Cartels, Antitrust Enforcement, and Industry Performance: Evidence from Mexico*

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Abstract: Forty percent of economic activities in Mexico weighed by sales have been investigated for illegal monopolistic practices since the Federal Competition Commission was established in 1993. By exploiting some unique features of the Mexican investigative system, and using a synthetic control approach, this paper examines the causal impact of antitrust sanctions on industry performance and aggregate outcomes. Sanctions cause sales and wages to increase and profit margins to fall in the sanctioned sectors, thus benefiting consumers and workers. Overall, antitrust enforcement contributes roughly half a percent of per capita gross domestic product growth. Outcomes of investigations that are closed without sanction fail to reject the hypothesis that some harmful conduct is not sanctioned because investigators lack resources to prove it conclusively. An implication is that the Commission could generate greater benefits with additional investigative resources.

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1 Introduction

Recent evidence of increasing market power (De Loecker, Eeckhout and Unger, 2020; De Loecker and Eeckhout, 2021) and growing profits has led to the recommendation of vigorous antitrust enforcement (Khan, 2017; Shapiro, 2018; Wu, 2018; Berry, Gaynor and Scott Morton, 2019; Marinescu and Posner, 2019; Rose, 2019; Salop, 2021). Implementation of this recommendation raises practical questions. How much can an antitrust authority expect to shape microeconomic outcomes using tools available under the law, like investigations and judicial discretion? If an antitrust authority can shape microeconomic outcomes, what could be its contribution to aggregate outcomes like productivity, inequality, and employment?

This paper answers these questions using outcomes of investigations by Mexico's Federal Competition Commission (in 2013 renamed the Federal Economic Competition Commission abbreviated as COFECE in Spanish) into alleged illegal monopolistic practices. A distinctive practice in Mexican competition law allows us to identify the causal effects of antitrust sanctions on economic outcomes. In jurisdictions like the United States, a description of the relevant market is not always known to researchers except after prosecutors bring it to a public trial, creating selection bias. In Mexico, the Commission describes the relevant market in a public document (*acuerdo de inicio*) for all ex-officio antitrust investigations and credible public complaints, including closed investigations that do not produce sufficient evidence to find agents responsible for illegal conduct (see Figure 1 for a schematic description of the investigative process in Mexico). Closed investigations allow us to measure changes in markets where illegal conduct was suspected but not punished, which we use as a counterfactual for assessing the impact of sanctions. Estimates of the effect of antitrust sanctions using this counterfactual deliver larger estimates than the approach of prior work, which compares sanctioned industries to others in the same sector with similar pre-treatment trends. We attribute roughly half a percent of per capita GDP growth in Mexico to the Commission's sanctions.

While antitrust authorities are long-established in high-income economies (the United States Federal Trade Commission was founded in 1914; the first European Commissioner for Competition was designated in 1958), their history in middle-income economies is more recent. Mexico established the Federal Competition Commission in 1993 as part of a broad reform agenda focused on economic modernization led by President Carlos Salinas de Gortari. An objective was to increase competition in line with participation in the multilateral system (GATT, now WTO) and later the North American Free Trade Agreement. The Commission today has investigated sectors that represent about 40 percent of all economic activity weighed by sales (Figure 2) and is regarded by peers as a member of the antitrust

vanguard (OECD, 2020). Narrowly, the Mexican experience is informative about what an antitrust authority can achieve in a relatively short period when starting from scratch and following international good practices. Broadly, the Mexican experience highlights the potential contribution of antitrust to aggregate outcomes, especially in an economy characterized by higher inequality, slower productivity growth, and greater misallocation (i.e., too much labor in unproductive firms) compared to other OECD countries (Levy Algazi and Walton, 2009; Hsieh and Klenow, 2014; Levy Algazi et al., 2018).[1]

The analysis begins using the synthetic difference-in-differences (SDID) approach proposed by Arkhangelsky, Athey, Hirshberg, Imbens and Wager (2021) to quantify the change in outcomes (e.g., sales, wage bill, operating profit margin) in relevant markets after the Commission's final decision to close a case or sanction agents for illegal conduct. Difference-in-differences are a common approach to ex-post economic evaluation of antitrust enforcement (see Kwoka, 2014; Ilzkovitz and Dierx, 2015). In each case, investigators define the relevant market in which defendants compete by grouping together all products that are close substitutes for the same group of consumers (Davis and Garcés, 2009).[2] As we apply it in Mexico, the difference-in-differences compares outcomes in the relevant market (the treated industry) after the Commission's decision with outcomes before the decision. To control for the possible influence of other factors, the estimator performs the same comparison in other never-treated industries in the same sub-sector that do not produce for the relevant market defined by investigators (the control industries). For instance, after a decision about an alleged monopolistic practice related to the manufacture of soft drinks, differences in that industry are compared to differences in the beverages sub-sector, such as the manufacture of rum and other distilled cane beverages. The approach weighs control industries so that the treatment industry and an average of the control industries (the synthetic control industry) have parallel trends in sales prior to the decision. By virtue of its parallel trend in sales, and cost and demand similarity to the treated industry due to its membership in the same sub-sector, the synthetic control industry is assumed to be similarly affected by all causal influences other than those leading to the Commission's final decision.

Relative to this control, on average sales rise after sanctions, consistent with either prices falling and consumers buying more quantity as they move down the demand curve, or consumers benefiting from higher quality that may or may not coincide with higher prices. The

[1] See Iacovone, Munoz Moreno, Olaberria and Pereira Lopez (2022) for a recent analysis of Mexico's productivity trends and dynamics.

[2] As a legal matter, in some cases prosecutors need not define a relevant market in which the defendants have market power in order to win a conviction. Nonetheless, investigators define the relevant market as a practical matter when assembling evidence, for instance, through interviews with agents and analyzing data from a specific product market.

wage bill also rises after sanctions, suggesting a benefit to workers. The wage bill result is consistent with cartel conduct restricting quantities and therefore labor demand, and is inconsistent with a model in which cartels share rents with workers.

After investigations are closed without sanctions, it is a different story. Sales and wages decline and profit margins rise. This is consistent with investigations targeting industries that are being monopolized, where firms are restricting output in order to raise margins. One might argue for an alternative interpretation that the Commission is investigating industries that are declining for exogenous reasons but are nonetheless competitive. However, this argument is not consistent with rising profit margins.

We use closed investigations as an alternative counterfactual for sanctioned industries. Instead of using, as in past literature, another industry in the same sector with plausibly similar cost and demand drivers, closed investigations allow us to use an industry in which illegal conduct is suspected yet unpunished. This approach corrects for a selection bias in the difference-in-differences approach, which arises because the illegal conduct targeted by investigations may emerge specifically in industries with certain tends that are different from those in the average industry that is used as a control. This approach also ensures that the counterfactual includes any potential deterrent or encouragement effect of a "shot across the bow" investigation that does not go to trial. To measure the effect of sanctions relative to the counterfactual of a closed investigation, we regress the synthetic difference-in-difference effects of all cases on a constant and a dummy for whether the case ended in a sanction, weighting the regression by the inverse variance of the estimator as in the meta-analysis literature, as has been recommended in the context of antitrust (Vita and Osinski, 2016).

The meta-analysis regression reveals the effect of sanctions relative to the alternative counterfactual of the case being closed. Relative to the difference-in-differences analysis of sanctions alone, this corrects for the presence of unobserved trends in industries that are being monopolized. In this specification, we find that the effect of antitrust sanctions on sales and wages is larger than estimated using only the difference-in-differences after sanctions. Relative to closed cases, sanctions increase sales by 6 percent annually in our preferred specification. The wage bill also increases by 5 percent annually, driven by both increases in employment and the wage rate. These magnitudes imply that sanctions reverse the negative trends that could be due to illegal conduct.

Together, these results suggest the Commission is pursuing its mandate effectively. Overall, its investigations target industries where trends suggest possible anti-competitive conduct, indicating an efficient allocation of investigative resources. When the Commission imposes sanctions there are benefits for both consumers and workers.

One might argue that the results with the closed case counterfactual do not have a causal

interpretation if the Commission uses its administrative discretion to sanction only industries that are growing for exogenous reasons, and which can afford it. This alternative hypothesis is rejected by the fact that positive effects of sanctions are economically significant only in the subset of cases where the Commission has no administrative discretion under Mexican law or cases where the alleged conduct is called an "absolute monopolistic practice" or collusive practice[3] or a cartel. Such practices, which include agreements to fix prices or divide a market, are per se illegal. In such cases, the Commission must impose a sanction when there is conclusive evidence of the conduct, for instance, evidence of an agreement to fix prices, and the defendant does not have the opportunity to bring evidence that their conduct did not harm consumers given limited market power. Since the positive effects of sanctions are concentrated in cases where sanctions are deterministic based on evidence of conduct alone, the effects of sanctions we estimate cannot be the result of administrative discretion. We also verify that results are not different in cases originating ex-officio, rather than from public complaints.

Could the Commission do even better by operating under its current legal mandate, or would a change to the law be required? A specific debate concerns cases of what are called "relative monopolistic practices" or abuse of market power,[4] such as price discrimination or vertical restraint, where under a rule of reason defendants have the opportunity to argue that their practices actually improve consumer welfare. In such cases, the authority can agree with defendants and choose not to impose a sanction, even if evidence of the conduct is conclusive. Debate about whether certain conduct should be per se illegal or subject to debate about intent or negative effects has a long history (e.g., Bork, 1966). Khan (2017) argues using a decision theory framework that in cases where judges have discretion, they are biased towards making Type II errors (false negatives, or not sanctioning agents when their conduct harmed competition) due to an incorrect presumption that Type I errors (false positives, or sanctioning agents when their conduct did not harm competition) are more costly. Salop (2017) provides several examples of when Type II errors can be more costly, for instance if the alleged conduct creates a reputation for prediction that raises entry barriers. If Type II errors are more costly, one policy response would be for judges to reject defense arguments more readily in cases of relative monopolistic practice. Another would be to change the law so that relative monopolistic practices are per se illegal, avoiding judicial discretion.

In Mexico, the outcomes of closed cases fail to reject the hypothesis that the Commission

[3]See Castañeda Sabido and Ruiz (2021) for further explanation of these practices in the context of telecommunications.

[4]See Castañeda Sabido and Ruiz (2021).

is making Type II errors. Declining sales and wages with rising profit margins are still observed when the Commission investigates but does not impose a sanction, suggesting harmful conduct could emerge and persist in those cases. If there were not false negatives, one would expect a uniformly negative or null change in sales, wages, and profit margins after the Commission closes a case without sanction. We also find that the negative changes in sales and wages after closed cases are more negative in cases of absolute monopolistic practices compared to cases of relative monopolistic practices. This implies that, in this context, Type II errors should be most costly in cases of absolute monopolistic practices, where judges already do not have discretion. Type II errors would emerge in such cases because investigators did not have the resources to develop conclusive evidence, leaving judges with no choice but to acquit. Under these circumstances, to reduce the costliest Type II errors requires greater investigative resources to develop more conclusive evidence, rather than different legal standards or administrative discretion. Rose (2019) outlines policy options from the United States context that apply globally to increase investigative resources.

In addition to documenting the outcomes of closed antitrust investigations and evaluating antitrust sanctions relative to this novel counterfactual, our paper makes several contributions to the empirical literature on the effects of antitrust.

First, we provide the first systematic empirical comparison of the effects of sanctions of absolute monopolistic practices with the effects of sanctions of relative monopolistic practices. In the ongoing debate about how much judicial discretion is optimal in cases of relative monopolistic practices (see Khan, 2017; Salop, 2017), there is very little evidence on the outcomes in such cases. Existing empirical analysis on the effects of antitrust have focused mainly on cases of absolute monopolistic practices like bid rigging and price fixing (Sproul, 1993; Porter and Zona, 1993; Asker, 2010; Ivaldi, Jenny and Khimich, 2016; Igami and Sugaya, 2021; Starc and Wollmann, 2022). We find evidence consistent with relative monopolistic practices doing harm, but also that the economic stakes are lower than in cases of cartels. A caveat is that the industries studied here are not high tech.

Second, we exploit the unique Economic Census of Mexico, which allows us to measure industry performance inclusive of every firm in an industry, including smaller and privately held firms. Previous studies of antitrust enforcement (Bittlingmayer, 1993; Duso, Neven and Röller, 2007; Aguzzoni, Langus and Motta, 2013) have used event studies to estimate changes in the equity prices of publicly traded firms subject to investigation immediately after news of an indictment is released. In contrast, we are able to measure the effects of antitrust sanctions on industry concentration, total factor productivity, and measured misallocation. We confirm that increasing sales and wages after antitrust are associated with increases in average total factor productivity at the establishment level. Interestingly, these

increases in productivity coincide with essentially zero changes in the market share of the four largest firms or measures of factor misallocation like the correlation of market share and total factor revenue productivity (Olley and Pakes, 1996) or the variance of total factor revenue productivity (Hsieh and Klenow, 2009). These results are consistent with a model in which rather than changing market structure, antitrust sanctions change the industry equilibrium from a collusive cartel to a competitive equilibrium, and that new equilibrium incentivizing increased productivity across the productivity distribution, moving cartel beneficiaries away from a 'quiet life' of limited innovation (Hicks, 1935, pg. 8). In a high income context, Babina, Barkai, Jeffers, Karger and Volkova (2022) use the Economic Census of the United States to study four decades of Department of Justice lawsuits in an event study framework.

Third, we add to the body of evidence on the outcomes of antitrust in emerging market economies. In cross-country analysis, Besley, Fontana and Limodio (2020) show that increased scope for antitrust law reduces profits in the non-tradable sector, but beyond this, most evidence on the outcomes of antitrust come from case studies in the European Union and the United States (see Baker, 2003; Crandall and Winston, 2003; Ilzkovitz and Dierx, 2015, for reviews of this literature). Exceptions are Castañeda Sabido and Elbittar (2013); Castellanos, Del Ángel and Garza-García (2016) who provide detailed case studies of government intervention in Mexico's telecommunications and banking sectors since 1980. In complement, we study antitrust in manufacturing and service industries. Sampi, Urrutia and Vostroknutova (2022) study the effects of antitrust enforcement using a staggered difference-in-differences design and establishment surveys from Chile, Colombia, and Uruguay.

