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SCHOOL CLOSURES AND PARENTAL MENTAL HEALTH

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ABSTRACT

Schools enhance the lives of families in various ways, and one potential consequence of their closures is worsened parental well-being. We study the effects of COVID-19 pandemic school closures on parental mental health by measuring consumption of products that are often used to cope with increased stress and depression. Using a cohort based difference in difference (DID) design and commercial claims data, we find an increase in maternal anti-depressant use by 1.5%, in zip codes with above median school closures; there are no statistically significant effects for paternal antidepressant use, and we are able to rule out fairly small values. Some parents may "self-medicate" as a coping mechanism rather than seek formal medical care. Using a county based DID design and retail scanner data, we find alcohol sales increased by 2% in counties with above median school closures. Both anti-depressant prescriptions and alcohol sales returned to base line levels as in-person schooling resumed. We explore whether the burdens of school closures were disparately concentrated in minoritized communities, and find that anti-depressant and alcohol use increases were concentrated in zip codes with above median Black and Asian populations, but not in zip codes with a predominantly White or Hispanic population. Overall, these results suggest that the school system plays an important role in maintaining population mental well-being outcomes and in helping families cope with stress.

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1 Introduction

The COVID-19 pandemic upended in-person K-12 education globally, as schools pivoted almost overnight to virtual modalities in Spring 2020 to curb the transmission of the virus (Almeida et al., 2022). School closures have been one of the widest-spread, and in some countries, longest-lasting and perhaps the most contested policy responses to the COVID-19 pandemic (Raviv et al., 2021; Viner et al., 2022; Vlachos et al., 2021). These closures have been documented to have a profound impact on students and families (Agostinelli et al., 2022; Jack and Oster, 2023); our work examines parental well-being, an understudied aspect of the literature. Specifically, we examine whether the 2020-2021 in-person schooling disruptions have impacted parental mental health, and especially whether they disparately impacted parents in already at-risk communities.

This paper explores the causal effects on parental mental health well-being of school closures, a novel question in the literature, using proxies of 1) demand for antidepressants and 2) adult alcohol purchases, both with commercial administrative data. We use a large nationwide private health insurance claims database (which contains zip code level geographic detail, along with gender and measures of parental status) and grocery store alcohol purchase data (which contains county level detail, and no further demographics), matched to school mobility restrictions data at the zip code or county level. The health claims data are from Optum's Clinformatics® Data Mart Database and the alcohol sales data are from the Nielsen Retail Scanner. We measure in-person schooling disruptions using cellphone mobility data from SafeGraph.

Our research design takes advantage of a cohort structure, in which we follow unexposed parents, between September 2017-December 2019, and compare them to an exposed cohort, followed between September 2019-December 2021. The terms exposed and unexposed refer to observations during the COVID-19 pandemic time period, so the treated and control groups are aligned in event time. This allows us to estimate event studies and difference-in-differences specifications, as has been done when studying the impact of school closures on children's outcomes (Freedman et al., 2024). Where possible, we disaggregate results by mothers and fathers, by area-level density of race and ethnicity, and by school type, separating the impacts from elementary, middle and high school closures.

Our estimates indicate that during the early stages of the pandemic, the demand for antidepressants by mothers increased by 1.5% on average in zip codes which experienced above median declines in in-person instruction. We find that the main effects can be attributed for the most part to elementary school closures, highlighting the importance of access to schools as formal childcare for parents during social isolation (Deryugina et al., 2022). Interestingly, the dynamic effects show that when schools gradually reopen, starting in the last months of 2020, antidepressant prescriptions for the exposed cohort return to pre-pandemic levels and are essentially identical relative to the unexposed cohort. We find that the worsening indications of mental health are experienced in zip codes or counties that are predominantly Black and Asian, with null effects for zip codes or counties predominantly White and Hispanic. In line with the literature on the disproportionate burdens of childcare between mothers and fathers and the distribution of tasks within the household, we find no statistically significant effects of school closures on fathers' antidepressant demand; all effects are among mothers. This complements research by Goldin (2022) on how women during this economic downturn bore a disproportionate burden with increased housework and care hours.

