

THE POWER OF EXAMPLE: CORRUPTION SPURS CORRUPTIONⁱ

Nicolás Ajzenmanⁱⁱ

April 10, 2020

Abstract

Does political corruption erode civic values and foster dishonest behavior? I test this hypothesis in the context of Mexico by combining data on local government corruption and cheating on school tests. I find that, following revelations of corruption by local officials, secondary students' cheating on cognitive tests increases significantly. The effect is large and robust and persists for over a year after malfeasance is revealed. These findings are validated by evidence from individual survey data, which documents that individuals interviewed right after corruption is revealed report being less honest, less trustworthy, and more prone to thinking that cheating is necessary to succeed, compared to similar individuals interviewed just beforehand.

JEL Codes: D73, P16, Z1, A13, O12, H72, K42.

Key words: Corruption, Social Norms, Culture, Leadership, Civic Values

ⁱI am grateful to Ruben Durante and Roberto Galbiati for their guidance and to Sergei Guriev for his help and support. I thank Rodrigo Soares, Filipe Campante, Sebastian Galiani, Ricardo Perez-Truglia, Martín Fiszbein, Claudia Senik, Juan Pablo Rud, Quoc-Anh Do, Ralph De Haas, Bernardo Guimarães, Phillip Keefer, Cevat Giray-Aksoy, Giovanni Prarolo, Max Viskanic, and Nicolò Dalvit for their valuable comments. This paper benefited from interactions with seminar participants at Louisiana State University, FGV-São Paulo School of Economics, Universidad de los Andes, CIDE, the Inter-American Development Bank, Sciences Po, the 2019 Annual Conference of the Society of Institutional and Organizational Economics in Stockholm, the 2017 Royal Economic Society Symposium of Junior Researchers in Bristol, the LACEA-LAMES Annual Meeting in Buenos Aires, the ASREC Europe Conference in Bologna, the European Bank for Reconstruction and Development research seminar in London and the 14th Workshop for Young Economists in Social Economy in Forli. I also thank Horacio Larreguy for sharing the audit data. Finally, I deeply appreciate the very constructive feedback and suggestions of the editor, Benjamin Olken, and three anonymous referees. All errors remain my own.

ⁱⁱAssistant Professor of Economics, FGV-São Paulo School of Economics - E-mail: nicolas.ajzenman@fgv.br - Phone: (+1) 415-316-2449 - R. Itapeva, 474, São Paulo, SP, 01332-000, Brazil. Total word count: 15420.

1 Introduction

Poor institutions and unethical norms are likely causes of political corruption. But what is the origin of these norms? And is there a reinforcement mechanism by which political corruption, in turn, fosters dishonest norms among citizens?

Although the theoretical literature on social norm transmission through leadership (Acemoglu and Jackson (2014), Tabellini (2008)) suggests that citizens may, in fact, adopt norms observed in their leaders' behavior, the empirical evidence is scarce. Various studies find a correlation between perceived government corruption and, for instance, citizen dishonesty (Fisman and Miguel (2007), Barr and Serra (2010), Gächter and Schulz (2016)). However, the underlying causality remains unclear or, at best, limited to laboratory experiments (d'Adda et al. (2017)).

I provide evidence on this causal relationship in the context of an anti-corruption program in Mexico that audits the use of federal funds by recipient municipalities.¹ In particular, I show that publicly exposed corruption perpetrated by leaders increases citizens' dishonest behavior, as measured by secondary school cheating rates on school tests. Additionally, such behavior spurs a significant deterioration in citizens' values related to honesty, rule observance, and trust. The evidence points to the self-reinforcing effect of corruption.

The context examined here is especially suitable to understanding the effect of leaders' negative conduct on citizen behavior. First, the independent audit reports were published two years after the actual corruption occurred. This allows to differentiate the effect of corruption itself from that of the disclosure of corruption (i.e., when the leader sets the negative example). Second, schools did not punish students for cheating. I am therefore able to rule out any interpretation related to a change in the perceived expected cost of breaking the rules, allowing a focus on the cultural channel.² Finally, because legal punishment for this type of corruption was virtu-

¹Several papers have used these and other audits as objective measures of corruption. See, for example, Avis, Ferraz and Finan (2018), Ferraz, Finan and Moreira (2012), Ferraz and Finan (2011), Ferraz and Finan (2008) in Brazil and Chong et al. (2014) and Larreguy, Marshall and Snyder Jr (2014) in Mexico.

²In the paper, I provide suggestive evidence to this regard. In particular, I show that the effect

ally nonexistent, I can interpret the results as the effect of unpunished corruption on dishonesty.

To identify a causal effect, I use different data sources and methods. First, I combine a municipality-level dataset containing the results of the audit reports, and a school-grade-year-level dataset that includes cheating data on compulsory standardized exams detected by software.³ I then exploit the time-space variation in the revelation of corruption audits and estimate a generalized difference-in-differences model with school, grade, and year fixed effects. In addition, I control for time-varying municipality characteristics and municipality-specific linear time trends, so as to take into account the presence of potential differential trends in cheating across municipalities due to unobserved local time-varying effects. I also control for a dummy indicating whether an audit report was released, allowing to compare cheating in municipalities that were audited in the same year but that showed different corruption levels.

Moreover, to support the internal validity of the estimations, I test the pre-treatment and post-treatment effects using an event-study type model in the spirit of Granger (1969). I show that there were no anticipation effects, meaning that people did not react to actual corruption but rather to the publicity about corruption two years later.

I find that, following revelations of corrupt behavior on the part of local officials two years after the latter occurred, secondary school students are 10% more likely to cheat on standardized tests (from 4% to 4.4%). The effect is more pronounced for older students, who are arguably more exposed to political discussions within and outside the family.⁴ The effect is also robust to the inclusion/exclusion of differ-

of corruption on cheating is large even in schools where the likelihood of catching a student is low. Interestingly, however, while there was no punishment for cheating, parents were informed about the potential misconduct of their children through the same means used to communicate grades. This means that students had an incentive to perform well and to be honest, where they perceived their parents would value honesty.

³The algorithm identifies only cheating among students and not teacher-led cheating. Refer to the data section for a detailed explanation or see Jacob and Levitt (2003).

⁴Other papers have shown conceptually similar results, where exposure to events have a different impact depending on the age of the subject. Take, for example, the findings of Madestam,

ent sets of controls (for example, municipality trends or time-varying municipality characteristics such as homicide rate, local tax collections, or political variables), definitions of corruption, and type of model. Using an event-study model, I furthermore show that the effect persists for one additional period after exposure to corruption.⁵

One possible mechanism to explain the main effect is a process of social learning whereby people observe their leaders' behavior (for instance, through the media), adjust their perspective relative to social norms, and change their own intrinsic values as a result.⁶ I provide suggestive evidence consistent with this hypothesis. First, I exploit within-municipality variation in household radio ownership rates and differences across municipalities in the number of local radio stations, showing that the effect is concentrated among those areas with potentially higher exposure to news. This result suggests that media was one channel by which news on government corruption was disseminated.

Second, I show that the impact of corruption on cheating is significantly larger in municipalities where the incumbent party was originally thought to be honest. This suggests that corruption scandals mean new social norm information for certain communities.

Third, I directly estimate the effect of corruption revelation on the personal values of the adult population. I combine the municipality-level dataset on corruption with the Mexican Family Life Survey (an individual-level longitudinal survey) and exploit the plausibly exogenous difference in the timing of the interviews relative to the timing of the release of the corruption reports. I then estimate a difference-in-

Yanagizawa-Drott et al. (2011) relative to the effects of attending a 4th of July parade on political preferences, where the largest effect is concentrated among students ages nine to 13. The psychology literature on children's behavior is also consistent with this idea. To this regard, Hays and Carver (2014) show that when parents lie to their children, they tend to be more dishonest, but this is true only for those old enough to internalize the treatment. As I explain below, the evidence of a differential effect on secondary versus primary school students is suggestive, but not definitive.

⁵Each year, students go on to the next grade and, therefore, the older cohort exits the sample and is replaced by a younger cohort less affected by the original exposure to corruption. For this reason, one would expect to see a phasing out of the effect.

⁶This is consistent with social learning theories in sociology (see Bandura and Walters (1977)) as well as the literature on the social transmission of values in economics, in line with Bisin and Verdier (1998), Bisin and Verdier (2010), or Acemoglu and Jackson (2014).

differences model using the month of the release of the audit reports as the threshold dividing “before” and “after” in each year. I control for municipality fixed effects, time-varying municipality variables, and a set of individual-level characteristics, thus comparing the answers of people of similar age, living in the same municipality, and with comparable job status and educational level, who were randomly interviewed before or after the date the audit report was released. I find that, after malfeasance becomes public, citizens significantly change their self-declared values related to honesty, trustworthiness, and the importance of abiding by the rules.

Finally, I present suggestive evidence against potentially competing hypotheses. An alternative explanation could, for instance, be that cheating was driven by teachers and not students. More specifically, it may be that mayors were politically connected with principals and teachers and attempted to inflate test scores around elections, especially if reports showed that they were corrupt (in line with evidence provided by Brollo, Kaufmann and La Ferrara (2017) and Akhtari, Moreira and Trucco (2017) in Brazil). I show, however, that the effect is not larger either among public schools (which are more likely to be politically influenced than private schools) or during election years. Another plausible hypothesis could be that students interpreted the release of corruption reports as a signal that punishment for misbehavior is mild (thus reducing the expected punishment for cheating). However, I demonstrate that the main effect is not influenced by the size of the classroom (a proxy for the risk of being caught cheating). This suggests that, even in environments in which the likelihood of being discovered is low (thus the harshness of the punishment should not be too relevant), the revelation of corruption has a large effect on cheating.

This paper contributes to three different strands of the literature. First, it builds on studies of the determinants of civic attitudes by showing how a transient factor (i.e., a leader’s negative example) can shape citizens’ values and honesty, and more broadly the effect of present events on different morals and beliefs. To this regard, Ananyev and Guriev (2013) show that recessions have a negative impact on trust, and Depetris-Chauvin, Durante and Campante (2018) demonstrate that individuals are more likely to trust people of other ethnicities after a victory of their country’s national football team. In a similar vein, Murthi and Tiongson (2009) find that

having experienced socialism increases the preference for redistribution; Alesina and Fuchs-Schündeln (2007) show that having lived under a communist regime affects preferences for redistribution amongst East Germans; Di Tella, Galiani and Schar-grotsky (2007) reveal that giving land titles to squatters has an immediate effect on their beliefs about free markets; and Giuliano and Spilimbergo (2013) furnish evidence showing that growing up during a recession affects future market beliefs.

Second, this paper adds to the growing literature on the role of leaders and the power of example. In particular, Acemoglu and Jackson (2014) provide a setting in which to study leadership-driven changes in social norms. My paper provides the first empirical evidence that this mechanism applies to the formation of civic norms. The findings here are also consistent with the sociological intuitions behind social learning theory (Bandura and Walters (1977), Akers et al. (1979) and Akers (2011)), according to which people engage in dishonest behavior in imitation of others. They also relate to the literature on the economics of organizations, especially to the models of leading by example developed by Hermalin (1998). The results of this paper particularly align with those of d’Adda et al. (2017). Using a laboratory experiment, the authors show empirically that leaders influence the ethical conduct of followers (firms) through their prominent corrupt statements.

Finally, this paper points to a new channel through which corruption harms development: its affect on the cultural formation of young generations. We already know that corruption affects efficiency (Olken and Pande (2012)), growth (Mauro (1995)), the allocation of government expenditures (Mauro (1998), Svensson (2005)), the provision of public goods (Del Monte and Papagni (2001)), education (Ferraz, Finan and Moreira (2012)), and private investment (Svensson (2003), for instance). This paper adds a novel, important aspect to the existing literature on the costs of corruption.

The paper is structured as follows. Section 2 describes the data. Section 3 introduces the main specification. Section 4 presents the main results of the effects of corruption on cheating, together with robustness checks, and the pre- and post-treatment analyses. Section 5 discusses the main results of the effect of corruption on civic values. Section 6 provides suggestive evidence on potential mechanisms, and

Section 7 concludes.

