Short-Term Health Effects of Soda Taxes in Areas with Low Access to Safe Drinking Water[*]

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We ask whether taxes on sugary beverages may have negative short-term health effects in areas where clean water is unavailable. Focusing on an excise tax in Mexico, we find a significant but focalized and temporary increase of 10% in outpatient gastrointestinal disease rates throughout the first year of the tax. We provide suggestive evidence of avoidance behavior through differential consumption of bottled beverages two years post-tax. The cost implied by our results is much smaller than the potential gains from the tax. However, our findings suggest the need for accompanying soda taxes with interventions that guarantee safe drinking water.

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1 Introduction

Obesity rates have been increasing at alarming rates in both developed and developing nations. One third of the world's population is now overweight or obese, and 60% of the world's obese population lives in the developing world (Ng et al., 2014). In particular, 40% of Mexican adults - the setting for this study - are now considered overweight, and a third are obese (National Health Survey ENSANUT, 2012).

Many policies have been suggested and implemented to prevent and reduce obesity (Cawley, 2015). However, most have only had small benefits on individuals' diet, weight, and related conditions such as diabetes (Cawley, 2016).[1] One of the measures that has received the highest support is the introduction of special taxes on sugar-added beverages (Brownell and Frieden, 2009). The World Health Organization (WHO) openly supports these taxes, both in developed and developing countries (WHO, 2016).[2]

The effectiveness of taxing sugary beverages (SBs) depends on consumers' substitution patterns. Some argue that substituting toward other high-caloric foods and beverages may invalidate their impact on dietary changes (Fletcher et al., 2010; Aguilar et al., 2016). Others have found evidence in favor of substitution between SBs and non-sugary beverages, mainly water (Nakhimovsky et al., 2016; Colchero et al., 2016, 2017).

The availability of low-calorie substitutes for SBs, such as water, is key for the effectiveness of SB taxes. However, a large fraction of the world's population, particularly in developing countries, may not have regular and affordable access to safe substitutes. Lacking clean drinking water is one of the main forces behind the high prevalence of gastrointestinal

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[1] This is mostly attributed to our relatively limited understanding of the shift in obesity trends over the past decades (Popkin, 2001). The general consensus indicates that there is no single solution to the epidemic, but that numerous interventions could jointly achieve the desired effect.

[2] For example, the soda tax introduced in April 2018 in South Africa was preceded by WHO support of the tax as a way to stop the rise of obesity and diabetes in that country (http://www.afro.who.int/en/south-africa/press-materials/item/9347-who-supports-proposed-sugar-sweetened-beverages-tax-in-south-africa.html, last accessed August 28, 2018).

diseases (GIDs) in developing countries (Kremer et al., 2011; Ashraf et al., 2017; Dupas and Miguel, 2017).[3]

This paper then asks whether the introduction of a soda tax in a developing country could lead to short-term negative health effects in areas where access to safe drinking water is low. As such, we implicitly ask whether soda consumption could potentially be a way for disadvantaged individuals to avoid unsafe water.

We are not the first to suggest a link between soda consumption and diarrheal disease in contexts without access to safe drinking water. In a review of the potential effects of soda taxes, Roache and Gostin (2017) recognizes the possibility of negative impacts in areas without clean water. Onufrak et al. (2014) presents the first piece of empirical evidence, documenting that Hispanics in the US that mistrust their local tap water are twice as likely to consume SBs than those who perceive it to be clean. In the context of a developing country, Ritter (2015) analyzes the introduction of Big Cola in Peru, finding a decrease in prices, higher soda consumption, and declines in GIDs.[4]

We add to this literature by causally estimating the impact of an increase in soda prices on GIDs. In particular, we are the first to link taxes on SBs to increases in GIDs in areas with low access to safe drinking water. The fact that these excise taxes have - until recently - only been implemented and analyzed in developed countries poses a challenge for the generalizability of findings to developing settings. This is especially relevant since the goal of these taxes is to combat the obesity epidemic, while inadvertently negatively impacting the health of some individuals, at least in the short-run.

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[3] For example, 57% of all GIDs worldwide are attributed to water and sanitation issues, and diarrheal disease accounts for 20% of all deaths of children under five (Prüss-Ustün et al., 2016). Furthermore, there is evidence in favor of implementing targeted policies. Interventions that improve drinking water, access to sanitation and hygiene efficiently reduce GID morbidity by up to 45, 28 and 23%, respectively (Freeman et al., 2014).

[4] Related work has analyzed the interaction between breastfeeding and water cleanliness. Keskin et al. (2017) finds that Bangladeshi women breastfeed longer in areas without access to safe drinking water, while Anttila-Hughes et al. (2018) documents the harmful effect of introducing baby formula when availability of clean water is low.

We focus our attention on Mexico, where a nation-wide tax on SBs was introduced on January 1st, 2014.[5] To the best of our knowledge, this policy was not accompanied by any campaigns for clean water or public service announcements with simple strategies for disinfecting water (such as boiling it). This particular setting - the first large-scale soda tax in a developing country - provides a unique opportunity to explore this question.

Despite being a middle-income country, many regions in Mexico still lack widespread access to piped water and have substandard groundwater quality (CONAGUA, 2016; DHAyS,2017). The under five death rate due to diarrhea in Mexico is 0.39 per thousand births, below other developing countries such as South Africa's 3.58, but four times higher than that for the US at 0.10 (UNICEF, 2016).

To motivate our question, we begin by documenting suggestive evidence that water and SBs are substitutes in Mexico, using both survey data and previous literature. In particular, Colchero et al. (2016) shows that for low-income households, consumption of taxed beverages declined by 9% post-tax, while untaxed beverages, mostly bottled water, increased by only 2%.[6] These numbers suggest that households may have substituted toward non-bottled drinking water, which might be detrimental if not clean.

We obtain data on water access from the census, data on groundwater quality from government monitoring stations, and health data from all public outpatient clinics. To answer our question, we need to identify areas where we can quantify both access to safe drinking water and health outcomes. Due to data restrictions, we conservatively assume that all piped water in Mexico is sufficiently clean. We therefore focus our attention on local hotspots where there is low access to piped water *and* bad groundwater quality, since this is where - theoretically - we would expect any effects to appear.

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[5] Since 2010, Mexico has implemented many policies to tackle the rising obesity rates. Some of these strategies include introducing healthy food options at schools, regulating the marketing of high-caloric food items to children, and requiring a clear, front-of-package labeling of nutritional facts on all foods and beverages (Barquera et al., 2013).

[6] Higher income households decrease their SB consumption by 6% and increase their consumption of other beverages by 4%.

We begin by contrasting GID rates over time in clinics located in areas with low access to piped water and bad groundwater quality against all other clinics in our sample within a difference-in-differences framework. We then expand this strategy to a triple differences approach, exploiting variation in GID rates along the three relevant dimensions (time, access to tap water, and groundwater quality). This effectively estimates the difference in outpatient GID rates between the differential trends in bad vs good groundwater quality clinics conditional on low access to tap water, and the same differential trends conditional on high access. Relative to the first approach, the triple difference simply controls for differential trends in all low access clinics, regardless of quality, and in all bad groundwater quality clinics, regardless of access.

Our specifications include time fixed effects to account for seasonality, and estimate the impact from changes within clinics over time by including clinic fixed effects. We present our results graphically in the main text, plotting lead and lagged indicators for weeks around the tax implementation date to clearly show the timing of the effect and its duration. Our main results are robust to other specifications as well as placebo checks.

We document a 10% statistically significant but focalized effect of the soda tax on GIDs for individuals in areas with both low access to tap water and bad groundwater quality. This effect is temporary (only present during the first year of the tax), robust to alternative specifications, and is prevalent among children and young adults. We present additional evidence suggesting that the effect is short-lived due to vulnerable households increasing their consumption of bottled beverages relative to other households two years after the tax. This seems to be driven by both changes in soda and bottled water consumption. We do not find any effects on hospitalization rates, indicating that although the soda tax increased outpatient GID rates, these did not translate into complications leading to inpatient care.

To contextualize our results, we conduct a simple back of the envelope calculation, and find that at most 33,000 GID cases in 2014 could be attributed to the tax, at a cost of around 467,000 USD. This cost, relative to the annual cost of obesity in Mexico, and relative to the potential health gains from the tax, is small. Therefore, these results do not warrant an argument against introducing these taxes.

However, our findings do indicate that in contexts where individuals lack access to safe drinking water, SB taxes may have unintended consequences. The magnitude of this problem increases with the prevalence of low-access hotspots. For example, the WHO was a vocal proponent of the soda tax introduced in 2018 in South Africa.[7] Given that the under five GID mortality rate in that country is an order of magnitude higher than in Mexico, the potential health costs in the absence of policies that guarantee safe drinking water could be far greater. We therefore suggest accompanying these taxes with aggressive, focalized policies aimed at guaranteeing clean water for these individuals.

Our main contribution consists in documenting the link between soda taxes and GIDs in areas where safe drinking water is not readily available. This finding informs the literature analyzing the consequences of soda taxes. Our paper also contributes to our understanding of the generalizability of policies from a developed context to a developing country, illustrating the unintended consequences from ignoring the specific characteristics of developing settings. Lastly, our paper adds to the vast literature linking water quality to diarrheal disease.

The remainder of the paper is organized as follows. Section 2 presents some context. Section 3 describes the data sources. Section 4 introduces the identification strategy. Sections 5 and 6 show and discuss the results. Section 7 concludes.

2 Background

2.1 Mexico's Soda Tax

In late 2013, the Mexican congress approved a fiscal reform, effective January 1st, 2014. The most salient item on the reform was an excise tax on sugary beverages (SBs) called

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[7] WHO press release, http://www.afro.who.int/en/south-africa/press-materials/item/9347-who-supports-proposed-sugar-sweetened-beverages-tax-in-south-africa.html, last accessed August 28, 2018.

the Special Tax on Production and Services (IEPS, by its Spanish acronym). The reform established that all SBs in the country would now be subject to a 1 peso (0.06 USD) per liter tax, which on average amounts to about 10 to 12% of the price (Colchero et al., 2016).

IEPS defines SBs as sodas, nectars and concentrates with added sugar, and powdered drink mixes. Beverages sweetened with non-caloric sugar substitutes and dairy products were exempt from the tax. Although many SBs are taxed, sodas garnered the most media attention, and the tax is commonly referred to as simply the "soda tax" (*impuesto a los refrescos*). We refer to it as such throughout this paper, keeping in mind however that other SBs were also taxed.

Grogger (2015) analyzes the effect of the soda tax on the average price of SBs, finding a passthrough of over 100%. Additional studies corroborate this finding (Aguilar et al., 2016; Colchero et al., 2015).[8] Colchero et al. (2016) calculates the effect of the tax on the consumption of taxed beverages, concluding that they declined 6% on average (around 12 mL per capita per day, equivalent to a regular-sized can of soda per month). Aguilar et al. (2016) confirms the magnitude of this decline, but also finds that substitution patterns across all food and beverage purchases led to a null effect on total calories consumed.[9]

2.2 Beverage Consumption Patterns in Mexico

Previous literature has characterized beverage consumption patterns in Mexico over the past years. Stern et al. (2014) uses dietary recall surveys in 1999 and 2012 to describe trends in caloric beverages consumed by demographic groups, finding that SBs increased among both children and adults. Strikingly, this study calculates that in 2012, SBs accounted for 17.5%

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[8] Note that total passthrough of the tax has not always been observed in other settings. For example, Cawley and Frisvold (2015) finds a passthrough of only 43% for the Berkeley, CA tax, attributed to consumers' avoidance behavior by making SB purchases in other jurisdictions. This is a particular advantage of the nation-wide implementation of the tax in Mexico.

[9] Notwithstanding these declines, there is still a sizable amount of taxed beverages being consumed. For example, in 2014, the total tax revenues from the soda tax amounted to 18 billion pesos, which averages to 163 liters of SBs per capita (http://finanzaspublicas.hacienda.gob.mx/es/Finanzas_Publicas/Estadisticas_Oportunas_de_Finanzas_Publicas, last accessed May 13, 2017).

of the total daily caloric intake for children and adolescents ages 1 to 19. Barquera et al. (2010) also finds that pre-school and school children obtain 28 and 21% of their energy from caloric beverages, respectively.[10]

We show descriptive drink patterns from the 2016 National Health Survey (ENSANUT) in Figure 1. Each panel shows a different beverage category: SBs (mostly made up of soda but also including other industrialized beverages with sugar added), juice (restricted to natural juice only), fruit water (non-industrial *aguas frescas* usually with sugar added), plain water (for which the survey does not specify the source), and unflavored milk. For each of these categories, we show the fraction of respondents that reported consuming at least one drink portion on a weekly basis. Each graph displays the data for the bottom and top socioeconomic status (SES) quartile, as reported directly by the survey.

Figure 1 indicates that SBs are a very prevalent drink choice among individuals of all ages and from all socioeconomic backgrounds. Fruit water is slightly more prevalent than natural juice. Low SES individuals tend to consume less natural juice and milk than high SES respondents, while there does not seem to be any difference for SBs, fruit water and plain water. Note that alternative definitions of consumption, such as fraction consuming at least one drink portion per day and average portions consumed, yield similar patterns.

Table 1 shows drink patterns using the same fraction that reported consuming at least one drink portion on a weekly basis, decomposed by age group and gender. This table documents very high consumption levels of SBs across all age groups. Gender differences are not generally statistically significant, except for a few cases. Overall, Figure 1 and Table 1 suggest high rates of consumption of caloric beverages, consistent with the previous literature, even after the introduction of the 2014 SB tax.

A significant caveat of these data is that the source of water consumed is not specified. Anecdotally, the market for bottled water in Mexico is large.[11] Therefore, a particular

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[10] It should be noted that this study includes whole, unflavored milk in their definition of caloric beverages. Reduced fat and nonfat milk are not included.

[11] See for example https://www.forbes.com.mx/agua-embotellada-el-negocio-multimillonario-que-mexico-no-necesita/, last accessed September 11, 2018.

concern would be that individuals are only consuming bottled water, rendering household water sources inocuous. Exploiting data from the 2012 National Household Income and Expenditures Survey (ENIGH), Table 3 presents summary statistics for households regarding purchases of both soda and bottled water. We stratify the sample into terciles by total household income.

Table 3 shows that 69% of high SES and 54% of low SES households purchased soda over the last week prior to the survey, compared to 42 and 20% of households making purchases of bottled water, respectively. Low SES households purchase about a third of the volume in liters of bottled water and more than half the liters of soda purchased by high SES households. These statistics show that not all households purchase bottled water, and less so if they fall in the left tail of the income distribution.