As a second form of verifiable purchase plausibly related to mental well-being, we also consider the role of self-medication through alcoholic beverages, which we observe from the Nielsen Retail Scanner Data. There are two drawbacks to these data relative to data on antidepressants: we observe only county, not zip code, and we do not observe parental status or gender. We find that adults do increase their demand for alcohol in counties that experienced above median decline in school mobility relative to more mobile areas, equivalent to a 2% increase by the end of 2020. Supporting the results from the use of prescriptions, the purchases of alcohol are concentrated in counties with above median decline in mobility to elementary schools, indicative of greater shortfalls in in-person instruction, during the pandemic (vs to other school types), and in predominantly Black and Asian communities.

The US experienced higher COVID-19 related mortality rates compared with peer countries (Bilinski and Emanuel, 2020; Viglione, 2020; Bilinski et al., 2023), and nearly all of the 55 million students in kindergarten through 12th grade in the U.S. were in some way affected by closures (Golberstein et al., 2020). Although Spring 2020 pandemic-related schooling disruptions were nationwide,[1] there was considerable variation across school systems in their subsequent response to the pandemic, including heterogeneity in reopening dates for the 2020-21 academic year and instructional modes (e.g., in-person, remote, or hybrid) (Parolin and Lee, 2021). The importance of high-quality K-12 education for post-secondary education opportunities, and long-term socioeconomic upward mobility is well-established and a growing body of recent literature finds that pandemic associated school closures not only created gaps in learning but has had a significant negative effect on children's behavioral and emotional difficulties, physical and social activities, with a possible increase in screen time, irregular sleep patterns, diminished exercise, and, for some, particularly children with fewer family resources, nutritional deficits (Golberstein et al., 2020; Hawrilenko et al., 2021). Research also finds that children's diagnosis of mental health changed, with fewer ADHD diagnoses (Freedman et al., 2024), a decrease in diagnoses and prescriptions for depression and anxiety (Björkegren et al., 2024) and reductions in bullying when schools closed (Bacher-Hicks et al., 2022).

School closures also have consequences for parents [2], as they increase childcare and homeschooling responsibilities, which leaves less time for work, leisure, and sleep. Additional time with children may increase family well-being, or, may, especially when combined with societal

[1] https://www.census.gov/library/stories/2020/08/schooling-during-the-covid-19-pandemic.html

[2] Throughout our paper, when we separate "mothers" and "fathers", we acknowledge that there is limited gender identity information in our data which does not go beyond female and male.

upheaval of pandemic times, negatively affect parental mental health. International research (Jo Blanden et al., 2021), descriptively documents the pandemic has had a significant negative influence on mental health of parents (effects are only among mothers) of elementary school age children, with survey evidence that parental stress surged at the beginning of the pandemic, and some parents reporting increased anxiety, depression, agitation, and sleep disturbances (Li et al., 2022; Moreland-Russell et al., 2022), and with adverse maternal mental health effects increasing with the number of elementary school aged children. Pandemic strains in the home environment, including from school closures, may at least partly explain the international descriptive reports of increased alcohol and other addictive substance use (Greenwood et al., 2023), domestic violence and child maltreatment or violence toward children during the pandemic. But evidence from small sample, single site or survey studies does not provide generalizable insights on how pandemic-related in-person schooling closures, beyond other pandemic stressors such as unemployment and financial difficulties, fears of sickness or death, and the effects of other social distancing policies, has adversely impacted parental mental health. Moreover, it is unclear whether school closures during the pandemic has imposed disproportionately higher burdens on mothers, in their continued roles as primary family caregivers. Additionally, we know from other pandemics that school closures disproportionately affect migrants, refugees, minoritized populations, and children with disabilities and special needs. Research also finds that in countries with weak educational infrastructure, the longer children are out of school, particularly children from at-risk communities, the less likely they are to return (Smith, 2021). Although in-person K-12 learning has gradually returned to near pre-COVID-19 pandemic levels, disproportionately higher COVID-19 morbidity and mortality in racially/ethnically minoritized communities (Feldman and Bassett, 2021; Miller et al., 2021; Aburto et al., 2022) has implied slower in-person schooling recovery. In addition to across-the-board adverse impacts of academic disruptions, since socioeconomically disadvantaged/minoritized families may rely more heavily on school provision of wrap-around services, it remains unclear whether school closures may have also disparately impacted the mental health of parents in racial/ethnic minoritized communities.