2 Data

2.1 Cheating Data

The main outcome tested here is the proportion of students that cheated on a specific standardized exam. The exam, called “*Prueba Enlace Básica*,” is a compulsory national standardized test (mathematics and Spanish plus an additional subject) implemented by the Federal Government each year between 2006 and 2013.⁷ Initially, from 2006 to 2008, the test was given only to primary students in third through sixth grade (8-11 years old) and to students in their third year of secondary school (14-15 years old). In 2009, students in their first and second year of secondary school were also included as test-takers. The exam was typically taken between mid-April and mid-July in both public and private schools. It was created and coordinated by the Federal Ministry of Public Education and organized locally by the equivalent authority in each state. For security reasons, each test was printed and distributed by the government established “National Commission of Free Text-Books.” Beyond the students, many people were involved in the administration of the test: an external coordinator (representing the Mexican Ministry of Public Education), schoolteachers, and other external viewers invited by the schools. Ultimate responsibility for the correct administration of the test rested with the external coordinator, charged with distributing the material within the school, verifying that there were sufficient external supervisors, and ensuring that each schoolteacher was allocated to a classroom other than the one they usually taught in. Finally, in an effort to identify students who still somehow managed to cheat, the results were analyzed using a software program designed to detect student-to-student cheating. The software uses

⁷Although the test is compulsory, the effective coverage varied slightly over these years due to administrative problems in its implementation. There are consequently some periods with more observations and others with fewer. The point estimates of the main results using only the schools that were present in each period of the sample are almost identical and the significance unaltered, even with larger standard errors. Tables available upon request.

two complementary algorithms: a K-Index and a Scrutiny Method, both designed to identify excessively similar patterns of incorrect answers between any pair of tests within a classroom. The variable used here as the objective measure of dishonesty is the proportion of cheaters per classroom identified by the software.⁸

In this paper, I focus on the years with valid observations of cheating and corruption: 2006 to 2013. Descriptive statistics on cheating can be found in Table A1 in the Appendix (Figure A1 in the Appendix also shows a kernel estimation of the distribution).

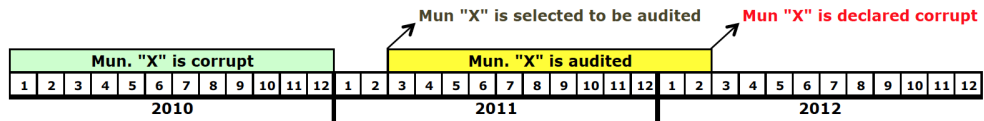
2.2 Corruption Data

The explanatory variable is government corruption. Instead of using perception variables, I focus on directly observed (and published) corruption, measured by a federal independent agency. In Mexico, a large proportion of public funds is spent at the local level. The 2,446 municipalities in the country thus represent a potentially important source of corruption. The audits, performed by the *Auditoría Superior de la Federación* (ASF), an autonomous, independent agency that is formally part of Congress, clearly reveal whether any deviation of the funds has occurred. For admin-

⁸The K-Index method has been widely used by testing companies. In particular, it is the method used to detect cheating on tests such as the GRE, the SAT, and the TOEFL. The original algorithm was popularized by Holland and Thayer (1986), and a comprehensive review of its methods can be found in Cizek (1999). Broadly, this kind of software computes a similarity index of wrong answers among all the possible pairs of exams within a classroom and compares the latter with a theoretical distribution of similarities under a no-copy hypothesis, conditional on a set of parameters that include the number of questions, options, students, correct answers, and confidence. The software then identifies as "cheaters," those pairs of exams in which the similarity index of wrong answers is unusually high. It is important to emphasize that while the software identifies cheating between two students within a classroom, it is not designed to capture teacher cheating, such as altering students' responses on answer sheets or directly providing correct answers. To detect this type of cheating, it is necessary to identify "unusually good" results, as opposed to a similar pattern of wrong answers (i.e., if a teacher dictates the correct answers, or some of the correct answers, then the wrong answers are unlikely to be particularly similar). Identifying "unusually good" results is not as straightforward because the counterfactual is difficult to assess. In principle, "unusually good" must be defined in terms, for example, of observable socioeconomic characteristics of the school, the students, and their community. Moreover, while the statistics literature on identifying whether a student copied answers from another student was developed as early as the 1970s, the first paper to propose a method to identify teacher-induced cheating is Jacob and Levitt (2003).

istrative reasons, until 2015, the results were published with a lag of around two years after the actual corruption occurred, which is crucial for the identification strategy of the paper. Each year, the ASF defined and announced which municipalities were going to be audited. The process itself took an entire year and, therefore, the results were not published until February of the following year, as Figure I illustrates with a hypothetical example of a corrupt municipality.

Figure I: Hypothetical Audit Timeline

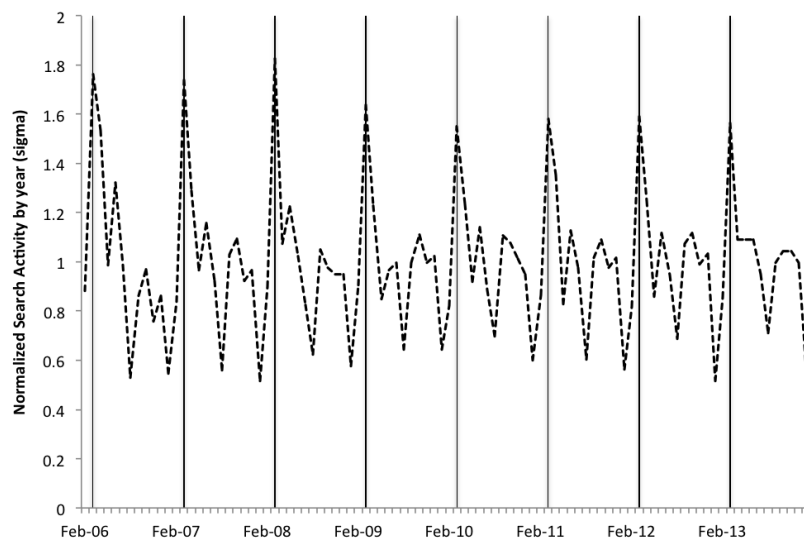


Note: The figure illustrates a hypothetical example in which the mayor of Municipality “X” diverts funds from their legal purposes during 2010. At the beginning of 2011, the municipality is selected to be audited; the process takes a year, and in February of 2012, the results, which correspond to 2010, are published.

One of the most relevant funds that is audited each year is municipal aid transferred from the federal government under the label “Municipal Fund for Social Infrastructure” (FISM in Spanish). The FISM is important not only because of the amount of money it represents (more than 25% of the local budget, on average), but also because it is earmarked to improve infrastructural projects targeting the poor; thus, events of malfeasance related to such funds tend to reach the media very quickly. Following Larreguy, Marshall and Snyder Jr (2014), Arias et al. (2018), and Chong et al. (2014), I focus on the results of these FISM audits to identify corruption, which report the share of “unauthorized use” of such funds - the variable I use to measure corruption in a municipality. Importantly, each year, the audit reports were released approximately three months before the school tests were taken. The release of municipal audit results each February is a popular event at the local level. Figure II shows the Google searches related to audit reports by month in the period 2006-2013, normalized by yearly average. There is a clear peak every year precisely

in the month of February, when the reports are released. News reports typically cover local mayors, with a focus on cases of unauthorized spending.⁹

Figure II: Google Searches of “*Auditoria*” in Mexico



Note: Extracted from Google Trends. The word “*auditoria*” means “audit” in Spanish. Horizontal bars mark the month of February. Values normalized by yearly mean.

Over the sample period, there were 930 audits, an average of 117 per year. More than 54% of the audits revealed some degree of corruption, and, on average, 8% of the funds were labeled as unauthorized spending (further details to this regard are presented in Table A2 in the Appendix). By 2013, more than a quarter of the municipalities had been audited at least once.¹⁰

⁹When there are irregularities, the results of these audits are typically published very promptly by the media, especially at the local level. Among the numerous examples online, see, for instance: “Abarca embezzles 66 million pesos in 2013” ([http://www.milenio.com/politica/Jose_Luis_Abarca_Guerrero\protect\discretionary{\char\hyphenchar\font}{-}{ASF_Abarca_desvio\protect\discretionary{\char\hyphenchar\font}{-}{Abarca_Iguala_Guerrero_0_467353346.html}](http://www.milenio.com/politica/Jose_Luis_Abarca_Guerrero\protect\discretionary{\char\hyphenchar\font}{-}{ASF_Abarca_desvio\protect\discretionary{\char\hyphenchar\font}{-}{Abarca_Iguala_Guerrero_0_467353346.html)) or “88 million pesos disappear under former Mayor of Huamantla” (<http://www.e-tlaxcala.mx/nota/2014-02-11/municipios/ex-alcalde-de-huamantla-desaparece-88-mdp>).

¹⁰Figure A2 in the Appendix shows a kernel density estimation of the amount of unauthorized expenditures.

2.3 Personal Values Data

To test the hypothesis regarding the channels, I complement the analysis with survey data on values. In particular, I use the longitudinal Mexican Family Life Survey (MxFLS).¹¹ Currently, the MxFLS contains information on an 11-year period (with some gaps), collected in three waves: 2002, 2005-2007 and 2009-2013. I focus on the questions related to values concerning honesty, rule abidance, and self-declared trustworthiness on five items relevant to the analysis.¹² I then build four different aggregated indices of civic-mindedness (including the first principal component of a PCA), which are summarized in Table A3 in the Appendix.

2.4 Political Perception Data

I exploit two data sources on political parties and party-corruption perception in order to estimate heterogeneous effects. First, I use a database containing all the mayors and their party affiliations for the period of analysis. This data comes from the National Institute for Federalism and Municipal Development (INAFED, in Spanish), an official federal but decentralized organization responsible for coordinating policies between different governmental levels. The institution maintains a database called the “Encyclopedia of Municipalities” in coordination with local government bodies such as municipalities and delegations. I combined the latter with official data from municipal government websites where any information was missing.

Second, to measure the perception of transparency and honesty of each party, I use a state-representative survey implemented by a private, independent firm (Gabinete de Comunicación Estratégica) that assesses public opinion. One question in the survey asks, “If the political parties in the State of X were persons, which party would you say is the most corrupt?”. People chose the adjectives they identified most strongly with each of the three main parties in every state. I use the percentages assigned to “Most Corrupt” to classify parties according to citizen perceptions. I

¹¹<http://www.enmvih-mxfls.org/english/index.html>.

¹²The exact wording of the questions can be found in the Appendix.

then match these percentages with data on the incumbent party in each period and municipality. This provides a measure of the proportion of people that perceives the party in power as corrupt. The survey is not performed on a regular basis, and the most recent version corresponds to the first year of this paper’s sample period. The results of the survey are normalized to a 0-100 scale and summarized in the Appendix (Table A4).

Although these three parties cover most of the sample (around 75%), many municipalities were governed by small (sometimes local) parties or by coalitions that, in some cases, were strongly influenced by one of the three large parties. To classify the smaller parties or coalitions according to their level of perceived corruption, I use an additional “Electoral Data-Base,” created by an independent think tank (CIDAC), which includes the main national party (usually one of these three) that supports the incumbent (if any) and the main parties represented in each coalition.

3 Empirical Model

I exploit the high detail and panel structure of the data (at the year-school-grade-level) and the fact that not all the municipalities are audited at the same time to estimate a multi-year difference-in-differences model. More specifically, I estimate the following equation:

$$PropCheat_{sgt} = \alpha Corrupt_{mt} + \nu ReportReleased_{mt} + \phi_s + \rho_g + \gamma_t + \lambda X_{mt} + \epsilon_{sgt} \quad (1)$$

Where $PropCheat_{sgt}$ is the proportion of students that cheated in school s , in grade g during year t ; and $Corrupt_{mt}$ is a variable indicating that there was corruption detected in the municipality m during the year t . This means that the year t corresponds to the period in which the results of the audit were published, not to the year in which corruption actually occurred. A full set of controls at various levels is included in the model, allowing to compare very similar observations: fixed effects at the school level ϕ_s to rule out the possibility of confounding the effect of corruption with the idiosyncratic time-invariant effect of the schools; grade fixed effects (1st ,

2nd , 3rd grade, ρ_g); and period effects (γ_t) to control for any year-specific shock. I also include a complete set of time-varying controls at the municipality level (X_{mt}): dummies for political parties - for both the year of the actual corruption event and the year when the report was released; a dummy indicating if the municipality was audited in the past; and a dummy indicating if the municipality was already corrupt in the past; plus homicides per-capita by municipality-year;¹³ the amount of local taxes collected in each year-municipality; and a dummy that takes a value of 1 if there was political alignment between the local and national governments, and 0 otherwise. I also include municipality time-specific linear trends so as to account for the presence of potential differential trends in cheating across municipalities due to unobserved local time-varying effects. All results are clustered at the municipality level to allow for intra-municipality serial correlation, and there are around 50 schools per municipality, on average.

