Apex Corruption Erodes Democratic Values *

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Abstract

We combine a field experiment with cross-country evidence to demonstrate that apex corruption—corrupt acts implicating top-level politicians—causes large decreases in democratic values and associated behaviors, including voter turnout, volunteering in election organization, and honesty and trust in incentivized games. These effects are strongest when corruption is revealed near elections and among incumbents ex ante viewed as honest. We further show, both experimentally and by drawing upon evidence from revelations across 17 countries, that apex corruption weakens explicit support for democracy. These findings reveal a transparency–legitimacy tradeoff: exposing apex corruption may strengthen accountability while eroding support for democracy.

JEL: D72, D73, C93, C72, D02, D91, K42, O10, P00, Z1
Keywords: apex corruption, democratic backsliding, institutions, norms, trust, voting.

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"The Presidency is not merely an administrative office. That's the least of it. . . . *It is pre-eminently a place of moral leadership.*"
- Franklin D. Roosevelt, September 11, 1932

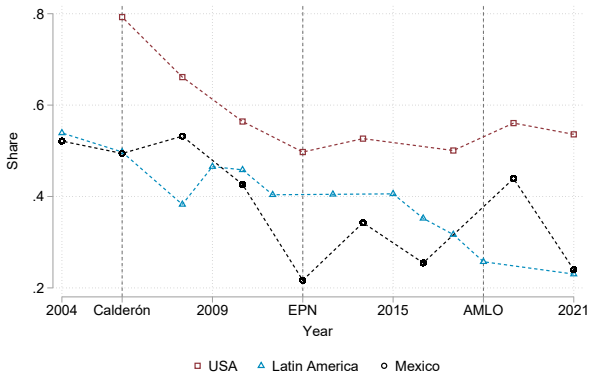
1 Introduction

Democratic institutions, once believed to be stable, now face increasing challenges in many countries around the world (V-DEM, 2023). Political parties openly skeptical of democratic institutions are part of ruling coalitions in roughly a quarter of countries (Funke et al., 2023). The willingness of citizens to stand up for democratic institutions is often viewed as a key driver of democratic resilience (Besley and Persson, 2019; Claassen, 2020); yet both globally and in Latin America, citizens' *dissatisfaction* with democracy has increased substantially since the mid-1990s (Foa et al., 2020, see also Figure 1a). Diagnosing the underlying causes of this erosion in democratic support is crucial for identifying ways to reverse it.

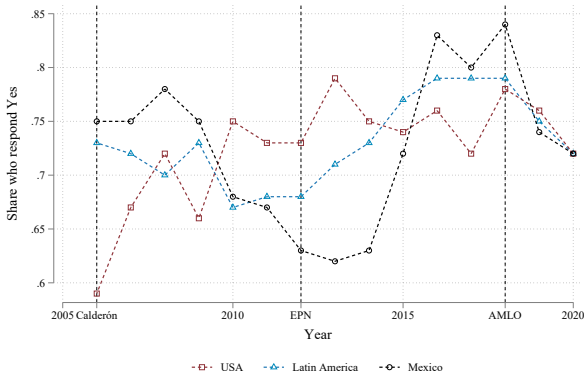
While democratic values are eroding, citizens increasingly perceive corruption as widespread, particularly in the wake of high-profile scandals implicating top-level politicians. In Latin America, the share of people who report corruption as their country's most important problem has more than doubled, from 4% in 2010 to 10% in 2018. Latin America is not alone. The United States, too, has seen dramatic falls in democratic satisfaction, while the share of citizens that claim that corruption is widespread in government has risen to rival that of Latin America (Figure 1b). Similar results appear using the Latinobarometer survey, or when focusing on support for democracy versus other systems rather than satisfaction with democracy.

Figure 1: Democratic Satisfaction and Perceptions of Widespread Governmental Corruption

(a) Satisfied with Democracy



(b) Corruption Widespread in Govt



Share of respondents "satisfied" or "very satisfied" with democracy (from Lapop, left) and answering "yes" to "Is corruption widespread throughout the government in this country, or not?" (from the Gallup World Poll, right); 0.4 stands for 40% of citizens. Reference lines represent Mexican presidential elections.

These macro patterns raise a key question: could exposure to *apex corruption*—corrupt acts implicating *apex* (top-level) politicians—be causally undermining democratic values? On one hand, exposing corruption is often regarded as essential to democratic accountability. Indeed, an important literature, though one focused almost exclusively on *local* political and bureaucratic corruption, emphasizes the role

of audits, watchdog organizations, political opposition, and a free press in strengthening governance and accountability (e.g. Ferraz and Finan, 2008; Chong et al., 2015; Dunning et al., 2019). On the other hand, revelations of *apex* corruption may have underappreciated downsides that can be particularly destructive of the complementary organizations, internalized norms, and beliefs that comprise liberal democratic institutions.[1]

We argue that this is because apex politicians differ from their local counterparts in key respects. First, because they directly oversee core state *organizations*, their malfeasance may discredit the very bodies meant to ensure accountability and electoral fairness. Second, as focal political role models, apex politicians are more likely to shape *social norms* (Bursztyn et al., 2020). Third, citizens may also update their *beliefs*, inferring from the success of corrupt leaders that such behavior is tolerated by both institutions and society. This channel is likely to be particularly germane in environments characterized by strategic complementarities in corruption, where incentives to cheat increase with beliefs that others will as well (Mauro, 1995; Klašnja et al., 2018), potentially generating multiple equilibrium levels of corruption. Moreover, apex corruption revelations typically have wider dissemination. These mechanisms may complement each other in explaining why revelations of apex corruption can markedly erode support for liberal democracy.

This paper provides the first direct causal evidence that exposure to evidence of apex corruption undermines the norms, beliefs, organizations, and voting behaviors that underpin liberal democratic institutions. We employ two complementary empirical strategies, combining a large-scale randomized controlled trial (RCT) to directly test the effects of the revelation of corrupt acts by apex politicians in Mexico, with a series of natural experiments exploiting corruption revelations across 17 Latin American countries to assess whether the pattern extends beyond the Mexican experimental setting.

The field experiment was implemented just prior to the 2021 Mexican federal congressional elections. We recruited more than 3,300 citizens door-to-door from 69 rural municipalities and the city of Oaxaca. We randomly assigned each individual to an unexposed control group or to view three minutes of real video footage showing high-level politicians exchanging stacks of cash. The footage featured apex politicians from either the ruling (and anti-corruption) Morena party or from opposition parties.[2] To test effect durability, in Oaxaca City, we further randomized the timing of the exposure to be either one month versus one week before the election. Our outcomes combine administrative data on *individual-level* voter turnout, with observed behaviors, incentivized lab-in-the-field games and survey-based attitudinal measures. Together, these outcomes allow us to test whether apex corruption affects each of the three institutional components emphasized by our framework: organizations, beliefs, and norms.

First, we show that exposure to apex corruption decreases actual voter turnout by $4pp$ to $6.2pp$ on average (or 6.7%–10.8%) and $15.6pp$ (or 27%) if the treatment occurred a week before the election. This contrasts with a number of important prior studies of *local* corruption revelations that tend to find null or muted effects on turnout (e.g. Dunning et al., 2019). Furthermore, and consistent with our pre-specified hypothesis (AEARCTR-000777), these declines are more pronounced when corruption revelations

[1] Here we build on Greif (2006)'s definition of *institutions* as a system of *(i) organizations, (ii) beliefs, and (iii) norms* that together induce regularities of behavior (in our case: support for the democratic process and voting itself).

[2] Oaxaca is large and diverse, encompassing 570 of Mexico's 2,476 municipalities. It includes both rural and urban areas, exhibits mid-to-high perceived corruption (ranked 6th out of 32 states), and features a competitive political environment with multiple parties represented in the local congress. As in much of Mexico, Oaxaca also showed strong support for the "anti-corruption" incumbent president, Andrés Manuel López Obrador (AMLO).

implicate the ruling Morena party, which successfully campaigned on an *anti-corruption* platform: footage of apex corruption implicating Morena decreased actual turnout by $22pp$ (39%) regardless of prior voter partisanship.

Second, beyond voting, we show that exposure to apex corruption, particularly when it implicates Morena, reduces pro-democratic behaviors mapped to each of the three institutional components emphasized by our framework. We find that treated citizens' willingness to support democratic *organizations* by volunteering as electoral observers immediately falls by 43% and their willingness to donate money to poll-booth volunteers drops by 16%. Using incentivized lab-in-the-field games, we find that *beliefs* about the trustworthiness of politicians and other citizens also diminish. Subjects exposed to apex corruption involving Morena entrusted less money to anonymous real politicians and neighbors, by 11.2% and 5.4%, respectively. Internalized *norms* of honesty also erode, despite high levels of prevailing honesty: we find that subjects behaved less honestly in a game in which they could steal money with impunity. Treated citizens stole 4.3% more when the rest would be donated to their municipal mayor for public projects, and (weakly) 2.6% more from an anonymous neighbor.

Third, based on pre-specified survey indices, we show that these behaviors reflect changes in attitudes and perceptions of which individuals are aware. The apex corruption treatments lead perceptions of corruption to strongly increase (0.23 SD - henceforth σ), even as support for democracy falls (by 0.16σ). These are accompanied by lowered self-reported trust in democratic institutions and in fellow citizens (0.19σ and 0.11σ, respectively). The effect on lowering support for democracy persists for at least four months after treatment, and appears to even cumulate with randomized exposure to a second scandal.

The randomized control trial also allows us to benchmark the apex-corruption effect against an important alternative explanation and to explore potential democracy-supporting interventions. Pervasive corruption often correlates with poor policy decisions and economic underperformance, which could also undermine support for democracy. To distinguish apex corruption from broader dissatisfaction with policy performance, we also randomly assigned a three-minute video detailing Mexico's poor economic and policy performance, but without providing any explicit evidence of apex corruption. We find that, in striking contrast to the apex corruption treatments, economic underperformance *increases* the probability an individual will vote (by 8–10pp, or 13%–17%).[3] We interpret these contrasting results as reflecting how economic underperformance —without direct evidence of apex corruption— instead tends to be viewed by citizens as a policy failure that can be addressed within the democratic system.

While the RCT provides strong causal evidence, a natural question is whether these results generalize beyond the Mexican setting and the specifics of the video treatments we employ. We therefore gather data on corruption scandals with different features from across 17 Latin American countries from 2008 to 2018. We then employ an interrupted survey design (following Bassi and Rasul (2017)), examining the effects on support for democracy among those who happened to be surveyed by Latinobarometro just after the breaking of an apex corruption scandal, relative to those surveyed just before. Consistent with the RCT, those surveyed just after an apex corruption scandal show significantly lower support for democracy (0.07σ) and lower trust in democratic institutions (0.07σ), while their perception that corruption is the main problem facing their country increases by 30%. We further find that the effects tend to be higher for scandals involving the *most apex* —i.e., heads of state. In contrast, and consistent with our hypothesis

[3] In further contrast to apex incumbent corruption, the economic underperformance treatment has no effect on willingness to donate time or money to support democratic institutions, though it does raise the propensities to steal opportunistically and to distrust.

on the differential importance of *apex* corruption revelations, those involving lower-level political officials show little or no effect on either corruption perceptions or democratic values.

This study contributes to our understanding in several ways. First, it complements other prominent explanations for democratic backsliding, which include financial crises and economic stagnation (e.g. Przeworski, 2019), austerity policies and government ineffectiveness (e.g. Howell and Moe, 2020; Fetzer, 2019), and the rise of social media and polarization. While an important literature in economics studies corruption (e.g. Banerjee et al., 2013; Fisman and Golden, 2017; Ajzenman, 2022; Gulino and Masera, 2023), including its extent (Svensson, 2003; Reinikka and Svensson, 2004), sources (Fisman and Miguel, 2007; Bertrand et al., 2007), organization (Shleifer and Vishny, 1993; Olken and Barron, 2009; Sánchez de la Sierra et al., 2024), detection, and policies to fight it (Olken, 2007), ours is the first study to comprehensively estimate the extent to which apex corruption undermines the norms, beliefs, organizations and voting behaviors that underpin liberal democratic institutions. As discussed above, conceptually, increased transparency about apex corruption could *increase* support for democracy, as it demonstrates that even the acts of even the most powerful politicians are being scrutinized and may be held to account. We show, however, that the opposite effect dominates.

Second, we innovate in our focus on *apex* corruption, adding a new dimension to an important literature that examines the effects of information disclosures about local governmental accountability and performance. Consistent with the accountability channel, seminal work by Ferraz and Finan (2008) shows that randomized audits of local government expenditure affects the re-election of mayors when irregularities are publicized by the media. Further, Chong et al. (2015) randomize the distribution of flyers that report the size of the municipality budget and the fraction that "did not comply with the [legal] norms" according to official government audits. They find that though the flyer had *no effect* on perceptions of corruption, there was a subsequent decrease— of $1.3pp$— in municipal election turnout. More generally, however, the Metaketa studies (Dunning et al., 2019) perform a coordinated multi-country RCT providing information to voters, and find "*no evidence overall that typical, nonpartisan voter information campaigns shape voter behavior.*" They attribute this to the information not having a discernible effect on perceptions of politicians' integrity. In contrast, we find strong effects on corruption perceptions. The experimental treatment we use—raw footage of apex politicians taking stacks of cash— comports with how these scandals often break in real world media (see e.g. McMillan and Zoido (2004)), while leaving little room for interpretation or confusion. This may explain why we find one of the largest effects on turnout in the literature.[4]

Third, we make several other methodological contributions. Our ability to observe voter turnout with administrative data and randomize at the individual level allows us to circumvent both potential social desirability and ecological inference concerns. Importantly, it also allows us to directly compare the impact of corruption implicating a ruling anti-corruption party (Morena), opposition parties, and economic underperformance. Moreover, our time-randomized design allows us to test for decay of the effect over time, as well as for longer-term persistence and accumulation effects.[5] We further contribute by assessing the generality and heterogeneity of these effects using a large natural experiment spanning 17

[4] A related literature provides information on characteristics of political candidates (Kendall et al., 2015; Banerjee et al., 2024), not apex corruption.

[5] We could only find one paper (Bobonis et al., 2016) that studies the persistent effect of corruption audits on subsequent corruption, but they do not study values. As we describe below, our paper also relates to other important papers examining the contagion of local corruption (Ajzenman, 2022; Gulino and Masera, 2023).

countries. Two important recent papers also compare across countries, examining the effects of exposure to 3G network expansion on reducing incumbent support in favor of populist alternatives (Guriev et al., 2020) and the number of years living in successful democracies on raising democratic support (Acemoglu et al., 2025b). Our paper complements these in terms of methods, treatment, outcomes, and findings.[6]

Finally, we provide evidence consistent with theoretical contributions emphasizing how leaders and prominent figures can shape culture and institutions (e.g. Hermalin, 1998; Akerlof and Holden, 2016), as well as expectations and social norms (Bursztyn et al., 2020; Acemoglu and Jackson, 2015). Our paper adds to a growing body of empirical work demonstrating that individual leaders can not only influence organizational performance (e.g. Bertrand and Schoar, 2003; Jackson and Yariv, 2011; Bandiera et al., 2020), but can also have macro-level political consequences (e.g. Jones and Olken, 2005; Jha and Wilkinson, 2012; Bai et al., 2022; Cagé et al., 2023; Jha and Wilkinson, 2023). We show that credible revelations of corrupt acts by apex politicians generate substantial negative spillovers, weakening not only the norms, beliefs, and organizations that sustain liberal democratic institutions, but also broader measures of social capital, including trust in neighbors.

Section 2 provides the experimental context, Section 3 the field experiment design and Section 4 the main results. Section 5 presents the broader cross-country analysis, while Section 6 concludes. The Appendix provides supplementary results, including evidence from a second RCT showing the accumulation effects of repeated exposure to apex corruption, as well as the effects of two sub-treatments designed to explore potential remedies for declining democratic values: a nation-building video priming shared identity and the provision of common financial exposures. We find suggestive evidence that the latter provides some hope.

2 Context

Revelations of corruption at the highest levels of government are increasingly common, often involving very large sums disbursed among extensive networks. For example, in 2019, it was disclosed that a single company (Odebrecht) paid almost $ 1 billion in bribes in order to obtain contracts. Alleged recipients include top politicians, such as Brazilian President Michel Temer and Ecuador's Vice President Jorge Glas, among others. In Peru a special set of prison cells was constructed to house *four* ex-presidents implicated in the scandal.[7] Media coverage of corruption has also risen: the *ratio* of corruption mentions to other news has doubled in English and tripled in Spanish-language news outlets since 2000 (Figure OA-1).[8] Citizens' perceptions of the pervasiveness of corruption have also risen to high levels: 68% of citizens in Latin America in 2018 believed that half or more of their politicians are corrupt (LAPOP 2018), and perceptions that corruption is the main problem of society have more than doubled. While the increased transparency implied by these revelations may suggest that democracy is working, satisfaction with democracy has been

[6] Guriev et al. (2020) find that the average level of corruption in society was an important mediating factor, with 3G raising incumbent support only in the cleanest societies (i.e. Denmark and Switzerland). Acemoglu et al. (2025b) use a difference-in-difference approach to show that individuals living for longer periods of time in 'successful' democracies—including states that have performed well economically and effectively controlled corruption—are more supportive of democracy. However, they find that the flipside: the relationship between those living longer in 'unsuccessful' democracies (including more corrupt ones) and democratic support is less clear.

[7] See: "*Odebrecht case: politicians world-wide suspected in bribery scandal*", BBC News, 17 April 2019.

[8] Alternative measures of *retail* corruption, including bribe paying by firms and reports by experts, have also increased (Figure OA-3), suggesting that the media reports may not be entirely divorced from on-the-ground practice. While actual corrupt behaviors and perceptions of corruption do not correlate perfectly (Olken, 2009), it is worth noting that our main empirical strategy does not hinge on the accuracy of corruption perceptions. The experiment shows citizens evidence of actual apex corruption and tests its effects.

falling (Figure OA-2).

Latin America is not alone. The Panama Papers implicated 140 prominent politicians in 50 countries, spanning both rich and developing nations.[9] Even in a country once perceived to have been in a low-corruption equilibrium like the United States, in each of the 2016, 2020, and 2024 election cycles, the leading presidential candidates of both parties (or close family members) have faced allegations and investigations for corruption. Close to 80% of both Americans and Latin Americans agree with the statement that "*corruption [is] widespread throughout the government in this country*" (Figure 1(b)). Deepening concerns about such apex corruption have been accompanied by a populist wave, with political figures gaining traction with promises "*to drain the swamp*", i.e., seeking national renewal at the expense of existing democratic institutions that are seen as corrupt and compromised (see also Guriev et al., 2020).

A salient example of this broader pattern is Andres Manuel López Obrador (AMLO), Mexico's incumbent president at the time of our study. AMLO ran his 2018 presidential campaign on a nationalist populist platform, with a strong anti-corruption message. Indeed, leading national and international media dubbed AMLO an "*anti-corruption crusader*".[10] Deeply critical of existing institutions of government ("*Al diablo con sus instituciones [to hell with you and your institutions]!*" (Averbuch, 2018)), AMLO pledged to use savings from eliminating corruption to finance social policies and fight poverty, and founded a new party as yet untainted by evidence of corruption: *Morena— the Movement for National Regeneration*.

This approach appears to have resonated with many voters, who voted for him in a landslide. AMLO's decisive victory was arguably a strong rejection by voters of continued apex corruption in which many would implicate AMLO's predecessor as president, Enrique Peña Nieto. Peña Nieto was alleged to have directed millions in bribes while in office (New York Times, August 3, 2022.). Instead AMLO has cultivated an image of *apex honesty* by using commercial aircraft, driving a cheap car, and constantly repeating his motto "*no somos iguales*" [we are not the same] and his campaign slogan "*honestidad valiente*" [courageous honesty].

Both voting behavior and official national surveys suggest that people's beliefs both in the probity of the person of the president and the prospect of lower institutional corruption did indeed change. AMLO won election by more than 30 percentage points, and between 2017 (the year before he took office) and 2019 (one year after he took office), there was a 21 percentage point drop in the fraction of people who believe the Mexican presidency to be corrupt (ENCIG, 2017,2019; Figure OA-4).[11] Indeed, in our baseline survey, only 4% considered the incumbent party *Morena* to be more corrupt than the opposition. These perceptions plausibly afforded AMLO greater latitude to sow distrust and threaten legislative action against the well-regarded independent electoral agency (INE), among other institutions.[12]

AMLO's movement provides a compelling case in which a single leader plausibly reshaped corruption perceptions and support for democratic institutions, and a valuable empirical setting to test how unmet

[9] See "*The Power Players: Politicians in the Panama Papers, International Consortium of Investigative Journalists*, 2016.

[10] "*López Obrador, an anti-corruption crusader, has dominated polls this election season, riding a wave of anger about government corruption...*" (Texas Tribune, July 1 2018). "*The left-wing anti-establishment candidate, Andrés Manuel López Obrador, will make tackling corruption his top priority after winning Mexico's presidential election on Sunday*" (BBC). "*Mexico's new leader is riding a wave of anti-corruption furor that's changing Latin America*", and that "*The new president insists that his anti-corruption campaign —including a 50-point plan to fight corruption and reduce privileges for officials — will be transformative.*" (Washington Post, Nov 29, 2018). "*If you ask me to express in one sentence what the new government's plan is all about: end corruption and impunity.*" (Christie and Botello, 2020).

[11] A well-regarded survey in Mexico showed after the 2018 election 52% of citizens thought corruption would decrease with AMLO vs 22% that thought it would increase (Reforma).

[12] See e.g. Anne Applebaum, "How Do You Stop Lawmakers From Destroying the Law?", the Atlantic, March 1, 2023.

expectations of *apex honesty* affect political behavior. Accordingly, we pre-specified that revelations of apex corruption involving the new *anti-corruption* Morena party would have stronger effects than comparable revelations involving opposition parties, where past revelations of apex corruption had already tarnished their reputations.

3 The Field Experiment

We implemented a field experiment involving 3,331 adult Mexican citizens in Oaxaca, located in the south of Mexico. Oaxaca is one of Mexico's largest states with 570 municipalities out of Mexico's 2471, and ranked 6th out of 32 States in Mexico in corruption perceptions (*Mexicanos Contra la Corrupcion y la Impunidad*, 2021). Not surprisingly then, AMLO's anti-corruption message also resonated strongly among Oaxaqueños: 65% voted for Morena, 11% for PAN, 17% for PRI in 2018, the year AMLO was elected. Within Oaxaca, 41% of the sample was allocated to the capital, Oaxaca City, while the rest was allocated across 69 rural municipalities (map in Figure OA-5).

3.1 Video Treatments

The experiment had four main objectives: first, to estimate the causal effects of informing citizens about apex corruption; second, to separately estimate the effects of exposing corruption involving party leadership where prior beliefs of corruption were relatively low versus ones already viewed as compromised; third, to distinguish these effects from those of policy ineffectiveness and economic underperformance; and finally, to test two remedies designed to strengthen democratic institutions.

To ensure that the information was credible, impactful, and communicated in a simple, homogeneous, and scalable manner, we delivered the treatments using video footage of politicians exchanging and counting large sums of illicit cash, which we randomly assigned to participants on electronic tablets (see links below). Video recordings of apex corruption are not uncommon in Latin America—sometimes even produced by the perpetrators themselves (e.g. McMillan and Zoido, 2004)—and corruption revelations in the region are therefore often disseminated through such footage. We anticipated that videos would also be more persuasive than audit-based flyers, which tend to be more technical and have shown mixed and relatively small effects in prior work (Dunning et al., 2019). Consistent with this, 90 percent of survey participants reported believing the veracity of the video evidence.

We designed four videos corresponding to the four objectives described above.[13] The videos were implemented as part of an in-person survey. Each day, a team of enumerators visited randomly selected election precincts and knocked on doors explaining that they were affiliated with academic institutions and conducting a survey lasting approximately 30 minutes. Respondents were informed that they would receive 50 pesos for participating and could earn up to 400 pesos through behavioral games at the end of the survey. The survey was administered on a high-resolution Samsung tablet.

In the middle of the survey, respondents were told that they would watch a video. Neither enumerators nor respondents knew in advance which video would appear, as assignment was randomized by the tablet software. All participants watched the assigned three-minute video, after which the survey continued,

[13] Links: Incumbent corruption video: https://youtu.be/BSdueriPSA4 ; Opposition corruption video: https://youtu.be/HyLrEmvzqPQ ; Economic underperformance video: https://youtu.be/6Y8EM9uGmwE ; Nation-building video: https://youtu.be/-fE_UgbDl34.

allowing us to measure the outcomes described below.

Apex Corruption: We designed two apex corruption videos. The first part of each video presents official statistics on the fraction of public procurement contracts that are awarded directly without competitive bidding—explaining how this practice facilitates corruption—as well as evidence on the amounts paid by citizens through bureaucratic extortion to obtain permits and government services.[14] Nearly three-fourths of contracts—including large contracts—are assigned directly without competitive bidding, and close to half of Mexicans report having paid a bribe when pursuing bureaucratic procedures.

The second part of the videos differs across the two versions. The **Incumbent Corruption Video** (CI) shows footage of a brother of the incumbent president (AMLO) from the Morena party receiving large stacks of cash. The **Opposition Corruption Video** (CO) similarly presents footage of senior senators and politicians from the main opposition party (PAN) also receiving large sums. This video further discusses corruption associated with the two previous presidents from opposition parties (Calderón and Peña Nieto), as well as testimony from a politician from the other major opposition party (PRI) regarding large bribes taken by top-level PRI cabinet members. The two corruption videos were designed to be as similar as possible in content, subject to the constraint that they rely on actual real-world footage involving apex politicians.[15]

Economic underperformance: This video also lasts close to three minutes but makes no mention of corruption. Instead, it describes the performance of the last three Mexican governments (PAN, PRI, Morena) along three dimensions: poverty, economic growth, and government relief during the COVID-19 pandemic. Using official income-based poverty measures from Mexico's CONEVAL, CEPAL, and the World Bank, the video explains that roughly one in four Mexicans lived in poverty between 2000 and 2019, with limited net progress over this period, in contrast to the substantially larger poverty reductions achieved in Chile, Panama, and Uruguay. In terms of economic growth, it explains that Guatemala, Paraguay, Peru, the Dominican Republic, and Uruguay all experienced average GDP per capita growth rates from 2010 to 2019 that were more than twice as high as Mexico's over the same period. Finally, it shows that Mexico was among the countries with the highest COVID-19 death tolls globally at that point, and that the federal government's fiscal response was relatively modest, with direct COVID-related relief amounting to well under 1 percent of GDP—substantially lower than in many other countries

Nation-Building: As our first remedy treatment, we hired a professional media designer to produce an emotive national narrative emphasizing Mexicans' shared identity and cultural heritage. The video traces the formation of the nation from pre-colonial civilizations to contemporary Mexican culture, highlighting shared symbols, traditions, and values—such as food, music, family, and the national flag—as well as revered poets and historical figures, collective resilience in the face of natural disasters, and the long and contested path toward democracy. The intervention was motivated by an emerging literature showing that national narratives delivered in audiovisual formats can shape national identity and political attitudes

[14] The data are from INEGI's *Encuesta Nacional de Calidad e Impacto Gubernamental* (2017 and 2019) and include obtaining permits (e.g., to open a firm, to build, to obtain a driver's license), paying for services (e.g., electricity, water, taxes), requesting government loans, and using the courts, among others. We piloted this information prior to the study and found that, when presented as official statistics, citizens perceived it as credible and non-partisan.

[15] Both videos mention presidential corruption. However, because they rely on real-world natural footage, the content of the second part of the videos is naturally not as precisely similar across arms as one might find in a more stylized lab experiment. We address these issues below. It is further worth noting that, no one featured in the videos was ultimately convicted, as is frequently the case with other apex corruption scandals in Latin America.