The paper unfolds as follows. Section 2 provides background on the Commission and the data. Section 3 describes our approach to estimating difference-in-differences outcome changes after each case, the triple difference approach to identifying the effects of sanctions, and three case examples. Section 4 reports the results, including the triple difference analysis of the effects of sanctions on sales, wages, employment, and profit margins, and heterogeneity by cases of absolute and relative monopolistic practices. Section 5 reports effects on total factor productivity and measured misallocation and discusses implications for aggregate outcomes. Section 6 concludes with policy implications.

2 Institutional Background and Data

This section provides background on Mexican competition law and the Federal Competition Commission, before describing the data used in our study.

2.1 Institutional Background

The Federal Competition Commission of Mexico The 1917 Political Constitution of the United Mexican States prohibits all but a few state monopolies (e.g., petroleum extraction, the telegraph) in the republic, with Article 28 requiring that the law "severely punish" the concentration of necessary consumption goods in the hands of one or a few agents. Yet it was not until 1993 that the Federal Competition Commission was established to enforce this law. A motivation for the legislation establishing an independent commission was that it would abrogate previous rules that concentrated enforcement of Article 28 in the Presidency, and had been used to justify measures like price controls.[5] The Commission was intended to bring antitrust enforcement up to date with internationally-agreed good practices, following for instance the United States Federal Trade Commission. Aydin (2016) provides further detail on the history of Mexico's competition laws, including reforms that provided indemnity for whistle-blowers, strengthened the role and independence of this authority, and also in 2013 renamed the Commission as the Federal Economic Competition Commission, abbreviated as COFECE in Spanish.

The Commission comprises a Board of seven Commissioners, nominated by the President and confirmed by the Senate, who are responsible for deciding on the Commission's cases; and an investigative authority empowered to investigate suspected illegal conduct and barriers to competition. As specified by law, strict operational separation is maintained between the two functions, with the investigative authority operating autonomously and interacting with the Board only when submitting recommendations to the Board's secretariat. The Commission is analogous to the Federal Trade Commission (FTC) in the United States in that it is responsible for both the investigation and adjudication of monopolistic practices, with operational separation between these two functions, though the agencies have different mandates. The FTC has an additional mandate on consumer protection, and unlike Mexico's Commission does not investigate economic cartels since in the United States those are prosecuted as criminal cases by the Department of Justice.

The Commission is highly regarded internationally. A peer-review by competition authorities in other OECD economies found that Mexico's competition regime is "equipped with strong powers, solid institutions and enforcement tools," and has staff that are highly qualified and viewed as professional and committed by Mexico's antitrust community (OECD, 2020). While antitrust enforcement actions in the United States have declined over the past 40 years (Babina et al., 2022), in Mexico they have expanded and touched all sectors of the

[5]The Law Granting Powers to the Executive Branch on Economic Matters (Ley de Atribuciones del Ejecutivo en Materia Económica in Spanish) gave discretionary power to the Executive branch in terms of controlling prices and even intervening in firms.

economy. Figure 2 shows that today about 40 percent of economic activities weighed by sales have been subject to at least one investigation into potential illegal monopolistic practices. The value of sales in each sector in 2018 is reported by a blue horizontal bar and a portion of the bar is shaded red to indicate the 6-digit industrial classifications within the sector that have been subject to investigation since 1993.

The investigative process Figure 1 describes the process through which the Commission investigates and decides on cases. As in other jurisdictions, investigations are initiated either by a formal complaint from the public (*denuncia*) or the Commission itself (*ex-officio*). After a complaint, a preliminary screen determines whether a full investigation is warranted. Given scarce investigative resources, if the alleged conduct does not constitute a violation of competition law, or if the complainant is not able to provide additional information, the case will be dismissed.

If the alleged illegal conduct is deemed possible, an investigation proceeds, and the Commission discloses a short note (*acuerdo de inicio*) with a description of the relevant market and the conduct under investigation. A formal definition of the legal market is not made in every case, but as a practical matter investigators must define the relevant market in order to assemble evidence, for instance through interviews with agents and analysis of data from a specific product market.

Once evidence is assembled, the investigative authority then acts as a prosecutor, gathering and organizing evidence to demonstrate agents' responsibility for illegal conduct. Once the investigation is complete, the investigative authority advises the Board as to whether it should proceed with a trial procedure (*procedimiento en forma de juicio*), or close the case in the absence of sufficient evidence of a violation. In practice trials are only recommended when the investigative authority is fairly sure the defendant is guilty, as with trial indictments by antitrust agencies in the United States Cases that are closed and which lead to a trial procedure are those that we consider.

The trial-like procedure In the case the investigators recommend a trial, the Board secretariat notifies the defendants of the charges, to which they have the right to respond and submit contrary evidence. The scope for administrative discretion depends on whether the alleged conduct is an absolute monopolistic practice or a relative monopolistic practice according to Mexican law. Absolute monopolistic practices, which include price fixing and agreements to segment a market, are *per se* illegal. In such cases, the Commission's Board must impose a sanction when there is conclusive evidence of illegal conduct, for instance a written agreement to fix prices. In contrast, in cases of relative monopolistic practices, such

as price discrimination or exclusive dealing, defendants have the opportunity to argue that their practices actually enhance efficiency, mitigate possible anti-competitive effects, and improve consumer welfare. In such cases, the authority can choose not to impose a sanction, even if evidence of the conduct is conclusive.

Lancieri, Posner and Zingales (2022) attribute a historical decline in United States antitrust sanctions to discretion by professional judges who retained a permissive "Chicago" school of thought that asserted many conspiracies by firms could promote efficiencies, and so should not be punished if they can be shown to benefit consumers. For instance Bork (1966), who was educated at the University of Chicago and taught at Yale University, argued that firms may fix prices if they use shared marketing services, to avoid a situation where a firm with a low-priced product benefits disproportionately from others' marketing expenditure. If firms did not fix prices in this example, firms might reduce spending on marketing, reducing overall sales. Mexican law adheres to a more skeptical "post-Chicago" school since it identifies price fixing and market division agreements as punishable without evidence of market power or consumer harm. Only in certain "relative monopolistic practices" can defendants argue their conduct was beneficial.

Once the arguments and evidence are presented, the secretariat proposes a resolution to the Board that includes the amount of a financial sanction (*sanción*) to punish the illegal conduct. The Board adopts a final decision based on the proposal. In rare cases, one of which is discussed in Section 3, the Board's decision can be appealed to a higher court, which may later ask the Board to reverse its decision and issue a new resolution.

2.2 Data

Relevant markets in antitrust investigations Our analysis begins with the universe of investigations into suspected monopolistic practices as of the end of 2018, since the founding of the Commission in 1993. The Commission's website provides a list of all cases and access to the disclosures of each investigation and final resolutions. Unlike in other jurisdictions including the United States, the Commission discloses the relevant market for each investigation, not just those that go to trial.

A task in antitrust investigation is the definition of the relevant market, or the set of products and geography in which the agent or agents under investigation compete (see Davis and Garcés, 2009). To analyze outcomes in investigated markets, we map the relevant market for each investigation to economic activities classified in the Economic Census. In doing so, we address two challenges highlighted by Shapiro (2018) and Benkard, Yurikoglu and Zhang (2021) that arise when mapping relevant markets in antitrust investigations to industrial

classifications in economic census data.

First, industrial classifications in censuses are based on points of production rather than consumption, and so may include or exclude goods and services that are not in the relevant product market. Even so, for purposes of data management, the Commission indicates a high-level industrial classification of the relevant market in each case and then provides further detail in a written description because the relevant markets in question are more specific than those covered by the high-level classification. We use this information to assign each relevant market a 6-digit industry classification from the North American Industry Classification System (NAICS), the lowest level of aggregation possible.[6] We define this as the *treated industry*. We then work forward and backward from the treatment date to define a consistent industry classification that captures the same set of activities from the first to last census in our dataset, including new activities that emerge during that time period. These classifications are generally more inclusive than a 6-digit classification, but there are many multiples within 3-digit classifications.

Second, relevant markets sometimes have specific geographic boundaries. In the majority of investigations, the relevant market is national (e.g., the national market for soft drinks). In the remaining investigations, we use relevant market definitions in the disclosures to define geographic boundaries for each market as a 6-digit industry within a set of municipalities or states. Within these boundaries, we aggregate establishment-level micro-data to construct series for the treated industry (e.g., all real estate brokers in the municipalities surrounding Lake Chapala in Jalisco state).

We define a pool of *control industries* to be all other 6-digit industries in the same 3-digit sub-sector as the treated industry. The motivation for this is that other industries in the treated industry should share common (unobserved) trends in demand and costs, while still being outside the relevant market defined by investigators. This approach to identifying controls in similar industries has been used in other studies of antitrust (Sproul, 1993; Kwoka, 2014). We successfully identify treatment and control industries for 95 of 261 investigations. Some cases are excluded because the decision of the Commission occurs after 2018, which means we do not observe treated outcomes, or because the decision occurs prior to 1998, which means we would not observe trends in prior outcomes, as required by the difference-in-differences estimator described in Section 3. For the other cases, control industries could not be identified in the same 2-digit or 3-digit sub-sector. These are industries with unique economic characteristics and no clear comparison group, such as natural gas pipelines and

[6]NAICS is a joint classification developed by the Statistical Offices of the United States, Mexico, and Canada. It offers high-level comparability across the three countries in terms of the conceptual framework but still leaves room for country-level specific codes.

commercial banks, or industries with rapid technological change over the study period, such as wired and wireless telecommunications. There are case studies of interventions in some of these industries (Castañeda Sabido and Elbittar, 2013; Castellanos et al., 2016).

We cover a greater subset of investigated industries than several previous studies of antitrust enforcement, which faced the similar challenge of matching relevant markets to plausibly similar controls. In his analysis of the effects of United States antitrust indictments, Sproul (1993) identifies relevant market control prices for 25 of "roughly 400" price-fixing indictments under the Sherman Act. In his analysis of United States antitrust decisions regarding horizontal mergers, Kwoka (2014) identifies 49 transactions for which rigorous retrospective studies of their effects are available. These transactions span the period 1976-2009 and during the period 1996-2011 alone the FTC investigated 264 horizontal mergers.

The Economic Census Having defined treatment and control industries in each case, we construct a series of economic variables for these industries using the establishment-level micro-data of the quinquennial Economic Censuses covering the calendar years 1993 to 2018. These censuses include every establishment in Mexico in every sector except agriculture, forestry, and hunting, and survey all physical establishments, including sole proprietorships and firms with so-called informal workers that do not contribute to the national social security scheme. The censuses are carried out by Mexico's national statistics body INEGI (*Instituto Nacional de Estadística, Geografía e Informática*), which provided us with access to the establishment-level microdata that we use to construct key variables: sales, employment, wages, operating profit margin, and total factor productivity (TFP). Sales and employment are measured as totals for each industry and year. Wages are measured as total remuneration to employees divided by total employment for each industry and year. The operating profit margin is measured as value added minus total remuneration, divided by sales for each industry and year. TFP is estimated for each establishment using an estimate of the production function as described in Section 5.

3 Empirical Design and Case Examples

In this section, we describe the difference-in-differences estimator used to measure outcome changes associated with each antitrust case, and the triple difference estimator used to measure the causal effects of the Commission's sanctions while correcting for selection of investigations into markets with unique characteristics. We close by reporting the difference-in-differences estimator graphically for three example cases and discussing the measurement issues and hypotheses that arise.

Informational setting Unanticipated news can lend a causal interpretation to a difference-in-differences estimator (Athey and Imbens, 2022). In our setting, the Commission initiates most investigations and decides whether to sanction the defendants based on the evidence within two years. The news of a final decision is plausibly unanticipated by market participants. Since the Commission's jurisdiction is national and there are many relevant markets, it would not be easy to predict which markets the Commission will investigate in a given year, and, given that many cases are closed without imposing a sanction, it would not be easy to predict how the Commission will decide in any one case.

We are interested primarily in the outcomes of the Commission's final decision to either sanction defendants or clear them of wrongdoing, so we identify the date of treatment with the date of the final decision by the Commission to either sanction the defendants or close the case. In most cases, investigations are initiated and cases closed between census years. In several cases, the investigation is initiated long enough prior to the final decision such that the existence of the investigation, but not its outcome, has been disclosed during some pre-treatment outcomes observed in the series. In these cases, the difference-in-differences estimator that we use to measure the outcome change after each decision matches trends in sales in the control industries during the pre-treatment period in which the investigation is disclosed, making it unlikely that the investigation disclosure causes a difference in treatment and control trends in the pre-treatment period that could contaminate the estimated effect of treatment. Even so, as a robustness analysis, in such cases, we will regress the difference-in-differences estimator on a measure of how long the case takes to close, and test whether it alters our conclusions.

Synthetic Difference-in-Differences The term y_{it} is the outcome of interest in industry i and time t. For each investigation j, let the term $\hat{\theta}_j$ indicate the observed outcome change after the final decision whether to sanction agents or not, and let $\hat{\sigma}_j$ indicate the standard error of that outcome change. We estimate these using the synthetic difference-in-differences (SDID)[7] proposed by Arkhangelsky et al. (2021), which can be represented as a weighted two way fixed effect regression of the form

$$y_{it} = \alpha_i + \beta_{jt} + \hat{\theta}_j^{SDID} W_{it} + \xi_{it} \tag{1}$$

[7]Like the difference-in-differences (DID) method, the SDID method is invariant to additive market-level shifts in the outcome, allowing more flexibility than the synthetic control (SC) method (Abadie, Diamond and Hainmueller, 2010) that requires that levels and not only trends in the outcome match exactly when comparing treatment and synthetic control in the pre-treatment period.

where $i \in \mathcal{I}^j$ indexes the set of treatment and control industries in case j ordered so that i =1 is the treated industry. Control industries include those in the same 3-digit sector that are not in the relevant market and have never been investigated by the Commission. Standard errors $\hat{\sigma}_j$ are calculated using the placebo method described by Arkhangelsky et al. (2021), in which the treatment effect is estimated in 400 industries drawn with replacement from the donor pool of control industries.