2 Related Research

2.1 Conceptual framework; How School Closures may Impact Parental Mental Health

School closures may operate through multiple economic pathways to influence parental mental health. First, school closures may require parents to invest greater time in child care, homeschooling and companionship. Comparing parental time inputs from the 2019 American Time Use Survey with the 2020 COVID Inequality Project literature shows the average daily hours parents spend with children increased four fold (from 1.26 hours in 2019 to 5.15 hours in 2020) during the pandemic induced school closures (Goldin, 2022). Spending more time with own children may be utility improving for parents. But many parents, work losses may pose substantial economic strain when facing unexpected childcare needs. In addition to childcare provision, facilitating remote learning during COVID-19 increased parental responsibilities in organizing and assisting their children's education (Agostinelli et al., 2022). Parental stress might also be affected by children's mental health experiences during school closures. Some children experienced reductions in bullying when schools closed (Bacher-Hicks et al., 2022), indicating beneficial mental health for affected children (for instance, Hansen et al. (2022b) find reductions in suicides during school closures) and their parents. But other children experiencing loss of structured school days and friends/peer interactions due to school closure experienced "boredom, worry, frustration, loneliness and irritability" (Dawes et al., 2021), needing additional interaction and support from their parents, who may be already experiencing anxiety due to pandemic induced financial insecurity and job and income loss (Moreland-Russell et al., 2022).

2.2 Prior Literature

There is no published causal research on the impact of school closures on parental outcomes we are aware of, apart from one paper that examined parental employment (Hansen et al., 2022a). Most prior evidence on the toll of school closures on parental mental health in the U.S. (and abroad) is descriptive and comes from survey data (Yamamura and Tsustsui, 2021b; Deryugina et al., 2022; Li et al., 2022; Moreland-Russell et al., 2022; Garcia and Cowan, 2022; Hansen et al., 2022a), that prevent observing administratively recorded mental healthcare use or to accurately follow individuals over time and track their changes in behavior and demand. For instance, Lamar et al. (2021) conducted a survey of 1,048 U.S. parents' in March and April 2020, asking about their depression, anxiety, stress, and alcohol and substance use behaviors in that period. They found severe levels of depression and stress and extremely severe anxiety. There was evidence of widespread alcohol use in the past month, with men reporting significantly higher alcohol consumption and substance use than women, and reports of depression, anxiety, and stress among those with substance use or a history of compromised mental health. Another study Czeisler et al. (2021) uses a survey of over 10,000 respondents that was fielded by the CDC in December 2020 to February/March 2021. This COVID-19 Outbreak Public Evaluation (COPE) Initiative reported adverse mental health symptoms. Suicidal ideation was particularly elevated among parents and caregivers of adults. Davis et al. (2021) use data from the National Panel Study of Coronavirus pandemic (NPSC-19) of 3,338 households conducted in March - April 2020 and found that the experience of parents and children were correlated: the mental health of parents was worse in families where children struggled with remote learning. This study used the Patient Health Questionnaire (PHQ-9) and General Anxiety Disorder (GAD-7) (Kroenke et al., 2001; Spitzer et al., 2006).

These studies provide valuable early signs of adverse parental mental health implications of school closures, reiterating early warnings from psychiatry experts calling for increasing mental health screenings of parents (Peris and Ehrenreich-May, 2021). However, due to the well known biases in survey data[3], existing survey evidence may not adequately capture the parental mental health implications of school closures in the population. Moreover, these early studies are not informative of potential long-term impact of school closures.

Although there are no prior papers examining the impacts of U.S. or international school closures on parental mental health using a causal design, one Canadian paper examines the impact on other parental outcomes (but not parental wellbeing), including labor market behaviors. Beauregard et al. (2022), using a DD design, exploiting regional variation in school closure, and comparing parents of younger children (who require greater parental supervision) to parents of older children, find a reduction in employment, with larger impacts for single parents and for those who do not have have telework options. The paper closest to ours, Blanden et al. (2021), uses a U.K. setting and survey data on self-reported mental health among parents before and after pandemic onset, and finds worse self-reported mental health during school closures among parents of children in ages 4-12 who were not prioritized to return to school. Similarly for Japan, Yamamura and Tsustsui (2021a) find that during the pandemic induced school closures, full-time employed mothers with primary school aged children were more likely to work remotely, while fathers tended to continue working in the office and spend less time with their children at home. In contrast, school closures had little impact on work-from-home patterns of parents with older children. The authors interpret