(Levendusky, 2018; Esposito et al., 2023; Rohner and Zhuravskaya, 2023). Related evidence from recent large-scale experimental studies in the United States suggests that priming shared national identity can be particularly effective in countering anti-democratic attitudes (Voelkel et al., 2024). Accordingly, we conjectured that the nation-building narrative would increase trust, foster pro-social behavior, and strengthen support for democratic institutions.

3.2 Randomization

We thus initially randomize 3,331 respondents into five experimental arms, corresponding to the four videos above plus a control group with no video. Figure 2 provides a timeline of the experiment, and Table OA-1 reports the sample sizes across arms. Randomization was conducted at the individual survey-folio level and loaded onto the tablets the day before the fieldwork began. Neither the enumerator nor the respondent knew which video would appear, with a notification message appearing on the tablet only at the moment the video treatment module started. The enumerator then ensured that respondents watched the video. After the video treatment, the survey continued, and we measured short-run effects.

Timing randomization. For the urban sample in the city of Oaxaca (41 percent of participants), we also randomized at the electoral precinct level the timing of exposure to the videos. A total of 1,055 citizens in 27 precincts were assigned to receive the videos approximately one month (3–5 weeks) before the election, while 311 citizens in 10 precincts received the videos about one week (5–8 days) before the election. This randomization allows us to measure the durability of the effect on voting, which has proven to be an enduring challenge for survey experiments in this domain. To our knowledge, only Gerber et al. (2011) have randomized the timing of political television advertisements during electoral campaigns.

Experimental integrity. One advantage of survey experiments is that they typically yield negligible attrition. Among respondents who started the survey, 100 percent of those assigned to a video treatment watched the video and completed the remainder of the survey. That is, we observe no attrition, and therefore no differential attrition across experimental arms. Randomization also achieved balance across arms and across time (Table OA-2).

3.3 Data for the experiment

The experimental analysis draws on five sources of data collected around the 2021 Mexican federal election. First, we fielded an in-person baseline-treatment-endline survey among 3,331 adult citizens in Oaxaca City (1,366) and 69 rural municipalities (1,965). In Oaxaca City, we randomly selected voting precincts and divided them into two groups: one surveyed several weeks before the election and the other in the days immediately preceding it. Between these two urban waves, we moved to 40 rural municipalities across the state, surveying households along pre-specified routes designed to cover a substantial share of dwellings.[16] We then returned to Oaxaca City to survey the remaining precincts. This sequencing generates variation in temporal proximity to the election, which we exploit to examine whether treatment effects differ when administered far from versus close to election day. Finally, we surveyed the remaining 29

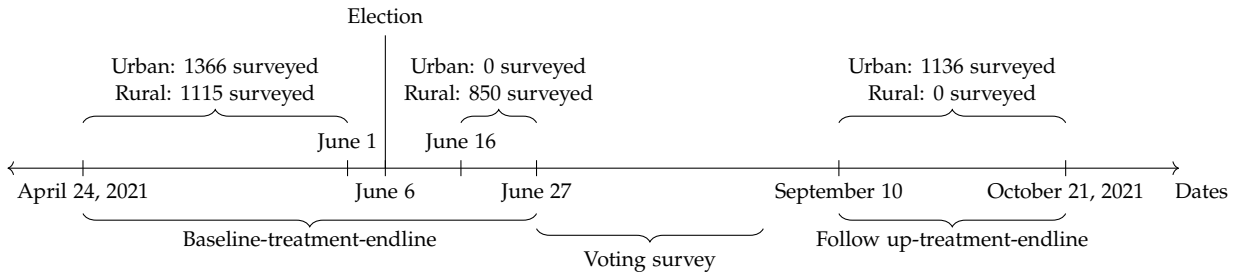
[16] Rural municipalities were selected using stratified random sampling to ensure that we included several municipalities with *usos y costumbres*—traditional indigenous governance institutions—and municipalities with weather suitable for growing cochineal Diaz-Cayeros et al. (2022), which we study in a separate project.

municipalities after the election (see Figure 2).[17] Midway through the survey, respondents were randomly assigned to the video treatments described above, after which the questionnaire continued. This survey, therefore, provides both pre-treatment covariates and immediate post-treatment measures of corruption perceptions, democratic values, trust in democratic institutions, and trust in other citizens.

Second, immediately after the survey modules, respondents completed a set of behavioral measures designed to capture the three institutional components highlighted in the introduction: organizations, beliefs, and norms. To measure willingness to participate in democratic organizations, respondents interviewed before the election were given two real opportunities to contribute to election administration: they could register as electoral observers and they could donate 10 pesos to purchase drinks and snacks for poll-station workers on election day. Registering as an electoral observer was a costly action rather than cheap talk. If respondents agreed to enroll, enumerators helped them begin the official registration process online, which required respondents to share their voter ID and email address and to devote about 30 minutes on the spot to initiating the application. To measure beliefs about trustworthiness, all respondents played two incentivized trust games, one with an anonymous politician and one with an anonymous neighbor. To measure internalized norms of honesty, a random subsample played two incentivized resource-allocation games: one involving funds earmarked for municipal public projects and the other involving an anonymous neighbor. The order of play was randomized across the identity of the counterpart. Taken together, these outcomes allow us to examine whether exposure to apex corruption reduces willingness to participate in democratic organizations, weakens beliefs about the trustworthiness of others, and erodes internalized norms of honest behavior.

Third, through a legal agreement with Mexico's electoral authority (INE), we matched pre-election respondents to individual-level administrative turnout records for the June 6, 2021 federal election. These records provide an objective measure of whether each respondent voted[18]. We successfully matched close to 80% of pre-election respondents (balanced across arms in match rates and observables), which yields the samples used in the administrative-turnout specifications in Section 4.1.

Figure 2: Timeline of treatment and surveys



This Figure shows the timing of the baseline/endline and followup surveys, compared to the election, and also urban and rural sample sizes.

Fourth, for the Oaxaca City sample, beginning on June 27, 2021, we fielded a post-election survey

[17] Survey rejection rates were below 15%, as we emphasized that the survey was non-partisan, conducted by academic researchers, short in duration, and that respondents could earn up to about 400 pesos from participation and the behavioral games.

[18] Because survey activity could not take place immediately before the June 6 election, pre-election fieldwork ended one week before election day and resumed on June 16 in rural municipalities. As a result, election-related outcomes are observed for the 2,481 respondents interviewed before the election, while the full experimental sample consists of 3,331 respondents

using phone interviews complemented by in-person visits. This survey asked whether respondents voted, which party they voted for, and, for those who did not vote, which party they would have voted for had they turned out; it also recorded null voting. These data allow us to distinguish between switching support to another party, abstention or null voting, and continued support for the same party, distinctions that underpin our analysis of voice and loyalty to democracy in Section 4.1.1. Because treatment timing was randomized only in Oaxaca City, the retrospective-vote analysis in Section 4.1.1 focuses on that urban sample.

Finally, beginning on September 10, 2021, we revisited respondents in Oaxaca City and fielded a follow-up survey roughly three to four months after the initial treatment. The follow-up re-measured the attitudinal outcomes used in the main survey and achieved a 77% response rate. It did not include the lab-in-the-field games or the electoral-observer enrollment module. After eliciting these persistence outcomes, we implemented a second randomized experiment (AEARCTR-0008170) that exposed a subset of respondents to a new incumbent-corruption video and then administered a second endline survey. These data allow us to study both the persistence of the original treatment effects and the accumulation of repeated exposure to apex corruption. The cross-country observational data used in the second part of the paper are described in Section 5.

4 Results

We estimate the following specification using Ordinary Least Squares:

$$Y_i = \alpha + \beta_{CI} CI_i + \beta_{CO} CO_i + \beta_{NB} NB_i + \beta_E E_i + \gamma X_i + \epsilon_i, \tag{1}$$

where CI_i indicates exposure to the incumbent corruption video involving the ruling Morena party, CO_i indicates the Opposition Corruption video (PAN-PRI), E_i the economic underperformance video and NB_i the nation-building video, while X_i includes controls and Municipality and Enumerator fixed effects.[19] As we describe below, the outcomes Y_i include indicators of individual voter turnout, donation of time and money to the organization of elections, choices in incentivized trust and honesty games, and survey measures of corruption perceptions and democratic values.[20]

4.1 Apex Corruption Revelations Lower Vote Turnout

Voting is the primary way citizens participate in representative democracy and hold their representatives accountable. Thus, the first outcome we study is individual voter turnout. While a number of existing studies focus on precinct-level outcomes in local elections, it is worth emphasizing that through a legal agreement with Mexico's electoral authority, we were able to gather *administrative data on individual-level* turnout in the 2021 election for the Federal Congress, the first federal election following AMLO's landslide

[19] Controls include age, gender, years of schooling, employment status, socioeconomic status, current economic satisfaction, baseline corruption perceptions, baseline perceptions of democracy in Mexico, and baseline support for the incumbent and opposition parties.

[20] The design of the survey experiment aligns closely with the recommendations of Stantcheva (2023) regarding the use of credible and legitimate information treatments, introducing incentivized questions, providing limited information about the purpose of the study, monitoring implementation in real time, introducing attention checks, and reducing fatigue by keeping the survey short and engaging. The order of the modules and the questions within the modules was randomized to avoid order effects.

2018 victory on an explicitly anti-corruption platform.[21] Combining individual-level randomization and individual turnout data is a rare but valuable feature of our study. It affords higher statistical power, allows us to examine treatment effect heterogeneity at the citizen level, and avoids the well-known problems of ecological inference that arise when using more aggregate, precinct-level measures (see e.g. King, 1997).[22] Using administrative turnout data also substantially mitigates concerns about social desirability biases or demander effects that can often plague studies that rely on survey responses to study individual voter decisions. Indeed, we document sizable gaps between actual turnout and self-reported turnout.

As discussed above, ex ante the effects of providing evidence of apex corruption on turnout are theoretically ambiguous. On one hand, exposure to evidence of apex corruption could *raise* turnout as voters seek to hold corrupt politicians accountable by voting for the opposing side. On the other, insofar as voting reflects support for democracy and formal democratic organizations, beliefs in the fairness of the democratic process, or internalized norms of voting as a civic duty (e.g Blais, 2000; Feddersen, 2004; Ali and Lin, 2013; Besley and Persson, 2019), one might expect apex corruption to reduce political participation and turnout. We build on Hirschman (1970)'s framework of responses to organizational or institutional decline encompassing exit, voice, and loyalty. In our setting, we adapt these concepts to voter behavior as follows: (a) exit through voting abstention; (b) loyalty to democracy with voice, understood as loyalty to the democratic system expressed through turnout while punishing exposed corruption by voting for an alternative party; and (c) loyalty to democracy *without voice*, understood as loyalty to the democratic system expressed through turnout without electorally sanctioning the corrupt party.

Apex corruption decreases voting behavior, consistent with *exit*. Table 1 provides our first main result, pooling both incumbent and opposition apex corruption treatments. We begin with a basic specification with no controls other than enumerator and municipality fixed effects (Column 1), before adding controls with relevant demographic and pre-treatment characteristics (Column 2), and focusing on the urban sample for which we randomized treatment timing (Columns 3 and 4)[23]. Columns 1-4 show that apex corruption reduces individual voter turnout by about 4.0 percentage points (pp) on average and 6.2pp in the urban sample (corresponding to 6.7% and 10.7% of the mean turnout rate, with p-values of a decrease: 0.066 and 0.043, respectively). This is a relatively large effect compared to the get-out-the-vote literature, which finds effects between 1pp from simple phone calls to 10pp for social pressure campaigns (Mann and Haenschen, 2024; Green and Gerber, 2023).

Columns 5 and 6 further examine treatment timing effects, exploiting the fact that, in the urban sample, participants were also randomly assigned to receive the treatment a week before the election rather than a month before. The effects of apex corruption led to a striking 15.6pp decrease in turnout (or 27.1%) when participants were randomly assigned to receive the treatment a week before the elections.[24] It is

[21] We successfully matched close to 80 percent of respondents to administrative turnout records. The matched percentage and respondents' characteristics are balanced across arms (see Table OA-2).

[22] On June 6th, 2021, Mexicans voted for 500 federal congresspersons (Diputados). Oaxaquẽnos had to choose 10 of those (with an average of roughly 170,000 voters per district) by simple majority and proportional representation. Morena won all but one of the 10 districts. It is worth emphasizing that our intervention was non-partisan, with citizens having an equal chance of being exposed to apex corruption by both incumbent and opposition parties. Further, the treatment group is also so small relative to the electorate that the chances of influencing the actual election outcomes are negligible.

[23] We cannot reject the null hypothesis that the effect of the corruption videos in columns 1 and 2 is the same for rural and urban locations (p-values 0.27 and 0.32, respectively).

[24] As the table shows the survey itself increases turnout among the control group if fielded a week before the elections, in tune with findings in the literature that surveys of this kind themselves make elections more salient (Persson, 2014; Zwane et al., 2011).

worth noting that even the one-week prior effects are durable relative to the only other study we are aware of that randomized treatment timing prior to elections: Gerber et al. (2011) show that TV ad campaigns in the United States also have substantial short-term effects (on self-reported voter intentions), but these entirely disappear within a week. Effects far from the election are also negative 4-5*pp* but more imprecise.

Table 1: Treatment Effects on Actual Voter Turnout

	(1) Vote	(2) Vote	(3) Vote	(4) Vote	(5) Vote	(6) Vote
Corruption (C)	-0.037	-0.040	-0.067*	-0.062*	-0.163**	-0.156**
	(0.027)	(0.026)	(0.038)	(0.036)	(0.080)	(0.077)
Nation Building (NB)	0.011	0.006	0.010	-0.003	0.025	-0.031
	(0.028)	(0.027)	(0.038)	(0.036)	(0.076)	(0.076)
Economic (E)	0.077*	0.081**	0.090*	0.099*	0.051	0.021
	(0.040)	(0.040)	(0.051)	(0.051)	(0.116)	(0.125)
Far from Election					-0.115**	-0.132**
					(0.055)	(0.054)
Far from Election x C					0.124	0.105
					(0.089)	(0.086)
Far from Election x NB					-0.022	0.018
					(0.086)	(0.084)
Far from Election x E					0.053	0.114
					(0.128)	(0.136)
Observations	2045	2045	1154	1154	1154	1154
R-squared	0.063	0.127	0.033	0.126	0.039	0.133
Mean dep. var	0.599	0.599	0.575	0.575	0.575	0.575
H0: C $\geq$ 0 (p-value)	0.086	0.066	0.039	0.043	0.021	0.021
Enumerator FE	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	Urban	Urban	Urban	Urban
Demography Controls		✓		✓		✓
Democratic/Economic Controls		✓		✓		✓

This table estimates Eqn 1 on individual voter turnout, pooling the apex corruption treatments. Controls include the following pre-treatment measures. *Demography*: Gender, Age, Schooling, Employment; *Democratic and Economic beliefs and attitudes*: Current Economic Satisfaction, Perception of Democracy in Mexico, Initial corruption perception, Support for Incumbent and Opposition parties, and Socioeconomic status. Robust standard errors are shown in parentheses. The bottom panel reports *p* values from a one-sided test that apex corruption reduces voter turnout. In Columns 3-6, the (urban) sample is restricted to Oaxaca city. Columns 5-6 include an indicator (*'Far'*) for whether a subject was randomly surveyed one month before the election (as opposed to a week before). The table includes only citizens interviewed before the election. We cannot reject the null hypothesis that the effect of the corruption videos in columns 1 and 2 is the same for rural and urban locations (p-values 0.275 and 0.321, respectively). *** p < 0.01, ** p < 0.05 and * p < 0.1

Information on economic underperformance *increases voice*. In striking contrast to the apex corruption treatments, as Table 1 also shows, instead of leading to exit, the economic underperformance treatment *increases* the probability that an individual actually turns out to vote (by 8–10*pp*, or 13–17%). Further, as discussed below, our post-election survey reveals that the economic underperformance video reduces the share claiming to have voted for the incumbent by 20.8*pp* when the treatment occurs a week before the election (Table OA-4). The primary beneficiaries are smaller parties that have not led the government in recent years, for whom the probability that treated respondents claim to have cast their votes rises by 6.3 percentage points (or almost three-fold).[25] The treatment also increases those reporting that they cast 'protest votes', with null or no-vote choices rising by about 10*pp*.

Thus, unlike the apex corruption treatments, evidence of economic and policy underperformance *spurs* voters' willingness to turn out and hold politicians accountable through the electoral process, including by claiming to have registered protest votes against ruling parties. This interpretation is further bolstered

[25] In contrast, the effect of the corruption video operates almost entirely through an increase in the no-vote/null-vote category—what we call exit—rather than through vote switching toward other parties not featured in the corruption video.

by the battery of evidence presented below.

The nation-building treatment has no effect on vote turnout. Finally, and contrary to our priors, the nation-building treatment does not raise actual voter turnout (Table 1). However, it lowered the share claiming to have voted for the main opposition parties (whose support falls by 8.5pp when treated close to the election, with a p-value of a decrease of 0.06 see Table OA-4).

4.1.1 The effect of apex corruption revelations by party.

We now distinguish between corruption revelations involving the incumbent Morena party or the opposition. As mentioned above, Morena and AMLO were widely believed to be less corrupt than other parties, so we pre-specified our directional hypothesis that the apex corruption treatments *"will diminish support, particularly for the culpable group, but also for the democratic system and trust more broadly"* (see our pre-analysis plan, p. 3)" We further anticipated "*more updating for the Incumbent video*" as "*while many people think the opposition parties are corrupt, the incumbent party–Morena– is viewed as much less so*."

Apex corruption implicating the anti-corruption incumbent leads to larger voter exit. Consistent with our hypothesis, Table 2, the Incumbent Corruption (i.e., Morena) video is mostly responsible for the striking close-to-the-election treatment differences found in Table 1. On average, the effects of incumbent and opposition apex corruption are statistically indistinguishable, with point estimates ranging from 2.4 to 5.6pp overall, and 4.5 to 7.9pp among the urban sample, respectively (Columns 1 and 2). Yet, there are starkly differential effects of treatment timing. The apex opposition corruption treatment proves to have durable effects, reducing turnout by similar magnitudes (of about 5pp) regardless of whether the treatment was randomly assigned to be delivered one week or one month before the election. In contrast, the apex corruption video implicating the ruling Morena party (CI) is mainly responsible for the striking effects on reducing actual turnout among respondents treated a week before the election (reducing turnout by a remarkable 22.4pp (38.9%)), while disappearing over time. A likely explanation for this is that Morena campaigned much more intensely than the opposition in the weeks between our short-run and long-run treatment, diluting the longer-run effects. Indeed, Figure OA-8 shows that Morena held 25.4% more campaign events nationwide and a remarkable 42.1% more events in Oaxaca than the opposition in the month leading up to the election.

Voter support for specific parties. Combining the administrative data with the post-election voting survey allows us to further unpack whether exit or loyalty (with or without voice) predominates in response to the different apex corruption treatments. Columns 4–8 of Table 2 use the voting survey data. Columns 3 and 4 highlight the importance of observing actual administrative data on a question like voter turnout: while 77% of respondents report having voted, only 57% actually did so. Despite this gap, the patterns of self-reported voter support are remarkably consistent with those obtained using administrative data, with the Morena apex corruption treatment exhibiting a similar negative effect on self-reported turnout —22.1pp (self-reported, Col 4) vs 22.4pp (actual, Col 3)— when delivered shortly before the election.

Columns 5–8 of Table 2 combine responses to the questions *"which party did you vote for?"* and *"which party would you have voted for had you voted?"* into indicators of support for specific parties. Particularly close

Table 2: Treatment Effects of Apex Corruption on Actual and Self-Reported Voting

	Actual vote			Retrospective vote				
	(1) Actual Vote	(2) Actual Vote	(3) Actual Vote	(4) States Went to Vote	(5) Vote Incumbent	(6) Vote Opposition	(7) Vote Others	(8) Null Vote or No Vote
Corruption Incumbent (CI)	-0.024	-0.045	-0.224**	-0.221**	-0.198**	0.005	-0.032	0.225**
	(0.032)	(0.047)	(0.090)	(0.089)	(0.095)	(0.081)	(0.037)	(0.090)
Corruption Opposition (CO)	-0.056*	-0.079*	-0.050	-0.139	0.047	-0.210***	0.071	0.092
	(0.033)	(0.047)	(0.104)	(0.096)	(0.096)	(0.065)	(0.064)	(0.094)
Far from Election			-0.137**	-0.080*	-0.014	-0.024	-0.030	0.068
			(0.053)	(0.046)	(0.054)	(0.044)	(0.021)	(0.046)
CI × Far			0.238**	0.221**	0.281**	-0.083	0.030	-0.227**
			(0.105)	(0.101)	(0.110)	(0.092)	(0.039)	(0.102)
CO × Far			-0.035	0.137	-0.095	0.229***	-0.078	-0.056
			(0.116)	(0.107)	(0.110)	(0.078)	(0.066)	(0.105)
Observations	2045	1154	1154	1162	1162	1162	1162	1162
R2	0.127	0.127	0.136	0.080	0.185	0.151	0.066	0.073
Mean dep. var.	0.604	0.575	0.575	0.772	0.541	0.209	0.024	0.226
$\mid CI \mid \geq 0$ (p-value)	0.229	0.166	0.007	0.007	0.019	0.476	0.809	0.006
$\mid CO \mid \geq 0$ (p-value)	0.045	0.047	0.314	0.074	0.312	0.001	0.136	0.163
$\mid CI + CI \times Far \mid \geq = 0$ (p-value)			0.399	0.496	0.939	0.967	0.569	0.518
$\mid CO + CO \times Far \mid \geq = 0$ (p-value)			0.051	0.485	0.815	0.675	0.717	0.231
Enumerator FE	✓	✓	✓	✓	✓	✓	✓	✓
Municipality FE	✓	Urban	Urban	Urban	Urban	Urban	Urban	Urban
Demography Controls	✓	✓	✓	✓	✓	✓	✓	✓
Democratic/Economic Controls	✓	✓	✓	✓	✓	✓	✓	✓

This table presents, in columns (1)–(3) the effects on actual voting from matching the citizens with administrative data using the same specifications as Table 1 but separating out the incumbent and opposition apex corruption treatments. In columns (4)–(8) we show treatment effect estimates on self-reported voting. For column (4) we ask *Did you vote in the last elections?*. For columns (5), (6) and (7) we use the questions *Which party did you vote for?* and *which party would you have voted for in case you did not vote?*. Column 6 only includes the opposition parties featured in the CO video (e.g. PRI, PAN, PRD). In column (8) the outcome is a dummy variable equal to 1 if the respondent said she did not cast a vote or she cast a null vote and 0 otherwise. Robust standard errors are shown in parentheses. Stars denote statistical significance: *** p < 0.01, ** p < 0.05 and * p < 0.1

to the election, both apex-corruption treatments reduce willingness to vote for the parties featured in the videos. The incumbent apex-corruption video reduces self-reported support for Morena by 19.8 percentage points in column 5, while the opposition video reduces self-reported support for the parties featured in that video (PAN-PRI-PRD) by 21pp in column 6. There is, however, an important difference between the two treatments. The decline in support for Morena is not offset by greater support for the main opposition parties; instead, it is associated with greater abstention or null voting (columns 3 and 8). By contrast, the opposition video has smaller exit effects and is associated with a modest, statistically insignificant increase of 4.7pp in support for Morena (column 5). One interpretation is that when the uniquely clean party is revealed to be corrupt, some citizens withdraw from electoral participation altogether, whereas revelations about opposition corruption leave voters with a clean electoral alternative in Morena. Under this interpretation, continued engagement in democratic competition may depend on whether voters still perceive at least one viable party as not corrupt.

Motivated reasoning? Given that affective polarization along Morena versus non-Morena lines is as strong as that between Republicans and Democrats in the United States (Greene et al., 2024; Castro Cornejo, 2023; Moreno, 2020), one might expect motivated reasoning, cognitive dissonance, or other identity-based attachments to lead citizens to ignore or down-weight evidence against their preferred party. Table OA-5 suggests otherwise. Revealing Morena-related corruption one week before the election reduces turnout by 23.2pp among ex ante Morena supporters and lowers the probability of a self-reported vote for Morena by 24pp.

To conclude, unlike the economic underperformance treatment, which induces greater voice through punishment voting for non-featured parties, revelations of apex corruption, especially those implicating Morena, substantially depress turnout. This pattern is more consistent with exit from, rather than loyalty to, the democratic system.

4.2 Effects on Other Pro-Democratic Political Behaviors

So far, we have established that apex corruption reduces voter turnout. This, however, does not necessarily imply a decline in democratic values. For example, the Morena corruption video may lead individuals to perceive that the parties are less differentiated on the corruption dimension, thereby making their vote seem less consequential and reducing turnout without indicating any broader erosion in democratic values.[26] Similarly, individuals may view abstaining when their preferred party is implicated in apex corruption as a weaker but less costly form of voice than punishing corruption by voting for the opposing party.

Thus, we now test whether apex corruption, particularly when it implicates Morena, affects outcomes corresponding to the norms, beliefs, and organizations that underpin liberal democracy, including the willingness to collaborate with the organization of elections, beliefs about the trustworthiness of politicians and other citizens, and internalized norms of honesty. In this section, we measure these components first through natural pro-democratic behaviors and then through more stylized lab-in-the-field experiments. Section 4.3 then estimates the effects on survey responses capturing support for democracy and trust in democratic institutions. Taken together, the results indicate how apex corruption can lead to a broad erosion of support for democratic institutions.

4.2.1 Natural Behaviors: Volunteering and Donating to Electoral Organizations

Beyond voting itself, we measure support for the formal organizations that sustain democracy through citizens' willingness to take two real-world actions that allow them to support the democratic system. Immediately after the video treatments, participants were given the opportunity to donate either time or money to support the administration of the elections.

Time: We asked citizens whether they would be willing to serve as official electoral observers. This required them to provide their email and voter ID to initiate the registration process on the webpage of the electoral authority (INE). We explained that the short-run cost would be approximately ten minutes to enroll, and the long-run cost would involve undergoing an official online training lasting a few hours. We recorded whether they initiated the registration process. Volunteering as an electoral observer is a costly action that reflects a willingness to participate in and safeguard the integrity of the democratic process.

Money: We asked citizens whether they would be willing to donate 10 pesos (1/16 of the average minimum wage) to buy water and snacks for poll booth workers, emphasizing that these workers play a critical role in administering elections. We provided a piggy bank on site, allowing respondents to deposit the money immediately. All donated funds were subsequently used by the research team to purchase and deliver water and snacks to poll booth workers on election day.

Table 3 shows the results. The willingness to volunteer as an electoral observer was low on average, with only 4.9% agreeing to do so (Cols 1–3). This indicates that participants did not view this choice as

[26] See for example, Arias et al. (2022), on the importance of such priors.

Table 3: Volunteering and Donating to Electoral Organizations

	(1) Electoral Observer	(2) Electoral Observer	(3) Electoral Observer	(4) Donate to Elec. Institution	(5) Donate to Elec. Institution	(6) Donate to Elec. Institution
Corruption Incumbent (CI)	-0.022**	-0.021**	-0.032**	-0.040*	-0.036	-0.043
	(0.011)	(0.011)	(0.013)	(0.024)	(0.024)	(0.037)
Corruption Opposition (CO)	0.003	0.005	-0.014	-0.000	0.001	-0.012
	(0.013)	(0.013)	(0.017)	(0.024)	(0.024)	(0.038)
Observations	2460	2460	1359	2481	2481	1366
R-squared	0.073	0.083	0.058	0.129	0.136	0.115
Dep. Var. Mean	0.049	0.049	0.054	0.228	0.228	0.243
H0: CI ≥ 0 (p-value)	0.017	0.025	0.008	0.045	0.063	0.119
H0: CO ≥ 0 (p-value)	0.599	0.642	0.201	0.499	0.512	0.371
Enumerator FE	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓
Demographic Controls		✓	✓		✓	✓
Democratic/Economic Controls		✓	✓		✓	✓
Sample	All	All	Urban	All	All	Urban

This table shows treatment effect estimates of the Corruption Incumbent and Corruption Opposition videos on the probability of registering as an electoral observer in columns (1)–(3) and on the probability of donating money to the electoral institution in columns (4)–(6). The estimating equation is $Y_i = \alpha_e + \eta_m + \beta_{CI}CI + \beta_{CO}CO + \beta_{NB}NB + \beta_E E + X_i^{'}\gamma + \varepsilon_i$. The vector $X^{'}$ includes demographic and democratic/economic attitudes controls such a respondent sex indicator, age, age squared, education level attained, indicator for being employed and SES, satisfaction with the economy, stated level of democracy in Mexico (1–10), perception of corruption improvement and indicators for supporting either the incumbent or opposition. We cannot reject the null hypothesis that the effects of both videos are the same across urban and rural localities, with p-values ranging from 0.26 to 0.86. Heteroskedasticity-robust standard errors are shown in parentheses. * p < 0.10 ** p < 0.05 *** p < 0.001

cheap talk and instead perceived the volunteering commitment as costly. However, the effects are striking. While exposure to opposition apex corruption had no effect, exposure to apex corruption by the incumbent reduces the probability of volunteering by about 2.1pp, or a remarkable 42.8% (p-value of a decrease = 0.025). While a larger fraction of citizens were willing to donate 10 pesos of money than to donate time (22.8%), incumbent apex corruption again induces a decrease of 3.6pp (or 15.7%, p-value of a decrease 0.063) in this willingness, with a somewhat smaller accompanying effect for the opposition treatment.