Recent work (see the helpful summary in Borusyak, Jaravel and Spiess, 2021) has identified several issues that arise when estimating a two way fixed effect regression with multiple units treated at different times. Our approach avoids such issues by identifying a never treated control group separately for each case, and estimating Equation (1) separately for each case. The case specific time fixed effects β_{jt} are estimable because we have a control group for each case.

3.1 Three Examples of Outcomes of Antitrust Enforcement Decisions

Three case studies demonstrate how the SDID estimator works in our setting, and the issues and hypotheses that arise. In this discussion we focus on (the log of) sales as an outcome because it is a basic indicator of the value of an industry, including value provided to customers. The three case studies here are selected non-randomly, specifically because they are outliers in certain ways. Later in Section 4 we will compare and contrast these cases with averages from across many cases in order to highlight the value of our meta regressions in disciplining inference from individual case studies.

First, we describe the largest positive effect of a sanction on (the log of) sales, which is a sanction of a real estate brokerage cartel around Lake Chapala. Second, we describe the largest negative effect of a decision on market revenue, which is a closed case regarding refusal to deal requirements in the wedding supply industry of Guadalajara. Third, we describe the effect of the Supreme Court reversing the largest-ever financial sanction on a firm in the national soft drinks industry.

Example A) Sanction for price fixing by real estate brokers at Lake Chapala This investigation began in 2007, after a complaint was filed alleging the real estate brokers' associations and their members were fixing brokerage rates on the shore of Lake Chapala, Mexico's largest freshwater lake. The accused parties were alleged to have arranged agreements to fix prices, which is per se illegal. One piece of evidence collected was written regulation of Del Lago Real Estate Group association that states explicitly that agents should

gather to share information and fix brokerage rates (i.e., at a minimum 7 percent rate for sales above a certain value). Internal fines were imposed against agents that do not comply with these rates. In fact, the individual filing the complaint had been associated until he was forced to resign after offering clients discounts relative to the agreed rates.

We identified the treated activity as #531210 "realtors and real estate brokers" in the municipalities around Lake Chapala. This treated activity is compared to other 6-digit activities in the same 3-digit classification called #531 "real estate services," which include, among other activities, housing and office units that are rented directly by the owners without the use of a broker. Panel A of Table 1 reports the control industries in real estate services and the synthetic control weights that give their weighted average the same pretreatment trend as the treated industry. Weights are roughly equal across control industries. For consistency with the cases that have national relevant markets, national sales rather than local sales are used as the data for the control group.

After investigation and several appeals by the defendants, in 2014 the Commission issued a resolution finding the defendants responsible for price fixing, sanctioning them a fine totaling 30,000,000 Mex$, and ordering the suppression of such practices. Figure 3 shows the SDID effect of this sanction on (the log of) sales. The thick blue line connects the average of (the log of) sales in the treatment industry after and before the sanction. The thick red line connects these same averages in the synthetic control industry. The counterfactual outcome in the treated industry is constructed by shifting the thick red line up along the dotted lines so that the pre-treatment average control outcome matches the pre-treatment average treatment outcome. This counterfactual outcome is indicated with a red dot. The SDID is the difference between this counterfactual and the observed outcome in the treated industry, indicated by the arrow. This corresponds to an annualized increase in sales of 5.05 (s.e. = 1.39) percent. Sanctioning the cartel apparently caused the industry to grow.

This case took seven years to be resolved because of countersuits for extraordinary constitutional protection of the defendants (*amparo*) and other legal maneuvers, raising the possibility of a potential anticipatory effect before the sanction, which could contaminate the SDID estimate. It would be concerning if there was a substantial jump in the difference between treatment and control groups during the period that the investigation was being contested, but there does not seem to be cause for concern. The thin blue line in Figure 3 connects the observations of (the log of) sales for realtors and real estate brokers around Lake Chapala, and appears to be flat or potentially declining during the period of the investigation. This slowdown in growth could potentially be due to the cartel's operation. The thin red line plots the same outcome in the synthetic control industry, which exhibits a very similar pre-trend in (the log of) market revenue prior to the decision. Since the synthetic

control industry is weighed to track the treated market even during the brief period after 2007 when the investigation was disclosed but a final decision had not been made, there is no apparent difference in pre-trends prior to the final decision.

Example B) Closing a case of exclusive contracting in the wedding planning and supply industry of Guadalajara In 2003, Revistas Novias y Eventos S.A. (Brides and Events Magazine) filed a complaint against Producciones y Convenciones S.A. (Productions and Conventions) for the alleged commission of relative monopolistic practices in wedding planning and supply services in Guadalajara city. Revistas Novias y Eventos is a company dedicated to the publication of magazines on topics related to weddings and other social events, as well as the organization of exhibitions promoting the sale of goods and services used in those kinds of celebrations. The complaint alleged that Producciones y Convenciones conditioned the sale of stands at its separate exhibition *Expo Tu Boda* (Expo Your Wedding) to various exhibitors on the requirement that they not participate in future events organized by Revistas Novias y Eventos. According to the complaint, the prestige of *Expo Tu Boda* would have led the exhibitors to agree to refuse to deal with Revistas Novias y Eventos.

Under the law, relative monopolistic practices like exclusive contracts should be punished unless they are shown to not harm consumer welfare. Here, the Commission did not find convincing evidence of harm, and so closed the case without sanctioning the practice in 2004. In interviews, various exhibitors supposedly withdrew from *Expo Tu Boda* after refusing to stop requesting the services of Revistas Novias y Eventos. This case highlights the ambiguity judges face in evaluating exclusive dealing arrangements (see, e.g., Gavil and Salop, 2019). Even if agents refuse an exclusive contract, should offering it still be punished if it was intended to restrict competition?

In this case, the treated industry is identified as #463214 "retail of wedding and regional dress" in the municipalities of Guadalajara. Firms in that industry would have participated in the exhibitions. Panel B of Table 1 shows the control industries and their synthetic control weights. Of six industries, one has 55 percent of the weight, retail trade of disposable diapers and sanitary products, and another has 20 percent, retail trade of hosiery and items used in sewing. It is not difficult to imagine young newly-weds driving a similar trend in wedding dresses and diapers, so this matching provides some proof of concept that the weighting approach selects industries with plausibly similar drivers of demand. Inspection of Figure 4 shows that the treatment and control lines do have a similar trend prior to treatment though the history is shorter than in the previous example. In this figure, the SDID effect of closing the case is an annualized decrease in sales of 12.3 (1.41) percent. Sales in the treatment industry also fall in absolute terms. Whatever happened at the time of the case

caused retail sales of wedding dresses in Guadalajara to decline. While the investigation did not find conclusive evidence of harm, the result here suggests it is possible, though not conclusive, that some went undetected. Yet, this case also illustrates that exogenous factors could drive differences between treatment and control industries, raising questions about the difference-in-difference alone as a means to identify the effects of antitrust. The divergence of diapers and wedding dresses could relate to population aging, as second children are born without additional marriages.

Example C) Reversing the largest ever financial sanction for monopolistic practices in Mexico's history (610M Mex$). This case provides an example of a nationally relevant market, that is, one of the majority in which the good or service is traded across the entire country. The relevant market is #312111, the manufacture of soft drinks. After the United States, Mexico ranks second in the world in per capita consumption of non-alcoholic carbonated beverages. In Mexico, the market was initially dominated by Coca-Cola. In 2000, competitors Aga and Pepsi-Cola filed a complaint against Coca-Cola and its bottling companies, accusing them of requiring retailers to carry Coca-Cola products exclusively. This allegedly prevented competitors from accessing the market, in particular, through grocery and miscellaneous stores, which are the most important sales channels for soft drinks. The alleged practices involved the sale of soft drinks subject to the condition of not selling competitors' products and the granting of discounts subject to the same condition. Initially, in 2002 the Commission found the alleged parties responsible for the denounced acts, fined the bottlers a total of 610,740,000 Mex$, the largest sanction in history, and ordered the suppression of the exclusive dealing practices.

However, appeals made their way to the Supreme Court. In 2010, the Supreme Court found that the Commission had proceeded illegally, not considering the evidence provided by Coca-Cola that its practices did not cause harm. In 2011, the Board issued a resolution stating that there is not enough evidence to support harm of consumers and closed the case.

Figure 5 shows the SDID outcome of the Supreme Court reversing the sanction is an economically and statistically imperceptible increase in sales of 0.7 (3.7) percent. Here is a case in which, unlike in Example B, a decision to close a case of relative monopolistic practices seems to have had little effect on the trajectory of the industry. Panel C of Table 1 reports the synthetic control weights. The largest weight (37%) is given to the manufacturing of rum and other distilled cane beverages, which are complements of soft drinks. Essentially zero weight is given to tobacco products, which are less obviously related to soft drinks.

Summary and implications for estimation of causal effects. These three cases studies illustrate heterogeneous outcomes across cases. They also raise questions about the validity of using the difference-in-difference approach alone to identify the effect of antitrust sanctions on industry performance. The average difference-in-differences effect of sanctions in Figures 6 and 7 assumes that the synthetic control industry is similarly affected by all causal influences other than those leading to the Commission's final decision. However, industries with similar pre-trends in the same sub-sector may not be the appropriate control.

Indeed, the Commission explicitly targets investigations towards industries where illegal conduct is suspected, as in Example A, with about half of investigations beginning because agents denounce conduct by others in their industry or firm. Treatment industries could be undergoing unique changes caused by or that cause the illegal conduct that is being investigated. If true, this means that difference-in-differences changes in sanctioned industries provide an incomplete picture of the effect of antitrust sanctions. One hypothesis is that illegal conduct emerges as a response to specific exogenous trends, such as declining demand. In Example B in wedding dress retail, for instance, the impetus for exclusive contracts may have been that the industry was declining, as the population aged and traditional weddings became less popular. In a very recent case, an agent in one sanctioned cartel suggested collusion was the only way to overcome a decline in the industry and was recorded saying to cartel members either "we cooperate or we exit."[8]

Example C illustrates a rare case in which a decision is reversed after a long appeals process. To account for such delays, in all trials procedures with appeals we measure the effect only of the Commission's final decision. Though such cases may generate potential anticipatory effects that would affect a DID estimator, the SDID estimator ensures the counterfactual industry also has pre-treatment trends that mirror any anticipatory effects. We will also verify that case duration does not shape our estimate of overall case effects.

4 Results

Our first result is to simply summarize the average of the difference-in-differences estimator in cases where the Commission imposed sanctions, as in Sproul (1993). Figure 3 reports the SDID changes in the log of sales after 35 cases, all of those for which we could identify: (i) the treated industry, and (ii) never treated control industries in the same sub-sector. The figure also reports the average change in the log of sales after a sanction, weighting cases by

[8]In a recent resolution, the pharmaceutical firms Casa Marzam (Marzam), Casa Saba, Fármacos Nacionales (Fanasa), and Nadro y Almacén de Drogas were fined 903 Mex$ for restriction of supply of medicaments to increase the price during 2008 to 2016.

the inverse variance of the SDID estimator for that case. This is a so-called "fixed effect" meta-analysis framework, which assumes the effect of sanctions on the log of sales is constant across industries. The overall effect of sanctions measured in this way is an annualized 1.3 percent with a tight 95 percent confidence interval [0.7,1.9], indicating sanctions are followed by an increase in sales growth of 1.3 percentage points annually. Although the inverse variance weighting does give as much as 20 percent weight to single observations, the point estimate of the average effect is within the confidence intervals of most cases. Figure 4 shows the same table where the outcome is the change in the log of the wage bill. Here we find a positive overall annualized average change in wages of 1 percent [0.2, 1.8] indicating sanctions are followed by a statistically significant increase in wage bill growth of 1 percentage point annually. Consumers and workers appear to benefit.

Unlike the United States context in Sproul (1993), in Mexico we are able to observe outcomes in industries where illegal conduct is suspected yet unpunished as in Example B. These outcomes provide an alternative counterfactual for the treated industries that allows us to disentangle the effect of the Commission's sanctions from the selection effect that arises in the difference-in-differences estimator due to the targeting of investigations. The only example of an analysis like this within investigated markets that we know of is Kwoka (2014). He exhaustively compiles from the literature difference-in-differences price changes after mergers, where control industries are chosen similarly as in our study from the same sector. In a triple difference analysis, he then compares the outcome changes after mergers where the regulator imposed a conduct remedy, which allows firms to merge but restricts their conduct, with outcome changes in mergers where the regulator did not. This approach controls for the fact that merging industries are undergoing unique changes in the competitive environment and could be different due to exogenous factors that lead to such change. He finds that most mergers raise prices, and that conduct remedies are not associated with lesser price increases. Unlike in Mexico, the authority in the United States does not disclose which mergers are investigated, rather, given their size, Kwoka (2014) is able to credibly assert with "virtually certainty" that an investigation is conducted.

In addition to providing an alternative counterfactual for industries treated with a sanction, the closed cases allow us to observe the effect of "a shot across the bow" by the competition agency. That is, the effect of an investigation of potential legal conduct and a public announcement that accused economic agents were not guilty of illegal monopolistic practices. In principle, this effect could go either way. On one hand, firms might be deterred from illegal conduct once they realize the threat of investigation. On the other hand, illegal conduct could be emboldened if the decision to close the case is, on average, an error as in the example provided by Khan (2017).

4.1 Meta-Analysis of Investigations by the Federal Competition Commission of Mexico

We use the SDID method to estimate the pair $(\hat{\tau}_j^y, \hat{\sigma}_j^y)$, where y indicates the outcome of interest, for instance (the log of) sales, (the log of) the wage bill, or (the percentage point) operating profit margin. To allow for a comparison in outcome changes across the same set of cases, we estimate the SDID for all variables using the same set weights, which match treatment and control on (the log of) sales. For robustness, we also report results using the DID and SC estimators, which weight control industries differently.