[3]Sample surveys have been known to suffer from biases such as self-reporting bias (respondents may under/over report their mental health conditions due to stigma, embarrassment, or misunderstanding of their symptoms), recall bias (respondents may have difficulty accurately recalling past mental health states), sampling bias (survey samples may be non-representative of the population, missing groups who may have different mental health experiences), response bias (respondents of mental health survey questions may differ systematically from those who do not), limited scope (survey questions may cover a narrow range of mental health questions), temporal variability (surveys may provide a snapshot in time of mental health but may not adequately capture changes over time), and cultural differences (related to sampling bias, cultural factors can influence perceptions and reports of mental health)

their findings as evidence that COVID-19 has increased the inequality in the burden of child care, disparately burdening mothers. Key differences in preexisting societal and schooling structures and pandemic experiences between other countries and the U.S., may make the evidence from other countries less informative of parental mental health implications of school closures in the U.S.

2.3 Disparate effects of school closures

There may be considerable variation in how burdensome additional caregiving and home-schooling responsibilities may be during the pandemic school closure. In addition to significant variation in duration and nature of in-person schooling disruptions, school closure related mental stressors may be greater for socioeconomically disadvantaged parents who may be less able to, for instance, purchase high-quality childcare, or were less able to work from home during the pandemic due to prepandemic sorting into jobs that did not allow as much remote work, than more educated and/or wealthier parents. Bacher-Hicks et al. (2022) show that early in the pandemic, internet searches for online learning resources rose much more quickly in high-income areas, which suggests that parents in affluent neighborhoods were more engaged with remote learning and may be better able to acquire remote learning resources. In contrast, parents with lower levels of formal education may be less comfortable assisting children with school work.

In the U.S., racial/ethnic minoritized populations experienced the highest rates of COVID-19 morbidity and mortality early in the pandemic (Alsan et al., 2021; Miller et al., 2021; Hamman, 2021; Benitez et al., 2020). Moreover, COVID-19 vaccine uptake was slower among racial/ethnic minoritized individuals, which may have deepened and extended the pandemic in these communities, further delaying return to in-person schooling. As a result, racial/ethnic minority communities in the U.S. may have experienced slower educational recovery and school closures impacts on parental mental health effects may be worse in racial/ethnic minoritized communities.

It may also be that the mental health of parents of younger children would be more severely impacted, than parents of older children, who may need less round-the-clock care and help with e-learning. Alternatively, older children may experience greater social isolation and might require greater emotional support and mentoring during school closures. Despite potentially heterogeneous impacts of school closures by children's age, most descriptive studies relying on survey data have only considered the impact of school closures on parents of elementary-school aged children or parents of high school children, but not both, ruling out the ability to differentiate between the impacts of school closures on parental mental health by age of children.

Pandemic-related school disruptions have also disproportionately impacted mothers compared to fathers. Although custodial fathers also increased their childcare time during the pandemic, the burden on working women was exceptionally heavy, especially with added housework. Childcare time for college-graduate women with full-time jobs and elementary school-aged children increased from 8.7 hours per week before the pandemic to 17.3 hours early in the pandemic and 22.4 hours by fall 2020 (Goldin, 2022), exacerbating preexisting disparities in division of household work (Zamarro and Prados, 2021). Women comprised 47 percent of the U.S. labor force just before March 2020. With 76 percent of women aged 25 to 54 in the labor force in 2019, and half having children under 18, childcare and education are critical issues (Berlinski et al., 2024). Compared to previous recessions, the COVID-19 induced recession witnessed greater job loss among women than men (Montenovo et al., 2022; Goldin, 2022), and were particularly concentrated among women with school-age children (but not for those with younger children), likely attributable to additional childcare responsibilities (the "COVID motherhood penalty") due to school closures (Couch et al., 2022), earning it the moniker "she-cession". However, the disparate impact of school closures on maternal mental health, and how it may differ from effects on fathers, has not been comprehensively studied in the U.S. using a systematic causal inference framework.