Table OA-6 shows the patterns for the other video treatments. Unlike apex corruption, the economic underperformance video has no detectable effect (β =-0.001, s.e.=0.016) on these behavioral measures of support for democratic organizations. Further, and contrary to our expectations, the nation-building treatment also fails to increase such support.[27]

4.2.2 Lab in the Field: Beliefs and Norms

To capture effects on the beliefs and norms that also underpin liberal democracy, we turn to incentivized lab-in-the-field methodologies.

Trust. As an incentivized measure of how individuals' beliefs in the trustworthiness of political figures and citizens change in response to apex corruption, we had participants play two trust games, one with an *anonymous politician*—an actual congressperson—and one with an *anonymous neighbor*. Participants were endowed with four 10-peso coins (totaling one-quarter of the minimum wage). They could then choose to send between zero and all four coins to the other player. Researchers multiplied any amount sent by three. We explained that their counterparts had also started with four coins, and had pre-specified a return

[27] Instead, and again consistent with a closer alignment with the messaging of the nationalist populist incumbent, the nation-building treatment has a *negative* impact on respondents' willingness to donate either time (β =-0.019, s.e.=0.010) or money (β =-0.027,s.e.=0.020) to the independent electoral authority, of which AMLO has long been highly critical. See e.g. David Agren, "Mexico bill threatens independence of electoral oversight...", the Financial Times, Nov 30 2022.

strategy as a function of the amount sent, but that this strategy was not disclosed to the participant.[28] We recorded the amount sent by each participant.

Table 4: Incentivized Games

	(1) Politician	(2) Politician	(3) Politician	(4) Neighbor	(5) Neighbor	(6) Neighbor
Panel A: Trust Games						
Corruption Incumbent (CI)	-0.185***	-0.182***	0.042	-0.120**	-0.113*	0.078
	(0.063)	(0.062)	(0.115)	(0.060)	(0.060)	(0.104)
Corruption Opposition (CO)	-0.115*	-0.120*	-0.165	-0.079	-0.084	-0.083
	(0.064)	(0.064)	(0.106)	(0.059)	(0.059)	(0.098)
Observations	3331	3331	1366	3331	3331	1366
R-squared	0.121	0.132	0.149	0.104	0.112	0.123
Dep. Var. Mean (USD 10's)	1.625	1.625	1.615	2.058	2.058	2.043
H0: CI ≥ 0 (p-value)	0.002	0.002	0.642	0.024	0.031	0.773
H0: CO ≥ 0 (p-value)	0.035	0.029	0.060	0.088	0.076	0.199
Panel B: Resource Allocation Games						
Corruption Incumbent (CI)	-0.677**	-0.654**	-1.407**	-0.439	-0.400	-1.114
	(0.324)	(0.329)	(0.649)	(0.368)	(0.371)	(0.695)
Corruption Opposition (CO)	0.143	0.150	0.351	-0.248	-0.274	-0.968
	(0.351)	(0.350)	(0.689)	(0.385)	(0.384)	(0.734)
Observations	1573	1573	493	1573	1573	493
R-squared	0.088	0.104	0.117	0.073	0.092	0.114
Dep. Var. Mean (num. coins)	14.752	14.752	14.920	14.898	14.898	15.046
H0: CI ≥ 0 (p-value)	0.019	0.024	0.015	0.117	0.141	0.055
H0: CO ≥ 0 (p-value)	0.658	0.666	0.695	0.260	0.238	0.094
Enumerator FE	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓
Demographic Controls		✓	✓		✓	✓
Democratic/Economic Controls		✓	✓		✓	✓
Sample	All	All	Urban	All	All	Urban

This table shows treatment effect estimates of the Corruption Incumbent and Corruption Opposition videos on trust games in Panel A and on resource allocation games in Panel B. Columns (1)–(3) show effects on trust games and resource allocation games with a politician, while columns (4)–(6) show effects on games with neighbors. We restrict the sample in columns (1), (2), (4) and (5) of Panel A to the sample of respondents for whom there are no missing covariate values in the most restrictive sample (columns 2 and 5). In columns (3) and (6) we further restrict the sample to respondents in Oaxaca City. We impose analogous restrictions in Panel B, with the difference that, to economize on the budget, we draw a random sample of respondents to participate in Resource Allocation Games. We cannot, at 95% confidence, reject the null hypothesis that the effects of both videos have the same effects across urban and rural localities in every column. Heteroskedasticity-robust standard errors are shown in parentheses. * p < 0.10 ** p < 0.05 *** p < 0.01

As Panel A Table 4 shows, on average, citizens were more likely to trust an anonymous neighbor than the anonymous politician, entrusting the former with almost exactly half of the endowment (20.6 pesos/ 40) on average, while the politician received 16.3 pesos—about 21 percent less. However, the apex corruption treatments strongly reduce the average amounts entrusted to the anonymous politician (by 1.8 pesos for the incumbent treatment, and 1.2 pesos for the opposition treatment, with p-values of a decrease of 0.002 and 0.029, respectively). Interestingly, revealing apex corruption decreases trusting behavior towards anonymous neighbors as well, suggesting a spillover of distrust from the political to the civic domain.[29]

Internalized Norms. In his study on democratic backsliding, Diamond (2019) writes "*Once ordinary people see that the political class is grabbing public wealth and advantage at every turn, they ask why not me?*". To assess whether apex corruption also affects behavior related to internalized honesty norms, we adapt

[28] Based on piloting, we decided that the best way to explain the trust game was with a video, which can be found at https://video.wixstatic.com/video/583ee0_c4911e24d46941f28217a204ad25b0bd/480p/mp4/file.mp4.

[29] Trust in the anonymous politician and anonymous neighbors are also undermined by each of the other treatments as well, though some of the effects are more muted (Table OA-7).

the resource allocation game (Lowes et al., 2017), which allows respondents to behave honestly or steal with impunity. We implemented this by giving citizens a non-transparent bag containing 30 coins and an opaque envelope. In private, the respondent chose heads or tails before flipping each coin and was entitled to keep the coin if their guess (unobserved by us) was correct. If they guessed incorrectly, they were required to place the coin inside the envelope. After doing this for the 30 coins, the respondent sealed the envelope and gave it back to the surveyor. Envelopes were labeled *"To the municipal mayor–to be allocated for public projects"* or *"To neighbor–to be sent to an anonymous neighbor"*. Envelopes were tamper-proof, and we explained that surveyors would not open them.[30]

Because there is no possibility of external punishment, following Lowes et al. (2017) and Fisman and Miguel (2007), we interpret stealing in this setting as consistent with the weakening of internalized norms against rule-breaking and theft.[31]

Panel B of Table 4 shows that, on average, citizens in the control group send 14.7 out of 30 coins to the municipal mayor and 14.9 coins to a neighbor, consistent with little or no stealing and strong internalized honesty norms. However, when exposed to apex incumbent corruption, citizens send 0.7 fewer coins—that is, they "steal" about 4 percent of the mean that would otherwise go to public projects—(p-value of a decrease = 0.024). Apex incumbent corruption also has a negative impact on honest dealings with neighbors, with respondents sending 0.40 fewer coins (p-value of a decrease = 0.14). Thus, though internalized norms can be deep-rooted, having emerged from institutions of the past (e.g. Greif, 2006; Nunn and Wantchekon, 2011; Jha, 2013; Lowes et al., 2017), apex politicians may be particularly positioned to affect these norms.

4.3 Self-Professed Values

So far, we have shown that apex incumbent corruption reduces democracy-supporting behaviors, including voter turnout and support for electoral organizations, as well as trust in politicians and neighbors, and increases the likelihood of stealing from public projects in incentivized games. We now ask whether apex corruption also affects democratic values of which respondents themselves are aware.

We now follow a large body of literature that assesses support for democracy by directly asking citizens (e.g. Acemoglu et al., 2025b; Claassen, 2020; Besley and Persson, 2019; Inglehart, 2003; Dalton, 1999; Mainwaring and Pérez-Liñán, 2014; Bartels, 2023). This approach allows both for direct comparability with prior work and enables us to examine whether the behavioral changes we document are reflected in respondents' own consciously-held attitudes.

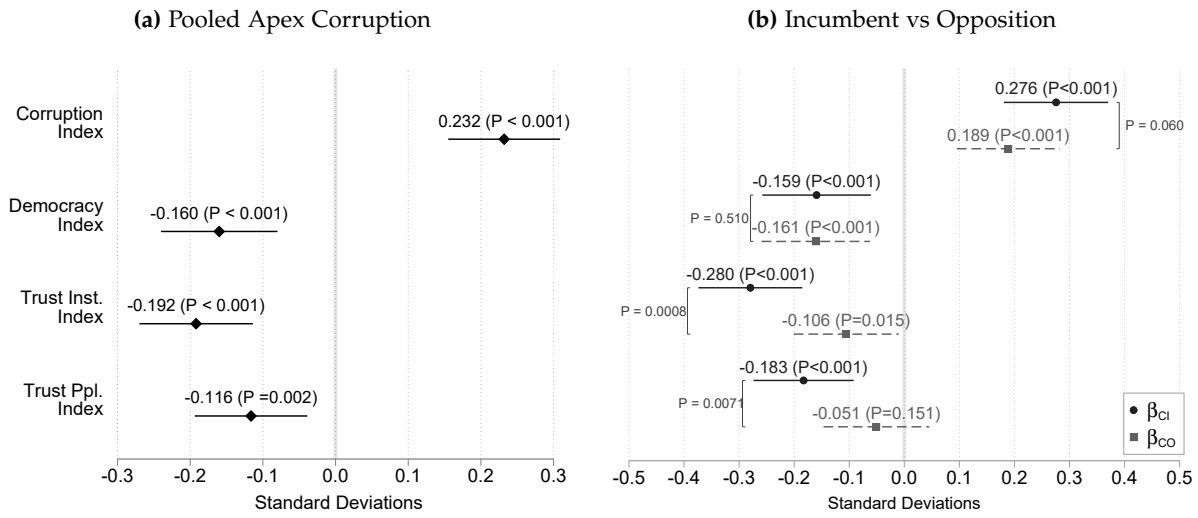
In particular, we estimate the causal effects of apex corruption revelations on four pre-registered indices

[30] We also varied the incentives for each toss by including a mix of denominations, ranging from 1 to 5 peso coins. We randomized whether the trust game or the stealing game was played first, and within each, we randomized whether the politician/municipal mayor or the neighbor module was played first. Because surveyors were handling money, we had a supervisor for every five surveyors. We told surveyors that we would audit a fraction of the surveys. We do not find systematic differences in outcomes across surveyors. The research team opened each envelope days later and counted the coins. Because the enumerator does not see or know how many coins are in the envelopes, and because a low number of coins is not a guarantee of stealing, it is unlikely that experimenter demand effects drive this result.

[31] These results also comport with important recent work that has found that corruption by *local* politicians increases cheating in secondary school level exams (Ajzenman, 2022) and stealing in supermarkets (Gulino and Masera, 2023). Our work builds upon and contributes to this line of research in a number of ways: our focus on apex corruption, using randomized variation, measuring effects in a context where citizens cannot be caught cheating (so behavior has to be due to norms internal to the person), varying the content of the corruption evidence revealed, as well as studying the effects on a different set of outcomes: support for democracy itself.

constructed from survey batteries measuring: (1) corruption perceptions[32], (2) democratic values[33], (3) trust in democratic institutions[34], and (4) trust in fellow citizens.[35] These effects were measured immediately after treatment during the baseline/endline survey round, with no attrition.

Figure 3: Effects of Apex Corruption On Immediate Conscious Perceptions and Values



Panel (a) shows the pooled estimates for the apex corruption treatments, from equation 1, along with their 95 percent confidence intervals. Panel (b) shows the separate estimates for the Incumbent Corruption (CI) and Opposition Corruption (CO) treatments, from equation 1. We show point estimates and their corresponding one-sided p-value above their corresponding confidence interval.

Corruption perceptions. Figure 3(a) summarizes the immediate treatment effects, pooling the two apex corruption treatments. Citizens experimentally exposed to evidence of corruption by apex politicians increase their perceptions of broader corruption by 0.225σ (p-value<0.001). While this may appear to be merely a manipulation check, it is worth emphasizing that this strong effect stands in contrast to an important body of synchronized studies of information on local corruption, (see e.g. Dunning et al., 2019), that find *no effect* on corruption perceptions. All components of the corruption index are affected, with

[32] **Corruption Index**: a standardized index (Kling et al., 2007) constructed from the following four variables: (a) *Progress fighting corruption*: "How much progress do you think has been made in reducing corruption in State institutions in the last 2 years?(4 pt scale)"; (b) *Extent politicians corrupt*: "What percentage of politicians in Mexico do you think are involved in acts of corruption? (0% to 100%)"; (c) *Extent money stolen*: "Of every $100 pesos in taxes collected by the government, what percentage do you think politicians steal?" (0% to 100%); (d) *All politicians are corrupt*: "Please state if you strongly agree (4), agree (3), disagree (2), or strongly disagree (1) with the following statement: "All politicians are corrupt". All components are standardized before aggregation.

[33] **Democratic Values Index**: a standardized index constructed from the following three questions: (a) *Voting essential*: "Does it seem essential and important to you to live in a country where rulers are elected by vote? Yes or No"; (b) *Satisfaction with democracy*: "In general, would you say that you are very satisfied (4), satisfied (3), not very satisfied (2), or not at all satisfied (1), with democracy in Mexico?"; (c) *Alternatives to democracy*: "Please tell me if you strongly agree (4), agree (3), disagree (2) or strongly disagree (1) with the following statement: Democracy can have its problems, but it is the best system of government". All components are standardized before aggregation.

[34] **Trust in Democratic Institutions**: We asked: "Please tell me, for each of the institutions mentioned in the list, how much trust do you have in them: a lot (4), some (3), little (2) or no trust (1) in [...]: the president, congress, media, and political parties". We then created a standardized index using the four resulting variables.

[35] **Trust in People**: a standardized index based on the following measures: (a) *General trust*: "Generally speaking, would you say that most people can be trusted or that one is never careful enough when dealing with others?"; (b) *Trust in specific people*: "Please tell me, for each of the groups or people mentioned in the list, how much trust do you have in them: a lot (4), some (3), little (2) or no trust (1). How much do you trust [...]: your neighbors; a Mexican that is not from Oaxaca." All components are standardized before aggregation.

treated citizens showing a 6% decrease in the belief that progress has been made in reducing corruption, a 5% increase in the share of politicians they believe are corrupt, a 4% increase in the perceived share of taxes stolen, and an increase of 4% in the belief that all politicians are corrupt (Table OA-10).[36]

Support for democracy. Further, the apex corruption treatments immediately reduce the index of support for democracy by 0.16σ (p-value<0.001). Again, every component of the index—including satisfaction with democracy and support for democracy relative to alternative political systems—moves significantly in the direction of lower support for democracy. Satisfaction with democracy decreases by 15%, $4.2pp$ fewer respondents (5.4%) report that democracy is the best form of government, and $3.7pp$ fewer respondents (4.6%) consider it essential to live in a democracy (Table OA-11).[37] Thus, the behavioral responses documented above—including voting behavior and willingness to support electoral institutions—are mirrored in immediate survey-based measures of consciously held democratic values.

Trust in elected officeholders and organizations. Beyond this, and again mirroring the lab-in-the-field behaviors above, apex corruption immediately reduces trust in democratic office-holders and formal institutions by 0.19σ relative to the control group (p-value < 0.001). This effect appears across all components of the index: trust in the President decreases by $5.6pp$ (10%), trust in Congress by $5.8pp$ (23%), and trust in political parties by $2.4pp$ (20%). Even trust in the media declines, with trust falling by $4.8pp$ (15%) (Table OA-12), suggesting that apex corruption revelations generate spillover effects across multiple formal organizations.

Trust in other citizens. Finally, the corrosive effects of apex corruption extend beyond formal institutions and the media, reducing self-reported trust in fellow citizens by 0.116σ. Once again, all components of the index decline, with effects that are particularly pronounced for trust in neighbors and non-neighbor Mexicans, and more muted effects for generalized trust (Table OA-13).

Effects by sub-treatment and partisanship Figure 3(b) decomposes the effects of apex corruption implicating Morena versus the opposition treatments (see also Table OA-8). While both the incumbent and opposition apex corruption treatments generate large increases in immediate general corruption perceptions and significant declines in self-reported support for democracy, once again the Morena-related apex corruption treatment has particularly pronounced effects on trust in democratic institutions and trust in fellow-citizens. Similarly, Figure OA-6 shows that the effects are not mitigated by partisan orientation, consistent with the video treatments proving sufficiently persuasive to overcome partisan bias and motivated reasoning.

Table OA-8 shows the effects of the economic underperformance and nation-building sub-treatments. Notice that the economic underperformance video also increases perceptions of overall corruption and reduces professed support for democracy, but does so without spillover effects on trust particularly in other citizens. Further, as we have seen, these perception changes are accompanied by strikingly different *behavioral* effects, consistent with an *increase* in voice, galvanizing actions that include higher voter turnout,

[36] By focusing on pre-registered indices, we already address potential concerns about multiple hypothesis testing. For completeness, we also report false discovery rate (FDR) q-values in Appendix B.2. Although q-values are not typically reported across families of outcomes, we provide these in Table OA-9 for interested readers.

[37] In terms of effect sizes, the impact of the three-minute apex corruption treatments on democratic support is approximately twice as large as that of the strongest eight-minute video treatments found to have a backlash effect on support for democratic practices among an internet panel of US partisans in Voelkel et al. (2024).

and decisions to vote *for* parties not implicated in apex corruption. We interpret these results as consistent with economic underperformance acting through a channel distinct from apex corruption, one that may also lead to institutional skepticism but induces voice rather than exit. Finally, the nation-building treatment does lead to a modest increase in professed support for democracy of 0.069σ, but with no detectable effects on the remaining indices.

4.3.1 Persistence and Accumulation.

Research rarely measures whether the effects of political persuasion from video treatments persist, and it is still rarer to find tests for accumulation (see e.g. Baum et al., 2025) of persuasion attempts or political events.

Persistence. Because of the logistical challenges of re-surveying the same individuals, measuring persistence has eluded many influential survey experiments on the formation of social preferences (e.g. Kuziemko et al., 2015; Alesina et al., 2018). Further, as we have seen, while the effects of apex corruption on actual voter turnout proved durable for those exposed to the opposition treatment a month before the elections, the differences in actual voter turnout were more short-lived among those exposed to the Morena arm, as its campaigning intensified. Nonetheless, the treatments may engender latent or actual persistent differences in perceptions and democratic values that are not fully captured by voter turnout. Thus, four months after the treatment, we returned to the city of Oaxaca and re-interviewed respondents from the original sample.[38] We achieved a 77 percent response rate, balanced across treatment arms (see Table OA-2).

Figure 4 presents the results. The apex corruption treatment and control groups had largely converged in terms of their broader corruption perceptions four months after treatment. Nevertheless, the effect on lowering support for democracy persists (an average effect of -0.22σ, with a one-sided p-value = 0.027). These effects are very similar for the two variants of apex corruption (-0.25σ for Morena vs -0.20σ for the opposition treatments. Furthermore, we find decreases of 0.14σ (one-sided p-value = 0.12) and 0.18σ (one-sided p-value = 0.06) in trust in democratic institutions and trust in people, respectively (results available upon request).

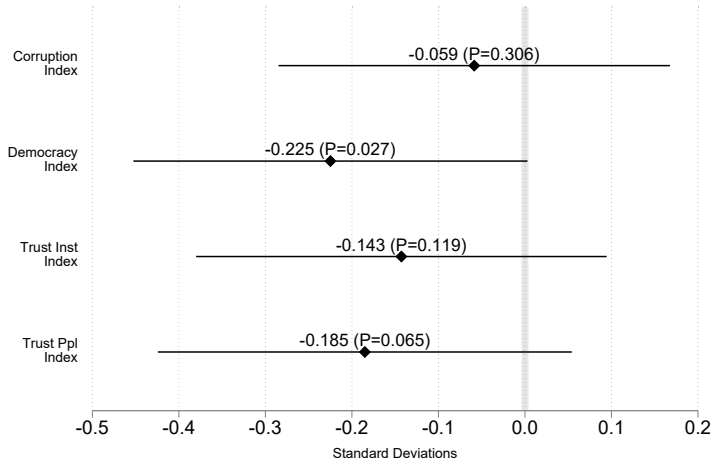
Accumulation. While planning fieldwork for the follow-up survey, a *second brother of the incumbent president* was caught on film taking bribes. This allowed us to create an additional 3-minute corruption video that we refer to as the *Two* Brothers Corruption treatment (henceforth CI_2). This video features sequential footage of bribes being exchanged not just by one but by two siblings of the president.[39] Therefore, after measuring the persistence of the effects from the previous videos, we randomized half of the follow-up (Oaxaca city) sample to receive the *Two* Brothers corruption video, stratifying by the treatment arms of the first experiment.

Because the comparison groups are half the size as before, this experiment has less power (see our follow-up trial registration, *AEARCTR-0008170*). Nonetheless, the effects are still informative. Receiving this two brothers-in-one treatment increases corruption perceptions by 0.28σ and leads to corresponding declines in the democracy index of -0.23σ. In fact, the decline in support for democracy is about 40

[38] We took special care to ensure that the same individuals were surveyed by revisiting the same addresses and verifying respondents' name, gender, and age using their IDs.

[39] See: https://www.youtube.com/watch?v=q6nheyGHc3g. Appendix B.5 provides more detail.

Figure 4: Persistence of Apex Corruption on Perceptions and Values



Effects from the follow up survey four months after treatment. Apex corruption treatments are pooled. OLS specification is the same as in Figure 3(a). We show point estimates and their corresponding one-sided p-value above its corresponding confidence 95 percent confidence intervals.

percent larger than the -0.159σ we previously found for the one-brother treatment (Figure OA-7).

Further, the effect of two doses—i.e., the effect of the two-brothers-in-one video among respondents whom we had previously exposed, four months earlier, to the video implicating the first presidential brother—is an even more striking decrease in support for democracy of 0.39σ. This is 65% larger than the effect of just the two-in-one video and 137% larger than the original single-brother corruption (CI) video. Taken together, these results suggest that apex corruption scandals can not only have persistent effects in undermining support for democracy, but also that these effects can accumulate.

4.3.2 Testing a Second Potential Remedy: The Effects of Common Financial Exposures

Appendix B.6 details the results from a second supplemental experiment aimed at rebuilding democratic values and trust: common financial exposure. After collecting the immediate survey outcomes, we provided a subset of subjects with exposure to the broad Mexican index fund, and thus a means to share in and learn about the gains of the common good—and common risks— of an important component of the national economy (e.g Jha and Shayo, 2019; Jha, 2025; Jha et al., 2025). Treated citizens received 200 pesos (about $10) of financial assets tracking the broad S&P BMV IPC Mexican stock index, receiving text messages allowing them to trade up to 25 pesos each week for three to four weeks. Take-up was relatively low at 28%, yet the results are nonetheless encouraging. While there is no effect on corruption perceptions, as one might expect, nonetheless, the treatment effect of stock exposure on the treated shows a durable effect on support for democracy by 0.8σ (p-value of an increase 0.04), trust in other citizens by 0.58σ (p-value of an increase 0.16), trust in institutions by 0.43σ (p-value of an increase 0.04). Further, the effect on those treated a month before the elections is to raise voter turnout by $12.6pp$, though this is not precisely estimated. We see these results as indicating a promising, but at best suggestive, direction for future research.

5 The Effects of Apex Corruption across Latin America

Our field experiment demonstrates the causal nature of apex corruption revelations in eroding democratic values in Mexico. However, like many RCTs, particularly those involving the provision of real information, a number of natural questions remain. The first is whether the central causal link between apex corruption and democratic erosion generalizes beyond our context. The second is whether the effects are sensitive to the specific features and format of the videos that were randomized in the RCT. For example, could the stronger effect of the Morena apex corruption video reflect the fact that a close relative of the incumbent president was implicated.[40] A further question is whether, as we argue, apex politicians have larger effects on democratic values than scandals implicating lower-level politicians, and whether scandals that emerge through judicial processes are any less corrosive to democratic support.

To shed light on these questions, we employ an interrupted survey design to study the effects of apex corruption scandals of different types on support for democracy across 17 Latin American countries between 2008 and 2018. We exploit the fact that the nationally-representative Latinobarometro surveys contain identical questions on corruption perceptions, support for democracy, and trust. We combine these data with the *exact* timing of apex corruption revelations in each of these countries, exploiting the fact that a set of apex corruption revelations happened to coincide with the days in which the Latinobarómetro surveys—which are planned well in advance— were being fielded (see Bassi and Rasul (2017) and Durante et al. (2020)). We thus compare democratic attitudes immediately before and after the revelation of apex corruption scandals.

Data on Apex Corruption Revelations. To identify the exact timing of corruption scandals, we scraped the Twitter news feeds of the four major news outlets in each of 17 Latin American countries between 2008 and 2018 (see Table OA-19 for a list), searching for corruption-related terms such as "corrupt," "corruption," "bribery," and "illicit enrichment," among others. We supplemented this with news reports from the LexisNexis Spanish-language newspapers database using the same keywords over the same period. This resulted in more than 142,000 news-related tweets and over 10,000 LexisNexis news articles across all countries and years.

To focus on apex corruption, we define an "apex corruption scandal" as an event meeting all of the following conditions: (i) the event was covered by at least two national news outlets on the same day; (ii) those involved were high-ranking political figures, such as presidents or former presidents, ministers or former ministers, opposition leaders, governors, federal legislators, senior federal officials, or federal judges; and (iii) the event constituted a scoop, meaning that it represented a new revelation, rather than continued reporting on an existing scandal.

We identified 176 scandals over the period—roughly one apex corruption revelation per country per year (Figure OA-11). Examples include the arrest of former President Lula da Silva on corruption charges (Brazil); former President Mauricio Funes accused of money laundering (El Salvador); President Otto Pérez's involvement in import bribery (Guatemala); the investigation of opposition leader Keiko Fujimori for money laundering (Peru); President Juan Orlando Hernández identified as a co-conspirator in a drug-trafficking scandal (Honduras); and evidence that Ricardo Martinelli received bribes from Odebrecht

[40] Because we worked with actual incidents of corruption, rather than hypotheticals, we are subject to available real footage and don't have families of former opposition presidents in the videos.

during his presidency (Panama). Our final estimation sample consists of 26 apex corruption revelations that broke during the Latinobarómetro survey fieldwork.[41] No country experienced more than one apex corruption revelation during a survey fielding period.