In each specification, I include a dummy indicating whether there was an audit report (which could show corruption or no corruption) released for any given municipality in each year ($ReportReleased_{mt}$). This allows to rule out any potential issue related to selection on auditing (e.g., if the timing of the audit was not random). The inclusion of this dummy is also important for the interpretation of the results: the parameter of interest (γ) measures the difference between cheating in a school located in a municipality that was audited and declared corrupt in a given year versus a school located in a municipality that was audited and declared non-corrupt in a given year.

Finally, I restrict the sample to only those municipalities that were audited at least once during the sample period to reduce any potential problem that could arise if the group of municipalities that were audited were systematically different from those that were never audited. Results are presented in the following section, including a set of different robustness tests.

¹³Crime in Mexico is a particularly relevant variable that has been shown to be correlated with a number of relevant economic outcomes such as firm-level activity (Montoya (2016)) or property prices (Ajzenman, Galiani and Seira (2015)). I use a dataset containing monthly municipality census data on homicides throughout Mexico compiled by the National Public Health Information System.

4 Main Results and Robustness

The main results for secondary school students are presented in Table I. The five columns reflect robustness tests, where a different set of controls is added in each column.

In every specification, I find positive and significant results at the 5% level and of similar magnitude, regardless of the inclusion/exclusion of municipality-specific trends or municipality time-varying controls (such as homicides per capita; local tax collection; whether the municipality had already been audited or already declared corrupt in the past; party in government dummies; and a dummy indicating party alignment between local and national governments).

The magnitude of the preferred specification (column V) is sizable: cheating in corrupt municipalities increased approximately 9-10% on average, with respect to the baseline. In Table I, I also show the estimation of the dummy variable “Report Released,” which takes a value of 1 if there was an audit report released for that municipality. Interestingly, although the estimation is not precisely estimated enough to detect a significant effect, the point estimate is negative and relatively sizeable. This suggests that there made be two sides to the effect of corruption on cheating: a positive result decreases cheating, while a negative result increases cheating (and the parameter of interest reflects the combination of both). Nonetheless, this interpretation should be viewed with caution. If the timing of the release of the audit reports is not completely random, then the only parameter that would have a causal interpretation would be that associated with the corruption variable. Controlling for the “Report Released” dummy allows to measure the effect of corruption on cheating among municipalities that were audited in the same year.

The findings of the main model are also robust to different specifications and definitions of the main explanatory variable. In Table II, I show the results hold when using different definitions of the corruption variable, when including fixed effects at the municipality level (instead of the school level), and when restricting the sample so as to keep observations of municipality-years in which a corruption report was

released. The first row (>0) presents the results of the preferred specification: a municipality is considered corrupt at any level of unauthorized expenditure greater than zero. In the second row (P15), a municipality is considered corrupt if the proportion of unauthorized expenditure is greater than the 15th percentile of the sample, and in the third row (P25), if the proportion of unauthorized expenditure is greater than the 25th percentile of the sample. Finally, in the last row (log), I use a continuous definition of corruption (the log of the proportion of corruption). For each of these models, I present the results with different sets of controls (columns I to IV) and using municipality fixed effects instead of school fixed effects (column V). Finally, I also estimate the main model but keeping only the observations corresponding to the municipalities for which an audit report was released in any given year (that is, dropping all the municipalities/years that did not have reports in a particular year). The results are presented in column VI.¹⁴

All the results remain significant and of similar magnitude, regardless of the definition of corruption and the inclusion of school or municipality fixed effects. The last row of the table, which presents the effects in logs, also adds an interesting interpretation of the results, as it shows that not only the extensive margin (corrupt versus not corrupt), but also the level of corruption, matter.

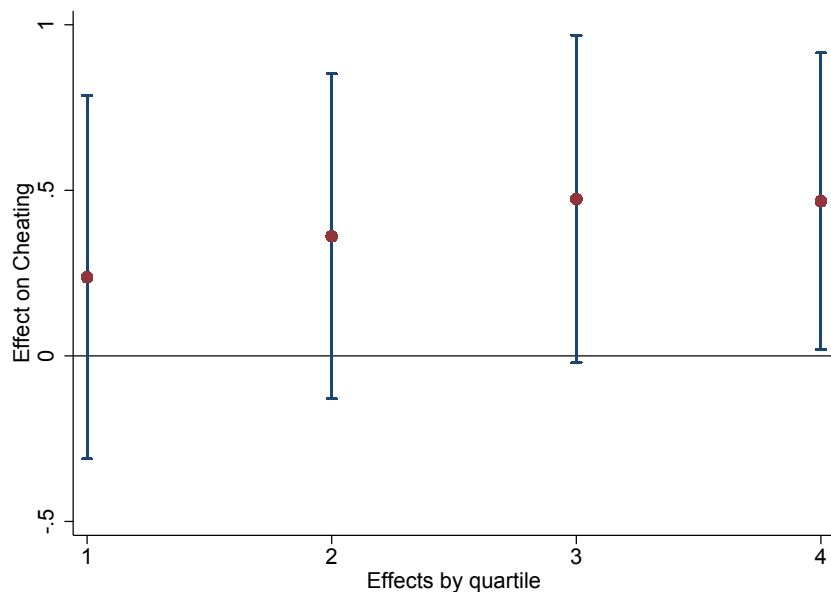
Along similar lines, it is interesting to see how corruption has a differential effect depending on the magnitude of malfeasance. There are a number of reasons to think that low levels of corruption should have a smaller effect. First, they could be interpreted as administrative faults rather than as corruption. Second, it may be more difficult for a corruption scandal to become massively public if the proportion of misallocation is very low (e.g, writing a corruption story about 0.1% of public funds being diverted).

Figure III shows the results of estimating the main equation but dividing the treatment into quartiles of corruption. The effect seems to be monotonically increasing in the level of corruption, although it becomes significant only at high levels. In

¹⁴Results controlling for municipality-specific linear trends are not presented in Column VI because most of the municipalities/schools are not repeated in the sample (as they were audited only once) and, therefore, trend dummies are collinear with municipality or school dummies.

other words, corruption seems to affect people's behavior only when its magnitude is sufficiently large.

Figure III: Main Effect by Quartiles of Corruption – 95% CI



Finally, another compelling question to analyze is how corruption affects cheating at different ages. Related papers in the literature (see, for example, Madestam, Yanagizawa-Drott et al. (2011)) have shown that certain events can have drastically different effects on behavior depending on the age at exposure.

In the case considered here, younger students are arguably less exposed to political discussions, particularly in Mexico (Segovia et al. (1975)). To test this, I estimate the main model using primary school students. Although the samples may not be strictly comparable (secondary and primary schools are qualitatively different, as they are different types of institutions), the results in Table III show that, as expected, primary school students were much less affected than secondary school students by corrupt leaders. Indeed, the effect on the former is indistinguishable from 0. The fact that older students (12-15 years old) changed their behavior but younger students (8-11) did not, suggests that the exposure to corruption influenced

only those who were more likely to have been exposed to politics and, thus, to the treatment.

4.1 Pre-treatment and Persistence

To formally test whether there were anticipation effects (leads, interpreted as a placebo) and/or persistence in the main effect during the years after corruption occurred, I estimate an event-study model along the lines of Autor (2003). More specifically, I estimate a model similar to Equation (1) but now including two leads and two lags:

$$\begin{aligned}
 PropCheat_{sgt} = & \sum_{l=-2}^{+2} \alpha_l Corrupt_{mt-l} + \alpha Corrupt_{mt} \\
 & + \sum_{l=-2}^{+2} \nu_l ReportReleased_{mt-l} + \nu ReportReleased_{mt} \quad (2) \\
 & + \phi_s + \rho_g + \gamma_t + \lambda X_{mt} + \epsilon_{sgt}
 \end{aligned}$$

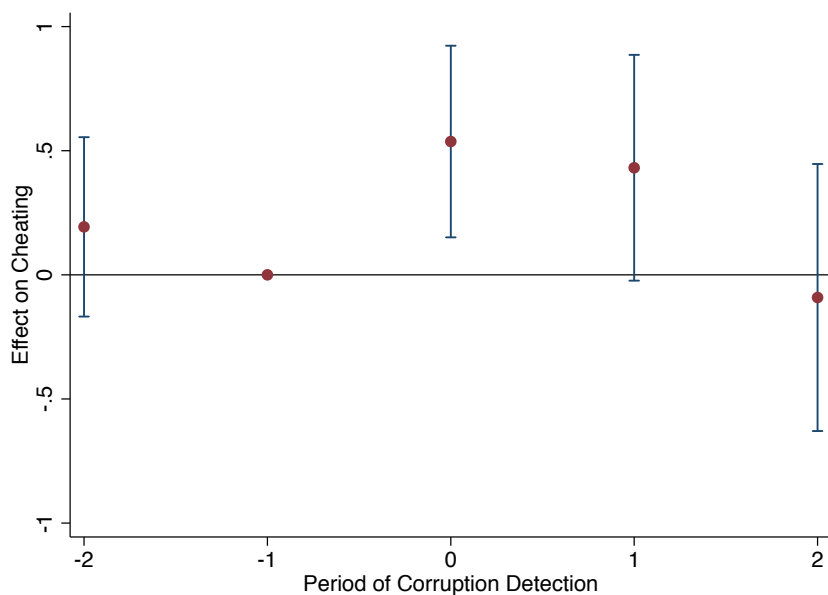
The model also controls for the "report released" dummy, indicating whether or not an audit report was released during a given year for each municipality (plus two leads and two lags of the "report released" dummy). The interpretation is thus identical to the main model for every period.¹⁵

¹⁵The law that regulates the allocation of funds from the national government to sub-national administrations significantly changed starting in 2014 (the decree is from December, 2013 - it therefore impacted the audit reports corresponding to 2014 and then released in 2016). In particular, the rules that regulate how the money from FISM could be spent were transformed, potentially affecting the measure of unauthorized funds. Thus, in order to ensure comparability of my corruption indicator, I restrict the years of my sample to 2015 (which does not affect my main analysis, but restricts the number of lags to two). The reform occurred due to complaints about imprecision in the way the regulation was written relative to what was authorized (or not) to be spent. As a result, the government altered a law through a decree, urging the Secretary of Social Development and the National Council for the Evaluation of Social Development Policy (CONEVAL, an independent government institute in charge of measuring poverty) to create and publish a detailed catalog of specific items on which FISM funds - meant to target the poor - could be spent. Since 2014, the ASF has consequently used this catalog as the instrument to determine the proportion

As Figure IV shows, pre-treatment effects for two of the periods are not significant at the 5% level (or 10% level). This is interpreted as placebo, as it demonstrates that there are no anticipation effects. The figure also suggests that the effect may have persisted for at least one period after the exposure to corruption, as the p-value for the first post-treatment period is 0.062. In the second period, after the audit reports are released ($t=2$), the effect becomes zero again. As students cannot be followed for more than two years, it is not possible to properly analyze the long-term persistence of the effect. However, the phasing out seen in the figure seems reasonable and consistent with a persistent effect. This is because, each year, a third of the students in the sample is replaced by a younger cohort, less affected by the exposure to past corruption, since they were of a younger age at the time. For example, in the second year post-exposure, a third of the sample consists of students who were affected by corruption when they were in their first year of secondary school, another third were in their last grade of primary school, and the remaining third were in their penultimate grade of primary school.

of FISM misallocation. This change is evident when reviewing the ASF reports, which, since the 2016 publication (analyzing the fiscal year of 2014), specifically document the proportion of money spent on items not included in the catalog. The fact that the rules became much clearer and that the catalog included a vast number of items likely impacted the proportion of unauthorized spending from 2016 onward (it fell to below 2% on average in 2018 versus, for instance, around 30% in 2011). The catalog and the new regulation is available online: https://www.coneval.org.mx/Evaluacion/NME/Paginas/acuerdo_lineamientos_fais.aspx. A non-technical explanation of the reform, described by the Ministry of Social Development is also available in the following presentation: <http://www.sedesol.gob.mx/work/models/SEDESOL/Sedesol/sppe/upri/dgap1/fais/Noticias/presentaciones/03.pdf>. Finally, the official decree is available at the following link: http://dof.gob.mx/nota_to_pdf.php?fecha=09/12/2013&edicion=MAT (page 13, referring to Article 33). The previous version of the law is available at the following link: http://www.diputados.gob.mx/LeyesBiblio/ref/lcf/LCF_ref18_31dic98.pdf (first page, referring to Article 33).