The results are reported in the meta regression

$$\hat{\theta}_j^y = \tau_0^y + \tau_1^y S_j + u_j^y + \hat{\epsilon}_j^y. \tag{2}$$

where $S_i \in \{0, 1\}$ indicates a sanction is imposed by the Commission. The term τ_0^y is the average outcome change after an investigation that does not lead to a sanction, and the term τ_1^y is the additional outcome change when there is a sanction. Under the assumption that news of each decision is unanticipated, we interpret τ_1^y as the causal effect of sanctions within the set of industries where illegal conduct is suspected. This effect is measured controlling for any unique characteristics of industries where sanctions may occur.

Vita and Osinski (2016) criticize Kwoka (2014) for not accounting for sampling variance in his analysis of difference-in-difference price changes after mergers. We incorporate such variance into our analysis. Measurement error $\hat{\sigma}_j^y$ is estimated in the first stage and included in the meta regression in the term $\hat{\epsilon}_j^y \sim N(0, \hat{\sigma}_j^y)$. In some specifications, the regression allows for heterogeneity in the difference-in-differences due to differences across industries through the term $u_j^y \sim N(0, \gamma_j^y)$. This is called a random effects (RE) meta-analysis framework, used in the medical sciences literature (Borenstein, Hedges, Higgins and Rothstein, 2021). The fixed effect (FE) meta-analysis framework would set $\gamma_j^y \neq 0$ and $u_j^y = 0 \forall j$, assuming that the effect is constant across the population. Random effects meta estimators that allow for $\gamma_j^y \neq 0$ are discussed by Vita and Osinski (2016) and are recommended by Borenstein et al. (2021) in their review of the meta-analysis literature. In the context of antitrust, random effects seem consistent with the approach in empirical industrial organization, in which industry specific case studies are preferred given that differences in demand and technology imply potentially substantial differences across industries in the economic effects of antitrust policy (Berry et al., 2019). Though the RE model makes use of more information, it also may have worse finite sample properties compared to the FE model, as noted by Angrist and Pischke (2009, footnote, p. 223). For this reason, we begin by reporting the FE specification that restricts $u_j^y = 0 \forall j$, and then move to the RE specification, which we prefer on the a priori ground

that the effects of antitrust should be different across industries. In practice, the random effect framework proves to be conservative, delivering larger standard errors and statistically similar point estimates, and placing more similar weight on each case as compared to the FE approach.

Antitrust and sales Sales summarize the value an industry provides to customers. Table 2 reports estimates of Equation (2) where the left hand side variable is the change in (the log of) sales after the Commission's decision to sanction or close a case. The different columns report alternative measures of the outcome change. Across all columns, the constant τ_0^{sales} is negative and the dummy for sanctions τ_1^{sales} is positive, while both are statistically significant. This indicates that investigated industries on average have declining sales, but that financial sanctions can reverse this trend.

Columns (1) - (3) report the fixed effect specification, where the meta regression is weighted by $\frac{1}{\sigma_j^2}$ and $u_j = 0 \forall j$. The constant τ_0^{sales} reports the average outcome change for investigations that end without a sanction. These are cases like Examples B and C in Section 3 in which the Commission ultimately decides that agents under investigation are not guilty of illegal monopolistic practices. Across all columns, the values of the constant τ_0^{sales} are negative and statistically significant. The first specification in Column (1) is the SDID specification where $\tau_0^{sales} = -0.025$ (0.002), indicating sales are on average declining by 2.5 percent annually in investigated industries relative to the control.[9] This coefficient estimate is smaller and has a smaller standard error compared to the DID specification in Column (2) and the SC specification in Column (3). This is consistent with evidence in Arkhangelsky et al. (2021) who show use of the SDID can lead to estimates that are smaller in absolute value than the DID and SC, but are more precise.

The contrast, when cases end in sanction Column (1) shows $\tau_1^{sales} = 0.038$ (0.004) and implying an increase in sales of 3.8 percent annually relative to cases that are closed without sanction. Moreover this coefficient is larger than the coefficient τ_0^{sales} indicating that a financial sanction on average completely reverses the decline in sales observed in investigated cases, increasing sales overall. We interpret this triple difference as the causal effect of sanctions within the pool of industries that are investigated. The closed cases allow us to control for selection of investigations into industries where illegal monopolistic practices are suspected.

In Column (4), we return to the SDID outcome change, but include in the meta regression

[9]SDID effects are annualized by dividing the SDID effect and its standard error by the number of years that elapse between the pre-treatment and post-treatment averages. This number varies slightly across cases because the SDID estimator weights pre-treatment periods differently in each case to improve precision (Arkhangelsky et al., 2021).

the random effects parameter $u_j \sim N(0, \gamma_j)$ and weigh the regression instead by the term $\frac{1}{\sigma_j^2+\gamma^2}$, distributing weight more equally across cases. This yields coefficients that are larger in absolute value, though they are estimated less precisely. The random effects model produces $\tau_0^{sales} = -0.071$ (0.011) and $\tau_1^{sales} = 0.058$ (0.018), reproducing the qualitative pattern that sales are declining after closed cases, but that this trend is reversed when there is a sanction. The variance of the random effect term estimated at $\hat{\gamma}^2 = 0.00625$, or 0.625 percent, indicating modest heterogeneity across industries in the effect of antitrust on sales.

For robustness, Column (5) reports the random effects specification where the outcome change is measured not using the SDID but the first difference (FD): the average value of (the log of) sales after treatment minus the average value (the log of) sales before treatment. There are more observations in this column reflecting a number of cases where we could not identify a never treated control industry in the same three digit sub-sector. In this way, the FD specification allows us to use a larger sample than the SDID specification. This value yields $\tau_1^{sales} = 0.046$ (0.009), which is positive and statistically significant like in Columns (1) - (4) . This suggests that the estimated effect of sanctions, relative to the counterfactual of a closed case, is unaffected by the choice of control group in the difference-in-differences. Notably, however, the sign of τ_1^{sales} is positive, indicating that investigated industries have positive trends in nominal sales. This highlights the importance of including a control group to identify the effect of closed cases, and control for sector specific trends, for instance in the price deflator.

Antitrust and workers The effects of antitrust on workers' outcomes are reported in Table 3. Our discussion focuses on the random effects specification using the SDID estimator in Column (4), though results are qualitatively similar in Columns (1) - (3), as in Table 2. Panel A reports results where the outcome is the industry wage bill. In Column (4) $\tau_1^{wagebill} = 0.055$ (0.016) and $\tau_0^{wagebill} = -0.054$ (0.010). This implies a statistically significant 5.4 percent annual decline in wages relative to the synthetic control when an investigation is closed, but, relative to this counterfactual, a 5.5 percent annual increase in wages when there are sanctions. Panel B reports results when the outcome change is the log of employment, or number of workers, yielding $\tau_1^{employment} = 0.033$ (0.014) and $\tau_0^{wage} = -0.047$ (0.009). Panel C reports the results when the outcome change is the log of the total wage bill divided by total employment in the industry, yielding $\tau_1^{wagerate} = 0.028$ (0.008) and $\tau_0^{wage} = -0.014$ (0.005). The results in Panels B and C are quantitatively similar to Panel A. The values in Panel C (wage rate) are not exactly the values in Panel A (wage bill) divided by the values in Panel B (employment) because the inverse variance weights differ across panels. Investigations target markets where wages and employment are falling, and that sanctions can reverse these trends.

Evaluated relative to the counterfactual of an industry where collusive conduct is suspected but not sanctioned, antitrust sanctions create additional growth in jobs of 3.3 percent per annum, and annual wage growth of 2.8 percent per annum. In Section 5, we will explore the extent to which this increase in the wages reflected increased improvements in productivity, rather than movement up the labor supply curve.

While Sproul (1993) and Kwoka (2014) focus on effects of antitrust on prices and consumers, Marinescu and Posner (2019) survey recent literature identifying potentially large negative effects of monopsony power in labor markets for workers. In our setting, antitrust in a product market has a substantial effect on workers in that industry. Such effects could be motivated by collusive conduct to restrict sales, which requires hiring fewer workers. Marinescu and Posner (2019) note that labor market concentration need not be correlated with product market concentration. In our setting, product market power appears to be directly related to labor market power.

Antitrust and the operating profit margin In Table 4 we turn to the effects on the operating profit margin, measured in percentage points. If the mechanism for increased sales after antitrust sanctions is lower prices, and for increased wage bill, a higher wage rate, we would expect antitrust to put downward pressure on the operating profit margin. We exclude any (net) financial costs when calculating profit to control for differences in capital structure and facilitate comparisons across industries that have different fixed and sunk costs. Column (4), again reporting the SDID and random effect specifications, shows that without sanctions, the average profit margin of an establishment increases by investigated industries by $\tau_0^{profit} = 1.630$ (1.313) percentage points. We do not annualize this effect, so it reflects a change over about 15 years. This suggests that investigators are targeting investigations towards industries where profits are rising by 1.63 percentage points. Sanctions appear to reverse this with $\tau_1^{profit} = -0.677$ (2.028). Unlike the effects on sales and wages, these results are not statistically significant at standard levels. This is because the random effects model estimates much greater heterogeneity in the effect of antitrust on profit margins, relative to effects on sales. In Column (4) we estimate $\gamma^2 = 56.16$, equivalent to 56 percentage points of profit margin. This result indicates that while effects of antitrust on sales and wages can be fairly homogeneous, effects on profits may be more variable. In the FE specifications in Columns (1) - (3) the coefficients are estimated much more precisely. The SDID estimator with fixed effect is quite similar in magnitude to Column (4) with $\tau_0^{profit} = 1.604$ (0.333), and is statistically significant. This gives us confidence that, on average, closed cases do have rising profit margins, in addition to declining sales and wages.

An interpretation of the sales and wages results so far is that they are consistent with the

classic anti-competitive conduct of firms restricting quantities to raise prices. One might argue for an alternative interpretation that, despite its mandate to investigate anti-competitive conduct, the Commission is investigating industries that are declining for exogenous reasons. This explanation accounts only for declining sales and wages, but not the rising profit margins in treated industries documented in Table 4. Considering the results on profits, prima facie it appears that industries where investigations are closed are characterized by emergent anti-competitive conduct.

4.2 False Negatives or False Positives?

Antitrust enforcement can be viewed as a decision theory problem, where the competent authority tries to identify and sanction all harmful conduct under incomplete information using available investigative resources and administrative discretion (Beckner and Salop, 1999; Salop, 2017). One area of debate regards what presumptions judges should make about guilt in the presence of incomplete information.

Khan (2017) argues that judges are biased towards making Type II errors (false negatives, or not finding violations when the conduct harmed competition) due to a presumption that Type I errors (false positives, or finding violations when the conduct did not harm competition) are especially costly relative to Type II errors. Salop (2017) provides examples of when Type II errors are especially costly, suggesting such a presumption, if it exists, could be inappropriate in at least some cases. One policy response would be for judges to instead presume that Type II errors are more costly than Type I errors, and reject defense arguments more readily.

In Mexico, the fact that decreases in sales and wages and increases in profit margins are observed when the Commission investigates but does not to impose a sanction fails to reject the hypothesis of Type II errors. If there were no false negatives, one would expect a uniformly negative or null change in sales, wages, and profit margin after the Commission decides not to sanction, rather than the pattern we observe. The results are also not consistent with harmful false positives. In the presence of a harmful false positive, one would expect a greater decline in sales in wages after sanctions relative to after closed cases, which is the opposite of what we find.

4.3 Assessing Threats to Identification through Analysis of Heterogeneity by Type of Investigation

Relative to the sample of only cases ending in sanctions, the sample including closed cases helps to correct for a sampling bias due to the fact that only cases likely to end in sanction are

brought to a trial (or administrative "trial-like procedure" in the case of the Commission). This analysis makes the assumption that sanctions are quasi-random within investigations, effectively assuming it is by chance whether investigators collect enough evidence of illegal conduct for the Plenary to impose a financial sanction on the agents. In support of this assumption, we present evidence that the effects on sales, the wage bill, and profit margins hold in two subsets of the data: (i) cases of "absolute monopolistic practices" where administrative discretion to sanction is limited by law, (ii) cases of ex-officio investigations, which were initiated by the Commission, and in which investigators plausibly (though not necessarily) have a greater incentive to search for all available evidence, (iii) case duration, and (iv) whether the case includes a conduct remedy.

In this subsection, we report results from the meta regression

$$\hat{\theta}_j^y = \theta_j^y + \tau_0^y + \tau_1^y S_j + \tau_2^y X_j + \tau_3^y (S_j \times X_j) + u_j^y + \hat{\epsilon}_j^y. \tag{3}$$

We are particularly interested in τ_3^y which gives the different effect of sanctions in subsets of investigations defined by the covariates in X_j. In the table we report results for three outcomes, sales, the wage bill, and operating profit margin, and different covariates X_j.

Absolute vs. relative monopolistic practices Table 5 reports an estimate of Equation (3) where in place of X_j is used a variable indicating whether the conduct in question is an absolute monopolistic practice under Mexican law. As described in Section 2, Mexican law makes clear that certain conduct is subject to a different legal standard. Cases of absolute monopolistic practices are per se illegal. In such cases, the Commission must impose a sanction when there is conclusive evidence of the conduct. In contrast, in cases of relative monopolistic practice, defendants have the opportunity to argue that their practices actually improve consumer welfare and the authority can choose not to impose a sanction even if evidence of the conduct is conclusive. In Table 5, Columns (1), (2), and (3) report results where the effect of sanctions is interacted with a dummy variable indicating an absolute monopolistic practice case. The results suggest the effects of sanctions are much more economically significant in absolute monopolistic practice cases. For instance we have $\tau_1^{sales} + \tau_3^{sales} = 0.071(0.027)$. Further, in closed cases of absolute monopolistic practices, wages appear to decline by more than in relative monopolistic practice cases, with $\tau_2^{wages} = -0.038(0.022)$. The pattern is similar for wages and the profit margin.

There are two inferences from these results. First, this mitigates concerns that the previous estimates of τ_1^{sales}, τ_1^{wage}, τ_1^{profit} are driven by administrative discretion, wherein the Plenary sanctions industries that are growing for exogenous reasons. Since the positive

effects of sanctions are concentrated in the absolute monopolistic practice cases where there is no administrative discretion, the effects cannot be due to administrative discretion. Rather sanctions in these cases occurred because there was conclusive evidence that per se illegal conduct had taken place.