Latinobarómetro. Latinobarometer (2018) conducts a series of yearly nationally representative surveys covering most Latin American countries. Interviews are conducted in person through house-to-house fieldwork, which typically takes one to three months to complete (see Figure OA-10). The questionnaires are standardized, enabling comparison across countries, and consist of core questions and questions that vary across years. We use individual-level survey data from Latinobarómetro waves conducted between 2008 and 2018 in 17 Latin American countries.[42] Because Latinobarómetro fieldwork lasts approximately 28 days on average, we focus on the sample of over 27,000 individuals interviewed in the 15 days before and after an apex corruption revelation. Paralleling the outcomes in our RCT, we create measures of the perceived importance of corruption[43], support for democracy[44] and trust in democratic institutions.[45]

We also add a supplemental outcome that we did not measure in the RCT: 'Support for Authoritarian Alternatives'. Although we believe this is a useful measure of a decline in democratic support (following Acemoglu et al. (2025b)), it may be noisier for two reasons. Because there is no single question on authoritarianism repeated across years and countries, we use different questions in different years.[46] Further, some years did not include any related questions, which restricts the sample size.

5.1 Empirical Strategy

We employ the following estimation equation to implement the interrupted survey design:

$$Y_{ictd} = \alpha + \beta \mathbf{1}\{PostCorruption_{ictd}\} + \gamma' X_i + \Gamma_{ct} + \epsilon_{ictd} \tag{2}$$

where Y_{ictd} denotes stated support for democracy or related outcomes for respondent i in country c, interviewed in country-year survey round t on day d relative to the corruption scandal; $\mathbf{1}\{PostCorruption_{ictd}\}$

[41] Table OA-20 provides detail. To the best of our knowledge, this list of apex corruption revelations is comprehensive. Given the narrow time window used in the regressions, the omission of some events would reduce the sample size but should not bias the estimated effects of the included revelations. In a companion study (González-Téllez et al., 2025), we analyze the full set of 176 apex corruption revelations and show that exposure increases the likelihood of violent—but not non-violent—protests, again consistent with a decline in democratic support.

[42] Argentina, Bolivia, Brazil, Chile, Colombia, Costa Rica, Ecuador, Dominican Republic, Guatemala, Honduras, Mexico, Nicaragua, Panama, Paraguay, Peru, El Salvador, and Uruguay. We exclude Venezuela because it is not considered a democracy. Latinobarómetro was not implemented in 2019 and, given that COVID began in 2020, our sample ends in 2018. Because we use Twitter to identify corruption revelations, we start the analysis in 2008, when Twitter coverage becomes sufficiently dense.

[43] **Corruption:** we use the following question: *"In your opinion, which is the most important problem facing the country today?"*. Our corruption variable is an indicator = 1 if the person stated *corruption* as the main problem. We focus on this variable since it was asked consistently across years and reflects citizens' major concerns about corruption.

[44] **Democratic Index** is a standardized index of support for democracy using two variables: 'Support for Democracy' which equals 1 for those who agree that"*In some circumstances, an authoritarian government may be preferable*", 2 for those who agree that "*People like me, we don't care about a democratic regime or a non-democratic one*", and equals 3 for those who agree that "*Democracy is preferable to any other form of government*". The second variable is 'satisfaction with democracy', a 4-level response to the following question: *In general, would you say that you are very satisfied, satisfied, slightly satisfied, or not at all satisfied with democracy in your country?*

[45] **Trust in democratic institutions** is a standardized index of trust in the federal government, trust in political parties, trust in Congress, and trust in the electoral authority. To do this, we use *"How much do you trust..."* each of these institutions. Answers take values from "No trust at all"= 1 to "A lot of trust" = 4.

[46] The outcome **Authoritarian Alternatives** = 1 if respondents answer affirmatively to the questions *"I would support a military government instead of a democratic one"* (2009, 2010, 2011); *"I wouldn't mind if an undemocratic government came to power"* (2008, 2016); *"More than political parties and congress, what we need is a strong leader* (2013).

is an indicator that equals one if the interview occurred after the scandal; X_i is a vector of baseline individual controls; and $\Gamma_{c,t}$ includes country-by-year, month, and day-of-week fixed effects. We use heteroscedasticity-robust standard errors clustered either by country×year or by country×year×event day.[47]

To interpret the estimated β as causal, the identifying assumption is that within a narrow event window around the corruption scandal—±15 days in our case—the exact timing of the scandal is as good as random. That is, citizens interviewed immediately before and after the scandal would have otherwise exhibited similar potential support for democracy. This assumption is plausible because the logistics of implementing the Latinobarómetro survey—such as the selection of enumeration sites and the organization of field teams—require months of preparation and do not adjust in response to the occurrence of apex corruption scandals, making it unlikely that interview timing is correlated with citizens' latent democratic preferences. As we describe below, we confirm the comparability of the persons interviewed before and after the revelations on a range of predetermined characteristics (Table OA-16). Further, we show that the timing of apex corruption revelations is hard to predict (Table OA-15). Since, unlike in the RCT, we cannot guarantee that participants interviewed after an apex corruption scandal actually learned of it at the time of their interview, we interpret the results as intent-to-treat estimates, and thus as likely underestimates of the treatment effect of apex corruption revelations on those actually exposed.

5.2 Results: Interrupted Survey Design

Table 5 presents the results. First, note that the probability that a survey respondent considers corruption to be the most important problem in their country increases by $2.9pp$ (or 31% of the mean) in the 15 days after the revelation relative to the 15 days prior (Column 1). The effect size is equivalent to the difference between Costa Rica, a nation known for its relatively good governance, and Guatemala. Thus, mirroring our RCT, apex corruption revelations raise the importance of corruption among citizen priorities.

Further, and again consistent with the RCT, Column 2 shows that support for democracy declines by 0.069σ immediately following the apex corruption scandal. Although this is a (conservative) intent-to-treat effect, the magnitude is nevertheless substantial, comparable to the difference between Chile and Bolivia in our Democratic Index. Finally, note that this is the effect of a single scandal. The effect size implies that 4.7 revelations per country would be sufficient to explain the decrease in satisfaction with democracy that Latin America has experienced over the past decade (as measured by our index), assuming that the effects of repeated apex corruption on democratic values accumulate linearly.

[47] One can think of the treatment variable (the corruption scandal) as a random variable assigned at the country–year-day level. In such a case, clustering at the country-year-day level is justified (Abadie et al., 2022). For robustness, we also present results using a wild bootstrapping technique. Regressors include education, gender, age, employment status, size of the city, a proxy for socioeconomic status, and civil status.

Table 5: Effects of Apex Corruption on Corruption Perceptions and Democratic Values

	(1) Corruption Main Problem	(2) Democratic Index	(3) Trust Institutions	(4) Authoritarian Alternative
Corruption	0.029	-0.069	-0.062	0.039
	(0.011)**	(0.035)*	(0.031)*	(0.012)***
	[0.008]***	[0.033]**	[0.031]**	[0.013]***
Observations	25790	27016	27016	11865
Scandals	26	26	26	13
R-squared	0.073	0.022	0.033	0.223
Wild Bootstrap p-value	0.022	0.080	0.082	0.040
FDR q-value	0.025	0.031	0.031	0.025
Country x Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Mean dep. var	0.095	0.000	0.000	0.361

This table shows the effect of corruption revelations in Latin America by analyzing the responses of people interviewed for the Latinobarometer survey within 15 days before and after corruption revelations, estimating equation 2. Robust standard errors clustered by country × year are shown in parentheses, while robust standard errors clustered by country × year × relative distance to corruption scandal grouped in blocks of three days are shown in square brackets. The individual controls are gender, age, schooling, employment, civil status, size of town, socioeconomic status level, month and day-of-week indicators. The bottom of the table reports the p-value from a wild bootstrap test and q-values using the False Discovery Rate method, which adjusts for testing the hypotheses on the four indices (this correction may be overly conservative since indices constitute different families of outcomes). Significance is reported at levels *** p < 0.01, ** p < 0.05 and * p < 0.1. For computing the q-values we use the 4 p-values associated to the 4 null hypotheses that the coefficient of Corruption equals zero, for each of the four outcomes.

Table 5 (Col 3) estimates the effect on trust in political institutions. Again consistent with the RCT, we find that trust in democratic institutions declines by 0.062σ. Further, as Table OA-17 shows, trust in all four component institutions (Congress, federal government, political parties, and electoral authorities) decline by about 10 percent of their respective means.

A decrease in support for democracy need not mechanically translate into an increased preference for authoritarianism, as it may instead reflect a generalized loss of trust in all forms of government. Moreover, even disillusioned citizens may continue to prefer democratic institutions over authoritarian alternatives. Yet a decline in democratic values due to apex corruption may also increase the appeal of authoritarians who promise to end corruption, even at the expense of democratic norms. Column 4 shows that citizens' loss of trust in democracy in the aftermath of an apex corruption scandal coincides with a rise in support for authoritarianism. We estimate an increase of $3.9pp$ in the authoritarianism index, equivalent to 11 percent of its mean value.[48]

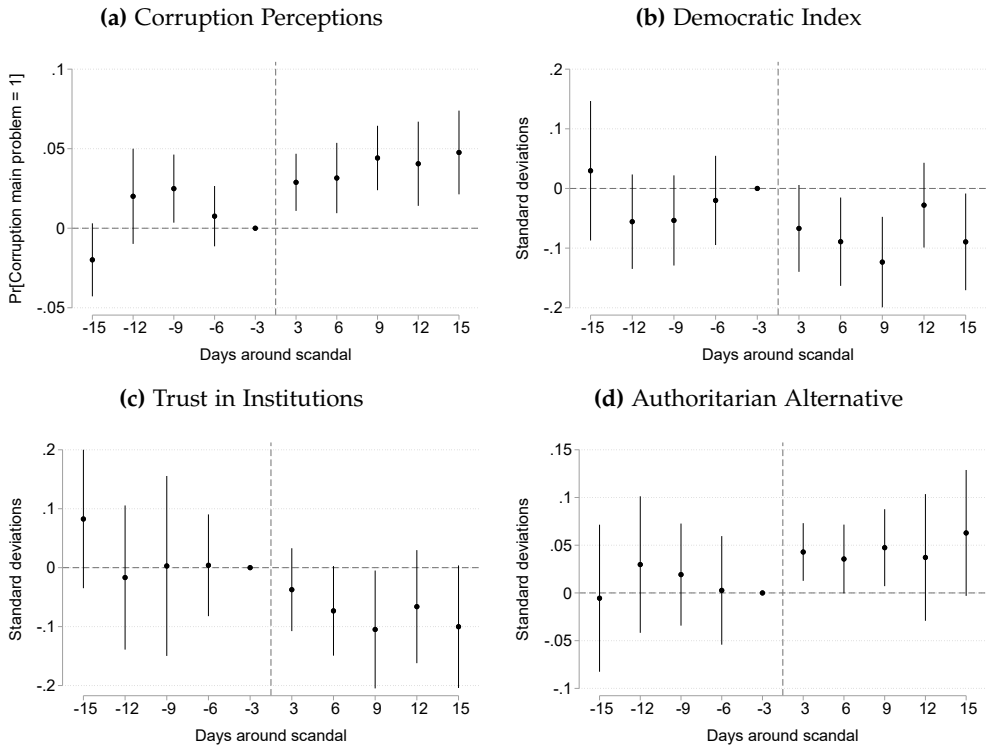
We next examine how the effects evolve in the days following the scandal. To do this, Figure 5 plots the estimated coefficients and their 95% confidence intervals for dummies representing three-day intervals before and after the apex corruption revelation, using individuals interviewed from one to three days prior to the scandal as the reference group. These coefficients are derived from a single regression that controls for individual characteristics and includes country-by-year fixed effects.[49]

As Figure 5 shows, the corruption revelation is accompanied by an *immediate* increase in corruption perceptions, a decrease in support for democracy, a decrease in trust in political institutions, and an increase in the preference for an authoritarian alternative. There is no evidence of decay in the magnitude

[48] This is consistent with broader patterns that find that, although authoritarian governments may themselves be highly corrupt, citizens often misperceive regime attributes (Acemoglu et al., 2024, 2025a).

[49] This approach benchmarks the responses of individuals, from the same country and year, interviewed one to three, four to six, seven to nine, ten to twelve, and twelve to fifteen days after the corruption revelation against those interviewed 1-3 days before the event. These plots serve to evaluate whether the effect is constant and to ensure that no changes in perception or attitudes occurred prior to the scandal. The number of days around the scandal is necessarily limited by the 1-3 month length of Latinobarómetro fieldwork, see OA-9.

Figure 5: Effects of Apex Corruption revelations Over Time



Coefficients and 95% confidence intervals in 3-day blocks from 15 days before to 15 days after an apex corruption revelation. The period from 3 to 1 days before the revelations is normalized to zero. Confidence intervals are based on heteroskedasticity-robust standard errors clustered by country × year × 3-day blocks.

of these effects in this time window; indeed, we cannot reject that the effects remain constant over the event window.

Validity Checks. One key potential concern with this approach is that citizens interviewed right after an apex corruption revelation may differ systematically. We test whether this is the case using observable characteristics. Table OA-16 in the appendix estimates equation 2, using predetermined demographics as dependent variables. We fail to reject the null hypothesis that citizens interviewed before versus after the scandal have equal characteristics on average. We also failed to reject the null hypothesis that the number of refusals to participate in the survey is equal before and after the scandal.

A second concern is that corruption revelations could be strategically released on particular dates—for example, on Mondays, when more people may be paying attention to the news. This would be problematic if, on those dates, Latinobarómetro systematically surveyed individuals who were already more disenchanted with democracy. This scenario is unlikely, as our comparisons are made within a 30-day window and control for month, day-of-month, and day-of-week fixed effects. Nonetheless, we explicitly test whether the timing of apex corruption revelations can be predicted using day-of-week, month, day-of-month, and proximity to the next election. The estimated coefficients on nearly all these predictors are statistically insignificant, and in-sample classification accuracy rates are below 1% (see Appendix Table OA-

15 and Figure OA-12).[50] In sum, the precise timing of apex corruption revelations appears orthogonal to these observable factors.

Another potential alternative explanation for the cross-scandal results is that apex corruption revelations generate general negative affect, i.e., that bad news causes people to exhibit pessimism across the board, rather than undermining democratic values. Notice that we already found that in the RCT, economic underperformance did not have these effects. Nevertheless, to assess this mechanism, we examine whether responses change on a range of non-political satisfaction indicators. In particular, we use the question *"In general, would you say that you are very satisfied, rather satisfied, not very satisfied, or not at all satisfied with the working of the economic system in the country?"* to generate an indicator that takes the value of one if the respondent answered very satisfied or rather satisfied, and zero otherwise. A second question is more concrete and asks: *How would you describe the country's present economic situation? Would you say it is... very good (=5), ..., very bad (=1)?"*. A third variable is more ideological and asks for agreement or disagreement with the following statement: *The market economy is the only system with which the country can become a developed country"*. Table OA-18 shows that apex corruption revelations have no detectable effect on these variables. The effects, therefore, appear concentrated on political institutions rather than on market institutions or the economy.

Staggered difference-in-differences. Our preferred method, the interrupted-survey design above, identifies effects from within-country-year before-after comparisons around each scandal, and requires only that, conditional on country-year fixed effects and controls, the *exact* timing of the scandal within the Latinobarómetro fieldwork window is as-good-as-random (see Bassi and Rasul, 2017; Durante et al., 2020, for similar implementatons). The appendix shows results for staggered difference-in-differences specifications that include cross-country and cross-year comparisons. These specifications require a stronger parallel-trends assumption that may not hold in our context. A potential concern in this setting is cross-country spillovers: respondents in nominal control country-years may themselves be affected by scandals revealed in other democracies, which would typically attenuate the estimated effects toward zero. By contrast, such spillovers would threaten the interrupted-survey design only under the unlikely scenario that foreign scandals differentially affect domestic respondents interviewed after, rather than before, the *domestic* revelation within the same narrow event window.

5.2.1 Local Corruption, Presidents, and Judicial Exposure

Our findings raise two further natural questions: first, whether the high-level nature of corruption is in fact crucial for the effects we find on democratic values, or whether lower-level corruption scandals might also have similar effects; and second, whether, among the highest-level corruption scandals we classify as apex, the effects are larger when they implicate heads of state.

Local corruption scandals have no detectable effects. In the introduction, we argued that compared to local corruption, apex corruption may be particularly damaging for democracy because apex politicians directly oversee core state organizations, and their malfeasance may discredit these. Apex politicians are more likely to be focal political role models who shape norms. Moreover, their national success suggests that society at large tolerates corruption. Apex corruption revelations also have broader media dissemination. While we cannot distinguish among these channels, to estimate the effect of *local* corruption

[50] There is one exception – apex scandals are less likely to break in December. However, as noted above, we compare within months.

revelations, we conducted an analogous interrupted-survey-design analysis. We systematically collected news reports on corruption involving city mayors, local congress members, and local party chapters, restricting attention to new "scoops", as above. We focus on local corruption revelations that occurred while Latinobarómetro was fielding the survey in the relevant country–state. Identifying local scandals is inherently more challenging, given their lower salience and more heterogeneous media coverage. Table OA-23 reports the 14 local corruption events that satisfy the criteria described above.

Table OA-24 estimates the effects of local corruption revelations on the same four indices analyzed above. In contrast to apex corruption, we find no effect on corruption perceptions—if anything, the point estimate is negative, consistent with findings in the local-corruption literature (e.g. Dunning et al., 2019). The estimated effect on support for democracy is also almost zero (0.002). We also find no significant effect on trust in democratic institutions; the point estimate is negative, but only about half the size of the corresponding apex-corruption estimate. Finally, the point estimate on support for authoritarian alternatives is reversed in sign and statistically insignificant.

Presidents versus other apex politicians. We can further investigate the importance of the apex dimension by asking whether scandals implicating heads of state or their family members have larger effects. Of the 26 apex corruption scandals that overlap with the Latinbarometro, however, only five involve heads of state, constraining the analysis that can be conducted. The descriptive patterns are nonetheless illuminating. Figure OA-14 presents box-plots of event-by-event estimates comparing the distribution of coefficients for apex corruption scandals implicating heads of state with those involving other high-ranking politicians. The median presidential corruption scandal raises the probability that corruption is viewed as the main problem of society by roughly three times as much as scandals involving other apex politicians ($6pp$ vs. $2pp$), while lowering the democracy index by roughly five times as much in absolute value (-0.10σ vs -0.02σ).

Does the judicial process ameliorate the effects of apex corruption? We have shown that revelations of apex corruption undermine support for democracy. However, it could be the case that some revelations, particularly those emerging through the judicial process, are indicative of greater transparency, and that no one is above the law. This may be consistent with a healthier democracy, and may be perceived that way. To examine this question, we classified each apex corruption scandal as either stemming solely from media reports, with no accompanying judicial process, or involving formal accusations, indictments, or sentences. Again, given the small number of each type of scandal that overlap with the Latinobarómetro survey, the findings should be interpreted with caution. However, the basic pattern is fairly clear. Instead of offsetting the effects of apex corruption, those involving indictments or formal judicial accusations actually generate larger decreases in support for democracy and trust in democratic institutions than those involving only media reports (Table OA-22). Because trials take many years and because of the small number of events, we cannot study whether final convictions restore trust in democratic institutions, which remains an important topic for future research.

In summary, these cross-country comparisons—covering nearly all of Latin America—complement our field experiment by showing that the effects of apex corruption are not confined to the specifics of the Mexican context, the particulars of the experimental design, experimenter demand effects, or the exact content of the corruption revelation. Instead, the evidence indicates that the effects of apex corruption

on support for democracy generalize across countries and institutional settings, and are also present in real-world corruption scandals, which incorporate endogenous responses by politicians.

6 Conclusion

This paper provides causal evidence that credible revelations of apex corruption can weaken citizens' support for democracy and reduce democracy-sustaining behaviors. Our evidence comes from two complementary empirical strategies. In a field experiment in Mexico, exposure to real video evidence of apex corruption reduced actual voter turnout, willingness to support election administration, trust in politicians and fellow citizens, and behavior consistent with honesty norms. These effects were especially large when the revelation implicated the incumbent anti-corruption party and when exposure occurred close to the election. The pattern is not simply a response to bad news about government: a comparison treatment on economic underperformance produced a different political response, suggesting that citizens respond to poor performance by engaging electorally, but to apex corruption by withdrawing from the democratic system itself.

The cross-country evidence points in the same direction. Using real-world corruption revelations across Latin America, we find that citizens interviewed shortly after an apex corruption scandal report lower support for democracy, lower trust in democratic institutions, and greater openness to authoritarian alternatives than citizens interviewed shortly before the scandal. This evidence suggests that the corrosive effects we document are not specific to one video or one national context. Additionally, although the sample of lower-level (non-apex) corruption scandals is smaller, analogous analyses do not reveal comparable effects.

The findings highlight a transparency–faith tradeoff at the heart of democratic accountability. Exposing apex corruption is essential: democracies cannot hold powerful politicians accountable if their misconduct remains hidden. Yet when corruption implicates the very leaders who are supposed to embody and protect democratic institutions, transparency may also weaken citizens' faith in democracy itself.

While our study provides unusually comprehensive causal evidence on the consequences of apex corruption revelations, it also raises critical questions for future research. Why do revelations of apex corruption increase openness to authoritarian alternatives, even though authoritarian leaders may themselves be corrupt? Are the effects of apex corruption revelations similar in authoritarian regimes, or are they distinctive to democracies, where citizens expect leaders to be constrained by institutions? Can judicial accountability and punishment restore democratic trust after apex corruption is revealed? Finally, which policies could counteract the corrosive effects of apex corruption and help strengthen democratic values?

Ultimately, democratic resilience depends not only on exposing corruption, but also on ensuring that citizens can interpret such exposure as evidence that democratic accountability is working rather than as evidence that democracy has failed. The challenge is to build institutions and informational mechanisms that allow citizens to punish corrupt apex politicians while preserving their commitment to the democratic system.

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Apex Corruption Erodes Democratic Values

Eduardo Rivera, Enrique Seira, Saumitra Jha

Appendix – For Online Publication

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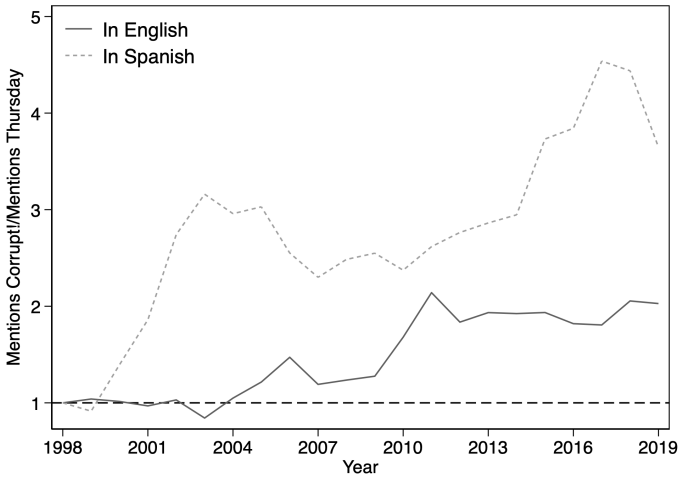
OA - A Corruption Exposure and Support for Democracy: Basic Patterns

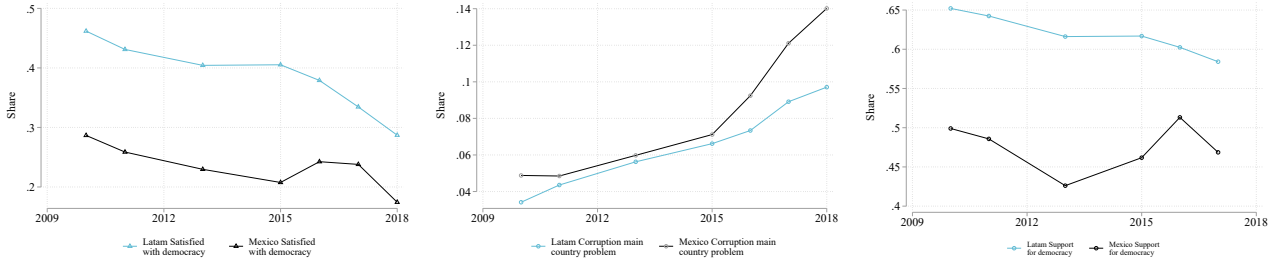
Figure OA-1: Media mentions of corruption



Source: Lexis-Nexis corruption-related newspaper articles (includes over 45,000 news sources from 15 countries). For the searches in English the prefix word is keyword "corrupt", while for Spanish we use "corrupt!" and "corrupción" also containing mentions of countries in our LA sample. We normalize both measures by the word "thursday" or "jueves", respectively, and normalize 1998=1.
Backreferenced (on pages): [5]

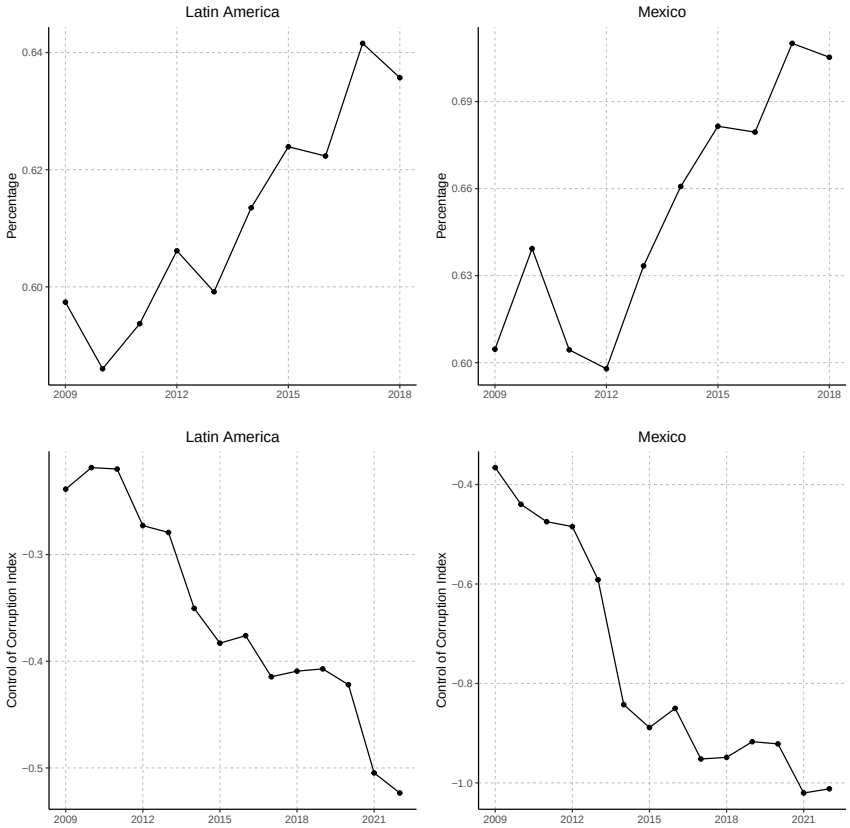
Figure OA-2: Concerns about Corruption Rise as Democratic Satisfaction Falls

(a) Satisfied with Democracy **(b)** Corruption **(c)** Support for Democracy



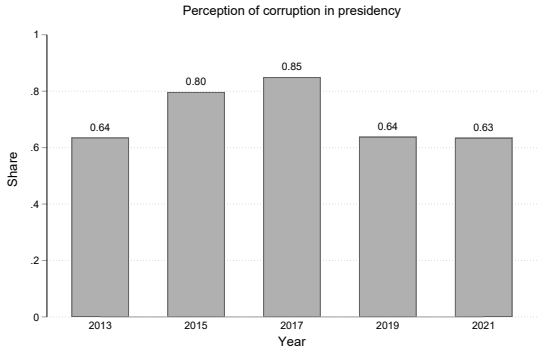
The figure depicts trends in corruption and satisfaction with democracy using Latinobarometer surveys from 2009 to 2018. Panel (a) reports the share that say they are very satisfied or satisfied with democracy. Panel (b) reports the share of respondents that answered "corruption" when asked, "What is the main problem in the country?" With more than 30 alternatives coded. We report the moving averages for the current and previous year.
Backreferenced (on pages): [5]