Figure IV: Leads and Lags – 95% CI



Note: 95% Confidence Intervals, 1st pre-treatment period normalized to 0. Corruption defined as "proportion of unauthorized expenditure greater than zero." Controls for school fixed effect, municipality-specific linear trends, and all of the controls described in Section 3.

For robustness, Figure A3 in the Appendix shows the estimations of the same models with different sets of controls. The results are robust to the inclusion/exclusion of municipality linear trends and time-varying municipality characteristics. As a robustness check, I also estimate the same models but keep only the observations corresponding to the municipalities for which an audit report was released in any given year (that is, dropping all the municipalities/years that did not have reports in a particular year). I present suggestive results in the Appendix (Figure A4): the pattern remains almost unchanged, although significance is lost, as the number of observations is heavily reduced.

5 The Effect of Corruption on Civic-Mindedness

A related question, crucial to the interpretation of the results, is whether corruption affects social norm values such as honesty and civic-mindedness (for instance, whether people learn that, in order to succeed in life, it is necessary to act unlawfully).

To test this, I use data from a longitudinal survey called the Mexican Family Life Survey (MxFLS). I work specifically with the five questions in the survey that refer to civic values (honesty, rule abidance, trustworthiness, cheating, etc.) and use them to construct four indices of incivic values:¹⁶ (1) at least one incivic answer; (2) at least two incivic answers; (3) first component of a principal component analysis of the five questions (normalized to a 0-1 scale); and (4) the count index: sum of incivic answers (min=1, max = 5).¹⁷

An advantage for the identification strategy is that, for administrative reasons, each wave includes three years of interviews that were conducted during different months. This means that some households were surveyed just before the audit reports were released and some just after.¹⁸

More specifically, I estimate a model in which I exploit the plausibly exogenous difference in the timing of the interviews relative to the timing of the release of the audit report. This allows to estimate a difference-in-differences model using the month of the release of the report as the threshold dividing before and after in each year. I am also able to control for municipality fixed effects and a set of individual-level characteristics so as to compare the answers of people of similar age, gender, job status, educational level, and living in a municipality that was audited

¹⁶The exact wording of the questions and criteria used to build the indices are provided in the Appendix.

¹⁷Table A3 in the Appendix shows the descriptive statistics for these indices and for the individual questions.

¹⁸The MxFLS is an academic project led by Mexican and American universities (Universidad Iberoamericana and CIDE in Mexico, UCLA and Duke University in the US), unrelated to any local government. Therefore, it seems reasonable to consider that for a given municipality and year, the decision to interview a family before or after the month in which the report was released was unrelated to the characteristics of the households.

in a particular year, with the only difference being that some families were surveyed before the report was released and some afterwards. Furthermore, the time effects (before or after the cut-off month) allow to control for seasonality. The model is as follows:

$$Values_{imtf} = \alpha CorruptAfter_{mtf} + \phi Corrupt_{mt} + \eta After_f + \gamma_t + \theta_m + \lambda X_{mt} + \phi Y_{imt} + \epsilon_{imt} \quad (2)$$

Where $Values_{imtf}$ represent the outcome variable(s) with answers to the different questions about values (answered by an individual i , living in municipality m , during period t , and interviewed before or after the month in which the audit report was published, f). $CorruptAfter_{mtf}$ is the variable of interest, and is the interaction between $Corrupt_{mt}$ (a dummy that scores 1 if the municipality m is corrupt during a particular period t) and $After_f$ (a dummy that takes on a value of 1 if the interview of the individual was performed before the month of February, when the reports were released, and 0 otherwise). The model also includes municipality fixed effects (θ_m), period effects (γ_t), all of the same municipality-level controls used in the main specification, plus individual-level controls (age, education, and labor status dummies, ϕY_{imt}). I restrict the sample to consider all municipalities that were audited (and therefore received a corruption report) in a given year. Thus, these models do not include a "released" dummy.¹⁹

The estimation of the main effects can be found in Table IV. The table presents four different specifications, including the various sets of controls defined in Section 3. I present the results normalized by standard deviations to make the indices comparable. As can be observed, all the point estimates of the main effect are positive and significant, most at the 1% level, with a few significant at the 5% level. The magnitude of the effects is generally sizeable.²⁰

¹⁹For any given year I consider a window of six months before the exposure to corruption and six months after the exposure, and exclude the month in which the report was released.

²⁰For robustness, I also present the results using different thresholds of corruption in Table A5 in the Appendix. In Table V, I present the results for the individual variables that comprise the indices. As expected, the estimation is positive in every variable and significant in most of them.

These results are consistent with the literature in economics (see, for example, Bisin and Verdier (1998), Bisin and Verdier (2010)), psychology (see, for example, Mazar, Amir and Ariely (2008)), and social learning theories (Bandura and Walters (1977)). There are also parallels to the work of Acemoglu and Jackson (2014), who show that when a leader is deemed a good (or bad) example, people learn new social norms and act accordingly.

6 Potential Mechanisms

Why do people change their behavior and self-reported values when they are exposed to government corruption? One plausible mechanism behind the main effect could be a process of social learning whereby people observe their leaders' behavior, learn something new about norms within their community, and change their own intrinsic values as a result. In this section, I provide suggestive evidence consistent with this channel.

6.1 Corruption and Local Media

A fundamental aspect to understanding the channels by which the treatment operated is how people found out about the corruption. Anecdotal evidence suggests an interest in revelations of corruption (the Google searches presented in Figure II are, for instance, consistent with this view). Moreover, as Larreguy, Marshall and Snyder Jr (2014) show, local media seems to be the most relevant actor in terms of dissemination (i.e., while there are a few TV stations that cover mostly national news, the focus of such media tends to be more local), a reasonable observation given the high level of media penetration in Mexico.²¹

Importantly, the estimated parameter associated with the question on the importance of cheating to get ahead in life is highly significant and large. In addition, I provide the results of a placebo analysis in Table A6, which show the insignificant effect of corruption on a set of questions unrelated to civic values.

²¹As an example, the 2010 Census reports that more than 90% and 80% of households had a TV or radio, respectively. According to the Latinobarometro (2010), more than 50% of the population

As in many developing countries, radio and television networks are the principal source of news in Mexico.

If corruption scandals are disseminated through the media, one would expect households with potentially higher exposure to media to show a larger treatment effect. To test this hypothesis, I use two data sources related to media broadcasting and consumption. First, I calculate the proportion of households that owned a radio in the 2010 census. The structure of the dataset allows to obtain data at the locality level and, thus, the possibility of observing within-municipality variability in potential exposure to news.²² Although average radio ownership is relatively high, there is still a considerable amount of variability (a mean of 0.59 radios per locality, a standard deviation of 0.14, a minimum of 0, and a maximum of 14).

Second, I utilize the number of radio stations that broadcast from each municipality, compiled by the Federal Electoral Institute (IFE) and accessed for this project. This data provides me with an objective measure of the supply of local media outlets.²³

Figure V shows the geographical distribution of radio stations. Considering only the municipalities under analysis, there are, on average, 3.5 stations per municipality (the standard deviation is 4.6 and the median is 2), and around half of the municipalities have at least one station.

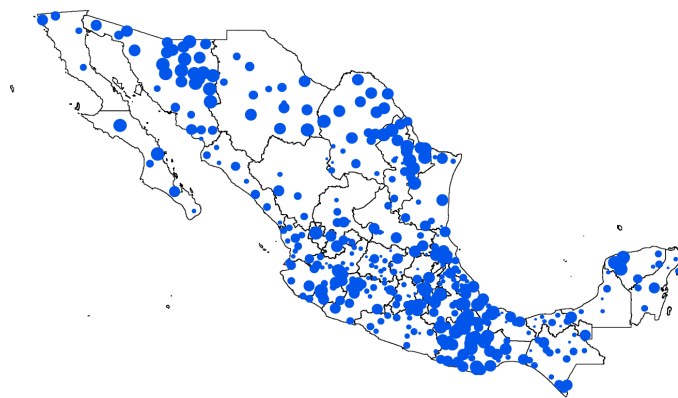
To measure potential exposure to news, I create a locality-level indicator of “high exposure” combining local radio station supply and household radio ownership, which takes a value of 1 if two conditions are fulfilled: a) the municipality in which the locality is found has an above-median number of per-capita radio stations; and b) the proportion of household radio ownership in the locality is above the sample median of

gets informed by listening to the radio.

²²Municipalities are divided into localities.

²³Unfortunately, it was not possible to retrieve reliable data on TV stations for the sample period (between 2006 and 2013). Only more recent data are available, but it would be misleading to use them because starting in 2011 there occurred a significant shift from analog to digital TV and, therefore, the current stations are not necessarily representative of those in existence up until 2011. However, as Larreguy, Marshall and Snyder Jr (2014) point out, radio and TV stations were highly spatially correlated, and, therefore, conclusions should be similar using either or both types of media.

Figure V: Geographical Distribution of Radio Stations



Note: The figure illustrates the number of radio stations (AM and FM) by municipality. Bigger dots represent a higher number of stations. There are 10 categories: 0, 1-2;2-5;5-8;9-11;12-13;14-16;17-20;21-24;25-28.

all localities. Slightly less than a quarter of the sample falls into the "high exposure" category.

I estimate equation 1 adding an interaction between corruption and a dummy ("high exposure"). In every specification, I include the interaction between the "Report Released" dummy and the "high exposure" variable as a control. I also control for the interaction of the treatment variable (corrupt) with several municipality controls (local tax collection, homicides per capita, and the dummy indicating if the party of the corrupt mayor was politically aligned with the national party in power). I also include interactions of these municipality controls with the "Report Released" dummy.

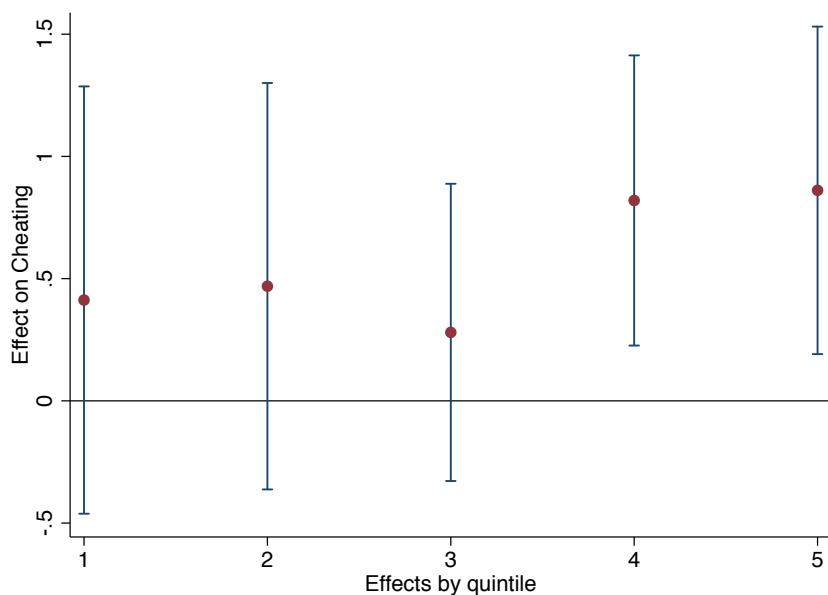
In Table VI (rows I and II), I show the results using a different set of controls. The effect of corruption on cheating becomes significant (and of a large magnitude) only

in those localities where there is a high exposure to media. The difference compared to the low-exposure municipalities is significant at (at least) the 10% level in every specification.