Another important point is to characterize whether SBs and water could be substitutes. If they are not, then we would expect to see no effect of the tax on GIDs. The 2016 ENSANUT asks about perceived changes in consumption after the implementation of the tax. Although we recognize that this is an imperfect measure, we believe that it sheds light on the possibility that water and SBs are substitutes to some degree in our context.

Table 2 shows the distribution of individuals reporting that their water and SB consumption went down, stayed the same or went up, for the top and bottom SES terciles. This table indicates that the majority of the increase in water consumption in the two years after the tax was implemented corresponds to individuals who decreased their SB consumption. Furthermore, the numbers suggest that substitution occurred across all SES groups, although perhaps to a lesser degree among low SES individuals.

Colchero et al. (2016) uses retail panel data to show that the consumption of taxed beverages fell by 6% on average, and up to 9% among low-income households. Untaxed beverages on the other hand increased by 4% on average, and just 2% for low-income households. Their data do not allow them to observe tap water consumption, and they argue that the increase in untaxed beverages is mainly driven by bottled water. However, as discussed above, purchases of bottled water by low-income households is actually not very prevalent (see Table 3).

Low-income households see both a larger decrease in SBs and smaller increase in untaxed beverages. This may suggest that these households indeed substituted toward non-bottled drinking water, which motivates the question of whether there were negative consequences from low access to safe drinking water. This possibility is especially salient given the descriptive evidence on bottled water purchases in the 2012 ENIGH. Overall, there is suggestive evidence that substitution did occur between water and SBs, and that households do consume non-bottled water.

3 Data

3.1 Water Access and Quality

We gather data on households' access to piped water from the 2010 census. This information is aggregated at the electoral section level.[12] For each section, we observe the fraction of households in 2010 obtaining their water from a variety of sources. Two measures stand out: fraction of households with water from a source outside the home, and fraction using groundwater. The former includes households obtaining piped water from a neighbor or a communal tap as well as non-piped water (from vendors or groundwater sources), while the latter restricts to households using water from wells, rivers, lakes, and dams only (see Appendix A for more information).

Publicly available data sources measuring tap water quality are not readily available. We explored a dataset containing chlorine efficiency measures of piped water at the municipality

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[12] Administratively, Mexico is divided into 32 states, which are in turn composed of municipalities, with a total of 2,456 in the whole country. For electoral purposes, municipalities are further divided into smaller geographic areas called sections (64,559 in our 2010 data). This is effectively the smallest administrative unit in Mexico.

level.[13] However, these data are not ideal for two reasons. First, it is unclear how local measures are aggregated to the larger geographic area of municipalities. This level of aggregation does not allow us to identify local hotspots of unsafe water within a municipality. Second, it seems there is not enough variation in this measure: 71% of municipalities comply with the government-established threshold of 95% efficiency.

Given this data limitation, we conservatively assume that all piped water in Mexico is sufficiently safe. We therefore shift our water quality focus toward non-piped water sources, specifically for all groundwater sources.

We obtain groundwater quality data from government monitoring stations throughout the country belonging to the regulator CONAGUA (National Water Commission). We focus on the yearly measures for 2013, although all our analyses are robust to using alternative years. In 2013, there were 2,976 monitoring stations at rivers, lakes, wells, and dams. We exclude another 1,500 stations that pertain to salt water sources. All stations are geocoded. Although we are unable to precisely characterize what determines the government's location choice for these stations, it seems that they are biased toward more polluted sources.

These data provide three distinct quality measures: biochemical oxygen demand (BOD), chemical oxygen demand (COD), and total suspended solids (TSS). For each, CONAGUA reports the precise measure, as well as a classification into five categories (very polluted, polluted, acceptable, good, and excellent), based on established thresholds. We summarize these data into a single binary measure per station, indicating bad groundwater quality if at least one of these three measures falls below good. However, our results hold under alternative specifications, such as restricting bad quality only to the polluted and very polluted classifications. See Appendix A for more details on water quality.

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[13] Chlorine efficiency is measured as a percentage from 0 to 100, and indicates the effectiveness with which chlorination eliminates water-borne pathogens. Mexico's government regulator COFEPRIS (Federal Commission for the Protection against Sanitary Risks) establishes a minimum efficiency level of 95% for good quality.

3.2 Health Outcomes

The health outcomes measuring outpatient rates of GIDs come from the Ministry of Health's Reported Cases Dataset. This information is collected by the Ministry of Health (SSA) on a weekly basis, and contains all new GID diagnoses at the outpatient clinic level. Each public outpatient clinic is legally required to report this information. Although some private clinics also report this data, their compliance rates are extremely low. Therefore, we restrict our attention to public outpatient clinics.

Note that the public healthcare system in Mexico is divided into separate, disjoint subsystems targeting different segments of the population. We restrict these data to the four principal subsystems: SSA (through *Seguro Popular* insurance), IMSS, IMSS-Oportunidades, and ISSSTE.[14] This amounts to 16,250 clinics, with the excluded ones making up around 1% of public healthcare services (ENSANUT 2012). All clinics are geocoded by merging information from SSA's Infrastructure Dataset for 2014.

The Reported Cases data records GIDs directly from doctors' diagnoses, based on ICD-10 codes. We use SSA's direct classification of GIDs, which includes all ICD-10 codes from A00 to A09. This is consistent with the literature (in particular for Mexico, see Agüero and Beleche, 2017). This information is also broken down by predetermined age groups. Finally, we collect the records for other unrelated diagnoses for a placebo exercise.

We also obtain hospital discharge records for a subset of public hospitals, corresponding to those administered directly by SSA. These data contain each patient's date of admission, as well as the final diagnosis based on ICD-10 codes. There are 762 SSA hospitals in this dataset. Unfortunately, the hospital discharge data for the other public subsystems only registers the year, and not the actual dates of hospitalization, which means we must restrict our attention to SSA hospitals. It should be noted however, that SSA tends to provide

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[14] IMSS provides healthcare for formal workers and their families; IMSS-Oportunidades is the rural branch of IMSS, linked to the cash transfer program Oportunidades; ISSSTE corresponds to government workers; and *Seguro Popular* provides coverage for informal workers and the unemployed, through SSA's own network of clinics and hospitals. The remaining smaller subsystems are for workers of the national oil company, the marines, and the army.

healthcare to lower SES groups, and that according to the 2012 ENSANUT, 40% of all hospitalizations occurred at an SSA hospital.[15]

4 Empirical Strategy

4.1 Setup and Raw Data Trends

To answer the question of whether the soda tax may negatively impact diarrheal disease, we need to identify areas where we can quantify access to safe drinking water and GID rates. We focus on identifying hotspots of unsafe drinking water, since both disease prevalence and where individuals get their water is geographically very localized. We then identify a potential control group.

To link our water and health variables, we begin by assigning piped water access and groundwater quality measures to each outpatient clinic. We construct a radius around each clinic akin to catchment areas. For our main results, we consider a 1 km radius, although we perform a robustness check with 2 km.[16]

We assign water access characteristics to our clinics based on a spatially-weighted average of the electoral sections that intersect each clinic catchment area. We also include other household characteristics from these data. We then assign groundwater quality variables from the monitoring stations within the catchment areas. If a catchment area has more than one station, we take the worst measurement (results are robust to taking the average).[17]

There are 16,250 public outpatient clinics, but only 2,976 monitoring stations. Therefore, we must exclude a great deal of clinics from our sample, since we cannot identify water

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[15] Private hospitalizations account for 15% of all inpatient care, which means that almost half of all public hospitalizations are at an SSA hospital.

[16] The government determines the location of all public clinics in an effort to maximize access. In general, SSA documents describe catchment areas as somewhere between 1 and 5 km radii, depending on population density (SSA-MASPA *Modelo de Atención a la Salud para Población Abierta*, 1995; SSA-MIDAS *Modelo Integrador de Atención a la Salud*), 2006). We conservatively choose the lower bound since we are also considering water quality measures.

[17] For the clinics included in our sample, 89% only have one monitoring station within a 1 km radius.

quality within a close radius. Likewise, not all stations are located in the vicinity of a public outpatient clinic. Overall, we are left with 624 clinics (around 4%) for which water quality is assigned from 536 different stations. On average, each monitoring station is assigned to 1.3 clinics, with 80% of them assigned to a single clinic.

This imposes an important limitation on our analysis in terms of the generalizability of our findings. Table 4 presents summary statistics for the clinics and stations included and excluded from the main analysis. Sample clinics are significantly different from the excluded ones (higher GID rates, different shares of IMSS and IMSS-Oportunidades clinics, surrounded by households with lower access to electricity, bathrooms, and sewage, and less likely to use groundwater). Monitoring stations in the sample are not statistically different from those excluded.

Regardless of these differences, we identify very focalized areas with precise measurements of both water and health variables. This is a great advantage due to the localized nature of GIDs (see Section 5 for further discussion). The tradeoff in our approach lies in the external validity of our estimates. In Section 6, when we consider the welfare implications of our results, we make sure to take the most conservative stance in how we generalize our findings.

For the analysis, we generate binary measures for tap water access and groundwater quality. For the former, we use the fraction of households obtaining water from wells, rivers, lakes, and dams, and consider the mean as the cutoff for low versus high access (results are robust to using the median instead). For the latter, we classify quality as bad whenever at least one of the three measures provided by the stations (BOD, COD and TSS) dips below the threshold for "good", as defined by CONAGUA (see Appendix A for more details).

The next step is identifying our treatment and control groups. The treatment should be the areas where an increase in water consumption could lead to more diarrheal disease. Due to the lack of reliable data on tap water quality, we conservatively assume that all piped water is sufficiently safe for human consumption. Therefore, our hotspots of unsafe drinking water are those that lack access to piped water *and* have groundwater that has been classified as bad quality.[18]

Using our measures, we first divide clinics into two groups: those characterized by low access to piped water *and* bad groundwater quality (our "treatment" group), and those with either high access to piped water *or* good groundwater quality (or both). Figure 3 plots the raw GID rate for each of these two groups of clinics for 2013-2014. This plot shows that prior to the tax, in 2013, GID rates were similar between both groups of clinics, with the treatment clinics displaying lower rates in the second half of the year. However, in 2014, GID rates start to spike in the treatment clinics, while the control remains relatively stable, and with a similar trend to 2013.

This initial exploration of the raw data brings up the issue of whether our treatment and control group exhibit parallel pre-trends. We are particularly concerned with the sharp drop in GID rates for the treatment group around week 30, while the control is gradually declining. In an effort to address this, we exploit both dimensions (piped water access and groundwater quality), and divide the sample clinics into four groups.

Figure 4 plots the raw GID rate for each of these four groups of clinics for 2013-2014. The panel on the left shows areas with low access to piped water, and the one on the right displays high access. Conditional on low access, GID rates at clinics with bad groundwater quality show a very different trend in 2014 relative to 2013 and relative to rates at clinics with good groundwater quality, with an increase in GID rates that subsides by mid-2014. However, within high access areas, GID rates for good and bad quality areas seem to closely track each other over time (with a small lag in mid-2013). This preliminary analysis of the data suggests that something different happened in 2014 relative to 2013, but only in areas with both low access to piped water and bad groundwater quality.

Table 5 shows some summary statistics for the clinics included in our sample, distinguishing between the treatment group, the pooled control, and the disaggregated control

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[18] Naturally, individuals that do not have access to tap water but live close to clean groundwater sources should not be at higher risk of GIDs if they increase their water consumption.

group. By construction, treatment and control clinics are significantly different in terms of access to piped water and groundwater quality. Furthermore, these groups of clinics differ significantly in terms of the outcome of interest and a few other variables of interest (namely, type of institution and household characteristics). Importantly, our identification strategy will rely on assuming similar trends over time across groups of clinics, not similar levels.

4.2 Identification

We begin by estimating the difference-in-differences (DD) effect of the soda tax on outpatient GID rates, using the pooled control group (clinics in areas with either high access to piped water or good groundwater quality, or both), under a flexible, parametric framework. Formally, we estimate the following equation:

$$rate_{ct} = \sum_{\tau=1}^{T} \beta_\tau (Low_c \times Bad_c \times \mathbb{1}_{[t=\tau]}) + \lambda_c + \theta_t + \varepsilon^1_{ct} \tag{1}$$

where $rate_{ct}$ is the GID rate per 100,000 at public outpatient clinic c in week-year t, Low_c and Bad_c are indicators for whether the clinic is in a low access and bad groundwater quality area, respectively, $\mathbb{1}_{[\cdot]}$ is the indicator function, λ_c are clinic fixed effects, θ_t are calendar week dummies and year indicators, and ε^1_{ct} is the error term. Standard errors are clustered at the clinic level.

Our coefficients of interest are given by β_τ, as they represent differential changes in GID rates for treatment clinics relative to our pooled control. The clinic and time period fixed effects imply that we are estimating changes within clinics over time, net of overall seasonal effects. Additional specifications also include state by calendar month fixed effects to account for regional epidemiological trends; institution (healthcare subsystem) by calendar month fixed effects to account for differential usage patterns; and flexible controls for household characteristics of the form $\sum_{\tau=1}^{T} \gamma_\tau (HH_c \times \mathbb{1}_{[t=\tau]})$.[19]

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[19] These characteristics include the fraction of households without electricity, without a bathroom, and without sewage.

The key identifying assumption is that trends in GID rates would be the same in both treatment and control clinics in the absence of the soda tax, regardless of any differences in levels, which will be captured by the clinic fixed effect. The fact that we observe multiple periods prior to the implementation of the tax allows us to test for the validity of the common trends assumption, by identifying whether the coefficients β_τ during the pre-tax period are small and statistically indistinguishable from zero.

Given the nature of our treatment group, which is defined along two conditions, we further explore a triple differences (DDD) approach. Conceptually, GID rates may vary in response to the soda tax along three dimensions: time, access to piped water, and groundwater quality. We can exploit this variation by estimating:

$$rate_{ct} = \sum_{\tau=1}^{T} \beta_\tau (Low_c \times Bad_c \times \mathbb{1}_{[t=\tau]}) + \sum_{\tau=1}^{T} \phi_\tau (Low_c \times \mathbb{1}_{[t=\tau]}) + \sum_{\tau=1}^{T} \xi_\tau (Bad_c \times \mathbb{1}_{[t=\tau]}) + \lambda_c + \theta_t + \varepsilon^2_{ct} \tag{2}$$

where all variables are defined as above, and ε^2_{ct} is the idiosyncratic error term. Standard errors are once again clustered at the clinic level to account for times series correlation within clinics.