Figure OA-3: Firms' increases in bribes and Control of Corruption



(Top) Global Competitiveness Survey from the World Economic Forum. The survey gathers data through a questionnaire distributed to domestic and foreign-owned firms worldwide. This graph shows the average percentage of yes in questions related to irregular payments in exports and imports, in public utilities, in tax collection, in public contracts, in judicial decisions. (Bottom) The Worldwide Governance Indicators project, WB. Household, expert, firm surveys + commercial providers, NGOs, and public sector organizations. Actual bribing + perceived corruption. It encompasses both minor and major corruption, as well as the state's domination by elites and private interests. The Index takes values from -2.5 to 2.5, where a higher score means more control of corruption, using Unobserved Components Model.
Backreferenced (on pages): [5]

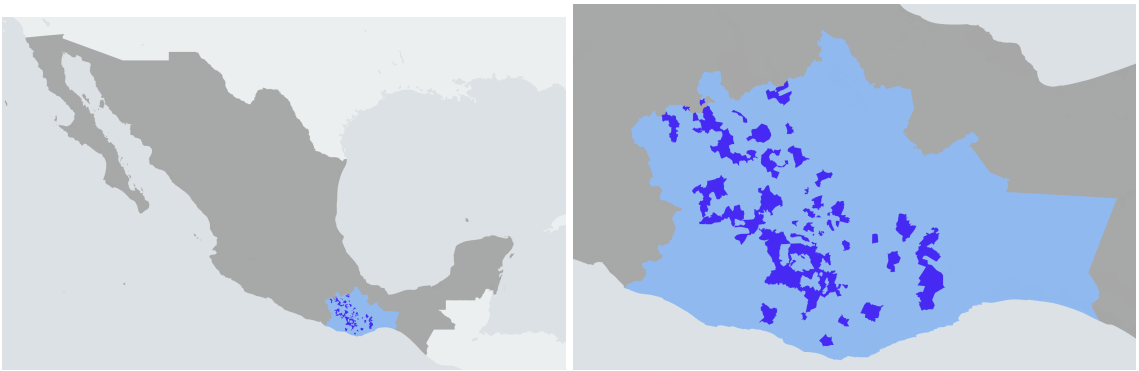
Figure OA-4: The incumbent president (since 2018) is perceived as less corrupt



Notes: This figure plots the citizens' perception of corruption in the presidency over time. Specifically, it plots the average of a dummy that takes the value of one if the surveyed answered "very frequent" or "frequent" to "In your opinion, how frequent are the acts of corruption in the presidency of Mexico?". Source ENCIG, INEGI
Backreferenced (on pages): [6]

OA - B Details of RCT and Experimental Integrity

Figure OA-5: Field Experiment sites: Oaxaca, Mexico



Notes: We sampled 69 urban and rural municipalities in Oaxaca
Backreferenced (on pages): [7]

Table OA-1: Basic experimental design

	City	Rural	Total
Incumbent corruption video	145	401	546
Opposition corruption video	159	387	546
Nation building video	308	523	831
Economic video	117	141	258
No video (control)	637	513	1150
Total	1366	1965	3331

The 5 experimental arms are listed in rows. Columns indicate the observations that belong to Oaxaca city (city) and outside Oaxaca city (rural).Originally we planned to have 650 observations in each of the corruption videos, 300 in the economic video, 1000 in the nation-building video, and 1400 in the control group. For budgetary and logistical reasons (including disruptions related to Covid), samples ended up being close but slightly smaller than these targets.
Backreferenced (on pages): [9]

Table OA-2: Balance Checks & Attrition

	(1) Male	(2) Age	(3) Completed Middle	(4) Employed	(5) High Middle Class	(6) Gvt. Satis. (1–4)	(7) Econ. Satis. (1–4)	(8) Support. Incumb. (1–10)	(9) Corruption Percep. (1–3)	(10) Democratic Scale	(11) Vote as Duty	(12) Attrition
Panel A: Balance — Main sample												
Corruption Incumbent	0.013	-0.842	-0.004	-0.001	-0.005	0.006	0.011	-0.305*	0.025	0.038	-0.060*	-0.015
	(0.026)	(0.853)	(0.023)	(0.025)	(0.011)	(0.043)	(0.036)	(0.169)	(0.041)	(0.125)	(0.031)	(0.037)
Corruption Opposition	-0.034	-0.005	-0.008	0.002	-0.006	0.050	0.050	-0.119	-0.012	0.014	-0.008	0.064
	(0.025)	(0.866)	(0.022)	(0.025)	(0.012)	(0.044)	(0.037)	(0.172)	(0.041)	(0.125)	(0.029)	(0.040)
Nation Building	-0.039*	0.131	0.006	-0.010	-0.003	0.025	0.009	-0.025	-0.028	0.133	-0.023	-0.019
	(0.022)	(0.742)	(0.019)	(0.022)	(0.010)	(0.039)	(0.032)	(0.150)	(0.035)	(0.105)	(0.026)	(0.028)
Economic	-0.005	1.044	-0.032	-0.010	-0.011	-0.045	-0.013	-0.102	0.021	-0.050	-0.001	-0.021
	(0.034)	(1.106)	(0.029)	(0.032)	(0.016)	(0.058)	(0.048)	(0.226)	(0.053)	(0.165)	(0.035)	(0.040)
Observations	3331	3331	3331	3331	3331	3321	3331	3331	3331	3331	3326	1366
All = 0 (p-value)	0.198	0.622	0.785	0.986	0.960	0.597	0.701	0.452	0.752	0.715	0.393	0.376
Control Mean	0.39	40.36	0.78	0.63	0.95	2.33	1.82	5.47	1.81	5.66	3.45	0.22
Panel B: Balance — Subjects matched to administrative data												
Corruption Incumbent	-0.001	-1.078	-0.004	-0.012	0.014	-0.034	-0.001	-0.219	0.013	-0.098	-0.117***	0.015
	(0.033)	(1.060)	(0.028)	(0.031)	(0.014)	(0.054)	(0.045)	(0.211)	(0.053)	(0.159)	(0.041)	(0.023)
Corruption Opposition	-0.071**	1.140	-0.027	-0.043	0.002	0.005	0.028	-0.239	-0.019	0.031	-0.051	-0.014
	(0.032)	(1.073)	(0.028)	(0.032)	(0.015)	(0.057)	(0.048)	(0.223)	(0.053)	(0.163)	(0.038)	(0.024)
Nation Building	-0.037	0.422	0.000	-0.000	0.012	-0.020	-0.017	0.114	-0.001	0.134	-0.033	0.007
	(0.028)	(0.911)	(0.023)	(0.027)	(0.012)	(0.049)	(0.039)	(0.189)	(0.045)	(0.132)	(0.033)	(0.020)
Economic	-0.061	0.459	-0.040	-0.031	0.017	-0.093	-0.130**	-0.269	0.029	-0.067	0.000	0.029
	(0.041)	(1.346)	(0.035)	(0.040)	(0.016)	(0.070)	(0.056)	(0.271)	(0.065)	(0.203)	(0.043)	(0.028)
Observations	2134	2134	2133	2133	2134	2127	2122	2134	2097	2084	2123	2481
All = 0 (p-value)	0.135	0.468	0.708	0.665	0.739	0.716	0.138	0.384	0.971	0.669	0.057	0.695
Control Mean	0.40	40.42	0.79	0.65	0.95	2.31	1.83	5.26	1.84	5.64	3.48	0.82
Panel C: Balance — Far vs. Close to Election												
Far from Elections	-0.008	1.514	-0.063***	0.035	-0.000	-0.075	-0.036	-0.305	0.067	-0.049	0.091**	
	(0.034)	(1.122)	(0.024)	(0.033)	(0.012)	(0.060)	(0.050)	(0.233)	(0.054)	(0.179)	(0.042)	
Observations	1154	1154	1154	1154	1154	1153	1154	1154	1154	1154	1152	
Close to Elections Mean	0.40	38.47	0.88	0.66	0.97	2.30	1.83	5.45	1.81	5.65	3.39	
Panel D: Balance — Two Brother treatment												
Treated Follow-up	0.016	0.657	0.007	0.007	-0.005	0.005	-0.037	-0.031	-0.043	0.094	-0.006	
	(0.029)	(0.985)	(0.023)	(0.028)	(0.011)	(0.053)	(0.042)	(0.197)	(0.047)	(0.142)	(0.038)	
Observations	1093	1093	1133	1132	1133	1129	1130	1133	1114	1121	1129	
Control Mean	0.37	40.60	0.82	0.68	0.97	2.27	1.79	5.17	1.90	5.63	3.45	

This table shows the balance in covariates for people interviewed in the RCT across the experimental conditions. Panel A shows balance across treatments in the main sample, Panel B shows balance across treatments within individuals matched to administrative data, Panel C shows balance across individuals interviewed far vs. close to the election, and Panel D shows balance across treatments for the Two Brother treatment in the follow-up. Each column shows the coefficient of regressing the individual covariate onto indicators for each of the treatment conditions, enumerator and municipality fixed effects. In Panel C we do not include municipality fixed effects as the far vs. close to elections randomization was only done in Oaxaca City. *Male* takes a value of 1 if the respondent identifies as male and 0 otherwise. *Age* measured the respondent age in years. *Completed Middle School* takes a value of 1 if the respondent completed middle school and 0 otherwise. *Employed* takes a value of 1 if the respondent is currently employed and 0 otherwise. *High-middle class* takes a value of 1 if the interviewer rated the socioeconomic level of the respondent as medium, good, or very good, based on the quality of the household, furniture and general appearance. *Government Satisfaction* measures the responses of the question "How satisfied are you with the performance of the government in this country?" from 1 to 4, where 1 is the most satisfied. In Panel A *Attrition* is an indicator equal to 1 if the individual is in the main sample in Oaxaca City but could not be contacted in the 4-month follow-up survey (there is no attrition in the short run survey). In Panel B it is an indicator equal to 1 if the individual is in the main sample, interviewed before the election and could not be matched to administrative data from the electoral institution.

Backreferenced (on pages): [9,11,22,OA-14,OA-14]

Table OA-3: Long run experimental design

Baseline Treatment	Follow-up CI_2 Treatment		
	Treated	Control	Total
Control	274	253	527
Corruption Incumbent (CI)	70	54	124
Corruption Opposition (CO)	57	67	124
Nation Building (NB)	127	131	258
Economic (E)	50	50	100
Total	578	555	1133

The table shows the cross-randomization of the Two Brothers Incumbent Corruption video CI_2 done in Oaxaca city at follow-up 3 months after the first intervention. The first experiment arms are displayed in rows, while columns display the allocation of CI_2.
Backreferenced (on pages): [OA-14]

B.1 RCT treatment effects

B.1.1 Vote

Table OA-4: Effect of Videos on Actual Voting: all treatment arms

	Actual vote			Retrospective vote				
	(1) Actual Vote	(2) Actual Vote	(3) Actual Vote	(4) States Went to Vote	(5) Vote Incumbent	(6) Vote Opposition	(7) Vote Others	(8) Null Vote or No Vote
Corruption Incumbent (CI)	-0.024	-0.045	-0.224**	-0.221**	-0.198**	0.005	-0.032	0.225**
	(0.032)	(0.047)	(0.090)	(0.089)	(0.095)	(0.081)	(0.037)	(0.090)
Corruption Opposition (CO)	-0.056*	-0.079*	-0.050	-0.139	0.047	-0.210***	0.071	0.092
	(0.033)	(0.047)	(0.104)	(0.096)	(0.096)	(0.065)	(0.064)	(0.094)
Nation Building (NB)	0.006	-0.003	-0.023	-0.082	0.046	-0.085	-0.004	0.042
	(0.027)	(0.036)	(0.073)	(0.062)	(0.069)	(0.055)	(0.030)	(0.058)
Economic (E)	0.081**	0.099*	0.004	-0.019	-0.208*	0.045	0.063	0.099
	(0.040)	(0.051)	(0.123)	(0.108)	(0.108)	(0.111)	(0.087)	(0.114)
Far from Election			-0.137**	-0.080*	-0.014	-0.024	-0.030	0.068
			(0.053)	(0.046)	(0.054)	(0.044)	(0.021)	(0.046)
CI × Far			0.238**	0.221**	0.281**	-0.083	0.030	-0.227**
			(0.105)	(0.101)	(0.110)	(0.092)	(0.039)	(0.102)
CO × Far			-0.035	0.137	-0.095	0.229***	-0.078	-0.056
			(0.116)	(0.107)	(0.110)	(0.078)	(0.066)	(0.105)
NB × Far			0.023	0.111	0.020	0.012	0.010	-0.042
			(0.085)	(0.072)	(0.080)	(0.063)	(0.032)	(0.069)
E × Far			0.121	0.066	0.169	-0.034	-0.031	-0.104
			(0.136)	(0.121)	(0.123)	(0.122)	(0.090)	(0.128)
Observations	2045	1154	1154	1162	1162	1162	1162	1162
R2	0.127	0.127	0.136	0.080	0.185	0.151	0.066	0.073
Mean dep. var.	0.604	0.575	0.575	0.772	0.541	0.209	0.024	0.226
$\mid CI \mid \geq 0$ (p-value)	0.229	0.166	0.007	0.007	0.019	0.476	0.809	0.006
$\mid CO \mid \geq 0$ (p-value)	0.045	0.047	0.314	0.074	0.312	0.001	0.136	0.163
$\mid NB \mid \geq 0$ (p-value)	0.413	0.465	0.378	0.093	0.252	0.063	0.448	0.236
$\mid E \mid \geq 0$ (p-value)	0.020	0.027	0.487	0.430	0.028	0.342	0.232	0.194
Enumerator FE	✓	✓	✓	✓	✓	✓	✓	✓
Municipality FE	✓	Urban	Urban	Urban	Urban	Urban	Urban	Urban
Demography Controls	✓	✓	✓	✓	✓	✓	✓	✓
Democratic/Economic Controls	✓	✓	✓	✓	✓	✓	✓	✓

This table presents, in columns (1)–(3) the effects on actual voting from matching the subjects with administrative data using the same specifications as Table 1 but separating out the incumbent and opposition apex corruption treatments. In columns (4)–(8) we show treatment effect estimates on self-reported voting. For column (4) we ask *Did you vote in the last elections?*. For columns (5), (6) and (7) we use the questions *Which party did you vote for?* and *which party would you have voted for in case you did not vote?*. Column 6 only includes the opposition parties featured in the CO video (e.g. PRI, PAN, PRD). In column (8) the outcome is a dummy variable equal to 1 if the respondent said she did not cast a vote or she cast a null vote and 0 otherwise. Robust standard errors are shown in parentheses. Stars denote statistical significance: *** p < 0.01, ** p < 0.05 and * p < 0.1
Backreferenced (on pages): [13,14,OA-33,OA-35]

Table OA-5: Actual and Retrospective Voting by Morena Support

	Actual vote			Retrospective vote				
	(1) Actual Vote	(2) Actual Vote	(3) Actual Vote	(4) States Went to Vote	(5) Vote Incumbent	(6) Vote Opposition	(7) Vote Others	(8) Null Vote or No Vote
Panel A: MORENA Supporters								
Corruption Incumbent (CI)	-0.041	-0.079	-0.232**	-0.206*	-0.242**	-0.008	-0.032	0.282**
	(0.040)	(0.058)	(0.105)	(0.106)	(0.112)	(0.090)	(0.048)	(0.113)
Corruption Opposition (CO)	-0.040	-0.054	0.070	-0.017	0.061	-0.199***	0.091	0.047
	(0.039)	(0.055)	(0.150)	(0.105)	(0.127)	(0.060)	(0.100)	(0.103)
Far from Election			-0.116*	-0.079	-0.004	-0.049	-0.046*	0.100*
			(0.064)	(0.056)	(0.067)	(0.051)	(0.025)	(0.053)
CI × Far			0.204	0.235*	0.311**	-0.049	0.050	-0.312**
			(0.126)	(0.120)	(0.130)	(0.101)	(0.051)	(0.126)
CO × Far			-0.139	-0.047	-0.158	0.185**	-0.084	0.058
			(0.161)	(0.118)	(0.141)	(0.076)	(0.102)	(0.117)
Observations	1454	816	816	803	803	803	803	803
R2	0.140	0.143	0.154	0.094	0.183	0.125	0.094	0.094
Mean dep. var.	0.620	0.593	0.593	0.798	0.618	0.172	0.017	0.192
$\mid CI \mid \geq 0$ (p-value)	0.152	0.085	0.013	0.026	0.015	0.462	0.745	0.006
$\mid CO \mid \geq 0$ (p-value)	0.151	0.164	0.320	0.434	0.316	0.001	0.182	0.324
$\mid CI + CI \times Far \mid \geq = 0$ (p-value)			0.340	0.299	0.859	0.887	0.856	0.706
$\mid CO + CO \times Far \mid \geq = 0$ (p-value)			0.120	0.124	0.943	0.619	0.671	0.033
Panel B: Non-MORENA Supporters								
Corruption Incumbent (CI)	-0.008	0.025	-0.199	-0.203	-0.037	0.030	-0.010	0.017
	(0.063)	(0.082)	(0.163)	(0.164)	(0.194)	(0.189)	(0.039)	(0.151)
Corruption Opposition (CO)	-0.112	-0.162*	-0.190	-0.385**	0.007	-0.295**	0.082	0.206
	(0.069)	(0.096)	(0.168)	(0.175)	(0.168)	(0.133)	(0.085)	(0.183)
Far from Election			-0.198**	-0.083	-0.066	0.042	0.029	-0.005
			(0.097)	(0.089)	(0.096)	(0.084)	(0.038)	(0.095)
CI × Far			0.302	0.132	0.146	-0.163	-0.043	0.060
			(0.189)	(0.196)	(0.222)	(0.215)	(0.051)	(0.187)
CO × Far			0.009	0.641***	0.195	0.401**	-0.133	-0.463**
			(0.201)	(0.202)	(0.204)	(0.176)	(0.093)	(0.208)
Observations	588	336	336	358	358	358	358	358
R2	0.213	0.235	0.254	0.195	0.182	0.267	0.121	0.179
Mean dep. var.	0.562	0.531	0.531	0.713	0.362	0.293	0.040	0.305
$\mid CI \mid \geq 0$ (p-value)	0.449	0.381	0.112	0.108	0.425	0.438	0.606	0.454
$\mid CO \mid \geq 0$ (p-value)	0.054	0.045	0.130	0.014	0.483	0.013	0.169	0.130
$\mid CI + CI \times Far \mid \geq = 0$ (p-value)			0.140	0.234	0.872	0.935	0.979	0.781
$\mid CO + CO \times Far \mid \geq = 0$ (p-value)			0.057	0.004	0.965	0.817	0.957	0.003
Enumerator FE	✓	✓	✓	✓	✓	✓	✓	✓
Municipality FE	✓	Urban	Urban	Urban	Urban	Urban	Urban	Urban
Demography Controls	✓	✓	✓	✓	✓	✓	✓	✓
Democratic/Economic Controls	✓	✓	✓	✓	✓	✓	✓	✓

This table replicates the specification of Table 2, subsetting among ex ante Morena supporters in Panel A and ex ante non-Morena supporters in Panel B. In particular, we ask *We would like to know your attitude towards certain groups. I will read out the name of some groups and ask you to evaluate each group from 0 to 10 according to the following criterion: An evaluation of 0 means that you totally reject that person or institution. An evaluation of 5 means that you are indifferent, and an evaluation of 10 means that you completely support that person or institution.* We consider an individual to be a non- Morena supporter if they evaluate Morena at 5 or below on this scale. Significance is reported at levels *** p < 0.01, ** p < 0.05 and * p < 0.1

Backreferenced (on pages): [15,OA-34]

B.1.2 Political behaviors and Incentivized Behaviors

Table OA-6: Political Behavior: All Video Treatments

	(1) Electoral Observer	(2) Electoral Observer	(3) Electoral Observer	(4) Donate to Elec. Institution	(5) Donate to Elec. Institution	(6) Donate to Elec. Institution
Corruption Incumbent (CI)	-0.022**	-0.021**	-0.032**	-0.040*	-0.036	-0.043
	(0.011)	(0.011)	(0.013)	(0.024)	(0.024)	(0.037)
Corruption Opposition (CO)	0.003	0.005	-0.014	-0.000	0.001	-0.012
	(0.013)	(0.013)	(0.017)	(0.024)	(0.024)	(0.038)
Nation Building (NB)	-0.019*	-0.018*	-0.018	-0.026	-0.028	-0.060**
	(0.010)	(0.010)	(0.014)	(0.020)	(0.020)	(0.027)
Economic (E)	-0.001	0.001	-0.009	-0.014	-0.014	0.013
	(0.016)	(0.016)	(0.021)	(0.031)	(0.031)	(0.042)
Observations	2460	2460	1359	2481	2481	1366
R-squared	0.073	0.083	0.058	0.129	0.136	0.115
Dep. Var. Mean	0.049	0.049	0.054	0.228	0.228	0.243
H0: CI $\geq$ 0 (p-value)	0.017	0.025	0.008	0.045	0.063	0.119
H0: CO $\geq$ 0 (p-value)	0.599	0.642	0.201	0.499	0.512	0.371
Enumerator FE	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓
Demographic Controls		✓	✓		✓	✓
Democratic/Economic Controls		✓	✓		✓	✓
Sample	All	All	Urban	All	All	Urban

This table shows treatment effect estimates of the Corruption Incumbent, Corruption Opposition, Nation Building and Economic performance videos on the probability of registering as an electoral observer in columns (1)–(3) and on the probability of donating money to the electoral institution in columns (4)–(6). The main specification, shown in columns (3) and (6), is $Y_i = \alpha_e + \eta_m + \beta_{CI}CI + \beta_{CO}CO + \beta_{NB}NB + \beta_E E + X_i'\gamma + \varepsilon_i$. The vector X' includes demographic and democratic/economic attitudes controls such a respondent sex indicator, age, age squared, education level attained, indicator for being employed and SES, satisfaction with the economy, stated level of democracy in Mexico (1–10), perception of corruption improvement and indicators for supporting either the incumbent or opposition. Heteroskedasticity-robust standard errors are shown in parentheses. * p < 0.10 ** p < 0.05 *** p < 0.001

Backreferenced (on pages): [17]

Table OA-7: Incentivized Games: Beliefs and Norms: All Video Treatments

	(1) Politician	(2) Politician	(3) Politician	(4) Neighbor	(5) Neighbor	(6) Neighbor
Panel A: Trust Games						
Corruption Incumbent (CI)	-0.185***	-0.182***	0.042	-0.120**	-0.113*	0.078
	(0.063)	(0.062)	(0.115)	(0.060)	(0.060)	(0.104)
Corruption Opposition (CO)	-0.115*	-0.120*	-0.165	-0.079	-0.084	-0.083
	(0.064)	(0.064)	(0.106)	(0.059)	(0.059)	(0.098)
Nation Building (NB)	-0.130**	-0.132**	-0.159**	-0.186***	-0.187***	-0.226***
	(0.053)	(0.053)	(0.077)	(0.052)	(0.052)	(0.076)
Economic (E)	-0.095	-0.097	-0.106	-0.118	-0.113	-0.076
	(0.082)	(0.081)	(0.122)	(0.077)	(0.077)	(0.120)
Observations	3331	3331	1366	3331	3331	1366
R-squared	0.121	0.132	0.149	0.104	0.112	0.123
Dep. Var. Mean	1.625	1.625	1.615	2.058	2.058	2.043
H0: CI ≥ 0 (p-value)	0.002	0.002	0.642	0.024	0.031	0.773
H0: CO ≥ 0 (p-value)	0.035	0.029	0.060	0.088	0.076	0.199
	Public Projects	Public Projects	Public Projects	Neighbor	Neighbor	Neighbor
Panel B: Resource Allocation Games						
Corruption Incumbent (CI)	-0.677**	-0.654**	-1.407**	-0.439	-0.400	-1.114
	(0.324)	(0.329)	(0.649)	(0.368)	(0.371)	(0.695)
Corruption Opposition (CO)	0.143	0.150	0.351	-0.248	-0.274	-0.968
	(0.351)	(0.350)	(0.689)	(0.385)	(0.384)	(0.734)
Nation Building (NB)	0.239	0.342	0.649	0.174	0.252	0.541
	(0.329)	(0.331)	(0.688)	(0.339)	(0.337)	(0.714)
Economic (E)	-0.728*	-0.663*	-1.098	-0.651	-0.634	-1.414
	(0.393)	(0.392)	(0.951)	(0.399)	(0.396)	(0.984)
Observations	1573	1573	493	1573	1573	493
R-squared	0.088	0.104	0.117	0.073	0.092	0.114
Dep. Var. Mean	14.752	14.752	14.920	14.898	14.898	15.046
H0: CI ≥ 0 (p-value)	0.019	0.024	0.015	0.117	0.141	0.055
H0: CO ≥ 0 (p-value)	0.658	0.666	0.695	0.260	0.238	0.094
Enumerator FE	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓
Demographic Controls		✓	✓		✓	✓
Democratic/Economic Controls		✓	✓		✓	✓
Sample	All	All	Urban	All	All	Urban

Notes: This table shows treatment effect estimates of the Corruption Incumbent and Corruption Opposition videos on trust games in Panel A and on resource allocation games in Panel B. Columns (1)–(3) show effects on trust games and resource allocation games with a politician/public project, while columns (4)–(6) show effects for games with neighbors. We restrict the sample in columns (1), (2), (4) and (5) of Panel A to the sample of respondents for whom there are no missing covariate values in the most restrictive sample (columns 2 and 5). In columns (3) and (6) we further restrict the sample to respondents in Oaxaca City. We do the analogous restrictions in Panel B, with the difference that, to economize on budget, we draw a random sample of respondents to participate in Resource Allocation Games. Heteroskedasticity-robust standard errors are shown in parentheses. * p < 0.10 ** p < 0.05 *** p < 0.01
Backreferenced (on pages): [18]

B.1.3 Effects on Conscious Perceptions and Values

Table OA-8: Effects on Corruption Perceptions, Support for Democracy, and Trust

	(1) Corruption Index	(2) Democratic Index	(3) Trust Institutions Index	(4) Trust People Index
Corruption Incumbent (CI)	0.276***	-0.159***	-0.280***	-0.183***
	(0.048)	(0.050)	(0.048)	(0.046)
Corruption Opposition (CO)	0.189***	-0.161***	-0.106**	-0.051
	(0.048)	(0.050)	(0.049)	(0.049)
Nation Building (NB)	0.045	0.069*	0.001	-0.011
	(0.042)	(0.041)	(0.043)	(0.043)
Economic (E)	0.179***	-0.281***	-0.252***	-0.051
	(0.062)	(0.063)	(0.060)	(0.061)
Observations	3331	3331	3331	3331
R-squared	0.201	0.186	0.211	0.170
Mun. and Enum. FE	Yes	Yes	Yes	Yes
Mean dep. var	0.000	-0.000	-0.008	-0.003
$\mid CI \mid \geq 0$	0.000	0.001	0.000	0.000
$\mid CO \mid \geq 0$	0.000	0.001	0.015	0.151

This table presents the effects of the four different videos on the four main outcomes described in the text. Individual Controls include: Gender, Age, Schooling, Employment, Current Economic Satisfaction, Perception of Democracy in Mexico, Initial corruption perception, Support for Incumbent and Opposition parties, and Socioeconomic status. Robust standard errors are shown in parentheses. The bottom panel reports p values associated to the test of treatment effects reducing (increasing) the corruption (democratic, trust in institutions and trust in people) index. The significance is reported at levels *** p < 0.01, ** p < 0.05 and * p < 0.1
Backreferenced (on pages): [21,21,OA-34,OA-35,OA-35]

Table OA-9: Q-values for Effects on Corruption Perceptions, Democratic Attitudes, and Trust Political Institutions, and Trust in People