To complement this assessment, I also study the heterogeneous effect by different levels of potential exposure to media. To this end, I construct a continuous indicator which takes a 0 value if there are no radio stations in the municipality where the school is located and X otherwise; where X is the average household radio ownership at the locality level. This indicator provides within-municipality variability of potential radio consumption (at-home radio availability) but is altered by the actual supply of local radio in the municipality. The underlying rationale is that - within a certain municipality - the higher the propensity to own a radio, the higher the likelihood of hearing about the corruption scandal, but only if there is a local radio broadcaster to actually disseminate the news.

In Figure VI, I present this heterogeneous effect by quintiles of potential exposure to media. The average effect seems to be increasing as access to media grows, and is only significant for the last two quintiles.

Figure VI: Main Effect by Quintiles of Exposure to Media – 95% CI



Note: Clustered standard errors in parentheses (municipality).

Model includes the full set of controls as described in Section 3, plus the interaction of the "corrupt" and the "Report Released" dummies with the included time-varying municipality variables (tax collection, homicide rate, and a dummy indicating party alignment between local and national governments). It also controls for the interaction of the "Report Released" dummy with the "high exposure" dummy.

These results are consistent with the local media being a crucial channel by which the treatment is disseminated, as well as are aligned with the findings of Larreguy, Marshall and Snyder Jr (2014), who show the importance of local media in spreading corruption news in Mexico. Moreover, they provide additional support to the claim of exogeneity, as they demonstrate that the main effect is significant only among those localities that were more likely to be affected given their higher potential exposure.

6.2 Test of Informational Update

If the corruption scandals were signals that people interpreted as new norms of (dis)honesty (or at least, new to them), then the main effect should have been

greater in municipalities in which the incumbent was thought to have been honest. Conversely, if the scandal did not reveal any new or surprising information, then citizens would not have updated their values and changed their behavior, as they would not have learned anything new about the social norms of their communities.

I test this hypothesis by showing that the effect of corruption on cheating was most pronounced in municipalities where the party in power was not the one perceived as the “corrupt party” ex-ante. More specifically, I once again estimate equation 1, but now add an interaction between corruption and a dummy (“perceived corrupt”) that takes a value of 1 if the party in power during time t in municipality m was perceived as corrupt (ex-ante), and 0 otherwise. To classify each political party as “perceived corrupt,” I use the combined datasets described in the Data Section. In particular, I classify as “perceived corrupt” municipalities in which the proportion of people viewing the incumbent party as corrupt in any given period exceeds 50% (on a normalized scale from 0 to 100).

In each specification, I include the interaction between the “Report Released” dummy and the “perceived corrupt” variable as a control. I also control for the interaction of the treatment variable (corrupt) with several municipality controls (local tax collections, homicides per capita, and the dummy indicating whether the party of the corrupt mayor was politically aligned with the national party). The models also include interactions of these municipality controls with the “Report Released” dummy.

Results are shown in Table VI (rows III and IV), including a different set of controls. The sign of the interaction term is, as expected, negative and significant at the 10% level. Moreover, the effect of corruption on cheating becomes notably larger and more significant for the municipalities where the party in power was not initially perceived to be the most corrupt. The effect of corruption on cheating seems, in fact, to be fully explained by the ex-ante “honest” municipalities. It then becomes indistinguishable from zero among the municipalities in which the incumbent party is ex-ante perceived as dishonest (row “Corrupt+Perceived Corrupt” of Table VI).

These results suggest - in line with other papers in the literature such as Arias et al. (2018) - that the exemplar effect of government malfeasance on cheating oc-

curred especially when there was an informational update.

6.3 Other Hypotheses

An alternative explanation is that cheating is driven by teachers (rather than students). One plausible story could be that mayors were politically connected with principals and teachers and attempted to inflate test scores around elections, especially if reports showed that they were corrupt (in line with evidence provided by Brollo, Kaufmann and La Ferrara (2017) and Akhtari, Moreira and Trucco (2017) in Brazil).

To rule out this potential mechanism, I present two pieces of evidence. In Table VII (rows I and II), I show the heterogeneous effect by type of school (private or public). In each specification, I include the interaction between the “Report Released” dummy and the “private school” variable as a control. I also control for the interaction of the treatment variable (corrupt) with several municipality controls (local tax collections, homicides per capita, and the dummy indicating if the party of the corrupt mayor was politically aligned with the national party in power). The models also include interactions of these municipality controls with the “Report Released” dummy.

If the “social norms” channel is the main explanation, I would expect to see a similar effect in private and public schools. On the other hand, if the main channel is related to mayors being politically connected with public schools principals, I would expect to see a much bigger effect among these schools (as private schools are less likely to be affected by politics). As the results in Table VII show, the effect is not significantly different in private and public schools.

The second piece of evidence is directly related to elections. In Mexico, public schools are managed by the state governments (as opposed to municipal governments). If the incumbent party at the state level is the same as the mayor’s party, the incumbent party at the municipal level could collude with the state government to put pressure on principals and teachers in the municipality’s schools to inflate grades during election years. In order to test this, I restrict the sample to municipal-

ities/years in which the state and municipal governments are from the same coalition (around half of the observations) and then estimate the heterogeneous effects by "local election years." If the effect is explained by politically connected principals/teachers and governments, I would expect to see larger effects during election years.

To this end, in each specification, I include the interaction between the "Report Released" dummy and the "election year" variable as a control. I also control for the interaction of the treatment variable (corrupt) with several municipality controls (local tax collections, homicides per capita, and the dummy indicating if the party of the corrupt mayor was politically aligned with the national party in power). The models also include interactions of these municipality controls with the "Report Released" dummy.

Table VII (rows III and IV) shows that the interaction terms are highly non-significant. The effect is not larger during election years. Combined, these two pieces of evidence suggest that the main effect was not, in fact, driven by politically connected principals and mayors.

Another plausible hypothesis competing with the social norms story could be that corruption, when discovered, affected the perceived expected cost for a cheater. More specifically, after observing that the mayor was corrupt, a student might have reasoned that, even if she were caught cheating, the punishment would not be very severe. While ideally, I would like to show that the effect does not vanish when a politician was punished, there is unfortunately not enough variability to conduct such an analysis (judicial punishment is highly unlikely and, in any case, the legal process would take too long).

That said, if the punishment story was valid, I would expect to see a larger effect of corruption on cheating in grades in which the likelihood of being caught was higher in the first place. In environments where, for logistical reasons, catching a student cheating is difficult, a change in the expected punishment shouldn't have a large effect on student behavior.

To formally test this, I run heterogeneous effects by the average size of the classrooms. Cheating in small classrooms is likely easier to catch, thus making the risk

of cheating higher. For students in such classrooms, a change in the punishment for cheating should be more relevant than for those in larger classrooms.²⁴ Although this test is certainly imperfect (classroom size could be associated with other variables that are also relevant for cheating), the result is at least suggestive.

I ran the main regression adding an interaction with the log of students per classroom (normalized to the mean for the sake of interpretation). In each specification, I also include the interaction between the “Report Released” dummy and the “students per classroom” variable as a control. I also control for the interaction of the treatment variable (corrupt) with several municipality controls (local tax collections, homicides per capita, and the dummy indicating if the party of the corrupt mayor was politically aligned with the national party in power). The models also include interactions of these municipality controls with the “Report Released” dummy.

Table VII (rows V and VI) shows that the interaction is highly non-significant, implying that classroom size does not diminish (or amplify) the main effect. Given that cheating is not formally punished at all, these results are not surprising.

7 Conclusions

Do political leaders lead by example? Do they influence our cultural attitudes? In this paper, I investigate one way in which honest behavior and values are significantly affected by the power of example: corrupt politicians generate corrupt citizens.

I show that when government malfeasance becomes public, people quickly behave more dishonestly (as measured by secondary school cheating rates on exams) and, perhaps even more worrisome, they modify their civic values.

First, I demonstrate that publicity about corruption in Mexico during the study period had an effect of around 10% (from 4% to 4.4% approximately) on secondary school cheating rates. I argue that this effect occurred as a consequence of a shift in

²⁴The number of students per grade and year is available for every year. However, the number of classrooms is available only for 2013. I compute a variable called “number of students per classroom” as the total number of students that took the test in a given school, grade, and year, divided by the number of classrooms of the same school and grade in 2013.

perceived social norms as part of a social learning theory process. To support this hypothesis, I use a Mexican values survey (MxFLS) to show that when households were exposed to public corruption, they significantly changed their thoughts on honesty: for example, they more often believed that to get ahead in life it is necessary to cheat.

Consistent with the idea that people act more dishonestly after having been exposed to new, debased standards of social norms, I show that the effect on cheating was significantly greater in municipalities where the government in power belonged to a party that was thought to have been honest.

Finally, I show that the effect was generally larger where there was greater access to local media, as well as present evidence of the implausibility of alternative channels. To this end, I demonstrate that the effect was not bigger during election years or in public schools (versus private schools), which suggests that the effect was not driven by politically connected mayors and principals. In addition, I show that the effect is equally big whether the classroom size was large or small. This supports the idea that, even when the likelihood of being caught was small (i.e., in a very large classroom), the effect remained significant. This finding helps to rule out a story in which students update their expected cost of cheating downwards after observing corruption.

A back-of-the-envelope calculation that extrapolates the results to the rest of the nation shows that, potentially, the effect of corruption on values and anti-social behavior is sizable. What would happen if all the municipalities of the country were audited?

For example, let us assume that the proportion of corrupt municipalities countrywide is similar to that found in the audits discussed in this paper (54% corrupt municipalities, on average). In addition, consider that the estimations in this paper show that the proportion of people with at least one survey answer revealing a lack of civic values increased by around 19 percentage points (from a base of 49%) due to being exposed to corruption. One can then extrapolate that around seven million additional individuals (of a population of 120 million people) will now have at least one answer reflecting a lack of civic values to statements such as “To get ahead in

life you need to cheat.” On the same note and with the same assumptions, 17,000 new secondary students would start to cheat as a consequence of being exposed to corruption in a given year.

These results are relevant for Mexico in particular, but also more broadly for the region as a whole, as corruption in Latin America is generally high. In the 2015 Corruption Perceptions Index published annually by Transparency International, Mexico ranked 123rd out of a sample of 167 countries. The issue of corruption also emerges as a primary concern when Mexicans are asked about major problems in their country. According to the Global Competitiveness Report (World Economic Forum, 2015), corruption is perceived as the main issue in doing business in the country. Moreover, misconduct at the top levels of government is not the only type of corruption that Mexico faces: according to the Mexican Competitiveness Institute (IMCO, 2016), Mexicans spend MX\$ 32,000 MM each year (\$ 1,600 MM) in small daily bribes, the result of 200 million small bribes of about \$ 8 each, per year. Corruption and dishonesty are, thus, highly relevant and correlated problems.

The findings of this paper show that the negative effects of a culture of corruption within a country should be added to the well-known and damaging effects of corruption on development, and that, together, they should be included in any cost-benefit analysis of policies aimed at fostering transparency. Indeed, corruption has a multiplier effect: it generates more corruption by setting a certain behavioral example. Given that people learn from their leaders, bringing about cultural change without reducing corruption at the top level seems a very difficult task.

8 Tables and Figures

Table I: Effect of Corruption on Cheating (Secondary Schools)

	Base	(I)	(II)	(III)	(IV)	(V)
Corrupt (>0)	4.07	0.44 (0.17) [10.7%]	0.47 (0.17) [11.7%]	0.41 (0.18) [10.1%]	0.41 (0.18) [10.1%]	0.38 (0.17) [9.4%]
Released		-0.19 (0.14) [4.6%]	-0.18 (0.14) [4.5%]	-0.12 (0.15) [3.0%]	-0.13 (0.15) [3.1%]	-0.23 (0.14) [5.6%]
Mun. Trend		NO	NO	NO	NO	YES
Grade FE		NO	NO	NO	YES	YES
Mun. Controls		NO	NO	YES	YES	YES
Pol. Controls		NO	YES	YES	YES	YES
Observations		340,009	340,009	340,009	340,009	340,009
R-Squared		0.21	0.21	0.21	0.22	0.22

Clustered standard errors in parentheses (municipality).