Second, these results are informative about how one might reduce Type II errors. The coefficients on the dummy for absolute monopolistic practice ($\tau_2^{sales}, \tau_2^{wage}, \tau_2^{profit}$) give the outcome change in absolute monopolistic practice cases that are closed without sanction, compared relative to monopolistic practice cases that are closed without sanction. These indicate the potential impact of Type II errors, or false negatives. Here we see that $\tau_2^{sales} = -0.009$ (s.e. $= 0.025$), $\tau_2^{wage} = -0.038$ (s.e. $= 0.022$), and $\tau_2^{profit} = -0.307$ (s.e.=3.161), implying that declines in the wage bill in closed absolute monopolistic practice cases are significantly more negative than in closed relative monopolistic practice cases. Evidence on the changes in sales and profit margin is inconclusive due to the larger standard errors.

Outcome changes in closed cases can be understood to reflect the cost of Type II errors. Looking at wages, the cost of a Type II error seems to be larger in cases of absolute monopolistic practice. These results suggest the best margin for improving welfare by reducing Type II errors may be an expansion of investigative resources to develop more conclusive evidence in cases of absolute monopolistic practice, rather than different administrative discretion in cases of relative monopolistic practice.

Public complaints vs. ex officio investigations Even though the results are robust in the sub sample with limited judicial discretion, sanctions could still fail to be quasi-random within investigated cases if the investigative authority brings different effort in certain cases. Interviews with Commission staff and past Commissioners suggest this is unlikely, as investigators have a professional responsibility to work equally hard to find evidence in all cases.

To assuage any remaining skepticism, Table 6 reports an estimate of Equation (3) where in place of X_j a variable is used indicating whether the origin of the case was an ex-offcio investigation, rather than a complaint from the public. Over the years, the Commission has targeted its own investigation based on various criteria, including whether the industry has linkages to other important industries, and whether there are large gaps between wholesale and consumer prices, or high prices relative to international markets. In such cases, (though not necessarily) investigators plausibly have a greater incentive to search for all available evidence, since they initiated the investigation in the first place. In the table, we find that sanctions do not have significantly different effects in cases where the investigation comes from a public complaint. However, the effects of a closed ex-officio investigation on sales and

wages, τ_2^{sales} and τ_2^{wage}, are negative and statistically significant. This suggests that Type II errors are potentially less costly in such cases and that it is in ex officio investigations where the stakes are the highest. One explanation for this is that the ex officio cases focus more on absolute monopolistic practice cases, which as we have shown above appear to have higher stakes for sales and wages.

Time between investigation and final decision Crandall and Winston (2003) observe that antitrust decisions may have a lesser impact on market outcomes if they occur a long time after the alleged anti-competitive conduct occurred. This could be due to the fact that enforcement takes so long after the alleged illegal conduct that market conditions change naturally and make intervention no longer effective. The authors provide the example that a final decision to break up Standard Oil in the United States took over a decade from the first indictment, during which time United States oil production became more competitive with new entrants from Texas, reducing the impact of the remedy on Standard Oil's monopoly position.

We test the hypothesis that case duration affects outcomes in the Mexican context in Table 7 where the sanction indicator is interacted with a variable called duration equal to number of years between the beginning of the investigation and the final decision. The duration is normalized as a Z-score by subtracting the average duration and dividing by the standard deviation. In these regressions we find that the interaction effects are not statistically significant, suggesting that effects of sanctions are not statistically different in longer cases. The interaction effects on sales and wages are positive however, suggesting that, if anything, the effect on production is positive rather than negative as hypothesized.

The coefficients on the duration variable τ_2^{sales}, and τ_2^{wage} however are statistically significant and negative. This suggests that, for closed cases, the ultimate decline in sales and wages is greater in industries where the cases have a long duration. An interpretation of this result is that cases that are most contested by defendants, and thus have longer appeals processes, are also the ones where conduct was most harmful. From this perspective the soft drinks Example C in Section 3.2. is not representative of all cases, because there we found only a shallow decline in sales after a long contested final decision to close a case.

Conduct remedies In addition to sanctions, a small number of cases require conduct remedies from market participants. Rather than additional requirements placed on firms as in the conduct remedies studied by Kwoka (2014) these are typically requirements that public agencies remove barriers to entry. For example, in one case, state authorities were required not to give preferential treatment to unions or business associations in the granting

of concessions. In another, a state was required to eliminate an authorization requirement for the entry of pasteurized and ultra-pasteurized milk into the state, a barrier to interstate commerce. In Table 8 we also include a dummy for such remedies. We find that, if anything, they reduce sales and wages, the opposite effect of sanctions. An interpretation of this result is that reducing such state-imposed barriers to entry in this context does not have as great an effect as sanctioning illegal conduct by private firms.

5 Aggregate Effects of Antitrust

The Mexican economy is characterized by significantly higher inequality and slower productivity growth than high-income countries (Levy Algazi and Walton, 2009; Hsieh and Klenow, 2014; Levy Algazi et al., 2018). What can we learn from the case of Mexico about the potential effects of antitrust on aggregate outcomes in this context?

The evidence presented so far suggests that antitrust contributes to reducing aggregate inequality. The Commission has investigated about 40 percent of industries, and in the investigations that do not end in a sanction, sales and wages are declining relative to a control while profit margins are rising. This indicates that a good share of the economy has characteristics consistent with possible monopolization. Sanctions appear to reduce the harmful effects of this conduct on consumers and workers. Increases in sales and wages after sanctions suggest a reallocation of surplus from shareholders to consumers and workers, which will reduce inequality if shareholders are those at the top of the welfare distribution.

Another measure of inequality is the dispersion of total factor productivity across establishments, which has a direct link to aggregate productivity. In this section, we first describe first the effects of antitrust on concentration and informality, which can shape productivity dispersion. Then we measure total factor productivity at the establishment level, and report how the productivity distribution changes after investigations. We close with a discussion and quantification of how this might aggregate into national productivity growth.

Antitrust, Concentration, and Informality Table 9 reports the RE meta-regression specification where outcomes are alternative measures of concentration. In Column (1) the left-hand side variable is the SDID change in the log of the number of firms. There $\tau_0^{firms} = 0.140$ (s.e. $= 0.083$) is negative and statistically significant, indicating industries have 14 percent fewer firms on average relative to those in the industry after a closed case. Investigated industries are those where firms are exiting, consistent with the declining sales and wage bill observed previously. The term τ_1^{firms} is positive, indicating after sanctions there is relatively less exit, though this effect is not statistically significant. In Column (2)

the left-hand side variable is the SDID change in the Herfindahl-Hirschman Index (HHI) or the sum of squared percentage market shares, which ranges from 0-10,000. Interestingly, the coefficient $\tau_0^{hhi} = -510.553$ (198.522) is negative and statistically significant, indicating sales after closed cases are becoming less concentrated, despite the industry having fewer firms as shown in Column (1). Similarly to the number of firms, τ_1^{hhi} is positive suggesting antitrust increases concentration, though this effect is not significant. Changes in concentration after closed cases are economically meaningful. Nocke and Whinston (2022) for example find that harms to consumers may outweigh potential efficiency gains from mergers when the HHI increases by 200, among industries that already have a low HHI of 1500-2500.

Changes in informality can explain this unexpected result. Columns (3) and (4) show specifications where the left-hand side variable is the SDID change in the concentration ratios, respectively CR4, the sales share of the four largest firms, and CR1, the sales share of the largest firm, each expressed as a percentage ranging from 0-100. The terms τ_0^{CR4} and τ_0^{CR1} are quantitatively small and statistically insignificant, suggesting the decrease in the HHI in closed cases is not coming from changes in the market shares of the largest four firms or the largest firm declining by a large amount and must be due to decreased concentration of sales among smaller (informal) firms. Columns (5) and (6) report specifications where the outcome change is in measures of legal and illegal informal employment proposed by Levy Algazi (2010) that can be measured in the Economic Census. Legal informal employment is unremunerated labor,[10] illegal informal employment is remunerated labor without employer contributions to social security. In Column (5) the left-hand side variable is the change in the percentage of workers in establishments with workers but no recorded remuneration. This indicates that workers are compensated out of profits, as in the case of a family business. Here we see $\tau_0^{legal_{informal}} = 1.445$ (0.643), suggesting closed cases see a 1.445 percentage point increase in the share of workers operating in firms that record no remuneration. In Column (6) the left hand side variable is employer contributions to social security as a percentage of the wage bill. The term $\tau_0^{illegal_{informal}}$ = -1.504 (0.551), suggesting closed cases see a statistically significant 1.504 percentage point decrease in employer contributions to social security, relative to the wage bill. These results suggest that investigators target industries with decreasing concentration of sales due to increased market shares of informal companies.

These results paint a nuanced picture of the relationship between antitrust and concentration. Overall, antitrust investigations tend to target industries where firms are exiting and the share of informal employment is rising, however these industries have no significant changes in the market share of the largest firms. Antitrust sanctions do not seem to alter these trends significantly, except in the case of the share of employment in firms with re-

[10]Typically unpaid family members working in the business.

munerated labor, which declines slightly after sanctions. We now turn to how these trends relate to the distribution of productivity.

Establishment-Level Productivity Measurement Total factor productivity is measured as the log of sales minus the logs of labor, capital, and materials, each multiplied by their output elasticities in the production function. We estimate production functions in each case using the control function approach of Levinsohn and Petrin (2003) with refinements by Ackerberg, Caves and Frazer (2015) in the longitudinal establishment panel compiled from the Economic Census by Busso, Fentanes Téllez and Levy Algazi (2019).[11] Specifically, for each treatment and control industry we estimate a distinct Cobb-Douglas production function for sales as a function of labor, capital, and intermediate inputs. To account for potential omitted variable bias when estimating the production function, materials are included in a control function to stand for the component of productivity that is unobserved to the econometrician, but was forecast by the firm when it made its decision to fix capital assets in the previous period. In the control function we also include the log of the firm's specific wage rate, which is set equal to zero if the firm has no remuneration; a dummy for whether the firm has no remuneration; and a dummy indicating whether the antitrust decision has occurred. These choices follow the advice of De Loecker and Syverson (2021) to control for factors that could affect firms' input decisions or the competitive environment.[12] The estimation also includes a selection function to account for potential attrition bias as in Olley and Pakes (1996), though in practice this made little difference to the results, nor did including investment instead of materials in the control function.

Distinct production functions are estimated for each treatment industry and all control industries in each case. When estimating the production function for control industries, fixed effects for each six-digit control industry are included in the control function. Total factor revenue productivity (TFPR) for each establishment k in case j in each year is measured by subtracting from (the log of) sales, (the log of) employment, (the log of) fixed capital assets, and (the log of) materials consumption where the three inputs are multiplied by their output elasticities in a Cobb-Douglas production function (with unrestricted returns to scale).

Effects of Antitrust on Revenue Productivity and Measured Misallocation Table 10 reports the results of the meta regression in Equation (2) where outcomes are changes in are alternative measures of productivity and misallocation. In Column (1) the outcome

[11] A match is not available between firms in the Economic Censuses of calendar years 1994 and 1999. Therefore the production function estimation only makes use of the censuses between 2003 and 2018.

[12] Including the wage bill in the control function also allows us to incorporate information about wages in estimation, while using the quantity of workers to measure the labor input in the production function.

is the annualized change in the average of the log of TFPR_{jk} within an industry, where the average is weighted by the market (sales) share of firm j. The effect of sanctions here is $\tau_1^{TFPR} = 0.009$ (0.005), a statistically significant increase in productivity of about 1 percent per year. In Column (2) the outcome is the annualized change in the unweighted average of the log of TFPR_{jk}. Here the result is similar with $\tau_1^{TFPR} = 0.011$ (0.006), suggesting that the increase in productivity was broad based, across all establishments in the industry, rather than just a few. In both columns the coefficient τ_0^{TFPR} is a precisely estimated zero, indicating investigations do not target industries with specific productivity trends.

The next columns present estimates where the outcomes are changes in alternative moments of the productivity distribution. In Column (3) the outcome is the change in the correlation of market share and the log of TFPR_{jk} (Olley and Pakes, 1996) and in Column (4) the outcome is the change in the variance of the log of TFPR_{jk} (Hsieh and Klenow, 2009). These measures proxy for changes in the extent of factor misallocation relative to the output maximizing allocation. In both columns we see no evidence that sanctions increase or decrease misallocation. Sanctions do not increase the correlation between market share and revenue productivity or reduce the variance of revenue factor productivity. Rather than a model in which antitrust changes industry concentration leads to a significant reallocation of factors, these results are consistent with a model in which antitrust sanctions change in the industry equilibrium from a joint profit maximizing cartel to a competitive equilibrium, and that new equilibrium incentivizing a broad-based increase in productivity across the productivity distribution, as firms benefiting from monopolistic practices move away from a 'quiet life' of limited innovation (Hicks, 1935, pg. 8).

Interestingly we do find that industries targeted by closed cases appear to be having an increase in the variance of revenue productivity. This is consistent with the evidence that closed cases have increasing informality. Since informal firms are numerous and less productive, greater informality increases productivity dispersion (Levy Algazi et al., 2018). At the same time these industries also experience an increase in the correlation of market share and revenue productivity, which can reflect a greater mass of informal low productivity firms with little scale or market share.

One concern with these results is that measures of TFPR may conflate profitability and productivity. Foster, Haltiwanger and Syverson (2008) distinguish TFPR, which is measured using revenues, from total factor quantity productivity (TFPQ), which is measured using physical output. If high markup firms are also the most productive, consumers will substitute away from high markup firms, and aggregate productivity will be less than if all firms had the same markup. Hsieh and Klenow (2009) show that it is possible to infer price vs. quantity, and therefore TFPR vs. TFPQ using an assumed demand function.