	(1) Corruption Index b/p/W	(2) Democratic Index b/p/W	(3) Trust Institutions Index b/p/W	(4) Trust People Index b/p/W
Corruption Incumbent (CI)	0.276	-0.159	-0.280	-0.183
	(0.000)	(0.002)	(0.000)	(0.000)
	[0.001]	[0.002]	[0.001]	[0.001]
Corruption Opposition (CO)	0.189	-0.161	-0.106	-0.051
	(0.000)	(0.001)	(0.030)	(0.302)
	[0.001]	[0.002]	[0.022]	[0.160]
Nation Building (NB)	0.045	0.069	0.001	-0.011
	(0.279)	(0.095)	(0.989)	(0.792)
	[0.160]	[0.055]	[0.448]	[0.359]
Economic (E)	0.179	-0.281	-0.252	-0.051
	(0.004)	(0.000)	(0.000)	(0.398)
	[0.004]	[0.001]	[0.001]	[0.166]
Observations	3331	3331	3331	3331
R-squared	0.201	0.186	0.211	0.170
Enumerator FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

This table shows the p-values in parentheses and q-values in brackets for the coefficients of the corruption, democratic, trust in institutions, and trust in others indices. We keep the same individual controls. The q-values are computed as first introduced in Benjamini et al. (2006). The procedure is as follows: First we compute p-values for the multiple hypotheses we are testing. We arrange these p-values in ascending order. Compute critical values according to the formula proposed in Benjamini et al. (2006). We then compare p-values to these critical values and consider the hypothesis associated to the p-value as significant if it is less than or equal to the critical value. Lastly, assign as a q-value for that hypothesis the minimum False Discovery Rate at which the hypothesis may be deemed significant. For computing the q-values we use the p-values associated to the 16 null hypotheses that the coefficient of each individual treatment is zero, for each of the 4 outcomes.
Backreferenced (on pages): [20]

B.2 Decomposing indices

Table OA-10: Decomposing Effects on Corruption Perceptions

	(1) Corruption Index	(2) Progress Fighting Corruption (1-4)	(3) Share of Corrupt Pol. (0-100)	(4) Share of Taxes Stolen by Pol (0-100)	(5) All Politicians Are Corrupt (1-4)
Corruption	0.232***	-0.144***	3.818***	2.619**	0.124***
	(0.039)	(0.037)	(0.982)	(1.097)	(0.033)
Nation Building (NB)	0.045	-0.014	0.752	1.630	0.004
	(0.042)	(0.038)	(1.045)	(1.138)	(0.036)
Economic (E)	0.179***	-0.168***	0.934	-0.607	0.180***
	(0.062)	(0.055)	(1.599)	(1.726)	(0.050)
Observations	3331	3266	3240	3222	3314
R-squared	0.201	0.168	0.102	0.109	0.172
Mun. and Enum. FE	Yes	Yes	Yes	Yes	Yes
FDR q-value		0.001	0.001	0.009	0.001
Mean dep. var	0.000	2.314	72.279	65.819	2.814

This table shows the decomposition of the effect across treatments on the variables measuring corruption. *Corruption index* is created by normalizing the variables in columns (2) to (5) measuring corruption, individually subtracting their mean and dividing by their standard deviation, adding them up, and once again normalizing the result by subtracting the mean and dividing by the standard deviation. *Progress Fighting Corruption (1-4)* is calculated through the answers of the question "How much progress do you think has been made in reducing corruption in State institutions in the last 2 years?" in the survey. *Share of Corrupt Politicians (0-100)* is measured from the answers to the survey question "What percentage of politicians in Mexico do you think are involved in acts of corruption?". *Share of Taxes Stolen by Politicians (0-100)* is calculated from the responses of the survey question "Of every $100 pesos in taxes collected by the government, what percentage do you think politicians steal?". *All Politicians Are Corrupt (1–4)* is calculated through the answers of the question "Do you strongly agree, agree, do not agree or strongly do not agree with the statement 'All politicians are corrupt'" in the survey. Same individual level controls. Robust standard errors are shown in parentheses.
Backreferenced (on pages): [20,OA-33]

Table OA-11: Decomposing Effects on Democratic Attitudes

	(1) Democratic Index	(2) Democratic Satisfaction	(3) Democracy best government	(4) Essential to live in a democracy
Corruption	-0.160***	-0.044**	-0.042**	-0.037**
	(0.041)	(0.018)	(0.018)	(0.018)
Nation Building (NB)	0.069*	0.001	-0.013	0.035**
	(0.041)	(0.019)	(0.019)	(0.018)
Economic (E)	-0.281***	-0.094***	-0.037	-0.092***
	(0.063)	(0.027)	(0.029)	(0.029)
Observations	3331	3307	3231	3300
R-squared	0.186	0.182	0.088	0.121
Mun. and Enum. FE	Yes	Yes	Yes	Yes
FDR q-value		0.035	0.035	0.035
Mean dep. var	0.001	0.278	0.782	0.788

Notes: This table shows the decomposition of the effect across treatments on the variables measuring democratic attitudes. *Democratic index* is created by normalizing the variables in columns (2) to (4) measuring democratic attitudes, individually subtracting their mean and dividing by their standard deviation, adding them up, and once again normalizing the result by subtracting the mean and dividing by the standard deviation. *Democratic Satisfaction* is calculated through the answers of the question "In general, would you say you are very satisfied, somewhat satisfied, a little satisfied or not satisfied with the democracy in Mexico?" in the survey. *Democracy best government* is measured from the answers to the survey question "Please, state if you strongly agree, agree, disagree or strongly disagree with the following statement: "Democracy may have its problems, but it is still the best form of government". *Essential to live in a democracy* is calculated from the responses of the survey question "Do you consider it essential to live in a country where governors are elected by the majority's vote?" Same individual level controls. Robust standard errors are shown in parentheses.
Backreferenced (on pages): [21,OA-33]

Table OA-12: Decomposing Effect of Trust on Formal Political Institutions and Offices

	(1) Institution Index	(2) Trust in President	(3) Trust in Parties	(4) Trust in Media	(5) Trust in Congress
Corruption	-0.192***	-0.056***	-0.024*	-0.048**	-0.058***
	(0.040)	(0.019)	(0.014)	(0.019)	(0.018)
Nation Building (NB)	0.001	0.002	0.003	-0.005	-0.020
	(0.043)	(0.021)	(0.015)	(0.021)	(0.019)
Economic (E)	-0.252***	-0.102***	-0.028	-0.024	-0.085***
	(0.060)	(0.031)	(0.020)	(0.031)	(0.025)
Observations	3331	3327	3326	3326	3321
R-squared	0.209	0.257	0.083	0.063	0.100
Mun. and Enum. FE	Yes	Yes	Yes	Yes	Yes
FDR q-value		0.005	0.022	0.010	0.005
Mean dep. var	-0.008	0.565	0.122	0.311	0.249

Notes: This table shows the decomposition of the effect across treatments on the variables measuring trust in political institutions. *Institutions index* is created by normalizing the variables in columns (2) to (5) measuring attitudes towards political institutions, individually subtracting their mean and dividing by their standard deviation, adding them up, and once again normalizing the result by subtracting the mean and dividing by the standard deviation. *Trust in President, Parties, Media and Congress* variables are calculated from the answers of the following question in the survey "Please tell me, for each of the groups, institutions or people mentioned in the list, how much trust do you have in them: a lot, some, little or no trust in: the president, congressman, media, and political parties." where each variable takes a value of 1 if the answer is a lot or some trust, and 0 otherwise. Same individual level controls. Robust standard errors are shown in parentheses. The significance is reported at levels *** p < 0.01, ** p < 0.05 and * p < 0.1
Backreferenced (on pages): [21,OA-33]

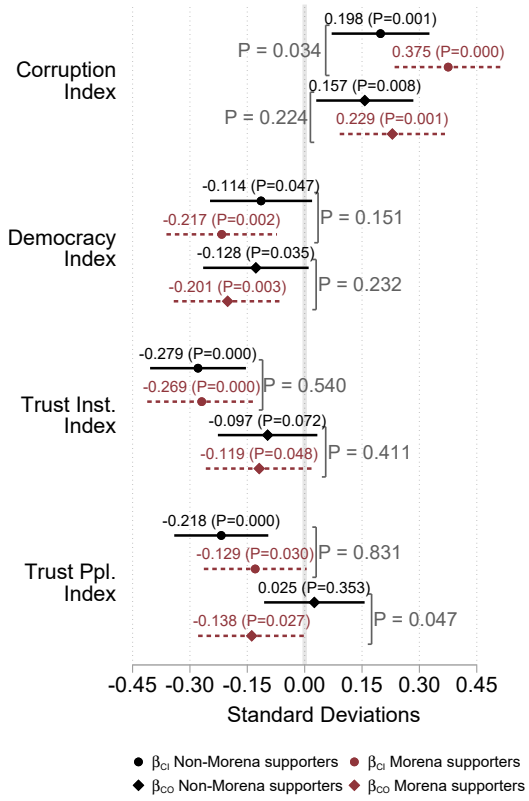
Table OA-13: Decomposing Effect on Trust in People

	(1) Trust in Others Index	(2) General Trust	(3) Trust in Neighbors	(4) Trust in Non-Neighbor Mexican
Corruption	-0.116***	-0.018	-0.041*	-0.043**
	(0.040)	(0.016)	(0.021)	(0.019)
Nation Building (NB)	-0.011	-0.000	0.001	0.012
	(0.043)	(0.017)	(0.022)	(0.021)
Economic (E)	-0.051	-0.036	0.057*	-0.038
	(0.061)	(0.024)	(0.033)	(0.030)
Observations	3331	3314	3329	3317
R-squared	0.169	0.105	0.100	0.122
Mun. and Enum. FE	Yes	Yes	Yes	Yes
FDR q-value		0.090	0.072	0.072
Mean dep. var	-0.003	0.175	0.550	0.332

Notes: This table shows the decomposition of the effect across treatments on the variables measuring trust in people *Trust in Others Index* is created by normalizing the variables in columns (2) to (4) measuring trust in other individuals, individually subtracting their mean and dividing by their standard deviation, adding them up, and once again normalizing the result by subtracting the mean and dividing by the standard deviation. *General trust* is calculated from the answers of the following question in the survey "Generally speaking, would you say that most people can be trusted or that one is never careful enough when dealing with others?". *Trust in Neighbors, Members of other community* are based on the responses of the survey question "Please tell me, for each of the groups, institutions or people mentioned in the list, how much trust do you have in them: a lot, some, little or no trust in: your neighbors, and a non-local Mexican.", each variable takes a value of 1 if the answer is a lot, or some trust. Same individual level controls. Robust standard errors are shown in parentheses. The significance is reported at levels *** p < 0.01, ** p < 0.05 and * p < 0.1
Backreferenced (on pages): [21,OA-33]

B.3 RCT: Heterogeneous Corruption Treatment Effects

Figure OA-6: By ex-ante support for the incumbent or opposition



Morena supporter is a dummy variable equal to 1 if the respondent's support for the political party is above the median. In particular, we ask *We would like to know your attitude towards certain groups. I will read out the name of some groups and ask you to evaluate each group from 0 to 10 according to the following criterion: An evaluation of 0 means that you totally reject that person or institution. An evaluation of 5 means that you are indifferent, and an evaluation of 10 means that you completely support that person or institution.* We consider an individual to be an ex-ante Morena supporter if they evaluate Morena above 5 on this scale. We show point estimates and one-sided p-values on top of the corresponding confidence interval. Grey brackets show one-sided p-values for the hypothesis that treatment effects are larger among non-morena supporters.
Backreferenced (on pages): [21,OA-34]

B.4 Other survey outcomes

We have presented a range of evidence suggesting that exposure to apex corruption reduces the legitimacy of democracy and erodes the organizations, beliefs, and internalized norms that support it. We have also shown that not all corruption revelations have the same effects. Revealing Morena-related corruption has stronger effects across several outcomes. We interpret this pattern as consistent with a learning mechanism. Citizens already believed that opposition parties were corrupt while perceiving Morena as relatively clean. The Morena apex corruption video provides new information that contradicts this prior, whereas the opposition corruption video largely reinforces existing beliefs and therefore has a more muted effect. Alternatively, or in addition, the stronger effects of the Morena video may reflect its apex nature, given that it directly implicates a close family member of the president. While we cannot empirically distinguish between these two mechanisms, both interpretations are consistent with the claim that apex corruption erodes support for democracy.

B.5 Do the Effects Accumulate? An Experiment with Two Brothers

While planning fieldwork for the follow-up survey, a second brother of the incumbent president was caught taking bribes on film. This allowed us to create an additional 3-minute corruption video we refer to as the *Two* Brothers Corruption treatment (henceforth CI_2). This video features sequential footage of bribes being exchanged not just by one but by two siblings of the president.[51] Therefore, after measuring the persistence of the effects from the previous videos, we randomized half of the follow-up (Oaxaca city) sample to receive the *Two* Brothers corruption video, stratifying by the treatment arms of the first experiment.[52]

We can measure two types of accumulation. First, by comparing half of the control group to the half that received the new CI_2 video, we can estimate the effect of the two-brother video. We can then benchmark the size of this effect against that of the *one*-brother CI video we estimated above, and ask whether a *two-in-one* treatment showing evidence for corrupt acts involving two brothers has a larger effect than a similar video showing evidence for just one.

Further, we can also examine if there is an accumulating *two separate doses* effect: i.e., whether people who receive two doses of evidence of apex corruption—one in the first round of the experiment and a second in this round—-suffer larger decreases in support for democracy than those who just got one, i.e., we can test if we can reject the hypothesis $\beta_{CI_2} \geq \beta_C + \beta_{CI_2}$. To do this, we estimate a regression analogous to equation 1 but interacting the arms of the first experiment with the *Two* Brothers Corruption video CI_2 as follows:

$$Y_{i,2} = \alpha + \delta CI_{2,i} + T_i' \beta + (T_i \cdot CI_{2,i})' \gamma + \epsilon_i \tag{3}$$

where $CI_{2,i}$ is an indicator for the *Two* Brothers Corruption video, with δ estimating *"replication/accumulation"*; T_i being a vector containing the indicators for the previous treatment arms. Coefficients $\beta = (\beta_{CI}, \beta_{CO})$ estimate *persistence*; the interaction terms $(T_i \cdot CI_{2,i})$ allow for the estimation of another measure of *accumulation* in $\gamma = (\gamma_{CI}, \gamma_{CO})$ plus β.

Because comparison groups are half the size as before, this experiment has less power and the findings are more tentative (see our follow-up trial registration, *AEARCTR-0008170*). Nonetheless, the effects are still informative. As Figure OA-7 reveals, the *two-in-one* treatment increases corruption perceptions by 0.28σ and lead to corresponding declines in the democracy index of -0.23σ. This decline in immediate expressed support for democracy is about 40 percent larger than the -0.159σ we previously found for the one-brother treatment (see Figure 3(b), p-value of a larger effect: 0.19).

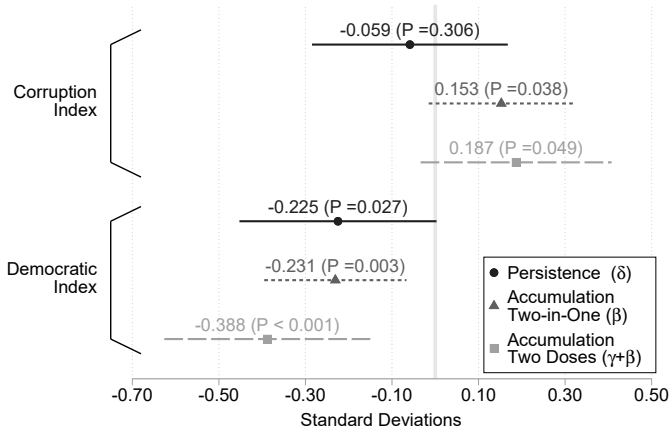
The effect of two doses is even more marked. As Figure OA-7 shows, the effect of the *two dose* treatment $(\beta_C + \beta_{CI_2})$ is -0.39σ. This magnitude is 65% larger than the effect of just the *two-in-one* video (CI_2) and 137% larger than the original corruption incumbent (CI) videos. Further, we can reject $\mid \beta_C + \beta_{CI_2} \mid \leq \mid \beta_{CI_2} \mid$, with p-value=0.03.

Taken together, these results suggest apex corruption scandals can have persistent effects in undermining support for democracy. Furthermore, these effects can even accumulate, re-triggering latent attitudes as repeated episodes come to light.

[51] See: https://www.youtube.com/watch?v=q6nheyGHc3g

[52] Table OA-3 shows the distribution of observations across treatment arms. Tables OA-2 verifies there is no differential attrition in response rates, while Table OA-2 Panel D shows the Two-Brother treatment achieved balance.

Figure OA-7: Testing for Accumulation



The figure shows the pooled estimates for the apex corruption treatments, from equation 1, along with their 95 percent confidence intervals on Follow-up outcomes. We show point estimates and their corresponding one-sided p-value on top its corresponding confidence interval. These estimates are for respondents who were in either of the corruption video treatments in the baseline but were not exposed to the second corruption video.
Backreferenced (on pages): [22,OA-14,OA-14,OA-36,OA-36]

B.6 Treatment Effects of Common Financial Exposure

Testing a Second Potential Remedy: The Effects of Common Financial Exposures As described above, instead of priming common identity, as in the nation-building treatment, after collecting the immediate survey outcomes, we provided exposure to a subset to the broad Mexican index fund, and thus a means to share in and learn about the gains of the common good—and common risks— of an important component of the national economy (e.g Jha and Shayo, 2019; Jha, 2025; Jha et al., 2025).[53] Treated citizens received 200 pesos (about $ 10) of financial assets tracking the broad S&P BMV IPC Mexican stock index, receiving text messages allowing them to trade up to 25 pesos each week for three-four weeks. Take-up was relatively low at 28%, yet importantly, as we follow both the compliers and non-compliers, we are able to estimate the treatment effect on the treated (TOT), and the results are nonetheless encouraging.

Table OA-14 Panel A shows the treatment effect on the treated of receiving common financial exposure on values three months after divestment. While there is no effect on corruption perceptions, as one might expect, nonetheless, the treatment effect of stock exposure on the treated shows a durable effect on support for democracy by 0.8σ (p-value of an increase 0.04), trust in institutions by 0.43σ (p-value of an increase 0.16), trust in other citizens by 0.58σ (p-value of an increase 0.04). Those exposed a week before the elections had no chance to begin trading, and the treatment does not affect their voting decisions. However, the effect on those treated a month before the elections is to raise voter turnout by 12.6pp, though this is not precisely estimated.[54] We see these results as promising but naturally suggestive at best. More (and more highly powered) research clearly needs to be done, yet these findings provide some hope that treatments that can be designed to incorporate *positive* social spillovers, including shared financial exposures, may be able to counteract some of the negative spillovers of apex corruption on democratic values and societal trust.

[53] This intervention differs from these in a number of important ways. First, we assigned the intervention after the corruption exposure to see if it could have offsetting effects. Second, we employed door-to-door sampling of citizens rather than employing an internet panel. Further, we study a much poorer developing country context with much weaker infrastructure, one where we use text messages to provide opportunities to trade rather than an internet platform (Israeli GDP per capita in 2021 was *five times* that of Mexico). Further, unlike in the Israel study, where individuals who missed an opportunity to record their weekly decision simply lost the 10% of their portfolio, the incentives were much more stark and binary: respondents had to register a decision each of four possible opportunities in order to receive the value of their portfolio at the end.

[54] Panel B reports the intent to treat results, which show consistent patterns, but not surprisingly are weaker given the relatively small shares that actually took up the treatment. Nonetheless, even the intent to treat raises the democracy index by 0.116σ (p-value of an increase 0.099).

Table OA-14: Treatment Effects of Common Financial Exposure

	(1) Corruption Index Follow Up	(2) Democratic Index Follow Up	(3) Trust Inst. Index Follow Up	(4) Trust Ppl. Index Follow Up	(5) Actual Vote	(6) Actual Vote	(7) Actual Vote
Panel A: Treatment on the Treated							
Stocks	0.168	0.804*	0.433	0.583*	0.065	-0.063	0.081
	(0.399)	(0.479)	(0.448)	(0.351)	(0.190)	(0.410)	(0.223)
Stocks × Far from Election						0.126	
						(0.478)	
Observations	1066	1063	1059	1062	1154	1154	890
R2	0.120	0.088	0.099	0.129	0.105	0.110	0.127
Outcome control mean	0.047	-0.111	0.006	-0.155	0.570	0.570	0.558
H0: Stocks offsets (p-value)	0.663	0.047	0.167	0.048	0.367	0.562	
H0: Stocks × Far offsets (p-value)						0.396	0.358
F stat - Stocks	234.995	237.257	232.318	235.115	297.835	189.668	
F stat - Stocks × Far						280.873	278.293
Panel B: Intent-to-Treat							
Stocks	0.084	0.116	0.040	0.110	0.031	-0.014	0.042
	(0.088)	(0.090)	(0.090)	(0.087)	(0.041)	(0.089)	(0.046)
Stocks × Far from Election						0.050	
						(0.100)	
Observations	1066	1063	1059	1062	1154	1154	890
R2	0.153	0.141	0.131	0.162	0.127	0.136	0.151
Outcome control mean	0.047	-0.111	0.006	-0.155	0.570	0.570	0.558
H0: Stocks offsets (p-value)	0.830	0.099	0.330	0.102	0.227	0.565	
H0: Stocks × Far offsets (p-value)						0.309	0.183
Sample	All	All	All	All	All	All	Far

This table shows treatment effect estimates of randomly exposing respondents to a common financial asset on pre-registered follow-up indices (Columns 1–4) and on administrative voter turnout (Columns 5–7), in the Urban sample. Panel A instruments actual portfolio activation with the random offer to trade assets interacted with the four video arms. Panel B shows the reduced form estimates of the effect of the randomized exposure to financial assets. Far from Election is an indicator for whether the respondent was interviewed a month before the election as posed to one week before it. Controls include the following pre-treatment measures. *Demography*: Gender, Age, Schooling, Employment; *Democratic and Economic beliefs and attitudes*: Current Economic Satisfaction, Perception of Democracy in Mexico, Initial corruption perception, Support for Incumbent and Opposition parties, and Socioeconomic status. Robust standard errors are shown in parentheses. We report p values from a one-sided test for the null hypothesis that the treatment effects offset the corruption effects (i.e., act in the opposite direction). We do not have always-takers because those who are not given the assets cannot trade. We also do not have defiers for the same reason. In this scenario, the set of compliers is the same as the set of treated individuals and LATE=TOT.

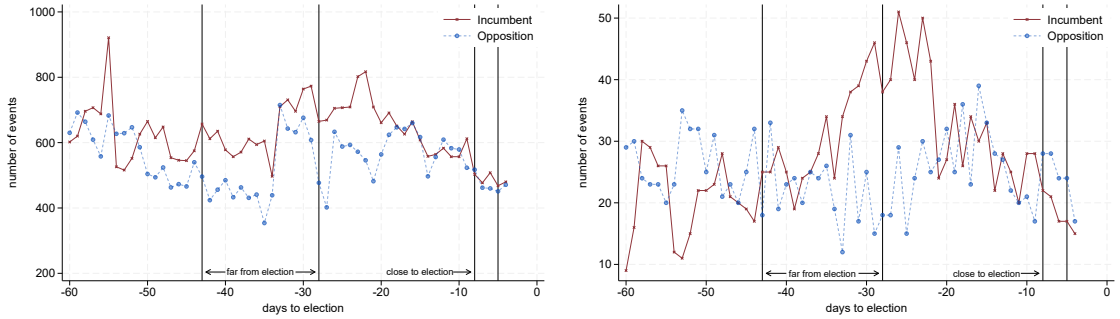
Backreferenced (on pages): [OA-15,OA-35]

OA - C Campaigning

We obtained publicly available data from the Proceso Electoral Concurrente for 2020–2021. The data contain information related to campaigning events during the electoral cycle. A campaign event can include, e.g., exposition of proposals to citizens, canvassing, delivery of banners, among others. We filter out events that were scheduled but that ended up not taking place. We restrict the time window of the analysis from 60 days before the election to the day of the election. For each event, we know the State where it happened, the party responsible for the event, start and end date of the event, whether the event took place or not, and a description of the event.