Results with the full set of controls (last column) include school fixed effects, grade fixed effects, year fixed effects, municipality-specific linear trends, and the set of controls at the municipality level described in Section 3.

In brackets: the estimated coefficient divided by the mean of cheating.

Table II: Effect of Corruption on Cheating (Secondary Schools): Robustness

	(I)	(II)	(III)	(IV)	(V)	(VI)
Corrupt (>0)	0.44 (0.17) [10.7%]	0.41 (0.18) [10.0%]	0.41 (0.18) [10.1%]	0.38 (0.17) [9.4%]	0.37 (0.17) [9.1%]	0.46 (0.25) [11.3%]
Corrupt (P15)	0.48 (0.17) [11.8%]	0.43 (0.17) [10.6%]	0.43 (0.17) [10.6%]	0.42 (0.18) [10.3%]	0.42 (0.17) [10.3%]	0.62 (0.23) [15.3%]
Corrupt (P25)	0.51 (0.17) [12.6%]	0.43 (0.18) [10.7%]	0.43 (0.18) [10.7%]	0.44 (0.18) [10.8%]	0.45 (0.17) [11.1%]	0.68 (0.26) [16.6%]
Corrupt (log)	0.23 (0.05)	0.19 (0.06)	0.19 (0.06)	0.16 (0.06)	0.15 (0.06)	0.23 (0.08)

Mun. Trend	NO	NO	NO	YES	YES	NO
Grade FE	NO	NO	YES	YES	YES	YES
Mun. Controls	NO	YES	YES	YES	YES	YES
Pol. Controls	NO	YES	YES	YES	YES	YES
School or Mun. Fixed Effects	School	School	School	School	Mun.	School
Sample	Full	Full	Full	Full	Full	Restricted
Observations	340,009	340,009	340,009	340,009	340,009	110,720

Clustered standard errors in parentheses (municipality).

Each row corresponds to a separate regression.

Results with the full set of controls include school fixed effects (column IV and VI) or municipality fixed effects (column V), grade fixed Effects, year fixed effects, municipality-specific linear trends, and the set of controls at the municipality level described in Section 3. All columns except Column VI control for the “Report Released” dummy. In Column VI the sample is restricted to year-municipalities in which there was an audit.

In brackets: the estimated coefficient divided by the mean of cheating.

Table III: Effect of Corruption on Cheating (Primary Schools)

	Base	(I)	(II)	(III)	(IV)	(V)
Corrupt (>0)	5.21	0.14 (0.10) [2.60%]	0.07 (0.10) [1.35%]	-0.03 (0.11) [0.50%]	-0.02 (0.11) [0.48%]	0.01 (0.12) [0.24%]
Released		0.01 (0.09) [0.30%]	0.07 (0.09) [1.40%]	0.09 (0.10) [1.70%]	0.09 (0.10) [1.70%]	-0.02 (0.09) [0.40%]
Mun. Trend		NO	NO	NO	NO	YES
Grade FE		NO	NO	NO	YES	YES
Mun. Controls		NO	NO	YES	YES	YES
Pol. Controls		NO	YES	YES	YES	YES
Observations		1,584,495	1,572,418	1,572,418	1,572,418	1,572,418
R-Squared		0.19	0.19	0.19	0.19	0.20

Clustered standard errors in parentheses (municipality).

Results with the full set of controls (last column) include school fixed effects, grade fixed effects, year fixed effects, municipality-specific linear trends, and the set of controls at the municipality level described in Section 3. All models control for the “Report Released” dummy.

In brackets: the estimated coefficient divided by the mean of cheating.

Table IV: Effect of Corruption on Values

Dependent Variable	Base	(I)	(II)	(III)	(IV)	(V)
Count Index (Sum 5 incivic values)	0.74	0.26 (0.10) [0.29 σ]	0.50 (0.20) [0.55 σ]	0.55 (0.17) [0.60 σ]	0.53 (0.17) [0.59 σ]	0.61 (0.14) [0.67 σ]
At Least 1 incivic value	0.49	0.12 (0.06) [0.24 σ]	0.18 (0.10) [0.36 σ]	0.18 (0.09) [0.37 σ]	0.20 (0.07) [0.40 σ]	0.19 (0.07) [0.37 σ]
At Least 2 incivic values	0.19	0.11 (0.04) [0.28 σ]	0.22 (0.07) [0.55 σ]	0.25 (0.05) [0.63 σ]	0.26 (0.05) [0.67 σ]	0.26 (0.05) [0.66 σ]
1st Component - PCA with 4 incivic values	0.16	0.08 (0.02) [0.38 σ]	0.12 (0.04) [0.60 σ]	0.12 (0.04) [0.63 σ]	0.14 (0.03) [0.72 σ]	0.14 (0.03) [0.71 σ]
Individual Controls		NO	NO	NO	YES	YES
Year FE		NO	NO	NO	YES	YES
Mun. Controls		NO	NO	YES	YES	YES
Pol. Controls		NO	YES	YES	YES	YES
Observations		9,150	9,150	9,150	9,150	9,150

Clustered standard errors in parentheses (municipality)

Each row corresponds to a separate regression.

Results with the full set of controls (last column) include year fixed effects and the set of controls at the municipality level described in Section 3, plus the individual level controls described in Section 5.3.

Definitions: “Count Index”: sum of five incivic answers, “At Least 1”: at least one incivic answer, “At Least 2”: at least two incivic answers, “PC”: First component of a PCA (normalized to a 0-1 scale). The exact wording of the individual questions is as follows: (1) “The one who does not cheat, does not get ahead” (Completely Agree, Agree, Disagree, Completely Disagree), (2) “Are you trustworthy?” (Completely Agree, Agree, Disagree, Completely Disagree), (3) “Laws were made to be broken” (Completely Agree, Agree, Disagree, Completely Disagree), (4) “How likely is it that you steal electricity from the public lines (illegally)?” (1 to 100), (5) “How likely is it that you return a wallet with 500 pesos in it?” (1 to 100).

In brackets: the estimated coefficients divided by the standard deviation of each variable.

Table V: Effect of Corruption on Individual Values

Dependent Variable	Base	(I)	(II)	(III)	(IV)	(V)
Cheat to get Ahead	0.21	0.14 (0.04) [0.34 σ]	0.20 (0.09) [0.48 σ]	0.20 (0.08) [0.50 σ]	0.24 (0.07) [0.58 σ]	0.23 (0.07) [0.57 σ]
Break Rules	0.21	0.18 (0.06) [0.44 σ]	0.16 (0.06) [0.39 σ]	0.17 (0.06) [0.41 σ]	0.21 (0.05) [0.51 σ]	0.22 (0.05) [0.54 σ]
Steal	0.06	0.04 (0.03) [0.17 σ]	0.13 (0.05) [0.55 σ]	0.12 (0.04) [0.51 σ]	0.11 (0.04) [0.44 σ]	0.10 (0.04) [0.44 σ]
Not Trustworthy	0.05	0.03 (0.03) [0.17 σ]	0.03 (0.03) [0.16 σ]	0.03 (0.02) [0.17 σ]	0.04 (0.03) [0.19 σ]	0.04 (0.02) [0.17 σ]
Not Return Wallet	0.22	-0.09 (0.09) [0.24 σ]	0.05 (0.08) [0.11 σ]	0.09 (0.05) [0.22 σ]	0.09 (0.06) [0.23 σ]	0.09 (0.06) [0.22 σ]
Individual Controls		NO	NO	NO	YES	YES
Year FE		NO	NO	NO	YES	YES
Mun. Controls		NO	NO	YES	YES	YES
Pol. Controls		NO	YES	YES	YES	YES
Observations		9.415	9.415	9.415	9.415	9.415

Clustered standard errors in parentheses (municipality)

Each row corresponds to a separate regression.

Results with the full set of controls (last column) include year fixed effects and the set of controls at the municipality level described in Section 3, plus the individual level controls described in Section 5.3.

The exact wording of the individual questions and the criteria to build the indices can be found in the Appendix.

In brackets: the estimated coefficients divided by the standard deviation of each variable.

Table VI: Heterogeneous Effect of Corruption on Cheating: Media and Perception

	(I)	(II)	(III)	(IV)
	(a)	(b)	(a)	(b)
(I) Corrupt	0.12 (0.27) [3.0%]	0.24 (0.26) [5.8%]	0.78 (0.33) [19.1%]	0.83 (0.30) [20.5%]
(II) Corrupt x "High Exposure"	0.65 (0.30) [16.1%]	0.63 (0.27) [15.4%]		
(III) Corrupt x "Perceived Corrupt"			-0.71 (0.38) [17.4%]	-0.68 (0.35) [16.7%]
Mun. Trend	NO	YES	NO	YES
Grade FE	NO	YES	NO	YES
Mun. Controls	NO	YES	NO	YES
Pol. Controls	NO	YES	NO	YES
(I) + (II)	0.77 (0.31) [18.8%]	0.86 (0.25) [20.9%]		
(I) + (III)			0.07 (0.26) [1.7%]	0.15 (0.23) [3.8%]
Obs.	340,009	340,009	321,201	321,201
R-Squared	0.21	0.22	0.21	0.23

Clustered standard errors in parentheses (municipality)

Regressions in Models (b) include school fixed effects, grade fixed effects, year fixed effects, municipality-specific linear trends, and the set of controls at the municipality level described in Section 3. All models control for the "Report Released" dummy.

All models control for the interaction of the "corrupt" and the "Report Released" dummies with two time-varying municipality variables (tax collection and homicide rate). It also controls for the interaction of the "Report Released" dummy with the "high exposure" dummy.

In brackets: the estimated coefficient divided by the mean of cheating.

Table VII: Heterogeneous Effect of Corruption on Cheating: Private Schools, Election Years and Class Size

	(I)	(II)	(III)	(IV)	(V)	(VI)
	(a)	(b)	(a)	(b)	(a)	(b)
(I) Corrupt	0.33 (0.25) [8.1%]	0.42 (0.23) [10.4%]	0.38 (0.23) [9.3%]	0.42 (0.25) [10.4%]	0.32 (0.24) [7.9%]	0.46 (0.22) [11.4%]
(II) Corrupt x "Private School"	0.30 (0.20) [7.4%]	0.25 (0.18) [6.1%]				
(III) Corrupt x "Election Year"			-0.24 (0.48) [5.8%]	-0.12 (0.49) [3.0%]		
(IV) Corrupt x "Class Size (log)"					0.08 (0.15)	0.05 (0.15)
Mun. Trend	NO	YES	NO	YES	NO	YES
Grade FE	NO	YES	NO	YES	NO	YES
Mun. Controls	NO	YES	NO	YES	NO	YES
Pol. Controls	NO	YES	NO	YES	NO	YES
(I) + (II)	0.63 (0.25) [15.4%]	0.67 (0.24) [16.5%]				
(I) + (III)			0.14 (0.43) [3.4%]	0.30 (0.47) [7.4%]		
Obs.	340,009	340,009	181,678	181,678	323,863	323,863
R-Squared	0.21	0.22	0.24	0.25	0.20	0.22

Clustered standard errors in parentheses (municipality)

Regressions in the (b) models include school fixed effects, grade fixed effects, year fixed effects, municipality-specific linear trends, and the set of controls at the municipality level described in Section 3. All models control for the "Report Released" dummy.

All models control for the interaction of the "corrupt" and the "Report Released" dummies with two time-varying municipality variables (tax collection and homicide rate). They also control for the interaction of the "Report Released" dummy with the "high exposure" dummy.

In brackets: the estimated coefficient divided by the mean of cheating.