Though only sales (revenues) are observed in the Economic Census, establishments with high physical output must have a lower price to explain why buyers would demand more physical output. It is possible to invert a constant elasticity of substitution (CES) demand function and recover from revenues, up to a constant, a counterfactual measure of TFPQ_j equal to the value of industry productivity if marginal products for all factors were equalized across establishments. This approach requires only an assumption about demand and profit maximization, and does not assume anything about the structure of factor markets.[13] Columns (5) and (6) show this measure of TFPQ_j for alternative values of the elasticity of substitution parameter. The effects of financial sanctions are positive and statistically significant in both columns, indicating that in addition to raising average TFPR, sanctions increase industry-wide TFPQ, with $\tau_1^{TFPR} = 0.076(0.030)$ in Column (5) which assumes a relatively low substitution parameter $\sigma = 3$, and $\tau_1^{TFPR} = 0.108(0.061)$ in Column (6) which assumes $\sigma = 5$. These results confirm that the mechanism behind the effects of antitrust on aggregate productivity is largely improvements in TFPQ at all firms rather than a reduction in the dispersion of TFPR. Further they suggest that in the absence of misallocation, effects of antitrust on productivity could be higher.

Aggregation of Effects on Productivity How much could be the Commission's contribution to aggregate productivity growth in the status quo, without additional reforms to reduce misallocation? GDP is simply the sum of value added in the Economic Census, plus some adjustments, and GDP grows at the growth rate of total factor productivity in the steady state. Roughly, the Commission has investigated 40 percent of output over 25 years, implying it can investigate about 1.6 percent of output per year. In our sample 40 percent of investigations lead to trial. Taking the estimate from Column (1) of Table 10 of the effect of sanctions on market share weighted average TFPR as the effect of these sanctions, this implies the contribution of the Commission to GDP growth is $0.016 \times 0.4 \times 0.009 = 0.0000576$, which divided by 0.01, the long run average GDP per capita growth rate, is about 0.576 percent of aggregate productivity growth in the steady state. This number is small in relative terms, but economically significant. We can attribute about half a percent of GDP per capita growth per year to sanctions by the commission. The evidence on false negatives suggests this effect could be larger if the Commission had more resources with which investigate more of the economy more frequently, and was also able collect sufficient evidence to secure sanctions at trial.

[13] Another method to assess whether TFPR overstates TFPQ due to a markup would be to estimate the markup itself using alternative assumptions. We experimented with the markup estimator of De Loecker and Warzynski (2012), but the results were noisy and so inconclusive.

6 Conclusion and Implications

This paper provides evidence on the outcomes and effects of a relatively new antitrust law enforcement authority in a middle-income economy over 25 years. Outcomes of investigations are estimated using the synthetic difference-in-differences estimator proposed by Arkhangelsky et al. (2021). Previous work has conducted ex-post evaluation of antitrust sanctions considering only difference-in-differences effects of investigations that go to trial (e.g. Sproul, 1993). Such results may be subject to selection bias, since antitrust investigations can target industries where sales and wages are falling, and firms are exiting, while profit margins are rising.

An empirically relevant practice is that the Federal Competition Commission of Mexico discloses the relevant market for all investigations. This provides an alternative counterfactual for an industry subjected to antitrust sanctions—the evolution of industry in which anticompetitive behavior is suspected but not punished. Using this counterfactual, the positive effects of antitrust on sales and wages are larger than when measured using difference-in-differences. Disclosure of all investigations by other jurisdictions will allow further research into the causal effects of antitrust.

These results provide insight into Mexico's slow growth story. Hsieh and Klenow (2014) and Levy Algazi et al. (2018) document that industries in Mexico are characterized by significant misallocation, which can account for slow GDP growth at less than 1 percent per capita per year since 1982 when liberalization began under the administration of President Miguel de la Madrid. Our results suggest that antitrust has contributed significantly to productivity growth. Part of this contribution could be attributed to a movement of workers out of informal firms, but this growth in productivity also coincided with no changes in standard measures of misallocation.

The results suggest practically how the Commission could increase its impact in the future. Falling sales and wages after closed cases leave open the possibility that some harmful conduct goes undetected, perhaps because investigators have insufficient resources to collect evidence and confirm all per se violations. Rose (2019) provides concrete recommendations from the United States context about how to increase enforcement resources, for instance by hiring more investigative staff. Such reforms can be implemented without a change in law, and simply require providing more budget to the Commission.

Our evidence is supportive of increased enforcement focus on conduct that is not per se illegal. Declines in wages in closed relative monopolistic practice cases are smaller than in absolute monopolistic practice cases, suggesting the stakes are possibly less high. Effects of sanctions are also quantitatively smaller in relative monopolistic practice cases. Nonetheless,

decreases in sales and wages and increases in profits are economically and statistically significant in closed relative monopolistic practice cases and so we fail to reject the administrative false negatives in relative monopolistic practice cases hypothesized by Khan (2017). Jury trials are a tool available to reduce false negatives conditional on the evidence collected by investigators.

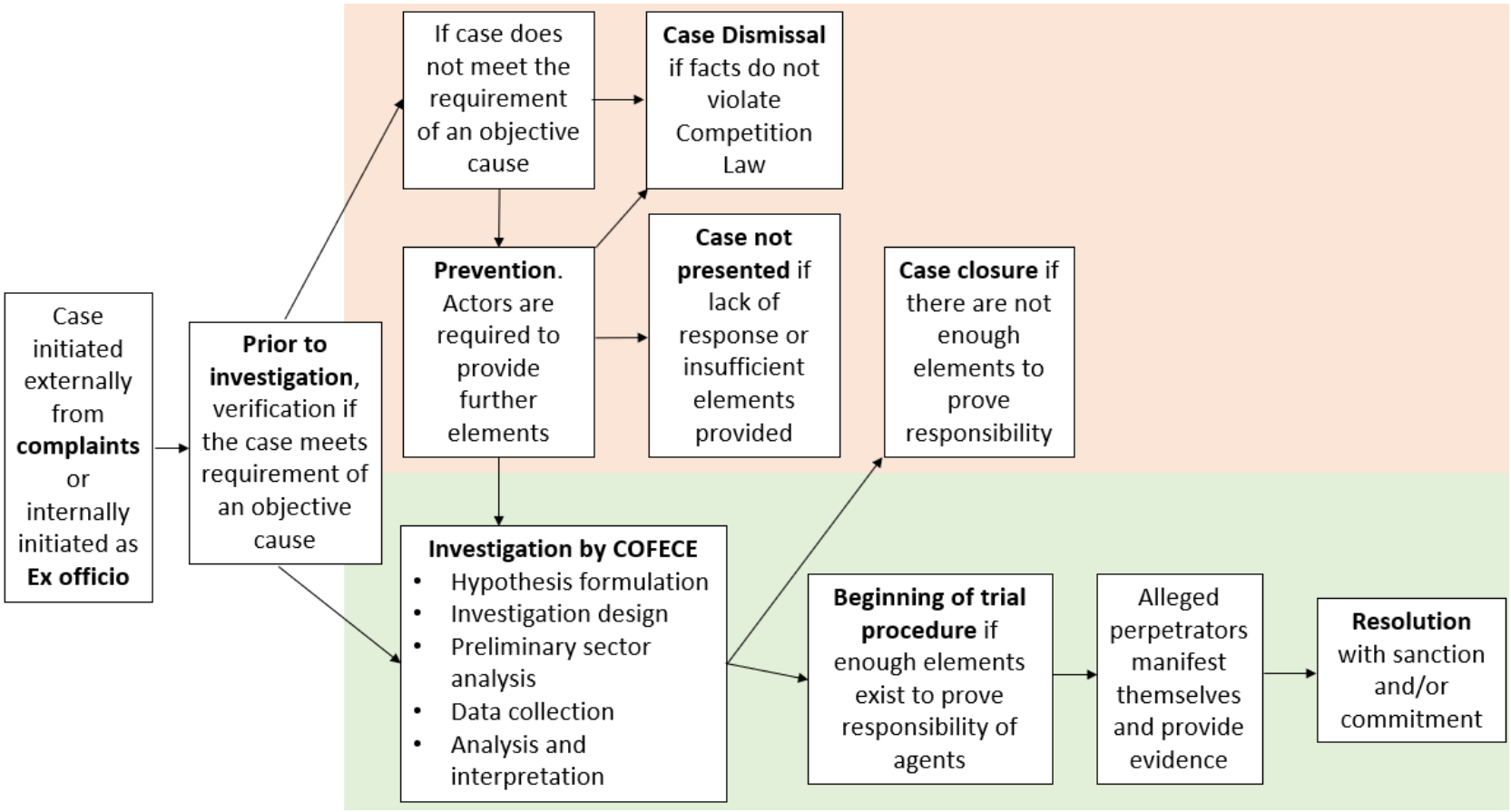

Figure 1: The Investigative Process. Diagram developed by the authors based on COFECE (2017). Case dismissal, case not presented, case closure, and resolution are terminology used to code cases in the database of cases. This paper studies outcomes of cases that have a investigation by the Commission, and therefore a description of the market. Investigations end either in case closure or a trial procedure with sanction and/or commitment.

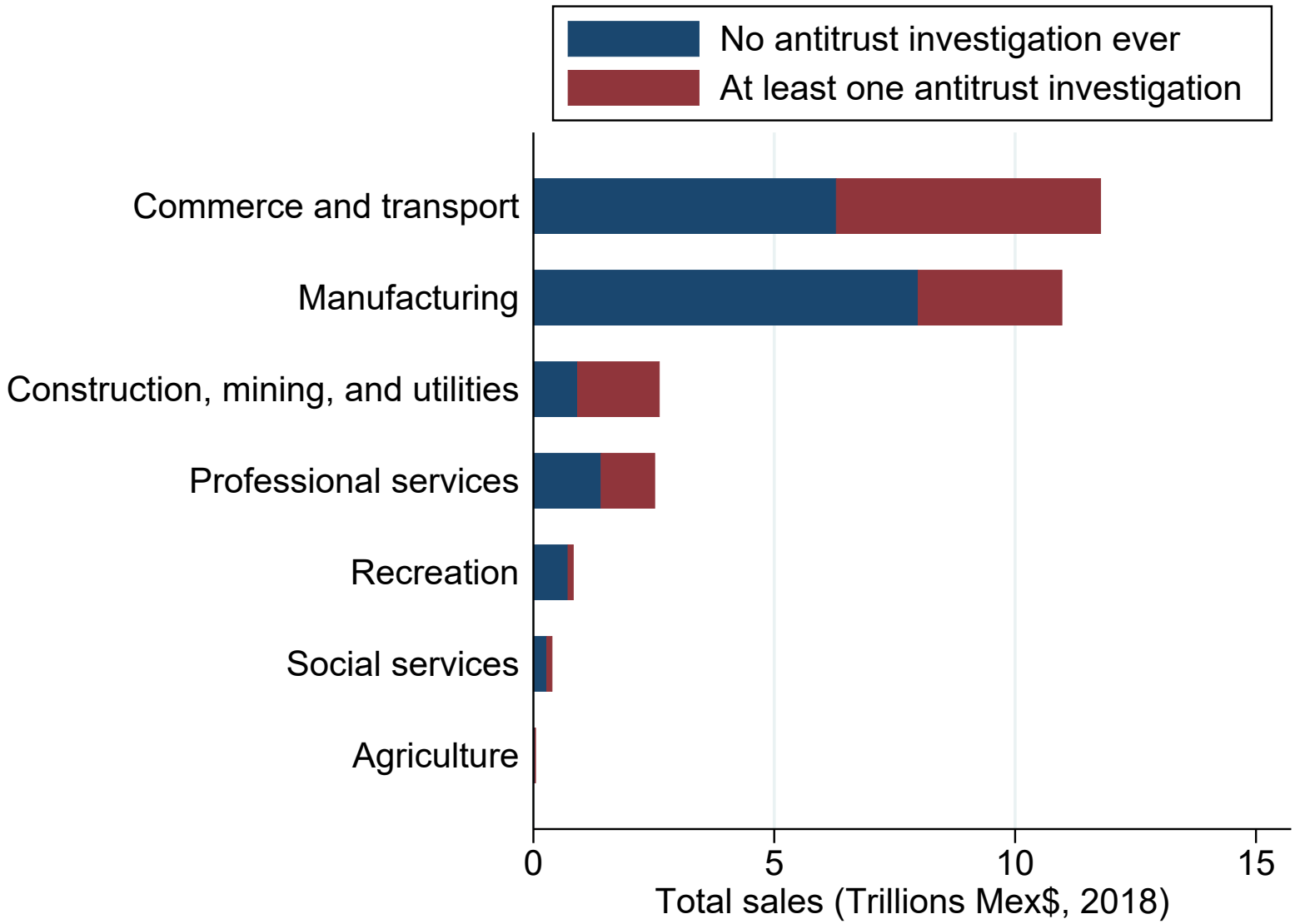

Figure 2: About 40 percent of economic activities have been subject to at least one antitrust investigation. Sales are coded as being derived from an activity that has had at least one antitrust investigation if the six digit activity corresponding to those sales has been investigated by the Federal Competition Commission. The source is the Economic Census (INEGI, 2020).