Our study fills several key knowledge gaps in the literature. First, although there is descriptive survey research, both domestic and international, on school closures and children's and parents' lives, to the best of our knowledge, our study is the first U.S. causal school closure paper examining implications for parental mental health. Prior research on parental employment include Garcia and Cowan (2022), who used the Monthly Current Population Survey data and county-level school closing information to find that parents of school-aged children, especially low-educated parents in non-telework careers, experienced reduced work hours and earnings. Hansen et al. (2022b) also examines parental employment, using CPS data and exploiting variations in reopening in an event-study framework and finds employment impacts only for married women. There are two papers we know of that causally examine outcomes among children. Considering the universe of older Swedish pupils, Björkegren et al. (2024) exploit the variation arising from mandated transition to remote instruction of upper-secondary students (ages 17–19) and continued in-person instruction of lower-secondary school students (ages 14–16) following pandemic onset as a natural experiment to analyze how modes of instruction affect student mental health. The authors find a 4.4 percent reduction in mental healthcare use from remote instruction, largely due to fewer diagnoses and prescriptions for depression and anxiety, that persists even 21 months after the initial closures and 9 months after resumption of in-person learning. Considering shortfalls in mental healthcare utilization by students during school closures in the U.S., Freedman et al. (2024) used granular geographic and time variation in in-person learning as a quasi-experimental research design to show that the pandemic school closures were associated with significant delays in diagnosis of ADHD among children, due to the school environment playing a major role in the diagnosis process.

Some of these earlier studies highlight the limitations of administrative school closure data in the U.S. context, in terms of completeness due to several states not releasing administrative data and the challenges in comparability across localities and other dimensions of comparability. Following Freedman et al. (2024)'s approach we create granular measures of realized disruptions in in-person schooling by space and time using foot traffic data from cellphone aggregator SafeGraph. Unlike Freedman et al. (2024), we consider the impact of school closures not on pupils but on their parents, thereby providing the first causally interpretable estimates of the impacts of school closures on parental mental health.

Second, our study considers two novel indicators of parental mental health - (1) prescription antidepressant use (from nationwide prescription drug claims database of a large commercial insurer) and (2) alcohol sales (nationwide retail scanner data). Unlike self-reported measures of mental well being in survey data, prescription antidepressant use is an objective measure of mental health not subject to recall bias or other challenges of survey data. The association between worse mental health and 'self-medication', with alcohol, illegal drugs and/or cigarettes, among others (Turner et al., 2018; Mc Hugh and McBride, 2020), has also been well established. People experiencing symptoms of mental illnesses are significantly more likely to misuse alcohol and drugs, though the direction of causality may be difficult to disentangle as each one can be exacerbated by the presence of the other (Kasten, 1999). Self-medication has been linked to the comorbidity of alcohol use disorders and depression (Mc Hugh and McBride, 2020) and also post traumatic stress disorders (Leeies et al., 2010). In particular, alcohol is seen as a way to cope or deal with psychological distress, mood issues and anxiety disorders. Individuals experiencing symptoms related to these conditions might develop a substance use disorder in an attempt at relief (Crum et al., 2013). Surveys conducted during the early stages of the COVID-19 pandemic show that the increased psychological burden of the health crisis increased self-reported mental health and substance use problems. For example, McPhee et al. (2020) show that the post-social-distancing conditions impacted depression severity, coping motives and alcohol consumption indices related to binge drinking and the frequency of solitary drinking. Notably, and by the global scale of the health emergency, the evidence on alcohol consumption and ways to find a relief from stress, anxiety and unpleasant emotions extends beyond the U.S., for instance, Chodkiewicz et al. (2020) reports changes in drinking habits during the initial COVID lockdown period in Poland. Thus, by considering changes in antidepressant prescriptions and alcohol use in response to school closures, our study extends the existing knowledge base and goes beyond existing small-sample survey based evidence on the impact of the school disruptions on self-reported well-being.

Third, by studying whether the COVID-19 school closures differentially impacted mothers than fathers, and racial/ethnic minoritized communities more than predominantly White communities, our study contributes to the recent literature that finds the pandemic may have widened preexisting disparities.[4] In doing so, our study findings contribute to two distinct strands of the literature - (1) non-COVID-19 related adverse health impacts of the pandemic and (2) establishing the key role of schools in driving parental well being, particularly for mothers in their role as the primary family caregivers, and parents in minoritized communities.