9 Appendix

9.1 Additional Tables and Figures

Table A1: Cheating Descriptive Statistics

Year	Grade	Obs.	Mean %	SD %	Min	P99	Max
2006	3	16,466	2.5	7.2	0	35.3	95.7
2007	3	17,586	2.9	9.2	0	46.2	100
2008	3	17,041	6.6	18.2	0	100	100
2009	1	18,515	1.7	7.2	0	100	100
	2	18,801	3.7	10.7	0	60	100
	3	18,443	6.1	14.2	0	78	100
2010	1	19,417	1.9	7.9	0	40	100
	2	19,368	5.0	13.5	0	76.5	100
	3	19,052	6.2	14.7	0	80	100
2011	1	20,343	3.4	11.1	0	63.6	100
	2	20,343	2.9	10.1	0	55.6	100
	3	20,343	3.5	11.1	0	65.3	100
2012	1	19,545	4	13.2	0	80	100
	2	19,384	6.3	16.6	0	93	100
	3	18,978	6.7	16.6	0	92	100
2013	1	20,793	1.9	8.1	0	43	100
	2	20,585	4.2	12.8	0	75	100
	3	20,298	4.1	12.4	0	72	100

Figure A1: Kernel Estimation - Cheating Rate Secondary School All Grades 2006-2013

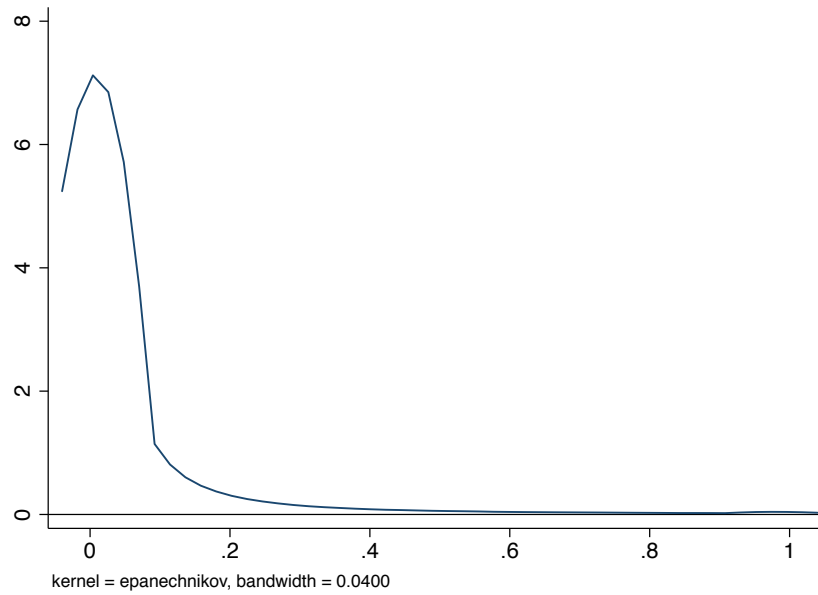


Table A2: Audit Descriptive Statistics

Year	Number Audits	Mean Not Authorized	SD Not Authorized	Proportion of Corrupt	Min. Not Authorized	Max. Not Authorized
2006	32	12.64	16.55	0.66	0	56.8
2007	94	9.21	13.1	0.57	0	46
2008	100	6.46	10.89	0.56	0	64
2009	111	11.15	15.71	0.61	0	87.5
2010	142	9.013	13.52	0.63	0	65.4
2011	130	6.96	14.27	0.52	0	100
2012	161	3.93	8.84	0.4	0	53.5
2013	160	3.2	7.95	0.38	0	52
Mean	116.25	7.82	12.61	0.54	0	65.65

Figure A2: Kernel Estimation - Unauthorized Expenditure 2006-2013, in %

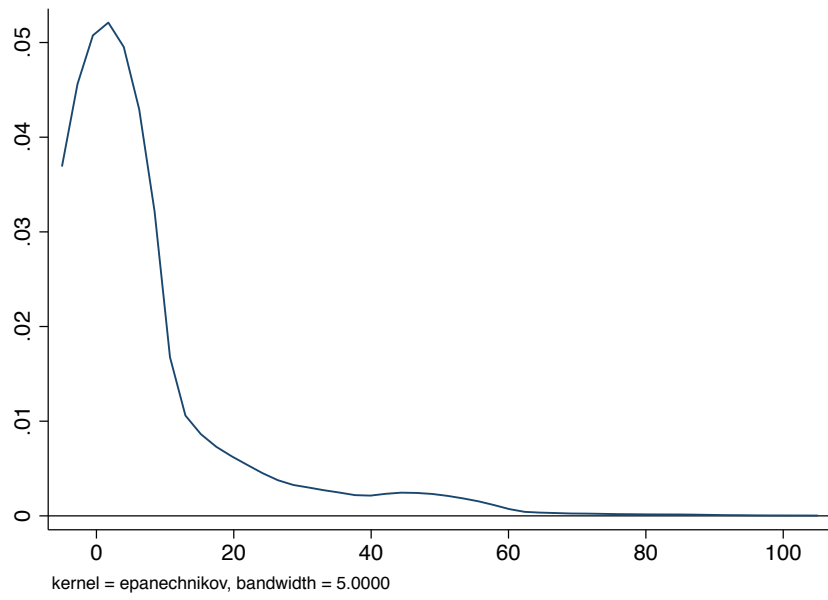


Table A3: Survey Descriptive Statistics (Mean and Standard Deviation)

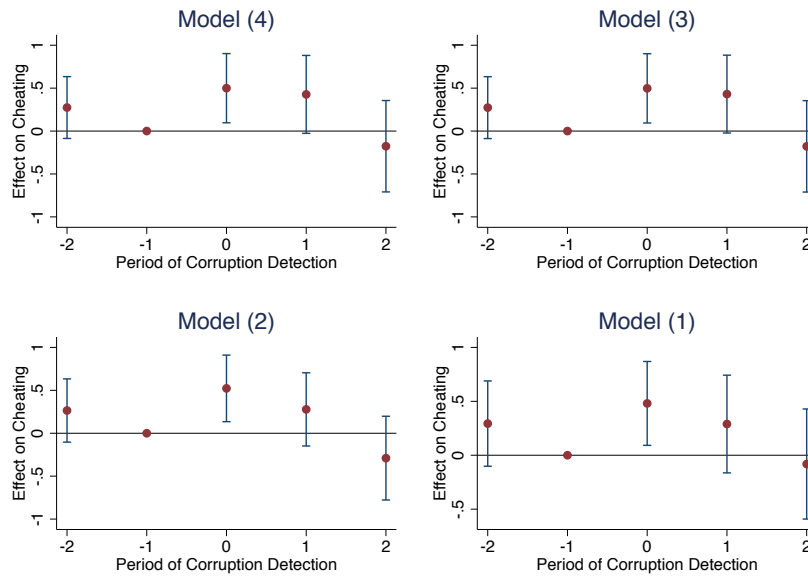
	Count Index	At Least 1	At Least 2	PC	Cheat to get ahead	Not Trust-worthy	Break Rules	Steal	Not Return Wallet
Mean	0.74	0.49	0.19	0.16	0.21	0.05	0.21	0.06	0.22
SD	(0.91)	(0.50)	(0.39)	(0.20)	(0.41)	(0.21)	(0.41)	(0.24)	(0.41)
Min	0	0	0	0	0	0	0	0	0
Max	4	1	1	0.87	1	1	1	1	1
Obs.	9,150	9,150	9,150	9,150	9,415	9,492	9,406	9,432	9,443

Definitions: “Count Index”: sum of incivic answers, “At Least 1”: at least one incivic answer, “At Least 2”: at least two incivic answers, “PC”: First component of a PCA (normalized to a 0-1 scale). The exact wording of the individual questions and the criteria to build the indices can be found in the Appendix.

Table A4: Public Opinion Descriptive Statistics

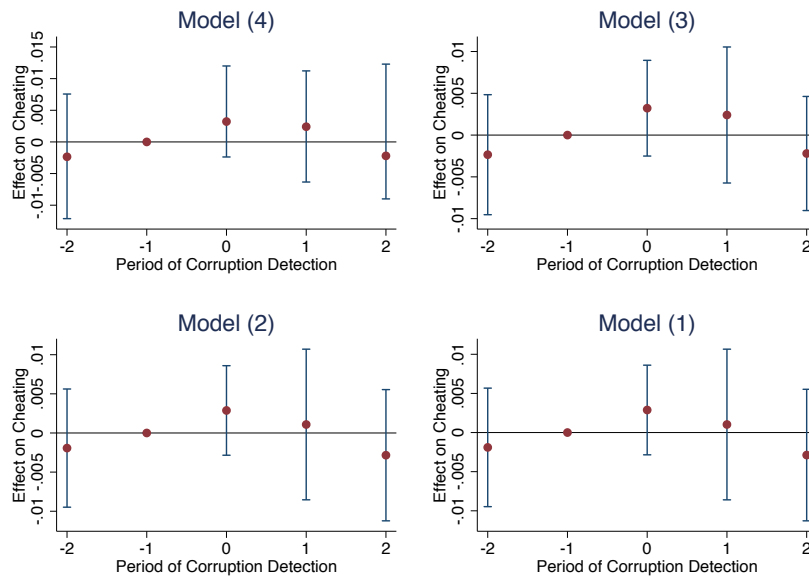
State	% Answering Corrupt			Corruption Ranking		
	PAN	PRI	PRD	1 st	2 nd	3 rd
Aguascalientes	34	48	20	PRI	PAN	PRD
Baja California	13	100	4	PRI	PAN	PRD
Baja California Sur	0	66	28	PRI	PRD	PAN
Campeche	9	2	27	PRI	PRD	PAN
Coahuila	28	46	30	PRI	PRD	PAN
Chihuahua	11	59	21	PRI	PRD	PAN
Colima	5	85	27	PRI	PRD	PAN
Chiapas	2.5	88	14	PRI	PRD	PAN
Durango	11	58	23	PRI	PRD	PAN
Guanajuato	6	79	27	PRI	PRD	PAN
Guerrero	4	81	32	PRI	PRD	PAN
Hidalgo	11	57	32	PRI	PRD	PAN
Jalisco	20	77	21	PRI	PRD	PAN
Mexico (State)	25	72	30	PRI	PRD	PAN
Michoacan	11	55	36	PRI	PRD	PAN
Morelos	8	78	19	PRI	PRD	PAN
Nayarit	15	56	16	PRI	PRD	PAN
Nuevo Leon	9	70	23	PRI	PRD	PAN
Oaxaca	0	97	19	PRI	PRD	PAN
Puebla	19	85	9	PRI	PAN	PRD
Queretaro	2	80	39	PRI	PRD	PAN
Quintana Roo	5	66	29	PRI	PRD	PAN
San Luis Potosi	6	82	24	PRI	PRD	PAN
Sinaloa	13	70	10	PRI	PAN	PRD
Sonora	8	67	23	PRI	PRD	PAN
Tabasco	5	40	45	PRD	PRI	PAN
Tamaulipas	12	62	19	PRI	PRD	PAN
Tlaxcala	12	51	12	PRI	PRD	PAN
Veracruz	32	40	24	PRI	PAN	PRD
Yucatan	32	58	15	PRI	PAN	PAN
Zacatecas	2	34	56	PRI	PAN	PAN

Figure A3: Leads and Lags (robustness) – 95% CI



Note: Model (1): excludes municipality linear trends, grade fixed effects, municipality time-varying controls, and political controls. Model (2): same as Model (1) but adds political controls. Model (3): same as Model (2) but adds municipality time-varying controls. Model (4): same as Model (3) but adds grade fixed effects.

Figure A4: Leads and Lags, only with Audit Reports – 95% CI



Note: 95% Confidence Intervals, 1st pre-treatment period normalized to 0. Corruption defined as "proportion of unauthorized expenditure greater than zero." Model (1) excludes grade fixed effects, municipality time-varying controls and political controls. Model (2): same as Model (1) but adds political controls. Model (3): same as Model (2) but adds municipality time-varying controls. Model (4): same as Model (3) but adds grade fixed effects.

Table A5: Effect of Corruption on Values: Robustness

	Count Index	At Least 1	At Least 2	PC
CorruptAfter (>0)	0.61 (0.14) [0.67 σ]	0.19 (0.07) [0.37 σ]	0.26 (0.05) [0.66 σ]	0.14 (0.03) [0.71 σ]
CorruptAfter (P15)	0.47 (0.21) [0.52 σ]	0.12 (0.09) [0.23 σ]	0.21 (0.09) [0.54 σ]	0.10 (0.05) [0.51 σ]
CorruptAfter (P25)	0.50 (0.21) [0.55 σ]	0.16 (0.09) [0.31 σ]	0.20 (0.09) [0.51 σ]	0.10 (0.04) [0.52 σ]
CorruptAfter (log)	0.07 (0.02)	0.02 (0.01)	0.03 (0.01)	0.02 (0.00)
Observations	9,150	9,150	9,150	9,150

Clustered standard errors in parentheses (municipality)

Each row corresponds to a separate regression.