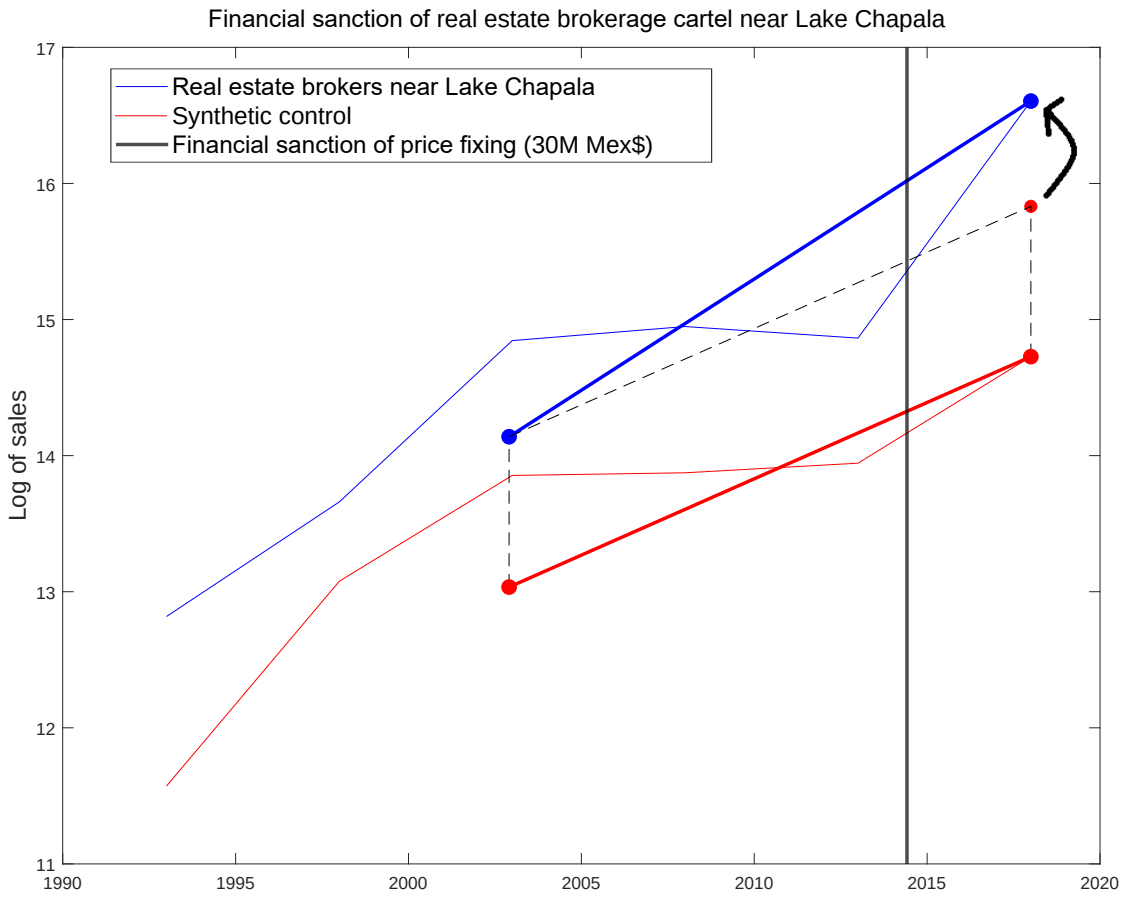

Figure 3: Change in industry sales after the sanction of a real estate brokers' cartel near lake Chapala. The vertical black line indicates the date when the real estate brokers' cartel near Lake Chapala was sanctioned. The thin blue line indicates the log of sales of real estate brokers near Lake Chapala, the treated industry. The thin red line indicates the synthetic control, or the weighted average of the log of national sales in control industries, with weights selected so the weighted average has a similar pretreatment trend to the treated industry. The thick blue and red lines connect average values of treatment and control before and after the sanction. The counterfactual for the treated industry, indicated by the arrow, is derived by shifting the thick red line upwards. The black arrow shows the implied synthetic difference-in-differences, which corresponds to an annual increase in sales between the pre- and post-treatment periods of 5.05 (s.e. = 1.39) percent. The case id (*expediente*) is DE-019-2007.

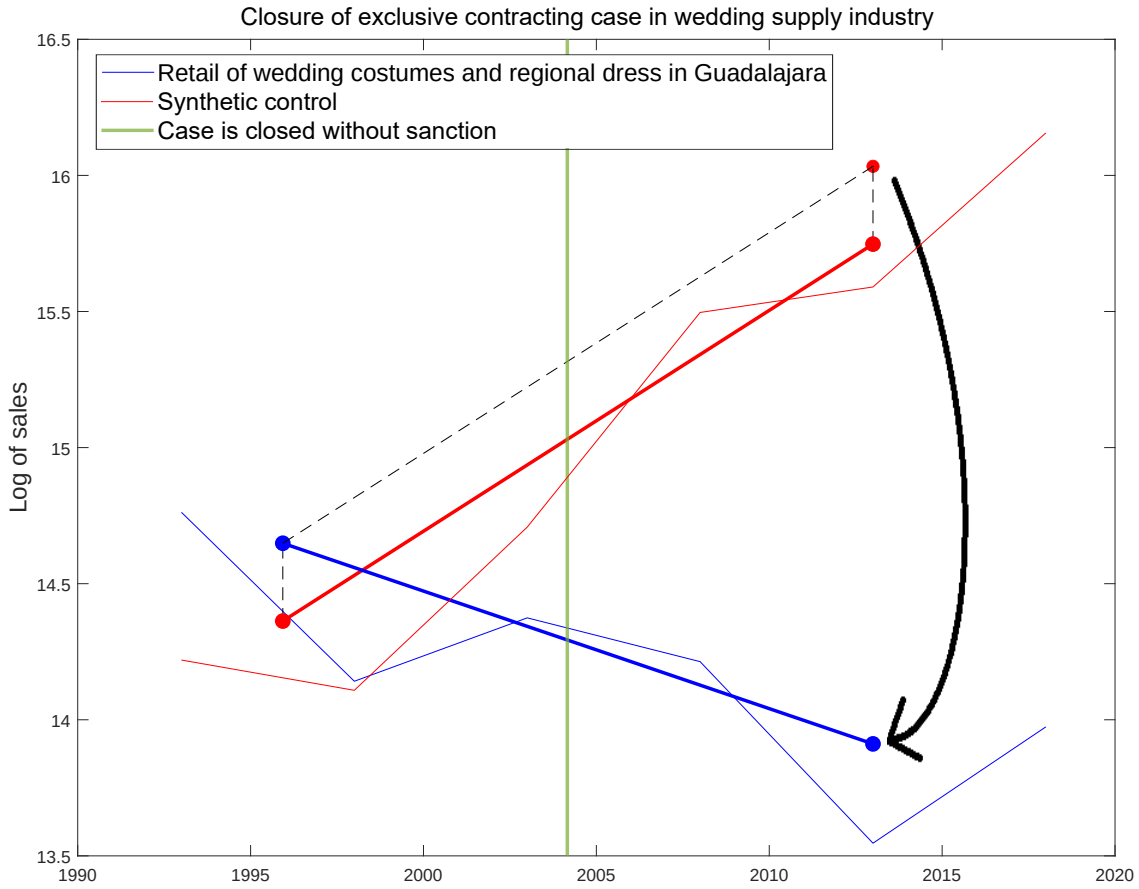

Figure 4: Change in industry sales after a closed case in the Guadalajara wedding industry. The vertical green line indicates the date on which the case was closed without a sanction. The synthetic difference-in-differences reveals an annual decrease in sales of 12.3 (1.41) percent. The case id (*expediente*) is DE-002-2003.

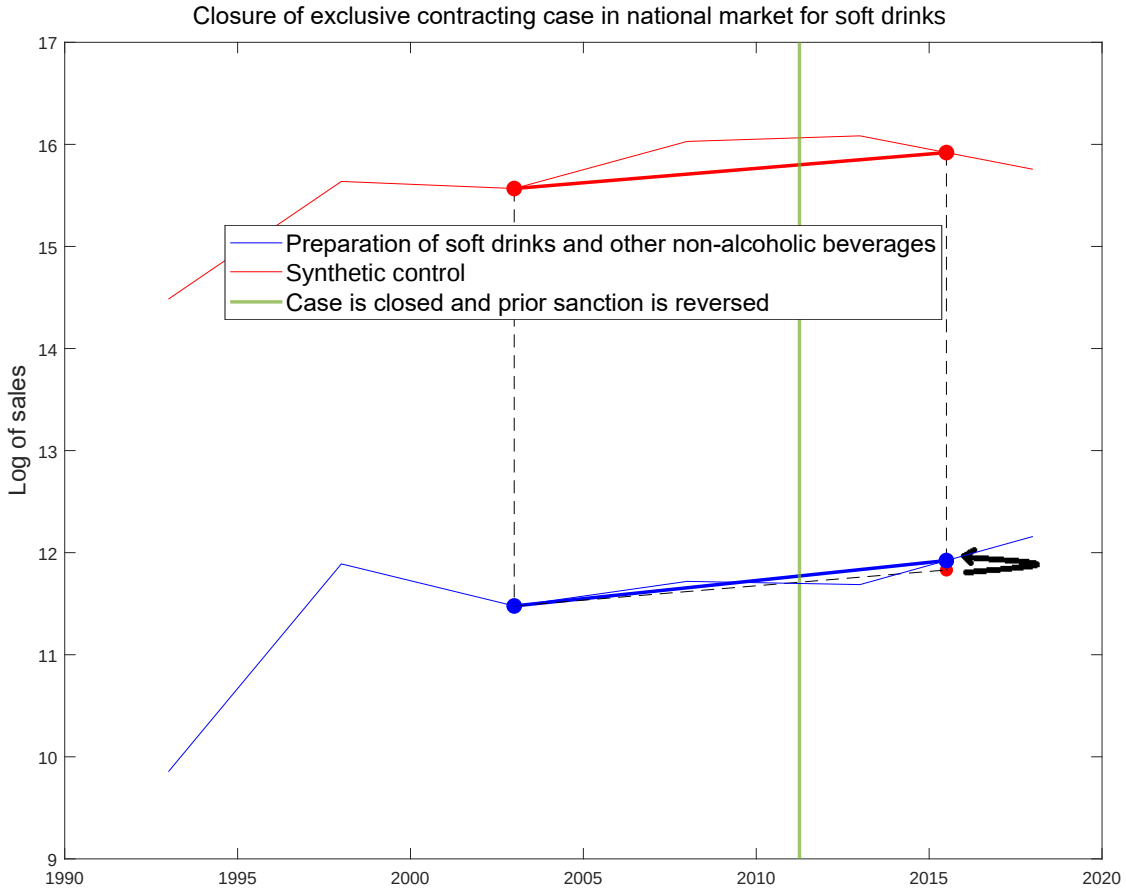

Figure 5: Change in the log of sales after the reversal of a sanction in the national soft drink industry. The vertical green line indicates the date on which the case was closed without a sanction, a reversal of a previous sanction. The synthetic difference-in-differences reveals an annual increase in sales of 0.7 (3.7) percent. The case id (*expediente*) is DE-006-2000.

Figure 6: Changes in the log of sales after antitrust sanctions in Mexico. The figure lists the SDID estimators for 35 cases ending in a financial sanction by the Commission.

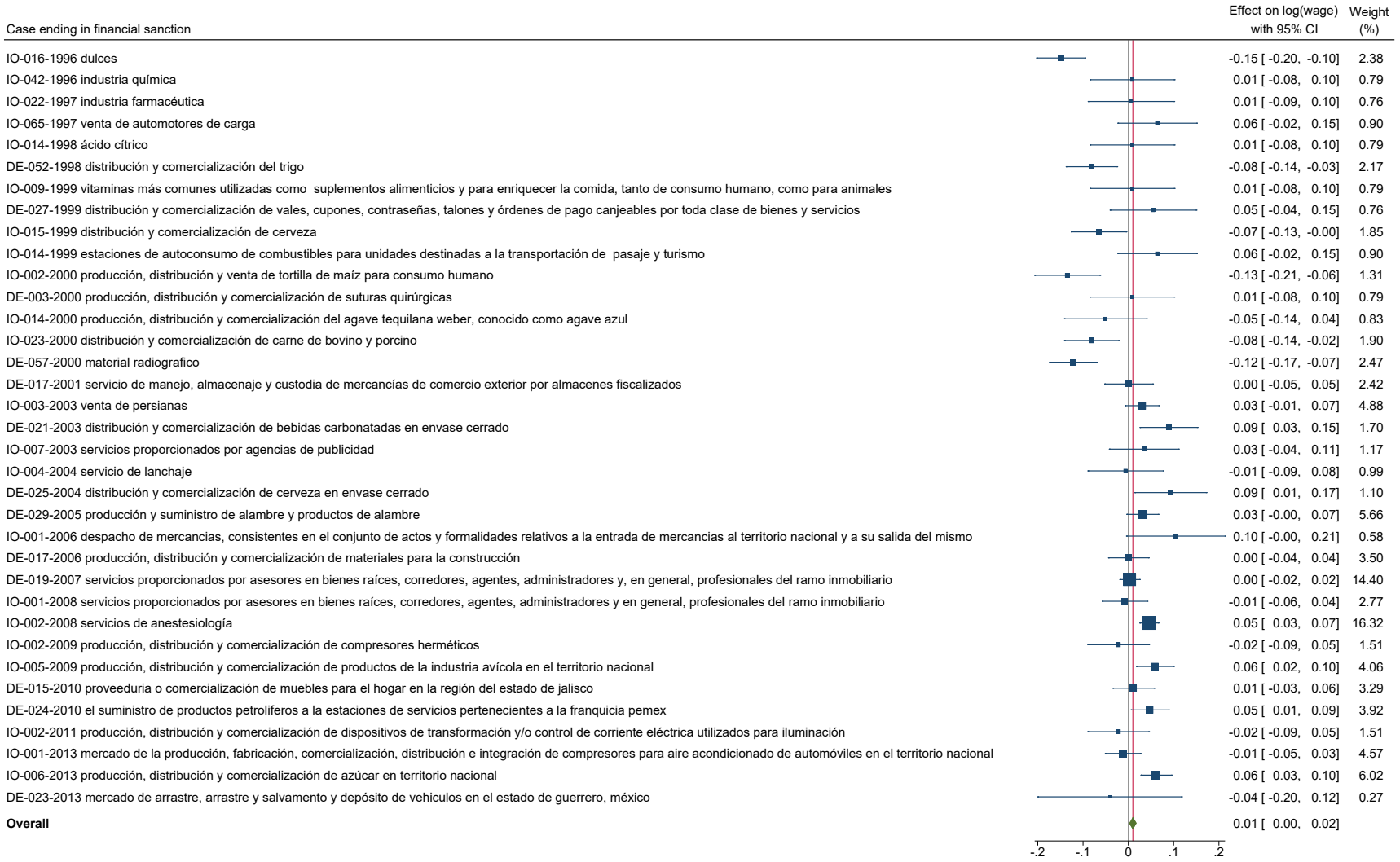

Figure 7: Changes in the log of wages after antitrust sanctions in Mexico. The figure lists the SDID estimators for 35 cases ending in a financial sanction by the Commission.