Our coefficients of interest are still given by β_τ. The DDD estimate corresponds to the changes over time for the clinics in areas with low access to tap water and bad groundwater quality, net of the changes over time for clinics with high access to tap water and bad ground water quality, and net of the changes over time for clinics with low access and good groundwater quality. This allows us to control for changes in GID rates related to bad groundwater quality across both high and low access to tap water clinics, as well as for changes in GID rates in low access clinics, regardless of groundwater quality. In essence, this approach simply includes additional controls to the DD in equation 1.[20]

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[20] In Section 5 we unpack this DDD by presenting the results for a DD conditional on low access to piped water, and a DD conditional on high access. This further allows for a better understanding of the dynamics in each subgroup.

Analogously, the main identifying assumption for equation 2 is once again the parallel trends condition. Under this setup, we require that the trends in GID rates prior to the tax are similar across the difference between bad and good groundwater quality clinics conditional on low access to tap water, and the same difference conditional on high access.

Note that the DD and DDD estimators will be biased if clinics "self-select" into our groups, as defined by access to tap water and groundwater quality. To minimize this issue, we use pre-tax data for this classification, keeping in mind that these clinics were already in existence prior to 2010. The effect will also be confounded if other relevant policies that affected GID rates were introduced in 2014. To the best of our knowledge, this was not the case. This would only be an issue if these additional healthcare policies affected our treatment and control clinics differentially. We provide evidence against this by performing placebo checks on unaffected diagnoses.

An additional concern is measurement error in our variables. Our outcome effectively measures GID cases at outpatient clinics, not the overall epidemiological prevalence of GIDs. This may be a concern only if the likelihood of seeking medical care at the clinic conditional on being sick changes differentially over time between treatment and control clinics. Using available survey data, we show that this is not the case in Appendix B.

In terms of our independent variable, we may be misclassifying clinics along the piped water access and groundwater quality dimensions due to the different moments in time when our data were collected. First, we argue that over such a short time period, any gains in these dimensions must be relatively small. As such, any measurement error around the mean is likely to be classical, with an equal probability of classifying the clinic correctly or incorrectly, therefore leading to attenuation bias. Second, if improvements in access and quality are indeed sufficiently large, then we would tend to classify high access/good quality clinics as low access/bad quality. This, too, would attenuate our results.

5 Results

We present our results from estimating equations 1 and 2. We construct all leads and lags relative to the tax in four-week intervals, and cluster our standard errors at the clinic level. For the main results, we focus our attention on 2013 and 2014 only. We start by showing the central finding regarding overall GID rates under both the DD and DDD approach. Then we perform a series of robustness checks: including more pre-tax years, redefining catchment areas as 2 km radii, and calculating the effect on placebo diagnoses and dates, which shouldn't have been altered by the soda tax. We then show heterogenous effects by age, and effects on hospitalization rates. We present these results only for the DDD strategy, although findings are robust to the DD alternative.

5.1 Main Effect on Outpatient GID Rates

The main results from the DD strategy are shown in Figure 5 (point estimates are shown in Table C1 in Appendix C). Each graph in this figure plots the coefficients for the lead and lagged indicators from estimating equation 1, using the last four weeks in 2013 as the omitted category. Error bars show 95% confidence intervals. These coefficients show the differential GID rate for clinics in areas with low access to piped water and bad groundwater quality relative to clinics with either high access or good groundwater quality (or both). Calendar week fixed effects and year indicators account for GID seasonality, while clinic fixed effects imply that we identify the effects from within clinic variation only.

The first panel presents the main specification as described above using 2013-2014 data. The results show an increase in GID rates for low access, bad quality clinics starting in 2014, although each individual estimate is not statistically significant at the 95% level. This increase reaches its peak around week 20 of 2014, and then slowly declines back to zero. Note that 2013 does not display this trend, with point estimates that are close to zero.

The next two graphs in Figure 5 add controls successively: first state by calendar month and healthcare institution by calendar month fixed effects, and then flexible household characteristics (fraction of households without electricity, bathroom, and sewage interacted with month indicators). The results hold: there are no differential trends between treatment and control in 2013, and a large rise and subsequent decline in GID rates in treatment clinics in 2014. The last panel in Figure 5 explores long-term effects by adding 2015 data. We still find an increase in 2014 for treatment clinics relative to the control, without any additional effects in 2015. Overall, large standard errors do not allow us to distinguish these effects from a statistical zero.

Given the lack of precision in our estimates and in order to exploit the full variation in the data, we turn our attention to the DDD approach. In an effort to clearly show the sources of the variation and build up to the DDD estimates, we begin by presenting DD results restricting to low access to piped water areas only. We effectively estimate:

$$rate_{ct} = \sum_{\tau=1}^{T} \xi_\tau (Bad_c \times \mathbb{1}_{[t=\tau]}) + \lambda_c + \theta_t + \varepsilon^3_{ct} \tag{3}$$

where all variables are as defined above, and we restrict to clinics with either low or high access to tap water. Analogously, our coefficients of interest here are ξ_τ.

The estimates are plotted in Figure 6. The graph on the left shows that conditional on low access clinics, those with bad groundwater quality experienced a significant increase in GID rates during 2014, relative to those with good groundwater quality. This effect is short-lived, with the estimates returning to zero by the end of 2014. The graph on the right in Figure 6 shows the estimates conditional on high access to piped water. These point estimates are mostly close to zero and do not exhibit the same pattern as those for low access areas.

The results in Figure 6 motivate the exploration of the effect of the soda tax on GID rates using the DDD specification. These estimates are shown in Figure 7 (point estimates are shown in Table C2 in Appendix C). The first graph corresponds to the main specification as described in equation 2. The results show an increase in GID rates for low access, bad quality clinics starting in 2014 that is statistically significant. This increase peaks around 20 weeks into the year, and then gradually declines back to zero. Importantly, 2013 does not display this trend. The coefficients indicate, at the highest peak, an increase in GID rates of about 27% of the mean GID rate. Overall, we find an average focalized effect of around 6 additional cases per 100,000 per four-week period in 2014, which is about a 10% decline relative to the mean at baseline in our treatment clinics. These magnitudes are consistent with the biology of locally focalized water-borne epidemics (Gadgil, 1998; Karanis et al., 2007; Mac Kenzie et al., 1994).

The following two graphs in Figure 7 successively add controls to the first specification. The second panel includes state by calendar month and healthcare institution by calendar month fixed effects. The third panel also adds household characteristics interacted with month indicators. In all cases, the result holds, with 2013 showing relatively stable coefficients around zero, and a temporary but significant increase in 2014.

The last panel adds 2015 data to the specification that includes all controls in order to explore long-term effects. These estimates indicate that the effect we find is short-lived, with no subsequent increase in GID rates in 2015 for areas with low access to tap water and bad groundwater quality. Overall, Figure 7 confirms the main findings of a temporary, focalized, significant increase in GID rates after the implementation of the tax on SBs.

Discussion

We discuss three important points regarding our results. First, we argue that these findings cannot be explained solely by seasonality effects. Second, we give plausible reasons for the fact that the effect is short-lived, and provide some suggestive evidence that speaks to this. Lastly, we comment on the generalizability of these findings.

An important concern is that the significant but temporary increase in GID rates in 2014 could actually reflect seasonal variations in the prevalence of infections. However, average temperatures for 2013 and 2014 indicate that the warmest months in Mexico are June, July and August, and the months with highest precipitation are July, August and September, for which the coefficients are already on a downward trend. Furthermore, the estimates include calendar week fixed effects, and the second panel accounts for regional epidemics by including state by calendar month fixed effects as well. Additionally, a robustness check below includes more pre-tax years. All of this evidence leads us to believe that this trend is not simply attributed to seasonality.

Turning to the short-lived character of the effect, a plausible explanation is that individuals learn about their local water quality and adjust accordingly. Potentially, these individuals could be switching back to sodas or bottled water after they realize that water made them sick.[21] We present some suggestive evidence that individuals without access to tap water switched back to consuming bottled beverages two years after the tax was introduced.

Using repeated cross-sections of household data from the 2012, 2014 and 2016 ENIGH rounds, we estimate the following equation:

$$liters_{hmt} = \delta_1(water_h \times \mathbb{1}_{[t=2014]}) + \delta_2(water_h \times \mathbb{1}_{[t=2016]}) + \delta_0 water_h + \chi_m + \theta_t + \nu_{hmt} \quad (4)$$

where $liters_{hmt}$ is the log of liters of bottled beverages consumed by household h in municipality m in year t, $water_h$ is an indicator for whether the household does not have access to tap water inside the home, χ_m are municipality fixed effects, θ_t are indicators for each survey year, and ν_{hmt} is the error term. Standard errors are clustered by municipality. Note that we do not observe the same households over multiple rounds.

Table 6 presents the estimates from this exercise. Panel A considers all bottled beverages (water, club soda, juice, sodas, and energy drinks), while Panels B and C restrict to sodas

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[21] Avoidance behavior by increasing consumption of bottled water has been identified in other contexts. See, for example, Graff Zivin et al. (2011).

and bottled water, respectively. Column 1 includes all households in the sample. Subsequent columns restrict to smaller communities, which tend to be more rural and have less access to tap water. Results are mostly similar across columns. In Panel A, the estimate for the interaction of no tap water and the year of the tax is positive and insignificant in all specifications. This indicates no differential change in consumption of bottled beverages in households with no access to piped water, relative to the change in households with tap water, from 2012 to 2014.

However, the estimate for the interaction with two years post-tax is positive, large and statistically significant. Statistical tests allow us to reject that both coefficients are equal. This suggests that households without access to tap water lowered their consumption in the first year of the tax at the same rate as households with access to tap water, but then differentially increased their consumption two years later in 2016.

The same pattern holds in Panels B and C. The estimates for soda are more imprecise. However, coefficients for the interaction with 2014 are negative, while those for 2016 are positive. This may suggest that households without access to tap water lowered their soda consumption by a slightly larger magnitude in the first year of the tax, but not two years afterward. For bottled water, all estimates are positive, although the interaction with 2016 yields larger and more significant estimates. Overall, the evidence indicates that households without access to tap water responded differentially two years after the tax, suggesting the intention of avoiding unsafe drinking water.

An alternative explanation for the short-lived effect is that doctors at public clinics do inform GID patients of simple measures they can take to decrease their likelihood of infection. We conducted a few informal interviews with public clinic doctors, confirming that it is common practice for doctors to share simple strategies, such as boiling water or using disinfectants, with GID patients.[22] Official government guidelines from the SSA also

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[22] We conducted 12 telephone interviews with IMSS doctors asking about common recommendations for GID patients. More details are available upon request.

suggest that this is the case.[23] Unfortunately, we cannot confirm individuals' knowledge on water quality pre-tax, although it seems unlikely that they are well informed, particularly since quality measures and reports are not widespread and readily available for the general population.

A final point in our discussion of the main results concerns the generalizability of our findings. Given that we are identifying effects around local hotspots of unsafe drinking water, we must rely on precise geographic measures of both access to tap water and groundwater quality. Unfortunately, the lack of extensive water quality data restricts the size of our sample clinics (see Section 4 above for more details). Furthermore, it seems unlikely that monitoring stations are randomly located throughout the country.

However, we do find very robust, localized effects at 100 public clinics in our sample. In an extreme case, these are the only affected public clinics in the country, representing 0.6% of the universe of clinics. On the other hand, if our sample of 624 clinics is an accurate representation of all clinics, then this effect is present in 16% of clinics. While monitoring stations seem to locate around areas with worse water quality, NGO reports (see for example DHAyS, 2017) suggest that unsafe drinking water is more prevalent than just around the 100 clinics in our sample. A robustness check below that doubles our sample size also supports this claim.

5.2 Robustness Checks

We conduct a series of robustness tests using the DDD specification of equation 2. Nevertheless, results using equation 1 are similar and available upon request. We perform three checks. First, we include additional years prior to the tax. Second, we expand the radius around each clinic, effectively allowing our sample size to grow. Lastly, we present estimates for placebo conditions and a placebo tax date.

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[23] See for example, https://www.gob.mx/salud/prensa/la-secretaria-de-salud-emite-recomendaciones-para-evitar-enfermedades-diarreicas-colera-y-golpe-de-calor, last accessed August 30, 2017.

The first panel in Figure 8 shows the estimates including additional pre-tax years all the way back to 2009. We show the estimates for the specification that includes all controls (as in the third panel of Figure 7). We only plot the coefficients for 2012-2014 for clarity. These estimates still show a spike in GID rates in 2014. Although there is more variability in the coefficients pre-tax, they are mostly statistically insignificant, while the results for 2014 are still significant.

The second panel in Figure 8 restricts again to 2013-2014 data, but now uses a 2 km radius around public clinics for the construction of water access and quality measures. Our sample size thus increases from 624 to 1,432 clinics. The tradeoff in augmenting our sample is that we may be losing precision in mapping our observable water quality (from monitoring stations) to the unobserved quality of water consumed by clinics' potential patients. The plot shows that our main results hold. Given that our sample size more than doubles and that the number of low access, bad quality clinics grows to 204, this finding further supports our claims for external validity.

Figure 9 conducts placebo checks. The first two graphs estimate the effect on unrelated diagnoses: chronic diseases and external injuries per 100,000.[24] If we are confounding the effect of the tax with other policies that affected healthcare, then we would expect to see effects on diagnoses unrelated to soda consumption. A tax on SBs should not have any immediate effects on chronic diseases or any effects on external injuries. The first two graphs in Figure 9 show that the estimates are statistically indistinguishable from zero and follow no particular trend, suggesting that our results are not confounded by other healthcare policies.[25]

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[24] Chronic diseases include ulcers, gastritis, duodenitis, alcoholic liver disease, dysplasia, breast cancer, diabetes (type 1, type 2, during pregnancy), hypertension, ischemic heart disease, venous insufficiency, cerebrovascular disease, asthma, and uterine cancer. External injuries include motor vehicle accidents, pedestrians involved in traffic accidents, dog bites, snake bites, bites by other mammals, and domestic violence incidents. Both classifications are defined directly by SSA.

[25] We also consider acute respiratory infections for a placebo exercise and find similar results. However, Agüero and Beleche (2017) shows a relationship between GIDs and respiratory conditions through changes in health behaviors in Mexico. Therefore, we exclude these estimates from the main text.