3 Data

Our study examining parental mental health extends earlier work by Freedman et al. (2024) on child mental health and Hansen et al. (2022b) on parental employment outcomes by exploiting granular cellphone pings based mobility measures and specific places of interest (schools), to construct a measure of in-person schooling disruptions by geography and time. We merge these data with individual-level healthcare records for a large US commercially

[4]For instance, Perry et al. (2021) use longitudinal survey data (2019–2020) and survey-weighted multivariate regressions to show that even after controlling for preexisting inequality, Black adults were over 3 times as likely as Whites to report food insecurity, being laid off, or being unemployed following pandemic onset. Similarly, Nguyen et al. (2022) show that the immediate decreases in recommended buprenorphine medicated assisted treatments for opioid use disorder (MOUD) at pandemic onset were concentrated among members of racial and ethnic minority groups but not White patients.

insured sample and national store level retail data, that together offer novel insights into the impacts of school closures and reopenings on parental mental health.

3.1 Measure of in-person schooling disruptions

The school mobility measures are constructed using data from the cellphone aggregator, SafeGraph. This is an anonymized dataset that exploits cellphones' specific locations to create aggregate measures of visits to points of interest like restaurants, stadiums, hospitals, offices, among many others[5]. The data reports mobility patterns for approximately 10% of cellphones in the United States, and the sample closely follows the official U.S. population by states and counties recorded by the Census and it also exhibits a high correlation with respect to racial composition, education level and household income (Parolin and Lee, 2021).

In order to create a measure of in-person visits to schools at the zip code level, we first restrict the number of daily visits to elementary, middle and high schools across the U.S. between September of 2017 to December of 2021.The process yields mobility to over 120,000 schools on average by year (public and private overall). Using the names of the schools and geographic identifiers, such as state, county, zip code and census tract, we then overlay the resulting dataset with the directory of public schools made available by the Urban Institute, following Parolin and Lee (2021). According to the National Center for Education Statistics (NCES), in the academic year 2019-2020 there were 98,469 public schools in the U.S. (National Center for Education Statistics, 2023), we are able to match nearly 87% of schools to the SafeGraph sample.

From the NCES directory, we are able to identify and classify schools by type, as elementary, middle or high. Around 14% of schools do not have a unique category, but fall into all three types. Some of this could be due to the actual scope of the schools, while others may be simply not reported accurately. Our main results include the small share of

[5]These are total visits by day, week or month to places identified by their specific state, city, county, address and coordinates.

schools that may fall in multiple categories of elementary, middle and high school, but we present in appendix E a sensitivity analysis using a restricted sample that cleanly identifies institutions; our estimates are broadly robust to the exclusion of these schools.

Our main mobility measure captures relative changes at either month-by-5-digit zip code level to study changes in antidepressant use or at the month-by-county level to study changes in alcohol purchases, between September of 2017 and December of 2021. Given that not just the number of people visiting schools might change, but also the number of devices captured by SafeGraph in each state and month during the study period, we adjust mobility to schools by the number of cellphones captured in SafeGraph in each state and month. The monthly relative mobility to schools metric is computed as follows:

$$MobilityChange_{zt} = \frac{(Visits_{Sept.2019-Dec.2021}/Devices_{Sept.2019-Dec.2021})}{(Visits_{Sept.2017-Dec.2019}/Devices_{Sept.2017-Dec.2019})} - 1 \qquad (1)$$

A relative school mobility metric close to zero indicates equivalence between school mobility in the pandemic exposed/ treatment cohort and in the pre-pandemic unexposed/ control cohort, indicative of no pandemic change for in-person schooling. Relative school mobility metric values progressively lower than zero would indicate greater shortfalls in in-person learning in the exposed/ treatment cohort compared to the unexposed/control cohort, and indicate greater schooling disruptions.