Table 1: Synthetic control weights for three cases. Weights match the pretreatment trend in the log of sales of control markets with the trend in the log of sales in the treated market.

Example A) Treated industry is real estate brokers near Lake Chapala

Control industries in real estate services (NAICS 513)	Weight
Rental without intermediation of housing	27.637%
Rental without intermediation of rooms for parties and conventions	26.888%
Rental without intermediation of offices and commercial premises	23.547%
Other real estate services including building management	21.928%

Example B) Treated industry is retail of wedding and regional dress in Guadalajara

Control industries in retail trade of textiles, jewelry, clothing accessories, and footwear (NAICS 463)	Weight
Retail trade of disposable diapers and sanitary products	55.051%
Retail trade of hosiery and items used in sewing	20.315%
Retail trade of footwear	9.267%
Retail trade of linens and fabrics	6.411%
Retail trade of hats	5.596%
Retail trade of jewelry and clothing accessories	3.360%

Example C) Treated industry is the manufacture of soft drinks

Control industries in beverages and tobacco (NAICS 312)	Weight
Manufacture of rum and other distilled cane beverages	37.166%
Manufacture of alcoholic beverages based on grapes	23.926%
Manufacture of other distilled beverages	23.224%
Manufacture of ciders and other fermented beverages	15.678%
Manufacture of beer	0.002%
Raw tobacco processing	0.001%
Manufacture of cigars	0.001%
Manufacture of other tobacco products	0.001%

Table 2: Antitrust and Sales. The regression left-hand side variable is the estimated change in the log of industry sales after a final decision by the Commission. The right-hand side variables are a constant and a dummy for whether the final decision is to sanction the agents. The coefficient on the constant therefore equals the average outcome change after a decision to close the case (and to not impose a sanction). In the fixed effect (FE) meta regression estimator, the regression is weighted by the inverse of the asymptotic variance of the outcome change. In the random effects (RE) meta regression estimator the regression is weighted by the inverse of the sum of the asymptotic variance of the outcome change and the estimated heterogeneity parameter γ^2 The ES, DID, SC, and SDID estimators are different ways to measure the outcome change on the left hand side of the meta regression. The event study (ES) estimator is the difference between the averages of the log of sales before and after the decision, controlling for a treatment industry-specific linear time trend. The ES estimator does not use a control group. The difference-in-differences (DID) estimator is the difference between the average of the log of sales before and after the decision, controlling for the average difference in industries in the same sub-sector. The synthetic control (SC) estimator is similar to the DID except that it weighs control industries so they have the same level and trend as the treatment group prior to treatment. The synthetic difference-in-differences (SDID) estimator is similar to the DID except that it weighs control activities so they have a parallel trend in (the log of) sales with the treatment group prior to treatment, though not necessarily the same level. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

	(1)	(2)	(3)	(4)	(5)
Change in (the log of)	sales	sales	sales	sales	sales
Financial sanction (=1)	0.038***	0.034***	0.032**	0.058***	0.046***
	(0.004)	(0.005)	(0.013)	(0.018)	(0.009)
Constant	-0.025***	-0.037***	-0.058***	-0.071***	0.032***
	(0.002)	(0.003)	(0.009)	(0.011)	(0.005)
γ^2				0.00625	
Number of cases	90	90	90	90	134
Meta analysis framework	FE	FE	FE	RE	FE
Outcome change estimator	SDID	DID	SC	SDID	FD

Table 3: Antitrust and Workers. Panels B and C do not sum to Panel A because the meta regression weights observations differently across panels. Synthetic control weights in the SC and SDID estimators match pretreatment trends in the log of sales for comparability with previous results. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)	(3)	(4)	(5)
Panel A) Change in (the log of the)	wage bill	wage bill	wage bill	wage bill	wage bill
Financial sanction (=1)	0.048***	0.095***	0.048***	0.055***	0.044***
	(0.005)	(0.004)	(0.016)	(0.016)	(0.009)
Constant	-0.038***	-0.091***	-0.060***	-0.054***	0.028***
	(0.002)	(0.002)	(0.011)	(0.010)	(0.005)
γ^2				0.00452	
Panel B) Change in (the log of)	employment	employment	employment	employment	employment
Financial sanction (=1)	0.065***	0.068***	-0.015	0.033**	0.011***
	(0.004)	(0.003)	(0.014)	(0.014)	(0.003)
Constant	-0.057***	-0.067***	-0.030***	-0.047***	0.021***
	(0.002)	(0.002)	(0.007)	(0.009)	(0.001)
γ^2				0.00368	
Panel C) Change in (the log of the)	wage rate	wage rate	wage rate	wage rate	wage rate
Financial sanction (=1)	0.013***	0.035***	0.025***	0.028***	0.017***
	(0.001)	(0.003)	(0.004)	(0.008)	(0.005)
Constant	-0.009***	-0.021***	-0.006*	-0.014***	0.042***
	(0.001)	(0.002)	(0.003)	(0.005)	(0.003)
γ^2				0.000721	
Observations	90	90	90	90	134
Meta analysis framework	FE	FE	FE	RE	FE
Outcome change estimator	SDID	DID	SC	SDID	FD

Table 4: Antitrust and Profits. The regression left-hand side variable is the estimated change in the operating profit margin after a decision by the Commission to either impose a sanction or close the case. Operating profit margin is value added (sales minus the value of intermediate inputs) minus the wage bill, divided by sales. Standard errors in parentheses. Synthetic control weights in the SC and SDID estimators match pretreatment trends in the log of sales for comparability with previous results. *** p<0.01, ** p<0.05, * p<0.1.

Change in (the percentage point)	(1) operating profit margin	(2) operating profit margin	(3) operating profit margin	(4) operating profit margin	(5) operating profit margin
Financial sanction (=1)	-0.539	-3.749***	-5.069***	-0.677	-0.030
	(0.628)	(0.652)	(0.531)	(2.028)	(0.618)
Constant	1.604***	8.127***	1.700***	1.630	1.429***
	(0.333)	(0.394)	(0.326)	(1.313)	(0.474)
γ^2				56.16	
Number of cases	90	90	90	90	134
Meta analysis framework	FE	FE	FE	RE	FE
Outcome change estimator	SDID	DID	SC	SDID	FD

Table 5: Antitrust and Legal Standard for Type of Monopolistic Practice. Absolute monopolistic practices are as defined in Article 53 of the Federal Economic Competition Law. Such practices "shall be null and void...and the Economic Agents that engage in such practices shall be subject to the sanctions provided in this Law, regardless of any criminal or civil liability that may arise." Practices that are not absolute monopolistic practices are relative monopolistic practices as defined in Articles 55 of the Law, which shall be punished "unless the Economic Agent proves these practices produce gains in efficiency and favorably impact upon the process of economic competition and free market access, thus overcoming their possible anticompetitive effects, and consequently result in an improvement of consumer welfare." Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Change in	(1) the log of sales	(2) the log of the wage bill	(3) operating profit margin
Absolute monopolistic practices X sanction(=1)	0.029	0.029	-0.570
	(0.039)	(0.035)	(4.597)
Financial sanction (=1)	0.042	0.049*	-0.174
	(0.028)	(0.025)	(3.130)
Absolute monopolistic practices (=1)	-0.009	-0.038*	-0.307
	(0.025)	(0.022)	(3.161)
Constant	-0.068***	-0.042***	1.721
	(0.014)	(0.012)	(1.515)
Observations	90	90	90
Meta analysis framework	RE	RE	RE
Outcome change estimator	SDID	SDID	SDID
Sanction in absolute case	0.071***	0.078***	-0.744
	(0.027)	(0.024)	3.368

Table 6: Antitrust and Source of Investigation. Ex-officio investigations are those initiated by the Federal Competition Commission itself, rather than requested by a member of the public or other another federal agency, as described in Article 66 of the Federal Economic Competition Law. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Change in	(1) the log of sales	(2) the log of the wage bill	(3) operating profit margin
Ex-officio investigation X sanction (=1)	0.020	0.028	-3.953
	(0.038)	(0.033)	(4.130)
Financial sanction (=1)	0.047	0.042*	1.134
	(0.029)	(0.025)	(3.128)
Ex-officio investigation (=1)	-0.011	-0.039*	4.639*
	(0.023)	(0.020)	(2.616)
Constant	-0.066***	-0.034**	-0.618
	(0.016)	(0.015)	(1.818)
Number of cases	90	91	90
Meta analysis framework	RE	RE	RE
Outcome change estimator	SDID	SDID	SDID
Sanction after ex-officio investigation	0.067***	0.069***	-2.819
	(0.024)	(0.021)	(2.696)

Table 7: Antitrust and Case Duration. Duration is measured as the number of years from the beginning of the investigation to the final decision. The average case duration is two years. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Change in	(1) the log of sales	(2) the log of the wage bill	(3) operating profit margin
Duration (years to final decision)	-0.001	-0.004	0.949
	(0.006)	(0.005)	(0.782)
Duration X sanction (=1)	0.002	0.005	-1.799
	(0.009)	(0.008)	(1.101)
Financial sanction (=1)	0.053*	0.047*	2.872
	(0.028)	(0.025)	(3.014)
Constant	-0.070***	-0.047***	0.151
	(0.015)	(0.014)	(1.778)
Number of cases	90	90	90
Meta analysis framework	RE	RE	RE
Outcome change estimator	SDID	SDID	SDID
Sanction after two year case	0.0574***	0.0557***	-0.786
	(0.0189)	(0.0169)	(2.072)

Table 8: Antitrust and Conduct Remedies. Compromises are conduct remedies designed to remove barriers to entry. There are seven compromises in the dataset, none of which coincide with a sanction. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Change in	(1) the log of sales	(2) the log of the wage bill	(3) operating profit margin
Financial sanction (=1)	0.053***	0.046***	-0.531
	(0.019)	(0.017)	(2.120)
Compromise (=1)	-0.034	-0.065**	0.998
	(0.033)	(0.029)	(3.656)
Constant	-0.067***	-0.045***	1.487
	(0.012)	(0.011)	(1.438)
Number of cases	90	90	90
Meta analysis framework	RE	RE	RE
Outcome change estimator	SDID	SDID	SDID

Table 9: Antitrust, Concentration and Informality. The regression left hand side variable is the estimated change in an outcome measured by the SDID estimator, where synthetic control weights match pretreatment trends in the log of sales for comparability with previous results. All specifications use a random effects meta regression. Firms with multiple establishments are treated as a single firm. The Herfindahl-Hirschman Index is the sum of firms' squared percentage market shares, and ranges from 0 and 10,000. The CR4 ratio is the market share of the four largest firms in a market, expressed as a percentage from 0 to 100. The CR1 ratio is the market share of the largest firm in a market, also expressed as a percentage. The legal informal share of employment is the percentage of workers in establishments with workers but no recorded remuneration, indicating workers are compensated out of profits, as in the case of a family business. The social security share of wage bill is employer contributions to social security as a percentage of the wage bill. Levy Algazi (2010) proposed definitions of legal and illegal informal employment that can be measured in the Economic Census. Legal informal employment is unremunerated labor, illegal informal employment is remunerated labor without employer contributions to social security. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Change in	(1) The log of the number of firms	(2) Herfindahl-Hirschman Index	(3) CR4: Sales share of four largest firms	(4) CR1: Sales share of largest firm	(5) Legal informal share of employment	(6) Social security share of wage bill
Financial sanction (=1)	0.056	253.237	-0.121	0.816	-2.405**	1.072
	(0.130)	(316.398)	(3.351)	(3.356)	(1.045)	(0.908)
Constant	-0.140*	-510.553**	-0.038	-1.535	1.445**	-1.504***
	(0.083)	(198.522)	(2.205)	(2.092)	(0.643)	(0.551)
Observations	90	90	90	90	90	90
Meta analysis framework	RE	RE	RE	RE	RE	RE
Outcome change estimator	SDID	SDID	SDID	SDID	SDID	SDID

Table 10: Antitrust, Misallocation, and Productivity. The term $TFPR_{jk} \equiv P_{jk}Y_{jk}/L_{jk}^{\alpha_j}K_{jk}^{\beta_j}M_{jk}^{\gamma_j}$ is the total factor revenue productivity of establishment k in the treated industry of case j, where P_{jk} is the price of sales, Y_{jk} is the quantity of sales, L_{jk} is the number of employees, K_{jk} is the value of fixed assets, and M_{jk} is the value of intermediate inputs. The term $TFPQ_j \equiv \sum_k \left((P_{jk}Y_{jk})^{\frac{\sigma}{\sigma-1}}/L_{jk}^{\alpha_j}K_{jk}^{\beta_j}M_{jk}^{\gamma_j}\right)^{\sigma-1}$ is industry total factor quantity productivity in the absence of distortions as in Hsieh and Klenow (2009), which is derived assuming a CES demand function and within industry substitution parameter σ. This corresponds to the productivity in a benchmark economy where there are no differences in markups or factor market constraints across firms. Production function parameters α_j, β_j, and γ_j are estimated separately for treatment and control industries using a control function approach in the Busso, Fentanes Téllez and Levy Algazi (2019) panel for each case j. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Change in	(1) market share weighted average ln(TFPR$_{jk}$)	(2) unweighted average ln(TFPR$_{jk}$)	(3) correlation of market share and ln(TFPR$_{jk}$)	(4) variance of ln(TFPR$_{jk}$)	(5) $\sigma = 3$ ln(TFPQ$_j$)	(6) $\sigma = 5$ ln(TFPQ$_j$)
Financial sanction (=1)	0.009*	0.011*	-0.013	0.034	0.076***	0.108*
	(0.005)	(0.006)	(0.056)	(0.115)	(0.030)	(0.061)
Constant	0.000	-0.002	0.133***	0.187***	-0.052***	-0.040
	(0.003)	(0.004)	(0.036)	(0.072)	(0.019)	(0.038)
Number of cases	90	90	90	90	90	90
Meta analysis framework	RE	RE	RE	RE	RE	RE
Outcome change estimator	SDID	SDID	SDID	SDID	SDID	SDID

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