The last graph in Figure 9 estimates the effect for a placebo tax date. We consider data from 2012-2013, and assume that the tax was introduced a year earlier, on January 1, 2013. The estimates show no significant effects. This exercise further supports the claim that our estimates are not due to simple seasonality effects.

5.3 Heterogeneous Effects by Age

We explore heterogeneous effects of the soda tax on GID rates by age groups. Many factors may influence the size of these effects. Acquired immunity suggests that younger individuals are at greater risk of water-borne pathogens, since their immune system may not be sufficiently developed. Likewise, beverage consumption patterns may indicate which age groups are less vulnerable to effects from a soda tax due to low baseline consumption. The magnitude of the effect by age group is therefore an empirical question.

Figure 10 shows the estimates of equation 2 including all controls, broken down by three groups: children ages 0-14, adults ages 15-44, and older adults ages 45 and over. The findings for the youngest age group are less precise. Nevertheless, we do find a similar pattern to the main result, with a significant increase in GID rates starting in 2014 in areas with low access to piped water and bad groundwater quality, which then subsides later in the year. The same is true for adults, although the effect is somewhat less significant. Lastly, older adults do not seem to experience any effects of the soda tax on their GID rates.

Overall, Figure 10 indicates that the burden of the effect falls mostly on children and adults. This is consistent both with the fact that they are more vulnerable to GIDs and with the relatively high rates of soda consumption among this group (see Table 1). In contrast, older adults seem to be unaffected, consistent with their lower soda consumption, and possibly their acquired immunity to local water-borne diseases.

5.4 Effect on Hospitalizations

Our main results corresponds to the effect of the tax on GID rates at outpatient public clinics. We now turn our attention to a worse health outcome: hospitalizations. As outlined in Section 3, hospital discharge data is only available for public hospitals belonging to SSA. However, SSA hospitalizations make up almost half of all public inpatient care cases, and since they target lower socioeconomic status individuals, SSA hospitalizations are an important group to consider in this context.

We follow the same strategy as above. From the full 762 SSA hospitals in the data, we can only assign water quality measures to 239 hospitals, using a 1 km radius. Figure 11 presents the results from estimating equation 2, including all controls. The graph on the left plots the coefficients for the full sample, while the graph on the right restricts to hospitalization rates for children under the age of six, who may be more vulnerable to complications from diarrheal disease.

Both plots indicate that the soda tax had no discernible effect on hospitalization rates in areas with low access to tap water and bad groundwater quality relative to the controls. It is especially striking that infant hospitalization rates are unaffected, since this particular age group is very vulnerable to the consequences of diarrheal disease, such as dehydration, that tend to necessitate inpatient care. These results indicate that, although the increase in soda prices did lead to more GIDs, these were successfully controlled and contained at the outpatient level.

6 Welfare Implications

We have identified a sizable but focalized effect of the soda tax on GIDs for individuals in areas with low access to tap water and bad groundwater quality. This effect is robust, prevalent among children and younger adults, and is only present at the outpatient care level, without any effect on hospitalization rates. We now address the welfare and policy implications of our findings.

The literature has shown that the soda tax in Mexico had a large effect on the price of taxed goods and decreased the consumption of taxed beverages (Grogger, 2015; Colchero et al., 2016; Aguilar et al., 2016). Colchero et al. (2016) calculates an average decline of 6 liters ($\sim$2,300 calories) per capita of SBs by the end of 2014. Aguilar et al. (2016) finds no significant effects on calories consumed when considering substitution across both food and beverage items, although sugar consumption does decline.[26]

Regardless of the full magnitude, the evidence suggests important shifts in the nutritional composition of Mexicans' diet after the introduction of the tax. It is beyond the scope of this paper to comment on the size of these effects. We simply understand the tax as a large-scale intervention effectively affecting the composition of the average diet, possibly toward a healthier one.

In this paper, we find a focalized average effect of around 6 additional outpatient GID cases per 100,000 individuals at each clinic in low access, bad quality areas, per four-week period in 2014. We do not find any effects in 2015. In the extreme case, this generalizes to 33,365 additional GID cases in 2014 in the whole country.[27] According to the 2012 ENSANUT, GID patients at public clinics paid on average 41 pesos in transportation, fees, and medicines, and spent an average of 129 minutes getting to the clinic, waiting, and with the doctor. Therefore, with an average hourly minimum wage of 65.5 pesos, the total cost of these additional cases is roughly around 6 million pesos or 467,000 USD.

Estimating the direct effect of the tax on weight, and ultimately obesity-related conditions such as diabetes and heart disease, is difficult given the lack of an obvious control group and

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[26] Both of these studies use home scanner data. As such, the effect on purchases outside the house is not estimated.

[27] The calculation is as follows. First, 16% of clinics in our sample are low access, bad quality, which extrapolates to 2,630 clinics in the country. The average population at a "treated" clinic is 16,201 individuals, and there is an average increase of 1.5 cases per 100,000 per week per clinic. This implies 0.24 extra cases per week per clinic ($\frac{16{,}201 \times 1.5}{100{,}000}$), and 0.24 cases × 52 weeks × 2,630 clinics implies 33,365 GID cases in 2014 attributed to the soda tax.

the chronic nature of obesity-related diseases. However, Molina et al. (2015) estimates that obesity costs Mexico around 120 billion pesos per year. As such, the negative short-term effects we estimate due to the introduction of the soda tax only represent 0.005% of this cost. Alternatively, our findings represent 0.03% of the 2014 soda tax revenues.[28] Therefore, our findings do not advocate against these excise taxes.

Nevertheless, our results do inform the need for focalized policies that guarantee affordable access to safe drinking water for populations that are negatively impacted by the soda tax. This insight may matter even more in countries where access to clean water is lower than in Mexico (for example, South Africa), since the potential cost in those settings may be far greater. The lesson we espouse is that policies attempting to incentivize water consumption over SBs can have negative health impacts on individuals without access to safe drinking water.

7 Conclusion

This paper asks whether a soda tax policy aimed at combatting obesity in a developing country could potentially lead to unexpected negative short-term health impacts in areas without access to safe drinking water. Focusing on Mexico, a middle-income country where clean water is still an issue in some areas, we find a significant but temporary increase in GID rates at public outpatient clinics in places with low access to tap water and bad groundwater quality. We find no effect on GID hospitalization rates at a subset of public hospitals.

Our findings are concentrated in very specific geographic areas, related to hotspots where safe drinking water is unavailable. We perform a simple back of the envelope calculation to provide some context for our results. In the worst case scenario, we attribute 33,365 new GID cases in 2014 to the introduction of the tax, without any additional cases in 2015. The relative cost implied by these estimates is very small with respect to the (potential)

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[28] Note that fiscal policy is not earmarked in Mexico. As such, the revenues from the soda tax are lumped together with all other fiscal revenues.

health gains from the tax. Therefore, our results are definitely not an argument against the introduction of such a tax in Mexico.

Our results do indicate however that in contexts where some individuals do not have access to clean water, a soda tax may have pernicious effects on this segment of the population. As such, this paper emphasizes the issues associated with implementing policies from developed countries in a developing context. Our findings inform the need for aggressive, focalized interventions that guarantee safe water access to these populations when introducing taxes aimed at incentivizing water consumption. While this may not be salient in high-income countries, where these taxes have traditionally been implemented and analyzed, the magnitude of this negative effect may be exacerbated in lower income countries, where a larger fraction of the population lacks regular access to clean water.

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Table 1:
Drink Patterns by Age and Gender

	Ages 0-5	Ages 6-12	Ages 13-19	Ages 20-64	Ages 65+
Sugary beverages					
Male	26.6	86.3	87.0	80.8	68.6
Female	26.6	85.1	84.7	76.3***	64.9
Natural juice					
Male	20.0	17.8	19.1	19.7	12.4
Female	20.9	19.9	22.0	17.2	20.2*
Fruit water					
Male	16.1	34.8	29.4	30.9	16.8
Female	18.8	34.0	33.8**	34.0	18.1
Plain water					
Male	20.3	90.8	88.7	87.2	94.2
Female	21.6	91.6	90.5	90.3**	89.9
Milk					
Male	80.4	77.6	68.2	59.8	64.2
Female	83.2	80.4	70.5	65.3***	61.7
Observations	1,440	1,464	1,936	2,547	425

Notes: This table shows the fraction by gender and age groups of individuals that reported consuming at least one portion of the drink on a weekly basis in the 2016 national health survey (ENSANUT). Sugary beverages are mostly made up of sodas but also include industrialized juices, flavored milk, and drinks with sugar added; fruit water is non-industrial *aguas frescas* usually with sugar added; plain water is both bottled and tap (unspecified in the survey); and milk refers to unflavored milk. T-tests for differences in means by gender are shown.

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

Table 2:
Perceptions of Consumption Changes After the Tax

		Water consumption			
Sugary drink consumption	Panel A: High SES				
		Went down	The same	Went up	Total
	Went down	2%	11	33	46
	The same	4	21	14	39
	Went up	5	5	5	15
	Total	10	37	53	
	Panel B: Low SES				
		Went down	The same	Went up	Total
	Went down	2%	13	24	39
	The same	3	29	15	47
	Went up	3	6	5	14
	Total	8	48	44	

Notes: This table shows the distribution of individuals answering questions about how they have perceived changes in their consumption of both water and sugary beverages during the two years after the tax, according to the 2016 national health survey (ENSANUT). Panel A shows high socioeconomic status individuals, corresponding to those in the top tercile. Panel B shows the bottom tercile.

Table 3:
Pre-tax Purchases of Beverages

	High SES			Low SES		
	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.
Fraction with soda purchases	0.69	0.50	3,000	0.54	0.50	3,001
Soda purchased (L)	4.43	3.49	3,000	2.27	3.49	3,001
Soda, if purchased (L)	6.39	5.84	2,082	4.21	3.80	1,621
Fraction with bottled water purchases	0.42	0.40	3,000	0.20	0.40	3,001
Bottled water purchased (L)	15.04	16.13	3,000	5.58	16.13	3,001
Bottled water, if purchased (L)	35.93	39.88	1,256	27.69	26.07	605

Notes: This table shows purchases of beverages in liters (L) prior to the soda tax, using household survey data from the 2012 national household income and expenditures survey (ENIGH). We show the fraction of households with positive purchases, the average amount purchased, and the average amount conditional on purchasing a positive quantity. We present statistics for the top and bottom terciles of total income reported in the survey.

Table 4:
Summary Statistics for Sample vs Full Universe

	In sample	Not in sample	T-statistic
Panel A: Public Outpatient Clinics			
GID cases per 100,000	12.16	11.04	-7.06***
	(0.034)	(0.132)	
Fraction SSA clinic	0.70	0.67	-1.20
	(0.018)	(0.004)	
Fraction IMSS clinic	0.13	0.06	-6.30***
	(0.013)	(0.002)	
Fraction IMSS-Oportunidades clinic	0.14	0.23	5.61***
	(0.014)	(0.003)	
Fraction ISSSTE clinic	0.04	0.03	-1.37
	(0.008)	(0.001)	
Fraction of HH without electricity	0.03	0.07	7.10***
	(0.002)	(0.001)	
Fraction of HH without bathroom	0.09	0.15	7.60***
	(0.004)	(0.001)	
Fraction of HH without sewage	0.15	0.31	12.76***
	(0.007)	(0.002)	
Fraction of HH using groundwater	0.13	0.19	6.15***
	(0.008)	(0.002)	
Number of clinics	624	15,626	
Observations	64,896	1,625,104	
Panel B: Monitoring Stations			
Biochemical Oxygen Demand (BOD, mg/L)	14.44	18.17	0.65
	(1.576)	(2.802)	
Fraction BOD below “good”	0.41	0.43	0.91
	(0.022)	(0.011)	
	[505]	[2,059]	
Chemical Oxygen Demand (COD, mg/L)	61.20	72.26	0.85
	(3.857)	(6.371)	
Fraction COD below “good”	0.77	0.77	0.19
	(0.019)	(0.009)	
	[505]	[2,063]	
Total Suspended Solids (TSS, mg/L)	85.04	81.84	-0.39
	(7.505)	(3.453)	
Fraction TSS below “good”	0.28	0.27	-0.45
	(0.279)	(0.269)	
	[534]	[2,401]	

Notes: This table shows averages comparing data included in the main analysis with the data not included (see text for details). Panel A considers public clinics from 2013-2014, and Panel B monitoring stations. Averages are shown, with standard errors in parentheses. In Panel B, brackets display number of observations. The last column shows the t-statistic for a difference in means test.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5:
Descriptive Statistics by Tap Water Access and Groundwater Quality

		Full control group	Disaggregated control group			
	Low access, bad quality	High access *and/or* good quality	Low access, good quality	High access, bad quality	High access, good quality	Full sample
GID cases per 100,000	14.70	13.74***	10.11***	12.35***	18.92***	13.82
	(48.02)	(35.13)	(30.58)	(30.71)	(44.87)	(36.43)
Fraction SSA clinic	0.83	0.67***	0.69***	0.68***	0.65***	0.69
	(0.38)	(0.47)	(0.46)	(0.47)	(0.48)	(0.46)
Fraction IMSS clinic	0.03	0.15***	0.06***	0.15***	0.18***	0.14
	(0.17)	(0.35)	(0.24)	(0.36)	(0.39)	(0.34)
Fraction IMSS-Oportunidades clinic	0.13	0.14***	0.24***	0.12***	0.12**	0.14
	(0.34)	(0.34)	(0.43)	(0.32)	(0.33)	(0.34)
Fraction ISSSTE clinic	0.01	0.05***	0.01	0.06***	0.04***	0.04
	(0.10)	(0.21)	(0.11)	(0.23)	(0.20)	(0.20)
Fraction of HH without electricity	0.05	0.03***	0.08***	0.02***	0.03***	0.03
	(0.07)	(0.05)	(0.11)	(0.02)	(0.03)	(0.06)
Fraction of HH without bathroom	0.15	0.08***	0.14***	0.08***	0.06***	0.09
	(0.14)	(0.10)	(0.16)	(0.08)	(0.05)	(0.10)
Fraction of HH without sewage	0.23	0.14***	0.30***	0.10***	0.13***	0.15
	(0.21)	(0.18)	(0.25)	(0.13)	(0.16)	(0.18)
Fraction of HH using groundwater	0.35	0.09***	0.38***	0.03***	0.04***	0.11
	(0.23)	(0.16)	(0.24)	(0.03)	(0.04)	(0.18)
Biochemical oxygen demand (mg/L)	8.48	14.84***	2.54***	24.10***	2.78***	14.33
	(12.49)	(35.55)	(1.37)	(45.03)	(1.64)	(34.34)
Chemical oxygen demand (mg/L)	66.73	56.16***	16.61***	86.04***	17.17***	57.00
	(92.16)	(75.32)	(11.29)	(88.55)	(10.81)	(76.84)
Total suspended solids (mg/L)	115.44	75.22***	20.63***	113.33*	22.65***	78.70
	(184.06)	(155.75)	(20.90)	(194.43)	(20.58)	(158.80)
Number of clinics	100	524	80	306	138	624
Observations	26,000	136,240	20,800	79,560	35,880	162,240