Figure OA-8: Campaigning Events

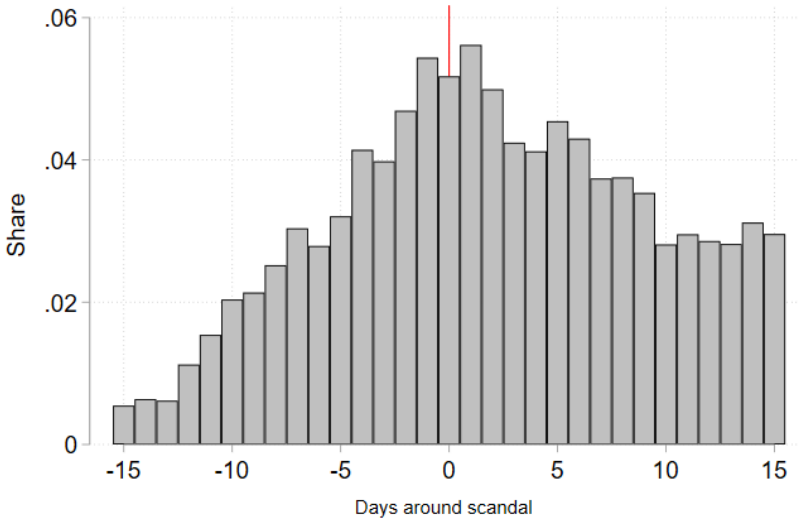
(a) Country **(b)** Oaxaca



This figure shows the number of campaigning events made by party in the two months leading to the election. Incumbent refers to MORENA, while opposition refers to PAN and PRI. We use data from Mexico's National Electoral Institution's audit reports for the Proceso Electoral Concurrente 2020-2021. Events include, e.g., exposition of proposals to citizens, canvassing, delivery of banners, discussion of proposals, among others. The vertical black lines delimit the period of respondents who were contacted more than one week before the election (Far from election), and the period of respondents who were contacted less than week before the election (close to election).
Backreferenced (on pages): [14]

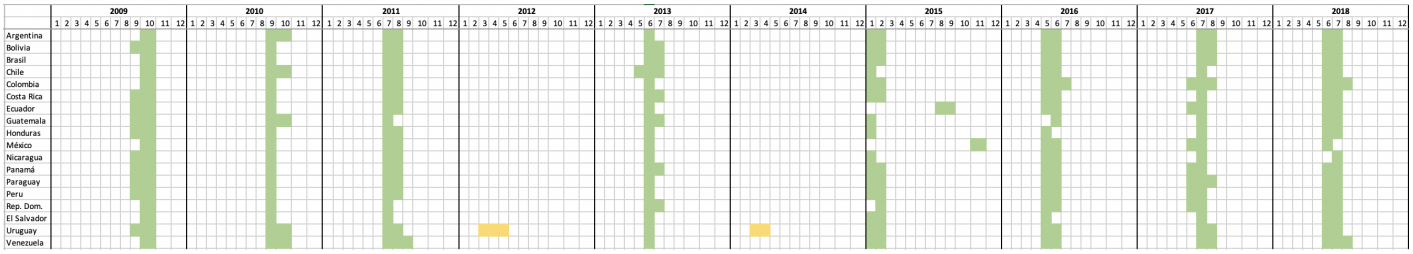
OA - D Apex Corruption in Latin America: Diagnostics

Figure OA-9: Distribution of interviews relative to timing of apex corruption scandals



This figure plots the distribution of interviews per day relative to the respective corruption scandal.
Backreferenced (on pages): [27]

Figure OA-10: Timeline for Latinobarometer surveys



Months of Latinobarometer field work by country and year.
Backreferenced (on pages): [25]

Table OA-15: Predicting Apex Corruption Scandals

	(1) scandal	(2) scandal	(3) scandal	(4) scandal
Mean dep. var	0.003	0.003	0.003	0.003
Accuracy (Out of sample)	0.002	0.002	0.002	0.002
AUC (Out of sample)	0.746	0.759	0.736	0.734
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Day of week	Yes	Yes	Yes	Yes
Month	No	Yes	Yes	Yes
Day of the Month	No	No	Yes	Yes
Close to Election	No	No	No	Yes

This table shows 4 different regressions trying to predict when corruption events happen. We take a random sample of 50% of all country-year observations (where 170 are true scandals) and estimate logit models using this sample. We then impute a prediction of 1 whenever the estimated probability is larger than the median probability of a country-year being a true scandal and a prediction of 0 otherwise. We use all 170 corruption events since we are not limited to those just falling within Latinobarometer. The dependent variable is $1(\text{Corruption})_{ictd}$, which =1 for the exact day in which the corruption scandal is revealed. Explanatory variables are different sets of indicators as described in the table and the text. Accuracy is measured as an indicator equal to 1 whenever the prediction equals the true value for the dependent variable. For column (1) the explanatory variables are country fixed effects, year fixed effects and day of week fixed effects, in column (2) we add month fixed effects, in column (3) we add day of the month fixed effects and in column (4) we add closeness to election dummies, that is, a dummy for whether the scandal occurred 1 month before the election, a dummy for whether the scandal occurred between 1 and 2 months before the election, a dummy for whether the scandal occurred between 2 and 3 months before the election and a dummy for whether the scandal occurred between 3 and 4 months prior the election. The omitted dummy is for scandals occurring more than 4 months away from the election. AUC is the Area Under the Curve measure useful for evaluating whether the model is good at classifying the occurrence of scandals. It is calculated by obtaining the area under the ROC curve, which plots the True Positive Rate on the vertical axis and the False Positive Rate on the horizontal axis.
Backreferenced (on pages): [26,28]

Table OA-16: Corruption Scandals: Covariate Balance Among Respondents

	(1) β	(2) Cluster 1 SE	(3) Cluster 2 SE	(4) Observations	(5) Mean dep. var
Male	0.022	(0.014)	[0.010]**	27080	0.482
Age in years	0.523	(0.572)	[0.442]	27080	40.239
Married	0.014	(0.012)	[0.009]	27016	0.548
Middle-school completed	0.021	(0.024)	[0.019]	27080	0.450
Employed	0.030	(0.011)***	[0.010]***	27080	0.574
Middle-High SES	-0.009	(0.012)	[0.011]	27080	0.886
Has sewer system	-0.029	(0.024)	[0.021]	26850	0.615
House owner	0.019	(0.014)	[0.010]*	26934	0.763
Car owner	-0.007	(0.016)	[0.012]	26873	0.306
Num. tries at interview	-0.071	(0.128)	[0.149]	27080	2.261

This table shows the balance in covariates for people interviewed by Latinobarometer before versus after the corruption scandals. The sample includes respondents interviewed within 15 days before or after the corruption scandal event. All variables except of age in years and number of tries to get an interview are indicators. All are self explanatory. An observation is an interview. We regress $1(\text{Post Corruption})_{ictd}$, the main explanatory variable in equation 2 separately on each of the above respondent characteristics. We control for country × year, month, and day-of-week indicators. Robust standard errors in parentheses are clustered by country × year in parentheses, while robust standard errors in brackets are clustered by country × year × days from scandal grouped in blocks of three days. The significance is reported at levels *** p < 0.01, ** p < 0.05 and * p < 0.1
Backreferenced (on pages): [26,28]

Table OA-17: Effects of Apex Corruption Scandals on Trust in Distinct Political Institutions

	(1) Institutions index	(2) Trust in fed. gov.	(3) Trust in pol. parties	(4) Trust in electoral inst.	(5) Trust in fed. congress
Corruption	-0.062	-0.031	-0.014	-0.030	-0.022
	(0.031)*	(0.014)**	(0.008)*	(0.015)*	(0.010)**
	[0.031]**	[0.012]**	[0.009]	[0.013]**	[0.010]**
Observations	27016	26597	26444	19938	26165
Scandals	26	26	26	20	26
R-squared	0.033	0.084	0.034	0.096	0.036
Country x Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Mean dep. var	0.000	0.315	0.172	0.396	0.236

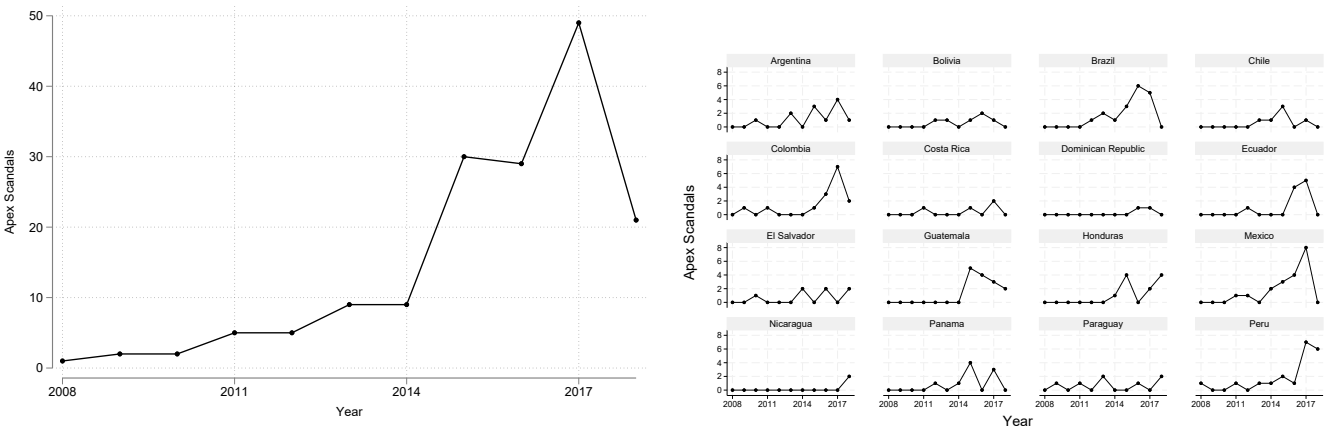
An observation is an interview. We regress each outcome on 1(Post Corruption)$_{ictd}$, the main explanatory variable in equation 2. We control for country × year, month, and day-of-week indicators. Robust standard errors in parentheses are clustered by country × year in parenthesis, while robust standard errors in brackets are clustered by country × year × days from scandal grouped in blocks of three days. For columns (2) to (5) the variables measure trust in the federal government, political parties, electoral institute and federal congress from the survey responses where respondents are asked to rate their level of trust on each of those groups from 1 to 4, where 1 indicates the most trust, each trust variable takes the value 1 if the response from the survey was 1 or 2, and the value 0 otherwise. *Institutions index* was defined in the text. Individual controls are included.
Backreferenced (on pages): [27]

Table OA-18: Effects of Apex Corruption Scandals on Economic Satisfaction Measures

	(1) Satisfaction with economic system	(2) Current macro situation (1-5)	(3) Support for a market economy
Corruption	-0.010	-0.017	-0.004
	(0.019)	(0.031)	(0.012)
	[0.016]	[0.025]	[0.013]
Observations	26095	26827	19988
Scandals	26	26	23
R-squared	0.046	0.127	0.027
Country x Year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Mean dep. var	0.227	2.761	0.747

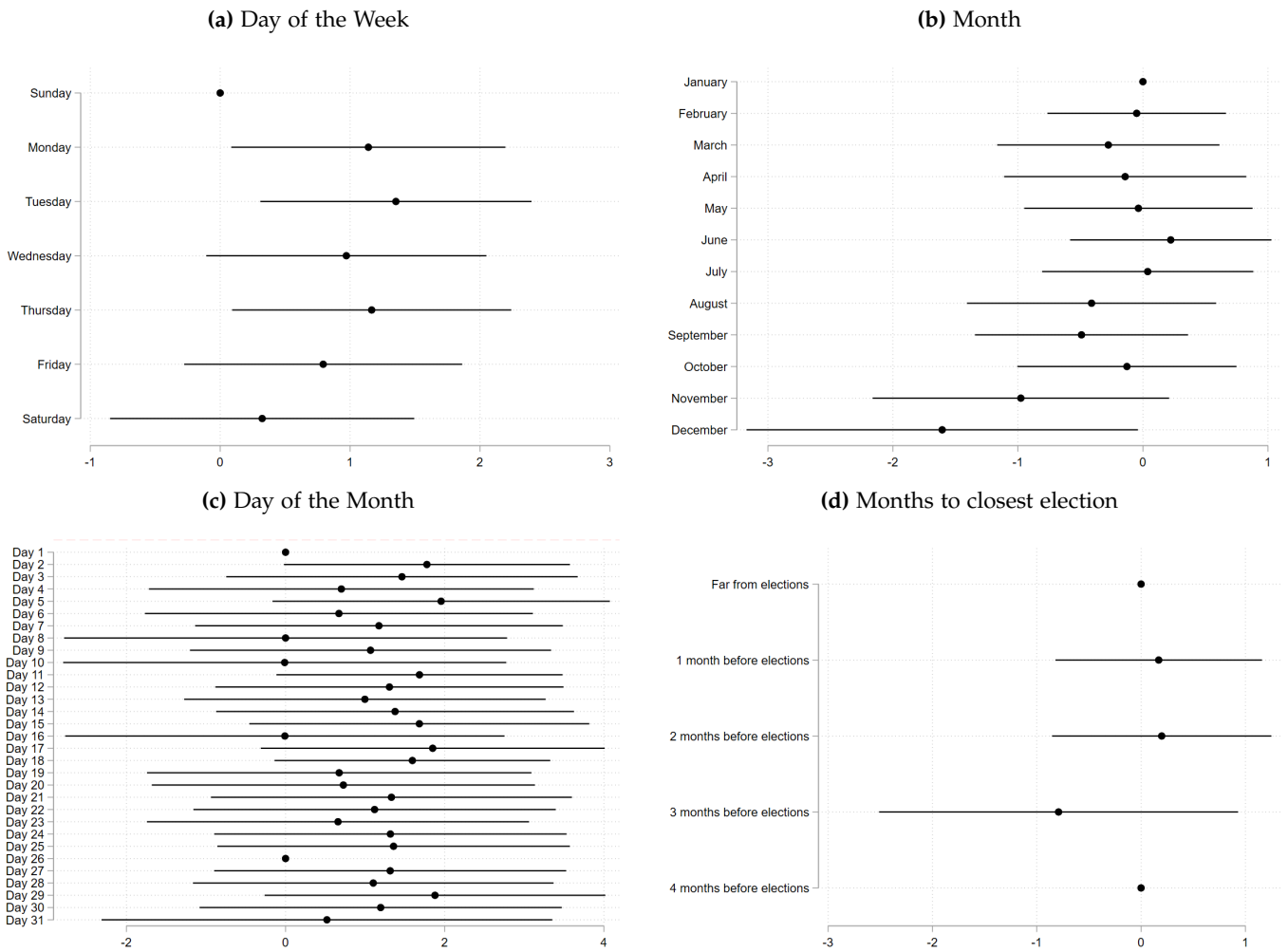
This table shows the effect of corruption scandals on non-political measures relating to satisfaction with the economy. Robust standard errors in parentheses are clustered by country × year, while robust standard errors in brackets are clustered by country × year × relative distance to corruption scandal grouped in blocks of three days. Individual Controls: Gender, Age, Schooling, Employment, Civil Status, Size of town, Socioeconomic Status, Month and Day. The significance is reported at levels *** $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$
Backreferenced (on pages): [29,OA-35]

Figure OA-11: Apex scandals frequency



This figure shows the number of apex corruption scandals by year in Latin American countries.
Backreferenced (on pages): [24]

Figure OA-12: Predicting Apex Corruption Scandals: coefficient plots



This figure plots coefficients from a single logit regression where the dependent variable is the occurrence of a corruption scandal $1[C]_{cdmty}$ in a on particular day-of-week d, on a particular month m, day-of-month t, year y and country c. Regressors include day-of-week, month, day-of-month, months from election. We plot the coefficients estimated on these indicators along with their 95 percent confidence intervals. All regressions control also for year and country dummies. The omitted category is shown as a dot without a confidence interval.
Backreferenced (on pages): [28]

Table OA-19: Scraped News Channels' Twitter Accounts

Country	Institution	Account	Followers	Country	Institution	Account	Followers
Argentina	Clarín	@clarincom	3.1 M	Guatemala	Prensa Libre	@prensa_libre	1.3 M
Argentina	La Nación	@LANACION	3.4 M	Guatemala	Diario La Hora	@lahoragt	328 mil
Argentina	Infobae	@infobae	2.8 M	Guatemala	Telediario Guatemala	@TelediarioGT	1 M
Brasil	O Globo	@JornalOGlobo	6.4 M	Guatemala	Nuestro Diario	@NuestroDiario	863 mil
Brasil	Jornal do Brasil	@JornaldoBrasil	617,7 mil	Nicaragua	LA PRENSA Nicaragua	@laprensa	356 mil
Brasil	Folha de S.Paulo	@folha	7.5 M	Nicaragua	Canal 10 - Nicaragua	@canal10nica	211 mil
Brasil	UOL Notícias	@UOLNoticias	3.4 M	Nicaragua	El Nuevo Diario	@elnuevodiario	220 mil
Bolivia	La Razón	@LaRazon_Bolivia	565 mil	México	Noticieros Televisa	@NTelevisa_com	3,1 M
Bolivia	EL DEBER	@grupoeldeber	641 mil	México	Reforma	@Reforma	3,1 M
Bolivia	Los Tiempos	@LosTiemposBol	352 mil	México	El Universal	@El_Universal_Mx	5.7 M
Chile	BioBioChile	@biobio	3.2 M	México	López Doriga	@lopezdoriga	7,6 M
Chile	TVN	@TVN	3.1 M	Paraguay	ABC Digital	@ABCDigital	999 mil
Chile	T13	@T13	3.2 M	Paraguay	Última Hora	@UltimaHoracom	1 M
Chile	La Tercera	@latercera	1.9 M	Paraguay	Diario La Nación	@LaNacionPy	319 mil
Costa Rica	La Nación	@nacion	612 mil	Panamá	La Prensa Panamá	@prensacom	538 mil
Costa Rica	Teletica	@teletica7	424 mil	Panamá	TVN Noticias	@tvnnoticias	846 mil
Costa Rica	La República	@La_Republica	166 mil	Panamá	Mi diario Panamá	@MiDiarioPanama	311 mil
Colombia	El Espectador	@elespectador	5.3 M	Panamá	La Estrella | Panamá	@EstrellaOnline	254 mil
Colombia	El Tiempo	@ELTIEMPO	7.1 M	Perú	El Comercio	@elcomercio_peru	464 mil
Colombia	Noticias RCN	@NoticiasRCN	7.9 M	Perú	Agencia Andina	@Agencia_Andina	616 mil
Colombia	Noticias Caracol	@NoticiasCaracol	8.8 M	Perú	Diario La República	@larepublica_pe	2.2 M
Ecuador	El Tiempo	@eltiempocuenca	219 mil	Perú	Diario Correo	@diariocorreo	1 M
Ecuador	La Hora Ecuador	@lahoraecuador	575 mil	Perú	Panamericanatv	@PanamericanaTV	1 M
Ecuador	El Comercio	@elcomerciocom	1.7 M	Uruguay	El País	@elpaisuy	720 mil
Ecuador	TC Televisión	@tctelevision	1.3 M	Uruguay	Subrayado	@Subrayado	727 mil
Ecuador	Teleamazonas	@teleamazonasec	2.1 M	Uruguay	La República	@larepublica_uy	110 mil
El Salvador	El Salvador.com	@elsalvador.com	778 mil	Uruguay	El Observador	@ObservadorUY	720 mil
El Salvador	Teledos	@teledos_tcs	650 mil	Rep. Dom.	Periódico Hoy	@PeriodicoHoy	698 mil
El Salvador	Diario El Mundo	@ElMundoSV	362 mil	Rep. Dom.	Diario Libre	@DiarioLibre	1.2 M
El Salvador	Noticias 4Visión	@noticias4vision	780 mil	Rep. Dom.	LISTINDIARIO	@ListinDiario	1 M
Honduras	Diario La Prensa	@DiarioLaPrensa	479 mil	Rep. Dom.	Noticias SIN	@SIN24Horas	1.2 M
Honduras	Diario El Heraldo	@diarioelheraldo	371 mil				
Honduras	Diario Tiempo	@DiarioTiempo	246 mil				
Honduras	Diario La Tribuna	@LaTribunahn	263 mil				

This table includes the Twitter accounts for the main news channels in Latin America that were used for scraping in Twitter, including country, institution, account, and follower count in 2022.
Backreferenced (on pages): [24]

Table OA-20: Apex Corruption Scandals that intersect with Latinobarometer Survey Fieldwork

Country	Date	Type	Corruption Scandal
Argentina	14/06/2016	Indictment	Nine million dollars and a monastery: Argentina's latest corruption scandal
	23/07/2017	Media Report	Governor of Santa Cruz was filmed receiving "a bribe of 2 million pesos"
Bolivia	11/06/2013	Indictment	Eight YPFB high ranked directors arrested for corruption
	13/05/2016	Media Report	Minister Juan Ramon Quintana denounced of influence peddling
	31/07/2017	Indictment	The National Director Jorge Bohorquez, arrested for acts of corruption and extortion
Brazil	17/06/2013	Media Report	Information leak of corruption in public works for the World Cup
	24/05/2016	Media Report	Minister Juca audio leak in which he said that would obstruct investigations
	12/07/2017	Sentenced	Lula Da Silva is arrested due to corruption accusations related with Lava Jato
Chile	11/05/2013	Media Report	Congresswoman supported a Fishing Law in exchange of a bribe
	22/01/2015	Media Report	SQM Case: Hugo Bravo is prosecuted for bribery, and money laundering.
Colombia	08/10/2009	Media Report	Former Minister Felipe Arias involved in scandal of Agro Ingreso Seguro subsidies
	14/07/2011	Indictment	Corruption network is discovered in the DIAN for millionaire fraud in the VAT refund
	05/02/2015	Media Report	WSJ reveals video showing bribes from senior PetroTiger executives to former Ecopetrol official
	21/06/2018	Formal Acc.	The Attorney General revealed investigation against congressmen for electoral crimes
Costa Rica	06/07/2017	Media Report	Prosecutor, Congressmen and President involved in a scandal for influence peddling in the import of Chinese cement
Ecuador	16/05/2016	Indictment	Former Petroecuador director is arrested for influence peddling.
Guatemala	26/05/2016	Formal Acc. + Indictment	CICIG discover a monumental blow to corruption in Congress for illegal hiring
	15/07/2017	Formal Acc. + Indictment	Micivi Corruption: Companies that paid bribes have received 6.7 billion from the Federal Government
Nicaragua	05/07/2018	Formal Acc.	The president's father-in-law and 2 officials are accused of corruption by the US Dep. of Treasury
Panama	28/01/2015	Formal Acc.	Cost overruns of more than 45 million dollars were detected in the National Aid Program. Investigations against Martinelli begin
Paraguay	29/09/2009	Indictment	Federal Prosecutor of Coronel Oviedo is charged with bribery request
	09/08/2011	Indictment	Federal Prosecutor of Ciudad del Este is arrested for alleged bribery
	23/05/2016	Media Report	Son of a Supreme Court judge receives bribes in name of his father
	05/07/2018	Indictment	Official is recorded asking for a bribe in the name of a Supreme Court judge
Peru	15/05/2016	Media Report	DEA investigates opposition leader Keiko Fujimori for alleged money laundering
	07/07/2018	Media Report	Audio leak reveals offers of reduced sentences, requests for favors, or negotiations for promotions in the Council of Magistracy

Notes: This table includes an overview of the political corruption scandals that happened in Latin America during times when Latinobarometer surveys were being conducted. The "Type" column refers to how the scandal broke. "Indictment" denotes that the news reported arrests related to the figures implicated in the scandal, "Media report" denotes that the scandal broke out by journalist reports, "Formal Accusation" denotes that the news reported on an accusation made by judiciary/audit officials, and "Sentenced" denotes that the news reported on a sentence made by a court. We exclude two scandals from the list since all respondents in that country in that year were either interviewed before the scandal, or all of them were interviewed after the scandal. Because of this we cannot leverage these respondents' answers to identify causal effects with our empirical strategy. In particular we exclude a scandal in Colombia on June 8, 2017 (Media Report - Audios prove that the Spanish company Inassa bribed Colombian politicians) and a scandal in Paraguay on June 1, 2013 (Indictment - Seam's administrative director arrested for bribery)

Backreferenced (on pages): [24]

OA - E Latinobarometro - Staggered Difference-in-Differences

In this section we show the results of apex corruption scandals on our main outcomes using a difference-in-differences approach. Our main result exploits the randomness in the scandal occurrences within a fielding of the Latinobarometro survey. Here, we include the survey waves in which no scandal occurs as a never-treated comparison group. We use the estimators proposed by Borusyak et al. (2024); Sun and Abraham (2021). These estimators relies on different assumptions, each of which we discuss here in the context of our setting.

Borusyak et al. (2024): This estimator relies on model based assumptions and an imputation. In particular, it uses the never-treated and the pre-scandal observations to estimate the potential outcomes under no treatment as a combination of country-year fixed effects and covariates. It then imputes the treatment effects as the difference between the observed and predicted outcomes for treated observations. That is $\hat{\tau}_{ictd} = Y_{ictd} - \hat{Y_{ictd}}(0)$, where $\hat{Y_{ictd}}(0)$ is the predicted counterfactual outcome based on the model. This estimator also relies on parallel trends and no anticipation assumptions. The never-treated units help pin down the model for the untreated outcomes with more precision. In particular, by including these, we can estimate more precisely the time trends in outcomes by leveraging information from more observations to estimate the month of the year and day of the month fixed effects included in the model.

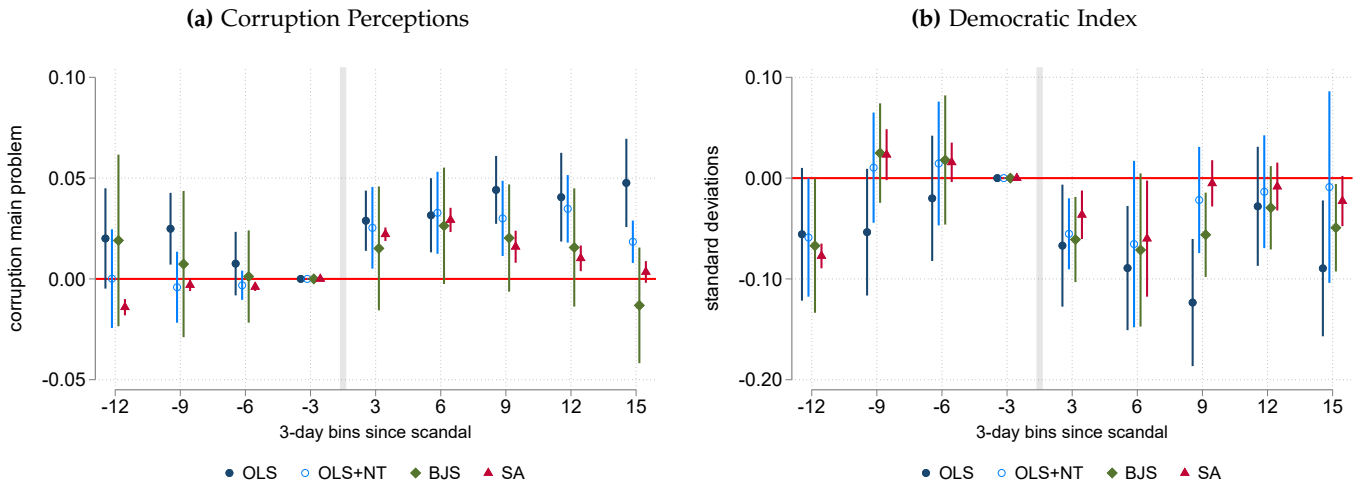
Sun and Abraham (2021): This estimator separately estimates average treatment effects by cohort for each relative time since event period. It does this by using a TWFE fully interacted specification between relative-time indicators and cohort indicators, and then re-weighting the estimates based on cohort-share weights. In particular, the estimator relies on parallel trends between each treated cohort and the never-treated cohorts as well as on no anticipation of scandal occurrence. By including never-treated cohorts we are able to estimate the treatment effect for each cohort —as opposed to losing at least one cohort when using the not-yet-treated as comparison groups.

Table OA-21: Effect of Apex Corruption Scandals: Staggered Difference in Differences

	OLS		OLS with Never-Treated		Borusyak et al. (2024)		Sun and Abraham (2021)	
	Corruption Main Problem	Democratic Index	Corruption Main Problem	Democratic Index	Corruption Main Problem	Democratic Index	Corruption Main Problem	Democratic Index
Corruption	0.029***	-0.069*	0.027***	-0.061***	0.041***	-0.066***	0.016***	-0.027*
	(0.011)	(0.035)	(0.009)	(0.024)	(0.009)	(0.005)	(0.003)	(0.015)
Observations	25,790	27,016	133,616	140,133	133,616	140,133	133,616	140,133
Scandals	26	26	26	26	26	26	26	26
Mean dep. var	0.091	0.001	0.056	0.001	0.057	0.001	0.057	0.000

This table shows estimates of the effect of apex corruption revelations on different outcomes. Columns labeled 'OLS' show estimates of β in $Y_{ictd} = \alpha + \beta \mathbf{1}(\text{Post Corruption})_{ictd} + \gamma' X_i + \Gamma_{ct} + \epsilon_{ictd}$ as in Equation 2. The sample for estimates of columns labeled 'OLS' includes only respondents in country-years where a scandal occurred. For Columns labeled 'OLS with Never-Treated', Borusyak et al. (2024), and Sun and Abraham (2021) the sample includes respondents (i) in country-years where a scandal occurred, and (ii) respondents in country-years where no scandal occurred. Columns labeled 'Borusyak et al. (2024)' show estimates of the ATT using the imputation estimator introduced in Borusyak et al. (2024), which imputes the counterfactual outcome for treated units based on estimates for the control sample (respondents in country-years without scandal plus respondents in country-years with scandals but who were interviewed before the scandal). Columns labeled 'Sun and Abraham (2021)' show the weighted average estimate of δ_s in $Y_{ictd} = \alpha + \sum_{s \in S} \sum_{\substack{l=-15 \\ l \neq -1}}^{15} \delta_{s,l} \mathbf{1}\{\text{Scandal}_i = s\} \cdot \mathbf{1}\{d_i = l\} + \gamma' X_i + \Gamma_{ct} + \epsilon_{ictd}$, as in Sun and Abraham (2021). The weight given to δ_s is proportional to the number of units used for its estimation.

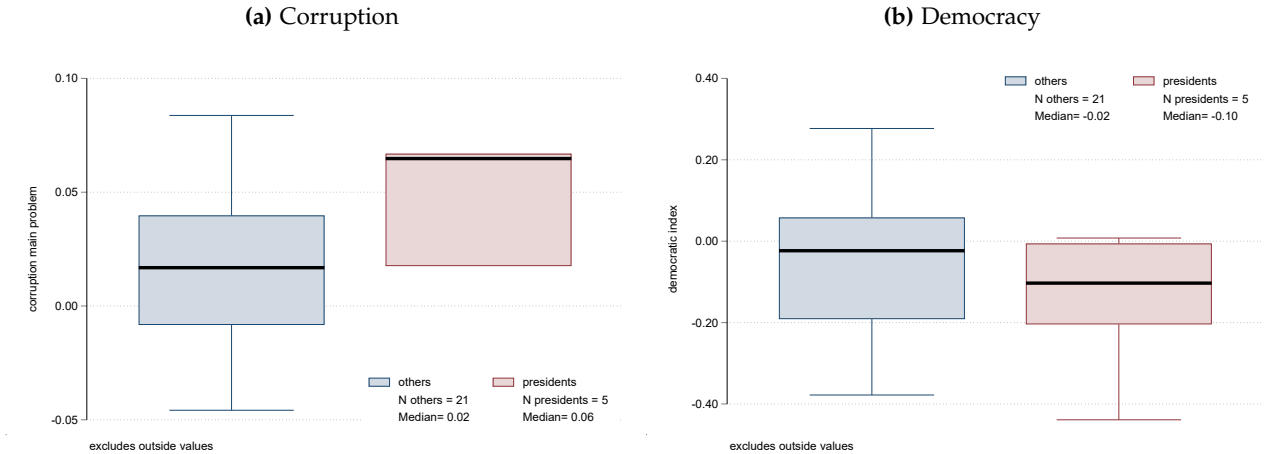
Figure OA-13: Effects of Apex Corruption Revelations Over Time: Staggered Difference-in-Differences



This figure shows estimates of the effect of apex corruption scandals on corruption perceptions and the democratic index. Filled circles report the interrupted-survey estimates, comparing respondents interviewed after a scandal to respondents interviewed before the scandal within the same country-year. The remaining estimates add respondents from country-years in which no scandal occurred during Latinobarómetro fieldwork. In these specifications, pre-scandal respondents in scandal country-years serve as not-yet-treated controls, while respondents in no-scandal country-years serve as never-treated controls. The Borusyak et al. (2024) estimator imputes counterfactual outcomes for post-scandal respondents using these untreated observations; the Sun and Abraham (2021) estimator compares post-scandal relative-time cells to not-yet-treated and never-treated observations.