Results with the full set of controls (last column) include year fixed effects and the set of controls at the municipality level described in Section 3, plus the individual level controls described in Section 5.3.

Definitions: “Count Index”: sum of incivic answers, “At Least 1”: at least one incivic answer, “At Least 2”: at least two incivic answers, “PC”: First component of a PCA (normalized to a 0-1 scale). The exact wording of the individual questions and the criteria to build the indices can be found in the Appendix.

In brackets: the estimated coefficients divided by the standard deviation of each variable.

Table A6: Effect of Corruption on Values: Placebo

	Tanda	Saving (I)	Saving (II)	Fear (day)	Fear (night)	Enough Money
CorruptAfter >0	0.02	-0.07	805.91	0.07	0.04	0.00
	(0.07)	(0.09)	(498.5)	(0.07)	(0.06)	(0.06)
	[0.04 σ]	[0.14 σ]	[0.25 σ]	[0.19 σ]	[0.10 σ]	[0.00 σ]
CorruptAfter (P15)	0.11	-0.20	1007.65	-0.03	-0.09	-0.07
	(0.10)	(0.10)	(536.50)	(0.09)	(0.09)	(0.09)
	[0.27 σ]	[0.40 σ]	[0.31 σ]	[0.07 σ]	[0.21 σ]	[0.15 σ]
CorruptAfter (P25)	0.11	-0.17	1117.46	-0.03	-0.09	-0.09
	(0.10)	(0.08)	(523.10)	(0.09)	(0.09)	(0.09)
	[0.26 σ]	[0.34 σ]	[0.35 σ]	[0.07 σ]	[0.21 σ]	[0.18 σ]
CorruptAfter (log)	0.00	0.00	110.78	0.01	0.00	-0.01
	(0.01)	(0.01)	(69.84)	(0.01)	(0.01)	(0.01)
Obs.	9,150	9,150	9,150	9,150	9,150	9,150

Clustered standard errors in parentheses (municipality)

Each row corresponds to a separate regression.

Results with the full set of controls (last column) include year fixed effects and the set of controls at the municipality level described in Section 3, plus the individual level controls described in Section 5.3.

Definitions: “Tanda”: How likely is it that you will invest all your monthly income in an informal savings group? (0-100). Takes a 1 if the probability is greater than the mean average. “Saving (I)”: Do you think about the future when you make decisions about spending and saving?. Takes a 1 if the answer is positive. “Saving (II)”: Imagine that you have a rich relative who gives you 20,000 pesos today. How much would you spend in the next 30 days? “Fear (day)”: Do you feel scared of being attacked or assaulted during the day?. Takes a 1 if the answer is positive (scared or very scared). “Fear (night)”: Do you feel scared of being attacked or assaulted during the night?. Takes a 1 if the answer is positive (scared or very scared). “Enough Money”: How likely is it that you will have enough money this year to cover all your household needs?. Takes a 1 if the probability is larger than the mean average. “Involved”: No one should get involved in a family’s or friends’ problems. Takes a value of 1 if the individual agrees or completely agrees.

In brackets: the estimated coefficients divided by the standard deviation of each variable.

9.2 Values Survey: Exact Wording of the Questions

In Section 6 (Interpretation and Channels), I use five questions related to civic values included in the Mexican Family Life Survey, which I combine to construct different indices. The exact wording of the five questions is as follows: (1) “The one who does not cheat, does not get ahead” (Completely Agree, Agree, Disagree, Completely Disagree), (2) “Are you trustworthy?” (Completely Agree, Agree, Disagree, Completely Disagree), (3) “Laws were made to be broken” (Completely Agree, Agree, Disagree, Completely Disagree), (4) “How likely is it that you steal electricity from the public lines (illegally)?” (1 to 100), (5) “How likely is it that you return a wallet with 500 pesos in it?” (1 to 100).

Using these questions, I then construct four synthetic indices of civic-mindedness: (a) Count Index: sum of incivic answers (min=1, max = 5) (b) At Least 1: takes a value of one if there is at least one incivic answer and zero otherwise, (b) At Least 2: takes a value of one if there are at least two incivic answers (c) Principal Component: First component of a principal component analysis of the five questions (normalized to a 0-1 scale). The answers to questions (1) and (3) are considered incivic if the individual agrees or completely agrees with the statements. The answer to question (2) is considered incivic if the individual disagrees or completely disagrees with the statement. The answer to question (4) is considered incivic if the probability is greater than the mean average. The answer to question (5) is considered incivic if the probability is smaller than the mean average.

References

- Acemoglu, Daron, and Matthew O Jackson.** 2014. “History, expectations, and leadership in the evolution of social norms.” *The Review of Economic Studies*, 82(2): 423–456.
- Ajzenman, Nicolas, Sebastian Galiani, and Enrique Seira.** 2015. “On the distributive costs of drug-related homicides.” *The Journal of Law and Economics*, 58(4): 779–803.
- Akers, Ronald L.** 2011. *Social learning and social structure: A general theory of crime and deviance*. Transaction Publishers.
- Akers, Ronald L, Marvin D Krohn, Lonn Lanza-Kaduce, and Marcia Radosevich.** 1979. “Social learning and deviant behavior: A specific test of a general theory.” *American Sociological Review*, 636–655.
- Akhtari, Mitra, Diana Moreira, and Laura Trucco.** 2017. “Political turnover, bureaucratic turnover, and the quality of public services.” *unpublished manuscript*.
- Alesina, Alberto, and Nicola Fuchs-Schündeln.** 2007. “Goodbye Lenin (or not?): The effect of communism on people’s preferences.” *American Economic Review*, 97(4): 1507–1528.
- Ananyev, Maxim, and Sergei Guriev.** 2013. “Causal effect of income on trust: Evidence from the 2009 crisis in Russia.” *Paris School of Economics (mimeo)*.
- Arias, Eric, Horacio Larreguy, John Marshall, and Pablo Querubin.** 2018. “Priors rule: When do Malfeasance Revelations Help or Hurt Incumbent Parties?” National Bureau of Economic Research.
- Avis, Eric, Claudio Ferraz, and Frederico Finan.** 2018. “Do government audits reduce corruption? Estimating the impacts of exposing corrupt politicians.” *Journal of Political Economy*, 126(5): 1912–1964.

- Bandura, Albert, and Richard H Walters.** 1977. *Social learning theory*. Vol. 1, Prentice-hall Englewood Cliffs, NJ.
- Barr, Abigail, and Danila Serra.** 2010. “Corruption and culture: An experimental analysis.” *Journal of Public Economics*, 94(11): 862–869.
- Bisin, Alberto, and Thierry Verdier.** 1998. “On the cultural transmission of preferences for social status.” *Journal of Public Economics*, 70(1): 75–97.
- Bisin, Alberto, and Thierry Verdier.** 2010. “The economics of cultural transmission and socialization.” National Bureau of Economic Research.
- Brollo, Fernanda, Katja Kaufmann, and Eliana La Ferrara.** 2017. “The political economy of program enforcement: Evidence from Brazil.” *Journal of the European Economic Association*.
- Chong, Alberto, Ana L De La O, Dean Karlan, and Leonard Wantchekon.** 2014. “Does corruption information inspire the fight or quash the hope? A field experiment in Mexico on voter turnout, choice, and party identification.” *The Journal of Politics*, 77(1): 55–71.
- Cizek, Gregory J.** 1999. *Cheating on tests: How to do it, detect it, and prevent it*. Routledge.
- Del Monte, Alfredo, and Erasmo Papagni.** 2001. “Public expenditure, corruption, and economic growth: the case of Italy.” *European Journal of Political Economy*, 17(1): 1–16.
- Depetris-Chauvin, Emilio, Ruben Durante, and Filipe R Campante.** 2018. “Building nations through shared experiences: Evidence from African football.” *National Bureau of Economic Research*.
- Di Tella, Rafael, Sebastian Galiani, and Ernesto Schargrotsky.** 2007. “The formation of beliefs: evidence from the allocation of land titles to squatters.” *The Quarterly Journal of Economics*, 122(1): 209–241.

- d’Adda, Giovanna, Donja Darai, Nicola Pavanini, and Roberto A Weber.** 2017. “Do leaders affect ethical conduct?” *Journal of the European Economic Association*, 15(6): 1177–1213.
- Ferraz, Claudio, and Frederico Finan.** 2008. “Exposing corrupt politicians: the effects of Brazil’s publicly released audits on electoral outcomes.” *The Quarterly Journal of Economics*, 123(2): 703–745.
- Ferraz, Claudio, and Frederico Finan.** 2011. “Electoral accountability and corruption: Evidence from the audits of local governments.” *The American Economic Review*, 101(4): 1274–1311.
- Ferraz, Claudio, Frederico Finan, and Diana B Moreira.** 2012. “Corrupting learning: Evidence from missing federal education funds in Brazil.” *Journal of Public Economics*, 96(9): 712–726.
- Fisman, Raymond, and Edward Miguel.** 2007. “Corruption, norms, and legal enforcement: Evidence from diplomatic parking tickets.” *Journal of Political Economy*, 115(6): 1020–1048.
- Gächter, Simon, and Jonathan F Schulz.** 2016. “Intrinsic honesty and the prevalence of rule violations across societies.” *Nature*, 531(7595): 496–499.
- Giuliano, Paola, and Antonio Spilimbergo.** 2013. “Growing up in a Recession.” *Review of Economic Studies*, 81(2): 787–817.
- Granger, Clive WJ.** 1969. “Investigating causal relations by econometric models and cross-spectral methods.” *Econometrica: Journal of the Econometric Society*, 424–438.
- Hays, Chelsea, and Leslie J Carver.** 2014. “Follow the liar: the effects of adult lies on children’s honesty.” *Developmental Science*, 17(6): 977–983.
- Hermalin, Benjamin E.** 1998. “Toward an economic theory of leadership: Leading by example.” *The American Economic Review*, 1188–1206.

- Holland, PW, and DT Thayer.** 1986. “Differential item function and the Mantel-Haenszel procedure.” ETS Research Report.
- Jacob, Brian A, and Steven D Levitt.** 2003. “Rotten apples: An investigation of the prevalence and predictors of teacher cheating.” *The Quarterly Journal of Economics*, 118(3): 843–877.
- Larreguy, Horacio A, John Marshall, and James M Snyder Jr.** 2014. “Revealing malfeasance: How local media facilitates electoral sanctioning of mayors in Mexico.” National Bureau of Economic Research.
- Madestam, Andreas, David Yanagizawa-Drott, et al.** 2011. “Shaping the nation: The effect of Fourth of July on political preferences and behavior in the United States.” *mimeo*.
- Mauro, Paolo.** 1995. “Corruption and growth.” *The Quarterly Journal of Economics*, 110(3): 681–712.
- Mauro, Paolo.** 1998. “Corruption and the composition of government expenditure.” *Journal of Public Economics*, 69(2): 263–279.
- Mazar, Nina, On Amir, and Dan Ariely.** 2008. “The dishonesty of honest people: A theory of self-concept maintenance.” *Journal of Marketing Research*, 45(6): 633–644.
- Montoya, Eduardo.** 2016. “Violence and economic disruption: Firm-level evidence from Mexico.” Working Paper.
- Murthi, Mamta, and Erwin R Tiongson.** 2009. “Attitudes to income equality: the ‘socialist legacy’ revisited.” *Comparative Economic Studies*, 51(3): 344–366.
- Olken, Benjamin A, and Rohini Pande.** 2012. “Corruption in developing countries.” *Annual Review of Economics*, 4(1): 479–509.
- Segovia, Rafael, et al.** 1975. “La politización del niño mexicano.”

- Svensson, Jakob.** 2003. "Who must pay bribes and how much? Evidence from a cross section of firms." *The Quarterly Journal of Economics*, 118(1): 207–230.
- Svensson, Jakob.** 2005. "Eight questions about corruption." *The Journal of Economic Perspectives*, 19(3): 19–42.
- Tabellini, Guido.** 2008. "The scope of cooperation: Values and incentives." *The Quarterly Journal of Economics*, 123(3): 905–950.