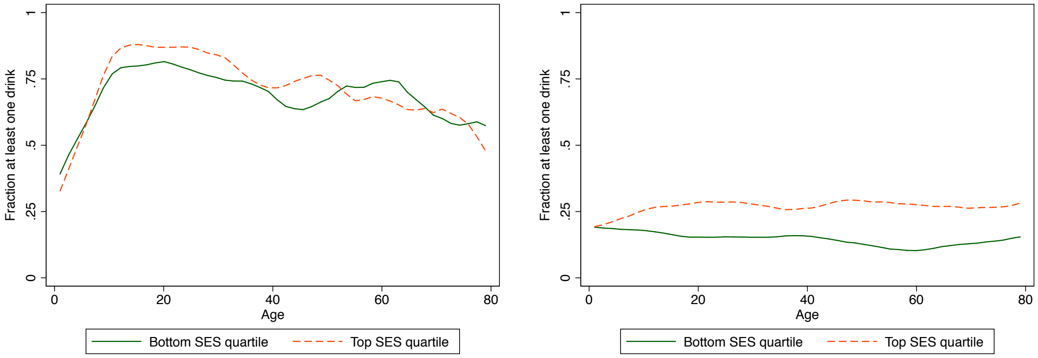
Notes: This table shows descriptive statistics for our sample, restricting to data for all years prior to the introduction of the tax (2009-2013). The first column shows our treated sample: clinics in areas with low access to tap water and bad groundwater quality. The second column shows the full (pooled) control group, while columns 3 to 6 decompose this into three subcategories. The final column shows the full sample. We use the overall mean of fraction with access to tap water as the cutoff for high vs low, and assign bad groundwater quality whenever at least one of the three reported measures falls below the "good" threshold. See text for more details. Difference in means tests performed for each control group relative to the treatment group (low access, bad quality).
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6:
DD Estimates of Changes in Bottled Beverage Consumption in Households with and without Access to Tap Water

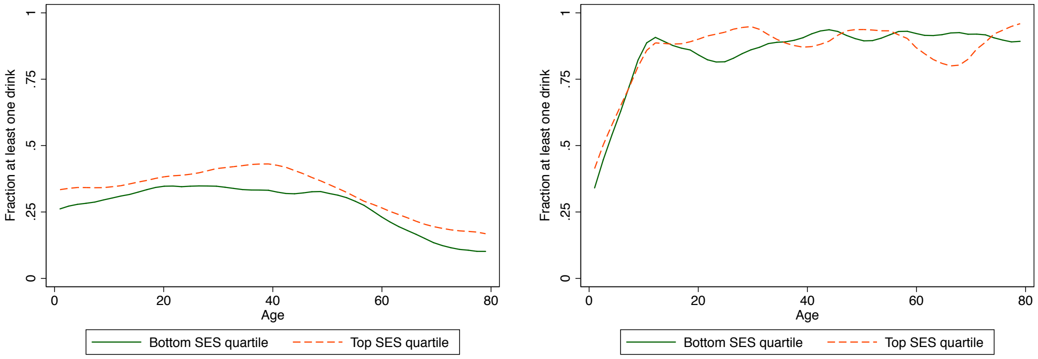
	(1)	(2)	(3)	(4)
Panel A: All bottled beverages				
No access to tap water × 2014	0.0388	0.0946	0.1918	0.0503
	(0.1110)	(0.1272)	(0.1430)	(0.1828)
No access to tap water × 2016	0.2632***	0.3156***	0.3249***	0.2908*
	(0.1019)	(0.1130)	(0.1229)	(0.1523)
R-squared	0.139	0.168	0.182	0.196
Panel B: Sodas				
No access to tap water × 2014	-0.0581	-0.0355	-0.0207	-0.1237
	(0.1030)	(0.1215)	(0.1335)	(0.1657)
No access to tap water × 2016	0.0892	0.1335	0.1328	0.1416
	(0.0938)	(0.1072)	(0.1188)	(0.1440)
R-squared	0.129	0.154	0.163	0.170
Panel C: Bottled water				
No access to tap water × 2014	0.0288	0.1977	0.3093*	0.0728
	(0.1296)	(0.1485)	(0.1748)	(0.2304)
No access to tap water × 2016	0.1741	0.2668**	0.2796*	0.1498
	(0.1112)	(0.1357)	(0.1606)	(0.2051)
R-squared	0.144	0.163	0.177	0.192
Observations	98,792	61,010	48,051	34,195
Communities included	All	$< 100,000$	$< 15,000$	$< 2,500$

Notes: This table shows DD estimates of changes in consumption over time for households with and without access to tap water. We show estimates from a regression of the log of liters purchased (plus 0.01 to deal with zeros) on an indicator for tap water access at the household level interacted with each year (2014, and 2016), an indicator for tap water access, indicators for each survey year, and municipality fixed effects (see equation 4 in the text). The unit of observation for each regression is a household-year. The first column includes all households in the survey. Subsequent columns restrict to smaller (i.e., more rural) communities. Robust standard errors clustered at the municipality level. Panel A includes all bottled beverages (bottled water, club soda, juice, sodas, and energy drinks). Panel B restricts to sodas and C to bottled water.
*** p<0.01, ** p<0.05, * p<0.1

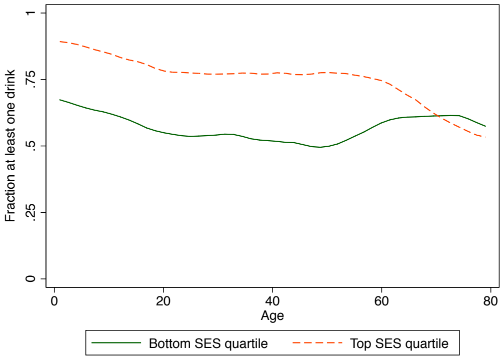
Figure 1:
Drink Patterns in Mexico by Age and SES Group



(a) Sugary beverages (b) Natural juice



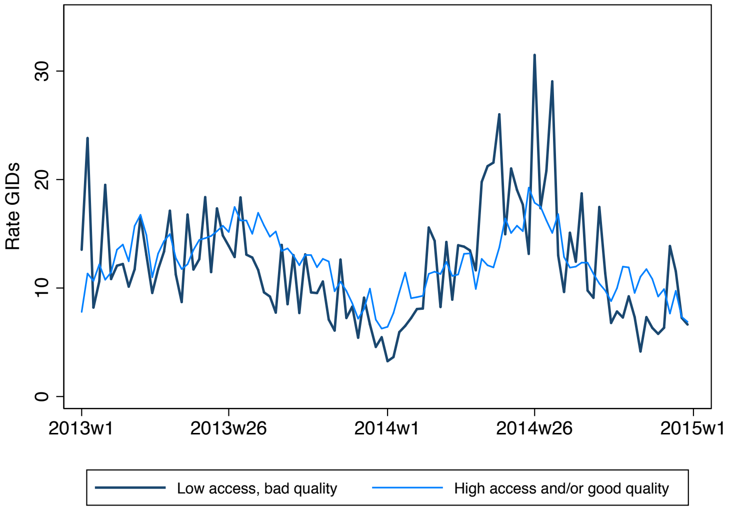
(c) Fruit water (d) Plain water



(e) Milk

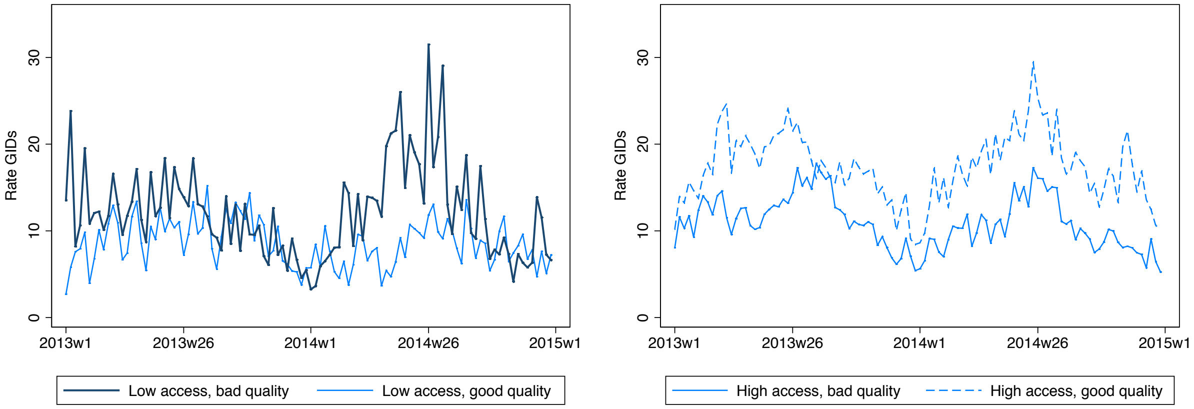
Notes: This figure uses data from the 2016 national health survey (ENSANUT) to show drink patterns in Mexico by age and SES group. The y-axis shows the fraction of individuals in a given age-SES group that reported consuming at least one portion of the drink on a weekly basis. The top left panel shows sugary beverages, mostly made up of sodas but also including industrialized juices, flavored milk, and drinks with sugar added; the top right graph includes all natural juices; the middle left graph shows non-industrial fruit water (*aguas frescas*) usually with sugar added; the middle right graph shows plain water (for which the source - bottled or tap - is not specified); and the last graph indicates unflavored milk.

Figure 2:
GID Rates by Treatment and Control Clinics



Notes: This figure plots the average weekly GID rate at public outpatient clinics in 2013-2014, dividing clinics into two groups: treatment areas with low access to piped water and bad quality groundwater, and control areas that have either high access to piped water or good quality groundwater (or both). Access to tap water is defined as the proportion of households within a 1 km radius of the clinic that report getting their water from wells, rivers, lakes or dams in the 2010 census. Low access to tap water is then defined relative to the average of this variable across the sample. Groundwater quality comes from monitoring stations within a 1 km radius of the clinic (the worst measurement is used when more than one station was present). Groundwater quality is bad whenever at least one of three available measures for 2013 drops below the “good” threshold (see text for more details). Sample restricted to clinics with at least one monitoring station within a 1 km radius (624 clinics total).

Figure 3:
GID Rates by Treatment and Control: Disaggregated Control Group

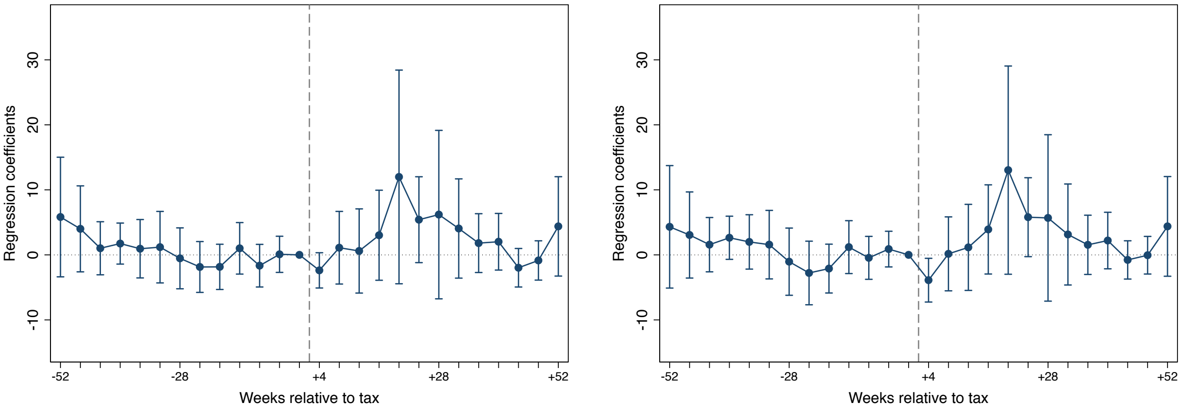


(a) Low tap water access

(b) High tap water access

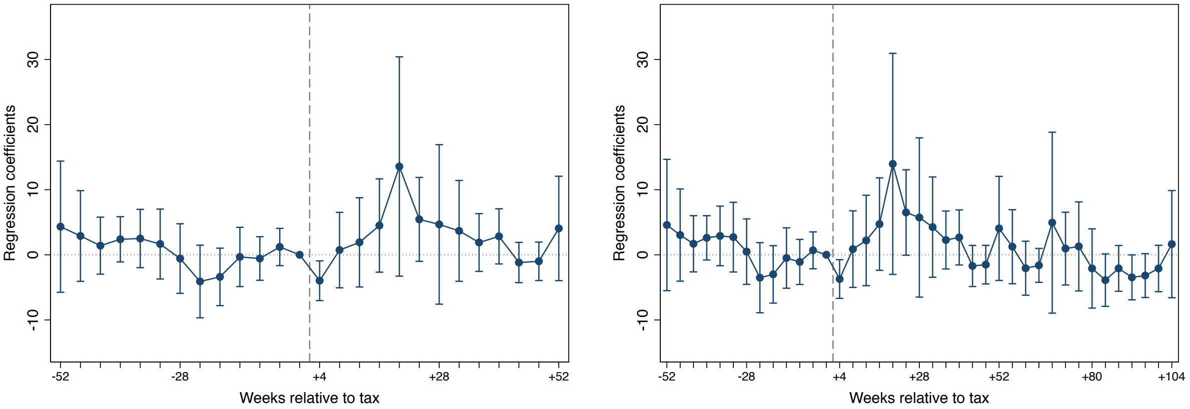
Notes: This figure plots the average weekly GID rate at public outpatient clinics in 2013-2014, dividing clinics into four groups by access to tap water and groundwater quality. Access to tap water is defined as the proportion of households within a 1 km radius of the clinic that report getting their water from wells, rivers, lakes or dams in the 2010 census. Low access to tap water is then defined relative to the average of this variable across the sample. Groundwater quality comes from monitoring stations within a 1 km radius of the clinic (the worst measurement is used when more than one station was present). Groundwater quality is bad whenever at least one of three available measures for 2013 drops below the “good” threshold (see text for more details). Sample restricted to clinics with at least one monitoring station within a 1 km radius (624 clinics total).