OA - F Comparing scandals by type

Figure OA-14: Distribution of Effects of Corruption Revelations: Presidents vs non-presidents



This figure shows the distribution of effects separating by whether the corruption exposure involves a president versus if it involves any other apex agent. We estimate equation 2 separately for each of our scandals to estimate the effect of apex corruption revelations.
Backreferenced (on pages): [30]

Table OA-22: Effects of Apex Corruption on Corruption Perceptions and Democratic Values by Type of Scandal Breakout

	(1) Corruption Main Problem	(2) Democratic Index	(3) Trust Institutions	(4) Authoritarian Alternative†
Panel A: Media Report				
Corruption	0.002	0.046	0.002	0.036
	(0.015)	(0.049)	(0.058)	(.)
	[0.012]	[0.033]	[0.050]	[0.021]
Observations	4256	4552	4552	836
Scandals	5	5	5	1
R-squared	0.080	0.022	0.029	0.070
Wild Bootstrap p-value	0.854	0.354	0.888	(.)
FDR q-value	1.000	1.000	1.000	(.)
Country x Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Mean dep. var	0.087	0.000	0.000	0.226
Panel B: Indictments				
Corruption	0.063	-0.110	-0.157	0.000
	(0.023)**	(0.058)*	(0.038)***	(0.031)
	[0.017]***	[0.051]**	[0.033]***	[0.031]
Observations	7384	7612	7612	4659
Scandals	9	9	9	6
R-squared	0.100	0.042	0.062	0.137
Wild Bootstrap p-value	0.024	0.110	0.000	0.968
FDR q-value	0.042	0.070	0.014	0.329
Country x Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Mean dep. var	0.093	-0.000	0.000	0.395
Panel C: Formal Accusations				
Corruption	0.020	-0.116	-0.042	0.054
	(0.013)	(0.056)*	(0.055)	(0.010)***
	[0.011]*	[0.058]**	[0.057]	[0.017]***
Observations	10596	11181	11181	5948
Scandals	12	12	12	6
R-squared	0.062	0.027	0.031	0.288
Wild Bootstrap p-value	0.206	0.132	0.530	0.026
FDR q-value	0.141	0.102	0.250	0.011
Country x Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Mean dep. var	0.100	-0.000	-0.000	0.353

This table shows the effect of corruption revelations in Latin America by analyzing the responses of people interviewed for the Latinobarometer survey within 15 days before and after corruption revelations, estimating equation 2. Robust standard errors clustered by country × year are shown in parentheses, while robust standard errors clustered by country × year × relative distance to corruption scandal grouped in blocks of three days are shown in square brackets. The individual controls are gender, age, schooling, employment, civil status, size of town, socioeconomic status level, month and day-of-week indicators. The bottom of the table reports the p-value from a wild bootstrap test and q-values using the False Discovery Rate method, which adjusts for testing the hypotheses on the four indices (this correction may be overly conservative since indices constitute different families of outcomes). Significance is reported at levels *** $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$

† Note that the measure of authoritarian alternative is not available for every country nor in every year.

Backreferenced (on pages): [30]

Table OA-23: Local Corruption Scandals that intersect with Latinobarometer Survey Fieldwork

Country	Date	Type	Corruption Scandal
Bolivia	16/09/2010	Indictment	Two former mayors in Cochabamba were accused of misappropriating public funds intended for community projects.
Bolivia	02/08/2011	Formal Accusation	The Ministry of Government accused a judge in Cochabamba of irregularly granting house arrest to a money-laundering suspect.
Bolivia	27/01/2015	Media Report	The interim mayor of Cochabamba reported uncovering multiple corruption cases within the municipal government.
Brazil	30/07/2011	Indictment	Prosecutors reopened a corruption case against the former head of São Paulo's state road company over misuse of public funds.
Brazil	04/07/2018	Media Report	Federal police investigated bid-rigging and overpricing schemes involving medical equipment suppliers in Rio de Janeiro's health system.
Chile	22/07/2017	Media Report	A former regional governor was accused of using political influence to favor approval of the GNL Talcahuano gas project.
Colombia	17/06/2016	Arrest	A judge in Valle del Cauca was arrested for allegedly inflating the value of public property in a state purchase.
Guatemala	20/07/2011	Media Report	Electoral workers reported threats and dismissals after denouncing irregularities linked to alleged electoral manipulation.
Honduras	19/05/2016	Formal Accusation	The national anti-corruption agency accused a municipal mayor of over-valued contracts and abuse of public office.
Mexico	01/06/2016	Media Report	Authorities investigated judicial corruption following the escape of inmates from a Mexico City prison.
Uruguay	07/06/2013	Formal Accusation	Opposition leaders filed a criminal complaint against a local legislative secretary for smuggling undeclared goods.

Notes: This table includes an overview of the local political corruption scandals that happened in Latin America during times when Latinobarometer surveys were being conducted. The "Type" column refers to how the scandal broke. "Indictment" denotes that the news reported arrests related to the figures implicated in the scandal, "Media report" denotes that the scandal broke out by journalist reports, "Formal Accusation" denotes that the news reported on an accusation made by judiciary/audit officials, and "Sentenced" denotes that the news reported on a sentence made by a court. One scandal in Brazil is related to two states, which is why OA-24 shows 12 scandals (*Sul, Sudeste*).
Backreferenced (on pages): [29]

Table OA-24: Effects of Local Corruption on Corruption Perceptions and Democratic Values

	(1) Corruption Main Problem	(2) Democratic Index	(3) Trust Institutions	(4) Authoritarian Alternative
Corruption	-0.023	0.002	-0.033	-0.045
	(0.027)	(0.063)	(0.092)	(0.073)
	[0.025]	[0.088]	[0.101]	[0.064]
Observations	2751	2841	2921	1955
Scandals	12	12	12	9
R-squared	0.066	0.077	0.108	0.167
Wild Bootstrap p-value	0.522	0.980	0.776	0.706
FDR q-value	1.000	1.000	1.000	1.000
Country x Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Mean dep. var	0.098	0.000	0.000	0.302

This table shows the effect of local corruption revelations in Latin America by analyzing the responses of people interviewed for the Latinobarometer survey within 15 days before and after local corruption revelations, estimating equation 2. The sample is composed of respondents who reside in the State where the corruption scandal occurs. Robust standard errors clustered by country × year are shown in parentheses, while robust standard errors clustered by country × year × relative distance to corruption scandal grouped in blocks of three days are shown in square brackets. The individual controls are gender, age, schooling, employment, civil status, size of town, socioeconomic status level, month and day-of-week indicators. The bottom of the table reports the p-value from a wild bootstrap test and q-values using the False Discovery Rate method, which adjusts for testing the hypotheses on the four indices (this correction may be overly conservative since indices constitute different families of outcomes). Significance is reported at levels *** $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$

Backreferenced (on pages): [30,OA-27]

OA - G Registered hypothesis

G.1 Registration record and timeline

The study was pre-registered with the AEA RCT Registry under two entries: AEARCTR-0007770 ("Rebuilding Trust, Social Cohesion and Democratic Values"), covering the main experiment, and AEARCTR-0008170 ("Does Political Corruption Cumulate in Its Effects?"), covering the second-video wave of September 2021.[55] The pre-analysis plan (PAP) referred to throughout this appendix is the document uploaded to the registry with the first entry on June 5, 2021; it has not been modified since.[56] Because the first registration was submitted after the pre-election survey wave had been fielded, we set out the chronology in full. While the AEA trial document was ready and the details were set, we waited for approval from Stanford's IRB before publishing it.

Design documentation before fieldwork. The experimental design was documented in writing at the implementing institution before any participant was contacted. The proposal submitted to ITAM's Institutional Review Board on April 2, 2021 describes the four video treatments and their content, a Latinobarómetro-style baseline covering attitudes toward democracy, perceptions of government effectiveness and perceptions of corruption, the trust and resource-allocation games, the electoral-observer item, a follow-up survey, and the link to administrative turnout data from INE. A revised version incorporating the committee's comments was submitted on April 13 and approved on April 18, 2021; the approval explicitly covered fieldwork in Oaxaca during April–May 2021 and recorded the board's acceptance that the study was time-sensitive and could not be postponed past the June 6 federal election. The Stanford protocol (60746), initiated in March 2021 and under review from April 20, describes the same design; it was approved on June 4, 2021. These documents fix the treatments, the outcome domains, and the administrative data link. They do not include hypotheses, outcome definitions, or an outcome hierarchy; those were fixed in the PAP.

Fieldwork and registration. The first interview took place on April 24, 2021, and the pre-election wave—baseline survey, video treatments and the immediate post-treatment survey—ended on June 1, 2021, as survey activity could not take place in the days immediately before the election (Figure 2). The registration and PAP were submitted on Saturday, June 5, 2021, the day before the election, and published by the registry on Monday, June 7, after its standard review. At the time of submission, the survey was being implemented in the field, but none of its data had been analyzed, and none of the following had yet been collected: the administrative turnout outcome (matched by INE in 2022, about a year after fieldwork ended), the post-election voting survey (from June 27), the remaining rural interviews (from June 16), or the follow-up survey (September 10–October 21, 2021). The PAP and registry give planned fieldwork start dates of May 1 and May 5; the paper reports the realized date.

Second registration. After footage of a second brother of the president (Martín Jesús López Obrador) receiving cash was published on July 8, 2021, we designed the second-video treatment and registered it on September 8, 2021. This is the raw material for the second experiment. The second-video wave was registered on September 8, 2021 and published on September 9, before its fieldwork of September 13–20, which was embedded in the follow-up survey of the urban sample after the persistence outcomes had been elicited (Section B.5). Its registration postdated the first wave's immediate survey outcomes—its power calculation uses the first-wave effect on a democracy indicator—but preceded the follow-up survey and the administrative turnout data, neither of which existed at the time.

[55] https://doi.org/10.1257/rct.7770-1.0 and https://www.socialscienceregistry.org/trials/8170.

[56] The registry records the file's upload date as June 5, 2021, with MD5 b34c8036be150fa7fde8a277e85d52fa and SHA-1 55195321c097584c3c22064d69deb46e82a01800.

@Rob – Confirm whether or not the June 27 voting-survey data had been collected and examined before September 8, 2021. ↑

Later edits. The registry entry for AEARCTR-0007770 was revised once, on August 27, 2024. The registry's change log for that revision lists three fields: trial status (changed from "ongoing" to "completed"), the visibility of the analysis plan (changed from private to public), and the "building on existing work" indicator (set to "no").[57] There was no change in the text, methods, measures, or hypothesis, or anything else. A line-by-line comparison of the registry's archived June 7, 2021 version with the current version confirms that the abstract, intervention description, primary and secondary outcomes, experimental design, sample sizes, power calculations and analysis-plan record are identical. The plan was therefore time-stamped at registration and kept on file, as the registry permits, until it was made public in 2024; the banner "Last registered on August 27, 2024" on the public page refers to us making this public, not to any change in content. The entry for AEARCTR-0008170 was revised on May 17, 2024. Comparisons in this appendix use the original versions of each registration and the June 5, 2021 analysis plan.

G.2 Registered hypotheses

Primary and secondary hypothesis. The pre-analysis plan (PAP) distinguishes primary from secondary outcomes and states its hypotheses in the text describing each treatment arm. We adopt the PAP's labels without modification. Primary outcomes are administrative turnout (and surveyed voting choices in the urban sample), the preference-for-democracy index, the resource-allocation game, the survey measures of trust, the corruption-perceptions index, and the survey measures of voting; secondary outcomes are electoral observer enrollment, the donation for electoral observers, the hypothetical questions on freedom versus surveillance and on cheating and helping, and the trust game. We label as exploratory only analyses that the registrations themselves describe as underpowered or exploratory. Three outcomes that the paper presents prominently—electoral-observer enrollment, the donation, and the trust game—were registered as secondary, for reasons the PAP states: the observer and donation items could be measured only for respondents interviewed before the election, and pilots had shown the one-shot, socially distanced trust game to be hard for enumerators and participants to understand. We keep their registered tier and return to this point below.

Direction of tests. Tests of the corruption treatments are one-sided. The PAP states the hypothesis for the corruption videos directionally—they "will diminish support, particularly for the culpable group, but also for the democratic system and for trust more broadly" (p. 3)—and lists administrative turnout as the first primary outcome and the study's behavioral measure of civic participation. The PAP does not contain a separate statement that turnout will fall; the direction we test is the one implied by the registered hypothesis for the construct that turnout measures. The registered design confirms that this was the planned test: the power calculation for turnout is specified for the pooled comparison of both corruption videos against control (p. 7), a comparison that is informative only under a common effect on participation, since partisan vote switching would offset across the two videos. We apply the same direction to every outcome in this family for the corruption arms, irrespective of the estimate, and report two-sided p-values alongside. For the economic-underperformance video, whose registered direction was also a decrease, the estimated turnout effect is positive; we report it with a two-sided test and treat it as contrary to the registered expectation. Of the paper's turnout results, the pooled estimate (one-sided $p = 0.066$; two-sided 0.13) and the urban estimate (0.043; 0.086) are significant at conventional levels only under the one-sided test; the treatment-timing results are not sensitive to this choice.

@ES – Confirm the two-sided values against the estout output; I doubled the one-sided ones. ↑

[57] https://www.socialscienceregistry.org/trials/7770/history/233348/changes.

Tables OA-25–OA-28 list every hypothesis stated in the two registrations, one row each: the registered statement, the arm and outcome it concerns with the PAP's tier, whether its direction was pre-specified explicitly, at the construct level, or not at all, where it is tested in the paper, and what we found. Table OA-25 covers the corruption treatments' hypotheses on primary outcomes; Table OA-26 their hypotheses on secondary outcomes and heterogeneity; Table OA-27 the economic, nation-building and stock arms; and Table OA-28 the second registration. Deviations in measurement or analysis are flagged in the last column and discussed in Section **??**.

Table OA-25: Registered Hypotheses and Findings I: Apex Corruption Treatments, Primary Outcomes (AEARCTR-0007770)

	Registered statement (PAP)	Arm	Outcome(s); PAP tier	Direction pre-specified	Tested in	Finding; status
H1	Corruption videos "will diminish support, particularly for the culpable group" (p. 3); "the incumbent video will reduce the intention to vote for the incumbent, and the analogous will happen for the non-incumbent parties."	CI, CO	Survey measures of voting (vote for the featured party); *primary*	Explicit	Table 2, cols. 5–8; Table OA-4	CI reduces self-reported support for Morena by 19.8pp; CO reduces support for PAN–PRI–PRD by 21pp (treatment one week before the election). Consistent. *Modified*: registered item was pre-election vote intention; the paper uses the post-election voting survey. **@ES** – Is the pre-election intention item reported anywhere? ↑
H2	"…but also for the democratic system" (p. 3)	CI, CO	Preference-for-democracy index (three items); *primary*	Explicit	Figure 3(a); Table OA-11	-0.16σ ($p < 0.001$); all three components move in the same direction. As registered.
H3	Same statement applied to the behavioral measure of support for the democratic system	CI, CO	Administrative turnout; *primary*	Construct-level (see text)	Table 1; Table 2, cols. 1–3	Pooled $-4.0pp$ (one-sided $p = 0.066$); urban $-6.2pp$ (0.043); $-15.6pp$ when treated one week before the election. As registered.
H4	"…and for trust more broadly" (p. 3); ten trust items plus generalized trust listed	CI, CO	Survey measures of trust; *primary*	Explicit	Tables OA-12, OA-13; Figure 3(a)	Trust in democratic institutions -0.19σ ($p < 0.001$); trust in other citizens -0.116σ; all components decline. *Modified*: items aggregated into two indices not pre-specified as indices; components reported.
H5	"We expect that the videos will cause people to update upwards on average on the level of corruption in the country"	CI, CO	Corruption-perceptions index (four items); *primary*	Explicit	Figure 3(a); Table OA-10	$+0.225\sigma$ ($p < 0.001$); all four components. As registered.
H6	"Stealing from politicians will be larger and more precise than stealing from neighbors … there may still be (small) spillovers to neighbors"	CI, CO	Resource-allocation game (mayor; neighbor); *primary*	Explicit	Table 4, Panel B	CI: -0.7 coins to the mayor (4%, $p = 0.024$); -0.40 coins to the neighbor ($p = 0.14$). As registered.
H7	"People will tend to keep for themselves higher denomination coins more than lower denomination ones"	CI, CO	Resource-allocation game by coin value; *primary* (sub-conjecture)	Explicit	—	*Not reported* in the current draft; the analysis exists (Table **??**, commented out). **@ES** – Result and decision: report or state why dropped. ↑

Quotations are from the pre-analysis plan registered with AEARCTR-0007770 on June 5, 2021; page numbers refer to that document. "Tier" is the PAP's own classification of outcomes. "Direction pre-specified" is *explicit* when the PAP states the sign for the outcome, *construct-level* when the sign follows from the registered hypothesis for the construct the outcome measures (see "Direction of tests"), and *none* when the PAP poses a two-sided question. p-values are one-sided for the corruption arms unless noted. "As registered" means the outcome, sample and comparison follow the PAP; "modified" and "deviation" flag differences discussed in Section **??**.
Backreferenced (on pages): [OA-31,OA-31,OA-34,OA-35,OA-36]

Table OA-26: Registered Hypotheses and Findings II: Apex Corruption Treatments, Secondary Outcomes and Heterogeneity (AEARCTR-0007770)

	Registered statement (PAP)	Arm	Outcome(s); PAP tier	Direction pre-specified	Tested in	Finding; status
H8	"Amount sent in a trust game either to a member of congress or a member of the community"	CI, CO	Trust game; *secondary*	Construct-level ("trust more broadly")	Table 4, Panel A	Amount entrusted to the politician falls by 1.8 pesos (CI, $p = 0.002$) and 1.2 pesos (CO, $p = 0.029$); neighbor: **@ES – fill from Panel A** ↑ . Consistent. *Deviation*: registered re-weighting procedure for comprehension not implemented (Section **??**).
H9	"Revealed preference willingness to participate as an electoral observer . . . or to donate money to for electoral observers"	CI, CO	Observer enrollment; donation; *secondary*	Construct-level (civic participation)	Table 3	CI: observer enrollment $-2.1pp$ (42.8%, $p = 0.025$); donation $-3.6pp$ (15.7%, $p = 0.063$); CO: no effect on enrollment, smaller effect on donation. Consistent. *Modified*: donation implemented as 10 pesos for poll-station workers rather than for observers.
H10	"There could be more updating with the Incumbent video" since Morena "is viewed as much less" corrupt (p. 3)	CI vs CO	All primary outcomes	Explicit, hedged	Figure 3(b); Table OA-8; Table 2	Larger CI effects on trust in institutions (0.28 vs 0.10σ, $p = 0.001$) and in people (0.18 vs 0.05σ, $p = 0.007$); corruption perceptions (0.25 vs 0.20σ, $p = 0.19$) and democracy similar; the close-to-election turnout effect is driven by CI. Largely consistent.
H11	"Differential effects among ex ante supporters of these parties (due to motivated reasoning/cognitive dissonance or greater informational content)"	CI, CO	Heterogeneity by baseline party support	None (two-sided question)	Figure OA-6; Table OA-5	Effects not mitigated among ex ante Morena supporters (turnout $-23.2pp$ and Morena vote $-24pp$ when treated one week before). Consistent with the informational, not the motivated-reasoning, alternative. As registered.
H12	Implementation note 1: "differential effects among those who ex ante are already aware of the informational content"	CI, CO	Heterogeneity by prior awareness	None	Table **??**	Effects on corruption perceptions, democracy and trust in institutions driven by respondents unaware of the videos. As registered.

Conventions as in Table OA-25. The trust game, observer enrollment and donation were registered as secondary outcomes; see "Hierarchy rule" for the reasons the PAP gives.
Backreferenced (on pages): [OA-31]

Table OA-27: Registered Hypotheses and Findings III: Economic, Nation-Building and Stock Arms (AEARCTR-0007770)

	Registered statement (PAP)	Arm	Outcome(s); PAP tier	Direction pre-specified	Tested in	Finding; status
H13	Economic video: "these effects will diminish support for incumbent party, for the democratic system, and mutual trust"	E	Turnout; vote for incumbent; democracy index; trust; *primary*	Explicit	Table 1; Table OA-4; Table OA-8; Figure **??**	Self-reported vote for the incumbent $-20.8pp$ (one week before); democracy index falls and corruption perceptions rise; no effect on trust in other citizens. Turnout *increases* by 8–10pp—*contrary* to the registered direction; reported two-sided. Mixed.
H14	"Test whether retrospective information about economic performance—i.e. failures in outcomes—have different effects than corruption—i.e. failures in processes"	E vs CI, CO	All primary outcomes	None (comparison)	Table OA-8; Table OA-18	Opposite behavioral responses: higher turnout and punishment of the incumbent (voice) versus lower turnout and participation (exit). As registered.
H15	Nation-building video: "we expect heterogeneous treatment effects by indigenous identity . . . with indigenous people experiencing a broader sense of community . . . reflected in more trust" (p. 4)	NB	Trust; democracy index; turnout; observer enrollment; heterogeneity by indigenous identity	Explicit (more trust)	Figure **??**; Table **??**; Table **??**	No effect on trust or corruption perceptions; democracy index $+0.069\sigma$ ($p = 0.09$); observer enrollment $-1.8pp$; no effect on turnout. *Contrary* to expectation. Indigenous heterogeneity *not reported* here (separate project, fn. 16).
H16	Narrative/perspective-taking exercise "for a small sub-sample" following Broockman and Kalla (p. 4); tailored indigenous and mainstream video variants "if feasible"	NB	Sub-treatment of the NB arm	—	—	**@ES** – Confirm whether the exercise was implemented and whether tailored variants were produced; if so, NB is a pooled arm and this should be stated. ↑
H17	Stock treatment: "We anticipate low compliance and thus will focus on treatment effects on the treated"; "sharing the economic future of the nation" as a means to (re-)build trust and support for democracy	Stock	Follow-up indices; turnout (urban sample); TOT and ITT	Construct-level (positive)	Table OA-14; Section B.6	TOT: democracy $+0.8\sigma$ ($p = 0.04$); trust in institutions $+0.43\sigma$ (0.16); trust in other citizens $+0.58\sigma$ (0.04); turnout $+12.6pp$, imprecise. Presented as suggestive. As registered (TOT focus).
H18	"Test for financial literacy effect differences by gender"	Stock	Financial literacy by gender	None	—	*Not reported*. **@ES** – Decision: report or state why dropped. ↑
H19	"Of particular interest is how these effects differ among historically marginalized indigenous groups and those with different histories of exposure and assimilation" (cochineal vs non-cochineal municipalities; rural migrants in the urban sample)	All	Heterogeneity by cochineal history, indigenous identity, migrant status	None	—	*Not reported* in this paper; the cochineal stratification is the basis of a separate project (fn. 16).
H20	Hypothetical questions on freedom versus surveillance and on cheating and helping (taxes, blood donation, vaccine queue-jumping)	CI, CO	Hypothetical items; *secondary*	None	—	*Not reported* in the current draft; a placebo-outcomes table exists in the source, commented out. **@ES** – Decision. ↑

Conventions as in Table OA-25. For the economic-underperformance arm, p-values on turnout are two-sided because the estimate is contrary to the registered direction.
Backreferenced (on pages): [OA-31]

Table OA-28: Registered Hypotheses and Findings IV: Second Registration (AEARCTR-0008170, registered September 8, 2021)

	Registered statement (registry text)	Arm	Outcome(s); tier	Direction pre-specified	Tested in	Finding; status
H21	"Whether political corruption cumulates in its effects. In other words, whether exposure to additional evidence of corruption has an effect relative to just one"	CI_2 vs control	Democracy preference; corruption perceptions; vote intention; *primary*	Explicit (sign follows the first wave); magnitude left open	Figure OA-7; Table **??**; Section B.5	CI_2: corruption perceptions $+0.28\sigma$; democracy index -0.23σ, about 40% larger than the one-brother effect (p of a larger effect = 0.19). Consistent. **@ES** – Vote intention item reported? ↑
H22	"Dormant effect: . . . the 'second brother' video will have greater effects for those who had already been treated relative to those in the control"	$CI_2\times$ prior CI	Democracy preference; corruption perceptions	Explicit	Figure OA-7; Table **??**	Two doses: democracy index -0.39σ, 65% larger than CI_2 alone. Consistent. Registered as underpowered (about 100 respondents received both incumbent videos; registered power 33%).
H23	"Differential treatment effects for ex ante stronger incumbent supporters . . . strengthened (due to greater updating) or weakened (due to motivated reasoning) . . . we consider it exploratory"	CI_2	Heterogeneity by baseline incumbent support; *exploratory*	None	**@ES** – location or — ↑	**@ES** – Reported? If not, state so. ↑

Quotations are from the registration text of AEARCTR-0008170, which has no separate analysis-plan document; its primary outcomes are the preference-for-democracy items, the corruption-perception items and vote intention, with trust in institutions and recall of the party voted for also listed. Conventions as in Table OA-25. The second registration was submitted after the first wave's immediate survey outcomes were known and before the follow-up survey and the administrative turnout data existed (Section G.1).

Backreferenced (on pages): [OA-31,OA-31]

Expected versus found. For the apex corruption treatments, every registered directional hypothesis in the primary tier is supported with the registered sign: vote for the featured party, the preference-for-democracy index, administrative turnout, the trust items, the corruption-perceptions index, and the resource-allocation game (H1–H6). The two registered two-sided questions resolve in the same direction as the directional ones: the incumbent video produces larger effects on trust (H10), and the effects are not attenuated among the incumbent's ex ante supporters (H11). Two outcomes the paper presents prominently were registered as secondary: the trust game (H8) and electoral-observer enrollment and the donation (H9). Ex post these measures performed better than the PAP anticipated—enrollment proved a costly and well-understood action, and the games produced effects consistent across counterparts—and the paper gives them more weight than their registered tier. The paper's conclusions do not depend on them: the primary-tier outcomes move in the same direction in every case.

@ES – Check that no headline claim rests only on H8 or H9; if one does, name it here. ↑

The misses are concentrated outside the corruption arms. The economic-underperformance video was registered as diminishing support for the democratic system; it lowers stated support for democracy as registered but *raises* turnout, the opposite of the registered direction, and we report this as a contrary finding (H13). The nation-building video was expected to increase trust and did not; it also reduced observer enrollment (H15). Among the corruption-arm conjectures, the coin-denomination hypothesis (H7) is the only registered primary-tier item not reported in the current draft. The registered heterogeneity analyses by cochineal history, indigenous identity and migrant status (H19), and the indigenous heterogeneity of the nation-building effect (H15), are not reported here because they are the subject of a separate project; the hypothetical items (H20) and the gender analysis of financial literacy (H18) are not reported. Section **??** lists each of these with its disposition, together with the differences in measurement and analysis flagged in the tables.