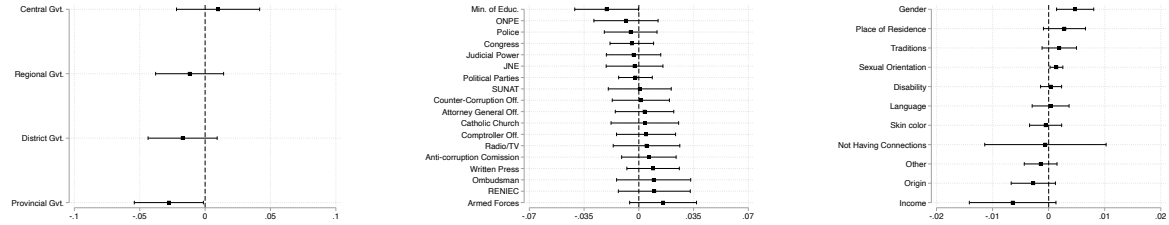
Figure C.6: Source of Discrimination.



(a) Skin color. (b) Origin. (c) Traditions.

Note: For each setting, we estimate a linear regression of the form $Y_{idt} = \beta_0 + \beta_1 \text{Post}_t + \varepsilon_{idt}$, where Y_{id} is a dummy variable indicating whether individual i in district d reported that specific setting as a source of discrimination, and Post_t is an indicator for the post-impeachment period. The regression is estimated on the subsample of treated individuals only, so that the coefficient captures the change in reporting behavior within the exposed group. Percentage change relative to the baseline mean by taking the ratio $\frac{\beta_1}{\beta_0} \times 100$. 90% confidence intervals are derived using the delta method.

Figure C.7: Effects of Regular Presidential Transitions on Government Approval, Institutional Trust, and Discrimination. Placebo Experiment.



(a) Effects on Gvt. Approval. (b) Effects on Institutional Trust. (c) Effects on Discrimination.

Note: The figure displays the β coefficients from equation 2, along with their 90% confidence intervals. These are canonical difference-in-differences estimates of the treatment effects across the period from April 2015 to November 2016. The outcomes include indicators of government approval (panel (a)), trust in institutions (panel (b)), and reported experiences of discrimination (panel (c)). Data comes from the 2015-2016 rounds of ENAHO.