Figure 4:
DD Effect of the Soda Tax on GID Rates: Treatment vs Pooled Control



(a) Main specification

(b) State×month, institution×month FE

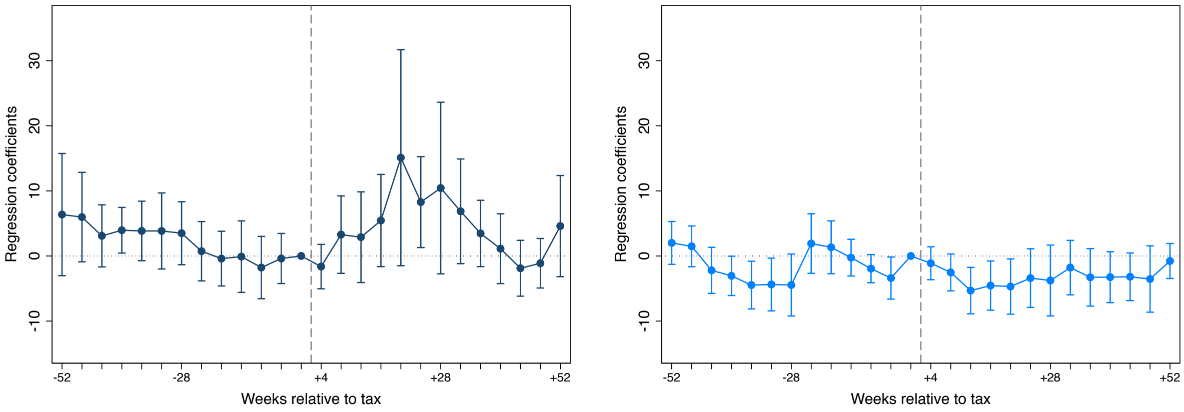


(c) Including all controls

(d) All controls, long-term effect

Notes: This figure shows the effect of the soda tax on outpatient GID rates per 100,000 from estimating equation 1. Estimates are based on a DD of the treated clinics against all other clinics. The figures plot the coefficients from a regression of rates on a vector of lead and lagged indicators for four-week periods relative to the soda tax's introduction on January 1, 2014 (dashed vertical line) interacted with an indicator for low access and bad quality, with the last four weeks of 2013 as the omitted category. Figure 5a shows the baseline specification. Figures 5b and 5c include additional controls. Figure 5d includes 2015 data to explore long-term effects. The unit of observation for each regression is the clinic-week, restricting to data for 2013-2014 (except for Figure 5d). Standard errors are clustered at the clinic level. Error bars show 95% confidence intervals. Each regression includes calendar week, year, and clinic FE. Estimates based on 64,896 observations (624 clinics × 104 weeks) for the first three graphs, and 162,240 for the last one.

Figure 5:
DD Effect of the Soda Tax on GID Rates: Conditional on Access

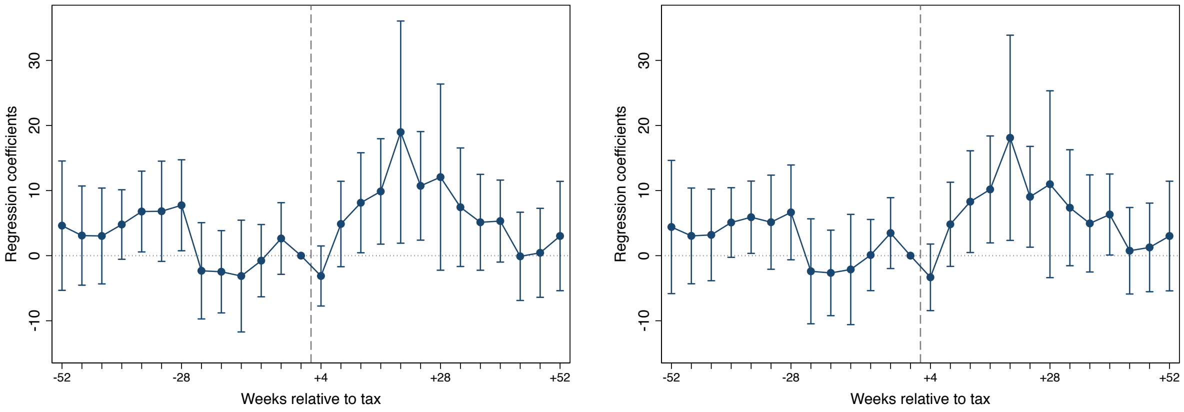


(a) Low access

(b) High access

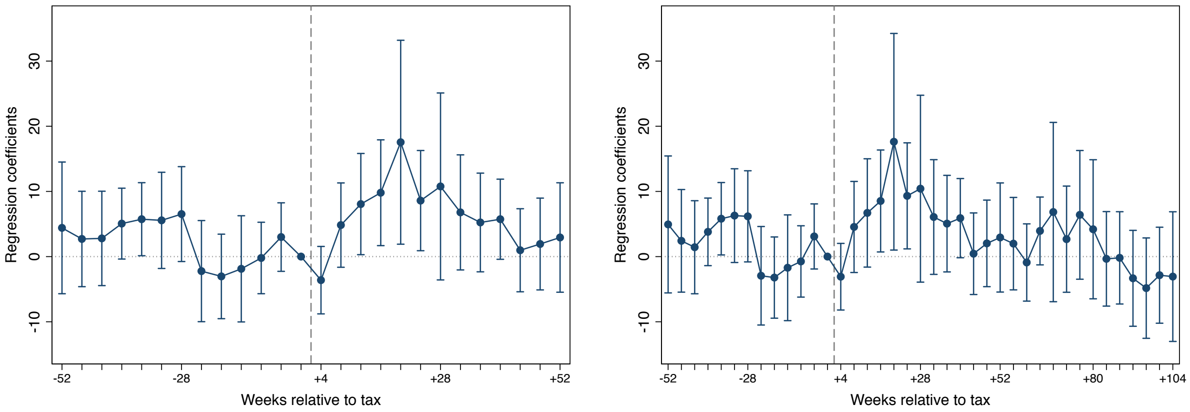
Notes: This figure shows the effect of the soda tax on outpatient GID rates per 100,000. Estimates are based on a DD, conditioning on access to piped water (see equation 3). The plot on the left conditions to low access (where we would expect to see effects), and the one on the right is conditional on high access to piped water. The figures plot the coefficients from a regression of rates on a vector of lead and lagged indicators for four-week periods relative to the soda tax's introduction on January 1, 2014 (dashed vertical line) interacted with an indicator for bad quality, with the last four weeks of 2013 as the omitted category. The unit of observation for each regression is the clinic-week, restricting to data for 2013-2014. Standard errors are clustered at the clinic level. Error bars show 95% confidence intervals. Each regression includes calendar week, year, and clinic FE. Estimates based on 18,720 observations for the graph on the left, and 46,176 for the one on the right.

Figure 6:
DDD Effect of the Soda Tax on GID Rates



(a) Main specification

(b) State×month, institution×month FE

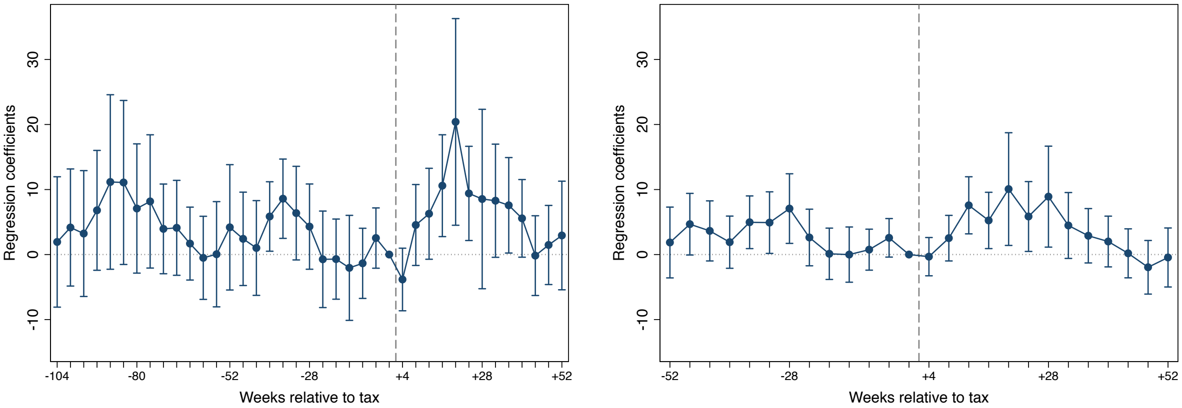


(c) Including all controls

(d) All controls, long-term effect

Notes: This figure shows the effect of the soda tax on outpatient GID rates per 100,000 from estimating equation 2. Estimates are based on a DDD design. The figures plot the coefficients from a regression of rates on a vector of lead and lagged indicators for four-week periods relative to the soda tax's introduction on January 1, 2014 (dashed vertical line) interacted with low access and bad quality indicators, with the last four weeks of 2013 as the omitted category. Figure 7a shows the baseline specification. Figures 7b and 7c include additional controls. Figure 7d includes 2015 data to explore long-term effects. The unit of observation for each regression is the clinic-week, restricting to data for 2013-2014 (except for Figure 7d). Standard errors are clustered at the clinic level. Error bars show 95% confidence intervals. Each regression includes calendar week, year, and clinic FE. Estimates based on 64,896 observations (624 clinics × 104 weeks) for the first three graphs, and 162,240 for the last one.

Figure 7:
Robustness Checks for the Effect of the Soda Tax on GID Rates

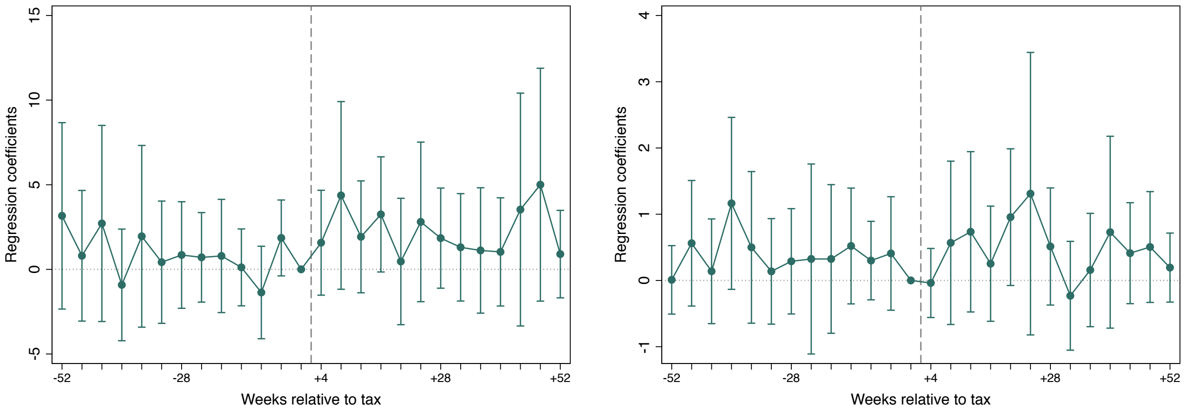


(a) Including 2009-2014 data

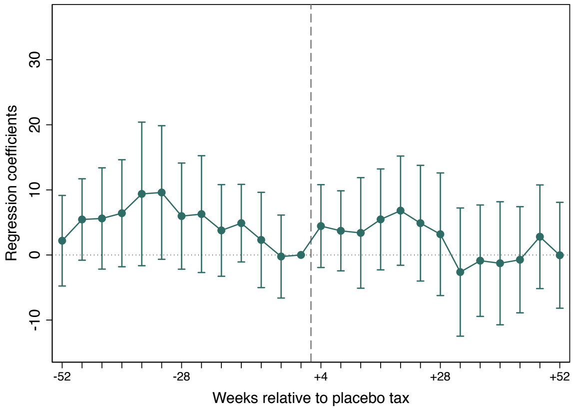
(b) Using a 2 km radius around clinics

Notes: This figure performs robustness checks on the main DDD results. The figure on the left includes data from 2009 to 2014, plotting only the estimates for 2012-2014. The graph on the right uses access and quality measures in a 2 km radius around clinics, restricting to data for 2013-2014. The figures plot the coefficients from a regression of rates on a vector of lead and lagged indicators for four-week periods relative to the soda tax's introduction on January 1, 2014 (dashed vertical line) interacted with low access and bad quality indicators, with the last four weeks of 2013 as the omitted category (see equation 2). The unit of observation for each regression is the clinic-week. Standard errors are clustered at the clinic level. Error bars show 95% confidence intervals. Each regression includes calendar week, year, and clinic FE, flexible trends for low access areas and bad groundwater areas, state-calendar month and institution-calendar month fixed effects, and flexible household controls. Estimates on the left based on 194,688 observations (624 clinics × 52 weeks × 6 years); estimates on the right based on 148,928 observations (1,432 clinics × 104 weeks).

Figure 8:
Placebo Checks for the Effect of the Soda Tax on GID Rates



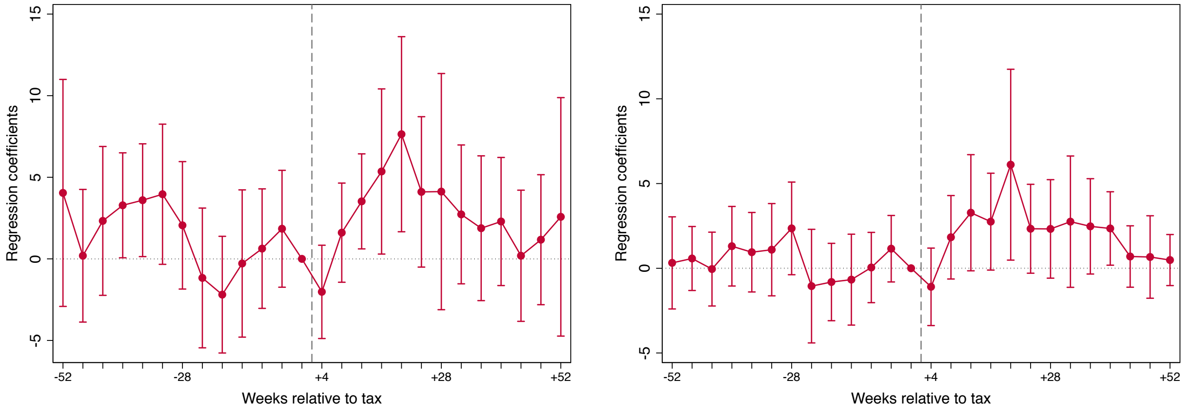
(a) Chronic diseases (b) External injuries



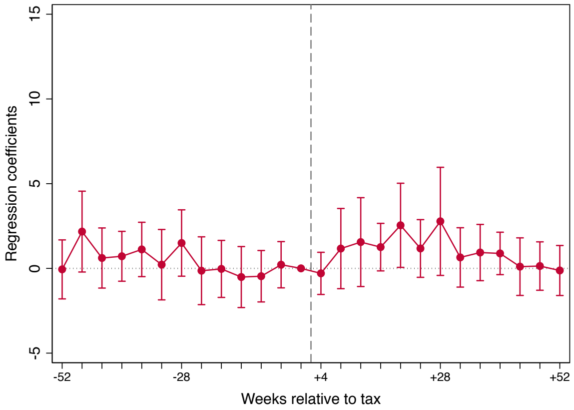
(c) Placebo tax date (Jan. 1, 2013)

Notes: This figure performs placebo checks on the main results by estimating impacts on unrelated outpatient visits (top graphs), and by estimating effects for a placebo date as if the tax had been introduced a year earlier on January 1, 2013 (bottom graph). Figure 9a considers chronic diseases, and Figure 9b external injuries (see text for definitions). The figures plot the coefficients from a regression of rates on a vector of lead and lagged indicators for four-week periods relative to the soda tax's introduction on January 1, 2014 (dashed vertical line) interacted with low access and bad quality indicators, with the last four weeks of 2013 as the omitted category (see equation 2). The unit of observation for each regression is the clinic-week, restricting to data for 2013-2014. The bottom graph uses the placebo date, restricting data to 2012-2013. Standard errors are clustered at the clinic level. Error bars show 95% confidence intervals. Each regression includes calendar week, year, and clinic FE, flexible trends for low access areas and bad groundwater areas, state-calendar month and institution-calendar month fixed effects, and flexible household controls. Estimates based on 64,896 observations. The mean baseline rate for chronic diseases is 2.44, and 0.27 for external injuries.

Figure 9:
Effect of the Soda Tax on GID Rates: Heterogeneity by Age Groups



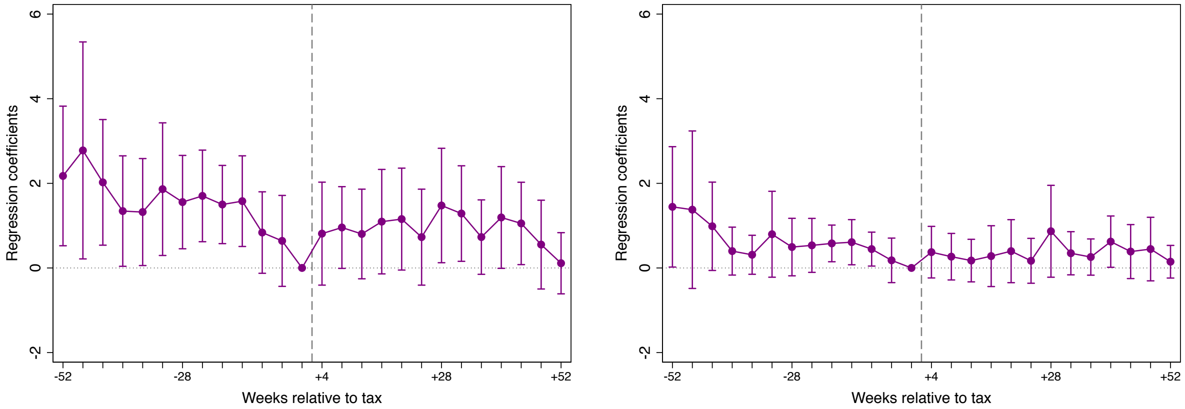
(a) Ages 0-14 (b) Ages 15-44



(c) Ages 45 and older

Notes: This figure explores heterogeneous effects by age of the soda tax on outpatient GID rates per 100,000. The figures plot the coefficients from a regression of rates on a vector of lead and lagged indicators for four-week periods relative to the soda tax's introduction on January 1, 2014 (dashed vertical line) interacted with low access and bad quality indicators, with the last four weeks of 2013 as the omitted category (see equation 2). The unit of observation for each regression is the clinic-week, restricting to data for 2013-2014. Standard errors are clustered at the clinic level. Error bars show 95% confidence intervals. Each regression includes calendar week, year, and clinic FE, flexible trends for low access areas and bad groundwater areas, state-calendar month and institution-calendar month fixed effects, and flexible household controls. Estimates based on 64,896 observations. The mean GID rate at baseline is 7.95, 3.84, and 1.92 for each age group, respectively.

Figure 10:
Effect of the Soda Tax on GID Hospitalization Rates at Public SSA Hospitals



(a) All hospitalizations

(b) Hospitalizations children ages 0-5

Notes: This figure shows the effect of the soda tax on hospitalization rates per 100,000 due to GIDs at public SSA hospitals. The left graph shows the overall rate, and the one on the right shows the rate for children under the age of 6. The figures plot the coefficients from a regression of rates on a vector of lead and lagged indicators for four-week periods relative to the soda tax's introduction on January 1, 2014 (dashed vertical line) interacted with low access and bad quality indicators, with the last four weeks of 2013 as the omitted category (see equation 2). The unit of observation for each regression is the hospital-week, restricting to data for 2013-2014. Standard errors are clustered at the hospital level. Error bars show 95% confidence intervals. Each regression includes calendar week, year, and clinic FE, flexible trends for low access areas and bad groundwater areas, state-calendar month and institution-calendar month fixed effects, and flexible household controls. Estimates based on 24,856 observations (239 hospitals × 104 weeks). The mean GID hospitalization rate at baseline is 1.98 for the general population, and 0.88 for children under six.

Appendices for Online Publication

A Access to Piped Water and Groundwater Quality

A.1 Access to Piped Water

Table A1 shows descriptive statistics at the electoral section level for the variables measuring access to piped water in the 2010 census. This table shows that on average 63% of households within a section have access to piped water inside their home. The remaining 37% is then broken down by other sources: around 24% obtain piped water from neighbors or a communal tap, 1% buy water from vendors, and the remaining 12% use groundwater from wells (8%) and from rivers, lakes and dams (3%).

The geographic distribution of access to piped water, as measured by the fraction of households using groundwater, is shown in Figure A1. As can be seen, there is a considerable amount of spatial variation in this variable. Similar maps for alternative definitions of access to tap water (as outlined in Table A1) show very similar patterns.

A.2 Groundwater Quality

Table A2 shows the criteria used by CONAGUA in classifying each of the three groundwater quality measures (biochemical oxygen demand, chemical oxygen demand, and total suspended solids) into five categories of cleanliness. While it is unclear how CONAGUA chose these thresholds (or even why five categories and not just two), we attempt to use both this classification and international standards in order to construct our binary measure of quality.

Standards in developed settings suggest that the "acceptable" category tends to fall above established thresholds. For example, the Canadian Ministry of the Environment establishes a threshold of 5.5-6.5 mg/L for BOD in warm-water ecosystems. Michigan's Department of

Environmental Quality indicates water appears cloudy for TSS levels between 40-80 mg/L. Utah, according to the EPA, establishes a threshold of 90 mg/L for TSS. Therefore, we consider water quality to be bad if at least one of the three measures drops below good. Note however that the results hold under alternative definitions, specifically, classifying quality as bad when at least one of the measures drops below acceptable.

Figure A2 shows a histogram for each of the three quality measures, while Figure A3 shows a map displaying each of the 2,976 monitoring stations. Each panel of the map shows each of the three measures, by level of pollution according to the thresholds established by CONAGUA.

Table A1:
Access to Piped Water at the Section Level

	Mean	Std. Dev.
Percentage of HH getting water from:		
Sources outside the home	37.1	36.8
Piped water from neighbors/communal tap	24.1	28.1
Water from vendors	1.2	6.3
Groundwater	11.8	24.6
Wells	8.6	19.9
Rivers, lakes and dams	3.3	12.0
Total observations	64,559	

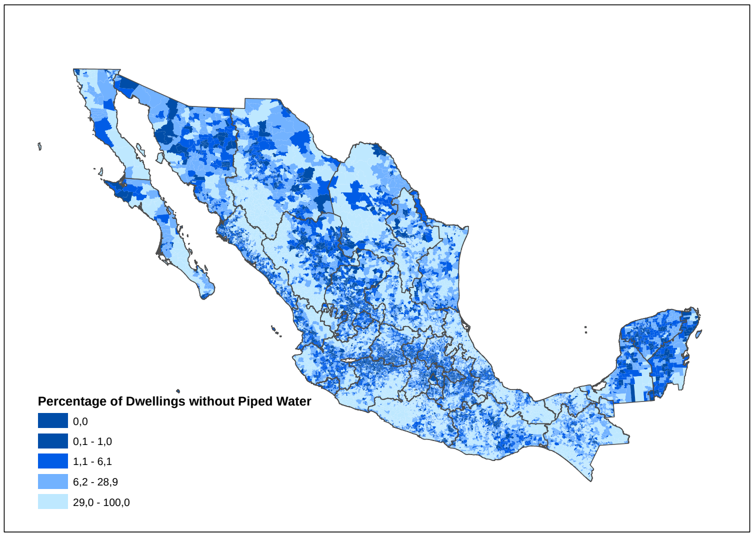
Notes: This table shows section-level averages for access to piped water according to the 2010 census.

Table A2:
Groundwater Quality Thresholds

	Biochemical Oxygen Demand	Chemical Oxygen Demand	Total Suspended Solids
Excellent	$\leq$3 mg/L	$\leq$10 mg/L	$\leq$25 mg/L
Good	3-6	10-20	25-75
Acceptable	6-30	20-40	75-150
Polluted	30-120	40-200	150-400
Very polluted	>120	>200	>400

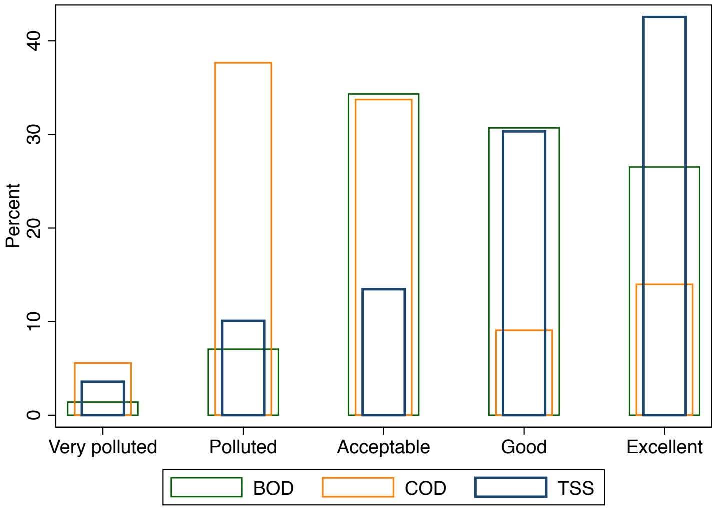
Notes: This table shows the thresholds established by CONAGUA in classifying each of the three groundwater quality measures.

Figure A1:
Geographic Distribution of Access to Piped Water



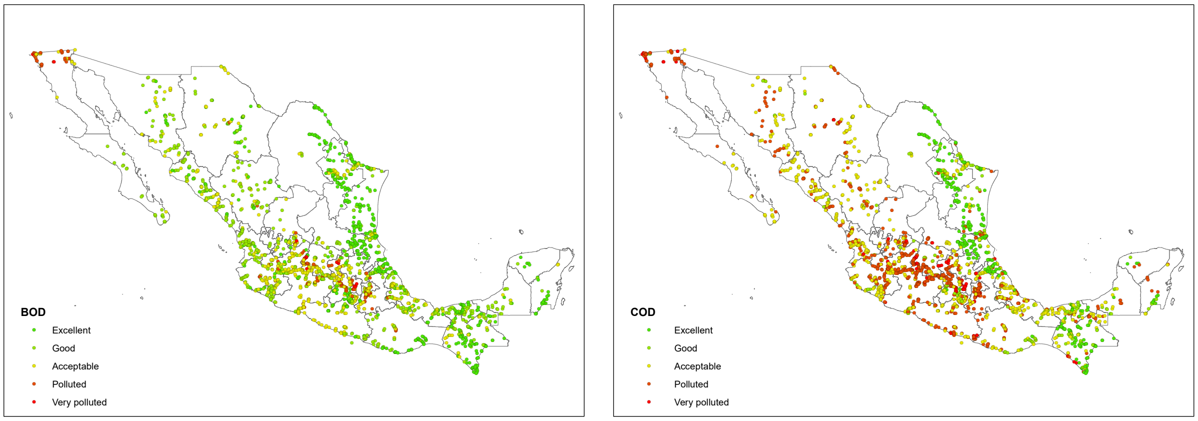
Notes: This map shows the distribution of households without access to piped water. The map shows the fraction of households within an electoral section that obtain their water from a ground source (wells, rivers, lakes and dams).

Figure A2:
Distribution of Groundwater Quality Measures



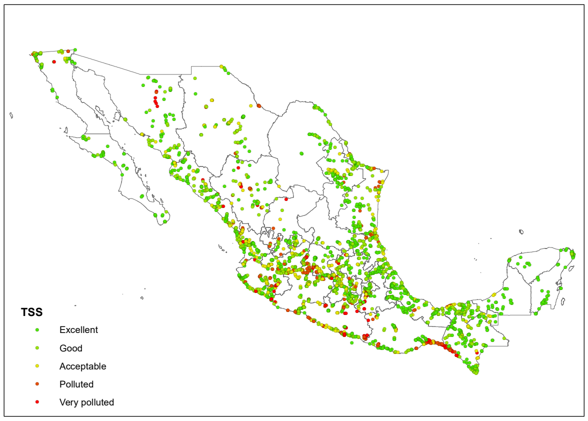
Notes: This figure shows the distribution of the three groundwater quality measures (BOD, COD and TSS) according to the five categories determined by CONAGUA. Each bar shows the percentage of monitoring stations classified into each category. There are 2,976 monitoring stations at wells, lakes, rivers or dams.

Figure A3:
Geographic Distribution of Groundwater Quality



(a) Biochemical oxygen demand

(b) Chemical oxygen demand



(c) Total suspended solids

Notes: This map shows each monitoring station's classification into each of the five categories (excellent, good, acceptable, polluted, and very polluted) for each of the three measures.

B Prevalence of GIDs and Likelihood of Seeking Outpatient Care

Our estimates rely on using outpatient GID rates as our dependent variable. Evidently, this may not be an accurate representation of the overall prevalence of diarrheal disease in a clinic's catchment area. In terms of the validity of our strategy, this is only a concern if individuals around treatment and control clinics differentially changed their likelihood of seeking medical care when sick. The question then is whether the mapping of unobserved GID prevalence to observed GID visits at public clinics is effectively changing over time, specifically as GIDs become more prevalent.

To shed light on this potential issue, we turn to survey data from the Mexican National Health Survey (ENSANUT). This is a nationally representative survey, usually carried out every six years. We explore data from the 2006 and 2012 rounds. Unfortunately, the 2016 round only focused on nutrition and chronic diseases, excluding questions on disease and healthcare utilization. Nevertheless, we believe that this exercise is informative.

We estimate the following equation using the individual-level data for both rounds:

$$y_{imr} = \beta_1 sick_{imr} + \beta_2 rate_{mr} + \beta_3 (sick \times rate)_{imr} + X'_{imr}\gamma + \lambda_m + \theta_r + \varepsilon_{imr} \tag{B1}$$

where y_{imr} is an indicator for whether individual i in municipality m in survey round r sought medical care at a public clinic, $sick_{imr}$ is an indicator for being sick with a GID, $rate_{mr}$ is the GID rate excluding individual i, $\frac{1}{N_{mr}} \sum_{j \neq i} sick_{jmr}$, X'_{imr} is a vector of controls, λ_m are municipality fixed effects, θ_r are indicators for each round, and ε_{imr} is the error term. Note that this is a repeated cross-section, where we cannot track the same individuals over time.

We recognize that these estimates only allow us to identify correlations within the data. However, these simple relationships may be very informative. The coefficient β_1 indicates by how much the observed probability of going to the public clinic increases when an individual is sick with a GID. The coefficient β_2 measures changes in the likelihood of seeking care as the prevalence of GIDs increases. Lastly, the coefficient β_3 indicates whether this probability changes differentially for individuals that are sick with a GID in areas with varying prevalence of GIDs.

We are especially interested in β_3. If we find a positive and significant coefficient, this would mean that the probability of seeking care when sick with a GID increases with the overall prevalence of GIDs in an individual's municipality. This would then suggest that clinic reports of GIDs increase mechanically whenever the prevalence of GIDs increases. If instead we find a statistical zero, then an individual's decision of seeking care when sick is independent of the overall GID rate, regardless of the general effect of GID rates on this likelihood. This would be reassuring, since it would indicate that the mapping of GID prevalence to our clinic reports does not change with changes in GID rates.

Table B1 shows the results from estimating equation B1. We begin in column 1 by simply showing the correlation between the likelihood of seeking care at a public clinic and being sick with a GID. In columns 2 and 3, we successively add the GID rate and base controls, as well as municipality and survey round fixed effects. These three columns show a positive and significant link between being sick with a GID and seeking care at a public clinic. The magnitude is relatively stable, increasing the probability of care by 30 percentage points. Columns 2 and 3 also indicate that an additional GID case per 1,000 individuals in a given municipality-year is associated with a small but significant increase in the likelihood of seeking care.

Column 4 adds the interaction between the indicator for whether the individual is sick with a GID and the local GID rate. The estimate is small, negative and statistically indistinguishable from zero. Column 5 includes additional individual-level controls. The results remain unchanged. The fact that we do not find a significant coefficient, and that the estimate is negative and not positive, suggests that there is no differential change in the likelihood of seeking care when sick with a GID as the local prevalence of GIDs increases.

As such, this suggests that the fact that we observe GID visits at public clinics, instead of the full prevalence of GIDs, does not introduce an important bias in our results.

Table B1:
Relationship Between Seeking Attention at a Public Clinic
and Being Sick with a GID

	(1)	(2)	(3)	(4)	(5)
Sick with GID	0.3071***	0.3039***	0.3042***	0.3239***	0.3094***
	(0.0108)	(0.0108)	(0.0107)	(0.0175)	(0.0175)
GID rate per 1,000		0.0004***	0.0005***	0.0005***	0.0004***
		(0.0001)	(0.0002)	(0.0002)	(0.0001)
Sick with GID × GID rate				-0.0023	-0.0027
				(0.0019)	(0.0018)
Observations	401,450	401,450	401,450	401,450	363,074
R-squared	0.0153	0.0185	0.0273	0.0274	0.0374
Municipality FE			X	X	X
Year FE			X	X	X
Base controls		X	X	X	X
Additional controls					X

Notes: This table shows the correlation between seeking medical attention at a public clinic and being sick with a GID, using data from the 2006 and 2012 ENSANUT survey rounds. Observations are individuals in a given municipality-year. The dependent variable is an indicator for seeking medical attention at a public clinic. GID rate per 1,000 is the prevalence of GID rates in a given municipality-year. Base controls include age, gender, whether the individual lives in a house with a dirt floor, electricity, piped water, and sewage, as well as municipality-year level averages of these last four household characteristics. Additional controls, for which a few missing values are recorded, include education indicators and indicators for health insurance status. Robust standard errors clustered at the municipality level.
*** p<0.01, ** p<0.05, * p<0.1

C Point Estimates of the Main Results

This appendix shows the point estimates in table format for the main results reported graphically in the main text. We only show the point estimates for the four main specifications presented for the DD and the DDD approach.

Table C1:
Point Estimates of the DD Effect

	(1)	(2)	(3)	(4)
Weeks 1-4, 2013	5.8224	4.3138	4.3210	4.5784
	(4.6840)	(4.7961)	(5.1322)	(5.1364)
Weeks 5-8, 2013	4.0036	3.0593	2.8932	3.0390
	(3.3672)	(3.3717)	(3.5465)	(3.6076)
Weeks 9-12, 2013	1.0182	1.5700	1.4076	1.6957
	(2.0781)	(2.1268)	(2.2296)	(2.1991)
Weeks 13-16, 2013	1.7389	2.6329	2.3861	2.6076
	(1.6064)	(1.6887)	(1.7699)	(1.7336)
Weeks 17-20, 2013	0.9461	1.9931	2.5029	2.9006
	(2.2866)	(2.1295)	(2.2809)	(2.3327)
Weeks 21-24, 2013	1.1884	1.5752	1.6583	2.7147
	(2.7999)	(2.6838)	(2.7363)	(2.7273)
Weeks 25-28, 2013	-0.5310	-1.0444	-0.5766	0.4864
	(2.3850)	(2.6313)	(2.7232)	(2.5610)
Weeks 29-32, 2013	-1.8616	-2.7820	-4.0986	-3.5173
	(1.9905)	(2.4906)	(2.8407)	(2.7396)
Weeks 33-36, 2013	-1.8436	-2.1069	-3.3902	-3.0050
	(1.7715)	(1.9119)	(2.2470)	(2.2473)
Weeks 37-40, 2013	1.0076	1.1927	-0.3278	-0.4947
	(2.0164)	(2.0711)	(2.3166)	(2.3614)
Weeks 41-44, 2013	-1.6592	-0.4546	-0.5638	-1.0957
	(1.6697)	(1.6806)	(1.7061)	(1.7627)
Weeks 45-48, 2013	0.0853	0.8913	1.2052	0.6889
	(1.4159)	(1.3964)	(1.4552)	(1.4420)
Weeks 1-4, 2014	-2.3805*	-3.8891**	-3.9771**	-3.7197**
	(1.3807)	(1.7121)	(1.5535)	(1.5156)
Weeks 5-8, 2014	1.1020	0.1577	0.7291	0.8749
	(2.8444)	(2.8950)	(2.9514)	(2.9953)
Weeks 9-12, 2014	0.6015	1.1533	1.9143	2.2023
	(3.2967)	(3.3714)	(3.4911)	(3.5367)
Weeks 13-16, 2014	3.0208	3.9148	4.4991	4.7206
	(3.5253)	(3.4921)	(3.6499)	(3.6164)
Weeks 17-20, 2014	11.9895	13.0366	13.5696	13.9673
	(8.3673)	(8.1535)	(8.5782)	(8.6488)
Weeks 21-24, 2014	5.4101	5.7969*	5.4450*	6.5014*
	(3.3634)	(3.0906)	(3.2819)	(3.3449)
Weeks 25-28, 2014	6.1960	5.6826	4.6722	5.7351
	(6.5965)	(6.5171)	(6.2397)	(6.2292)
Weeks 29-32, 2014	4.0568	3.1363	3.6769	4.2582
	(3.8866)	(3.9551)	(3.9450)	(3.9242)
Weeks 33-36, 2014	1.8085	1.5452	1.8902	2.2753
	(2.3031)	(2.3203)	(2.2593)	(2.2649)
Weeks 37-40, 2014	2.0208	2.2059	2.8355	2.6687
	(2.2179)	(2.2141)	(2.1592)	(2.1524)
Weeks 41-44, 2014	-1.9796	-0.7750	-1.1836	-1.7154
	(1.5083)	(1.4982)	(1.5732)	(1.6075)
Weeks 45-48, 2014	-0.8580	-0.0520	-0.9965	-1.5128
	(1.5390)	(1.4733)	(1.5041)	(1.5103)
Weeks 49-52, 2014	4.3821	4.3821	4.0534	4.0534
	(3.8911)	(3.9032)	(4.0822)	(4.0709)
Observations	64,896	64,896	64,896	97,344
R-squared	0.4014	0.4159	0.4173	0.4111
State-calendar month FE		X	X	X
Institution-calendar month FE		X	X	X
Household flexible controls			X	X
Includes 2015 data				X

Notes: This table shows the effect of the soda tax on outpatient GID rates per 100,000 from estimating equation 1 in the main text. Estimates are based on a DD of the treated clinics against all other clinics. The unit of observation for each regression is the clinic-week, restricting to data for 2013-2014 (except for column 4). Standard errors are clustered at the clinic level. Each regression includes calendar week, year, and clinic FE.

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

Table C2:
Point Estimates of the DDD Effect

	(1)	(2)	(3)	(4)
Weeks 1-4, 2013	4.6101	4.4038	4.4071	4.9360
	(5.0590)	(5.2105)	(5.1454)	(5.3506)
Weeks 5-8, 2013	3.0862	3.0379	2.7053	2.4182
	(3.8809)	(3.7457)	(3.7273)	(4.0077)
Weeks 9-12, 2013	3.0235	3.1896	2.7931	1.4359
	(3.7492)	(3.5829)	(3.6872)	(3.6353)
Weeks 13-16, 2013	4.7854*	5.0939*	5.0623*	3.7970
	(2.7197)	(2.7237)	(2.7677)	(2.6402)
Weeks 17-20, 2013	6.7790**	5.9053**	5.7375**	5.8089**
	(3.1607)	(2.8289)	(2.8543)	(2.8283)
Weeks 21-24, 2013	6.8215*	5.1402	5.5604	6.2890*
	(3.9222)	(3.6828)	(3.7577)	(3.6617)
Weeks 25-28, 2013	7.7410**	6.6490*	6.5231*	6.1780*
	(3.5582)	(3.7079)	(3.7054)	(3.5608)
Weeks 29-32, 2013	-2.3244	-2.4027	-2.2278	-2.9386
	(3.7655)	(4.1062)	(3.9499)	(3.8487)
Weeks 33-36, 2013	-2.4734	-2.6489	-3.0412	-3.2178
	(3.2185)	(3.3449)	(3.3022)	(3.1735)
Weeks 37-40, 2013	-3.1322	-2.1232	-1.8769	-1.7125
	(4.3722)	(4.3123)	(4.1478)	(4.1319)
Weeks 41-44, 2013	-0.7667	0.0937	-0.2118	-0.7429
	(2.8268)	(2.7835)	(2.7922)	(2.7867)
Weeks 45-48, 2013	2.6367	3.4669	2.9966	3.0924
	(2.8030)	(2.7712)	(2.6769)	(2.5433)
Weeks 1-4, 2014	-3.1224	-3.3287	-3.6138	-3.0849
	(2.3500)	(2.6038)	(2.6357)	(2.6031)
Weeks 5-8, 2014	4.8661	4.8178	4.8341	4.5470
	(3.3415)	(3.2922)	(3.2952)	(3.5578)
Weeks 9-12, 2014	8.1297**	8.2959**	8.0584**	6.7011
	(3.9121)	(3.9786)	(3.9543)	(4.2321)
Weeks 13-16, 2014	9.8684**	10.1770**	9.8004**	8.5351**
	(4.1250)	(4.1833)	(4.1333)	(3.9829)
Weeks 17-20, 2014	18.9814**	18.1077**	17.5491**	17.6205**
	(8.6907)	(8.0274)	(7.9663)	(8.4621)
Weeks 21-24, 2014	10.7238**	9.0425**	8.5911**	9.3196**
	(4.2461)	(3.9429)	(3.9144)	(4.1427)
Weeks 25-28, 2014	12.0669*	10.9750	10.7667	10.4216
	(7.2819)	(7.3084)	(7.3039)	(7.3022)
Weeks 29-32, 2014	7.4422	7.3639	6.7842	6.0733
	(4.6368)	(4.5359)	(4.4952)	(4.4813)
Weeks 33-36, 2014	5.1273	4.9519	5.2362	5.0595
	(3.7508)	(3.7985)	(3.8560)	(3.7728)
Weeks 37-40, 2014	5.3139*	6.3229**	5.7344*	5.8988*
	(3.2067)	(3.1679)	(3.1309)	(3.0937)
Weeks 41-44, 2014	-0.1053	0.7551	0.9729	0.4419
	(3.4522)	(3.3829)	(3.2449)	(3.1867)
Weeks 45-48, 2014	0.4378	1.2679	1.9322	2.0281
	(3.4813)	(3.4657)	(3.5868)	(3.3820)
Weeks 49-52, 2014	3.0157	3.0157	2.9320	2.9320
	(4.2760)	(4.2892)	(4.2748)	(4.2630)
State-calendar month FE		X	X	X
Institution-calendar month FE		X	X	X
Household flexible controls			X	X
Includes 2015 data				X

Notes: This table shows the effect of the soda tax on outpatient GID rates per 100,000 from estimating equation 2 in the main text. Estimates are based on a DDD design. The unit of observation for each regression is the clinic-week, restricting to data for 2013-2014 (except for column 4). Standard errors are clustered at the clinic level. Each regression includes calendar week, year, and clinic FE.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$