Figure 1, Panel (a) presents the nationwide monthly rate of mobility for the exposed/ treatment arm and separately for the unexposed/ control arms (numerator and denominator of the first part of the relative school mobility metric in 1, respectively). We note that the rate of school mobility in most of 2021 closely tracks the rate of school mobility in 2019. However, when we compare rates of school mobility in March-December of 2020 to the same months in 2018, it becomes clear that school closures severely restricted in-person visits for most of 2020 and well until February of 2021. Panel (b) presents the nationwide, monthly, relative school mobility metric in equation 1. Notably, after March of 2020, mobility to schools decreased by almost 100% compared to the same months in 2018, corresponding to complete school closures. Between August of 2020 and January of 2021, the decrease still amounted to a 50% decline, until schools reopened and transitioned from virtual or hybrid modes to in-person teaching by March of 2021. After March 2021 the relative school mobility metric fluctuates around zero percent, indicating an almost full recovery to in-person schooling to pre-pandemic levels.

In order to determine how well the SafeGraph visits to schools points of interest capture the actual conditions of enrollment and in-person learning mode, we compared enrollment data from the COVID-19 School Data Hub (2022) (CSDH) and create a series of scatter and time series plots. We caveat that the CSDH data is reported only from August of 2020 to June of 2021. Appendix Figure A.1 shows reassuringly that both measures are strongly positively correlated (a coefficient of 0.84). Further, Figure A.2 indicates that the two series also trend similarly. These descriptive plots provide reassurance of our mobility variable of interest, as we are able to capture a significant amount of variation in visits to schools that reflect the actual policies implemented by districts and states after March of 2020.

3.2 Measures of Mental Health

Our outcomes of interest include two novel and objective proxy measures of mental health - (1) prescription antidepressant use, and (2) alcohol purchases.

3.2.1 Prescription Antidepressant Use

The main analysis incorporates de-identified medical claims from Optum's Clinformatics® Data Mart Database. This comprehensive claims database captures 20% of the commercially insured population nationwide, comprising approximately 15 – 20 million annual lives across all 50 U.S. states during the study period. The retail pharmacy prescription claims data for all enrollees from January 2016 through December 2021 provides for each prescription claim the hashed patient identifiers, patient demographic information (gender and age)[6], family identifier to allow linkage of household members with shared coverage, National Drug Code, and date of fill. We identify prescription dispensing of antidepressants using the relevant National Drug Codes (NDC), which we obtained from the U.S. Food and Drug Administration. Demographic data indicates that individuals represented are comparable to the US commercially insured population (Lee et al., 2021).

We used family identifiers to create a sample of potential mothers and fathers as follows. First, we limited the sample to those continuously enrolled, female or male (there are only binary indicators for gender in the data) between the ages of 18 and 50 after September of 2016, with their individual and family identifiers. Second, we overlay this sample of unique women and men with the dataset that contains insurance coverage information and dates of birth for the whole Optum enrollee population using family identifiers as the key variable. This leaves us with a household-level dataset attached to the initial sample of women and men from the first step. Third, we compute the ages of the potential mothers/fathers and the members of their household and then proceed to calculate the difference between both. Since we include parents above 18 years old, the rule is to drop those cases in which the age difference is below 18. This leaves us on average with a sample of over 430,000 potential mothers/fathers between 2016 and 2021.

Panels (a) and (b) of Figure A.3 plot the distribution of the number of children below 18 years old for our sample of mothers and fathers and the sample of parents between 18 and 50 years old from the American Community Surveys (ACS) in 2018 and 2019. As the ACS

[6] In Optum's Clinformatics® Data Mart Database individual level race is available only for data at the state level. Since in-person schooling disruptions varied considerably within states, considering more granular geographic variation in school closures at the 5-digit zip code level is critical to examine changes in parental mental health in response to pandemic induced schooling closures. Hence, we use the version of the Optum Clinformatics® Data Mart Database that provides 5-digit zip code for each enrollee, to create community level measures of antidepressant use by aggregating antidepressant claims to the 5-digit zip code level which we correlate with zip code level pandemic schooling disruptions. To examine whether and how schooling disruptions may have disproportionately impacted minoritized communities, we identified predominantly Black and/or Hispanic communities using 5-digit zip code racial/ethnic distribution from the ACS, which we correlated with zip code level antidepressant use and in-person schooling disruptions.

is representative of the US population, the close overlap between both histograms suggests that we are capturing a sample that is similar to what we would find in a set of randomly selected households across the country.

To first demonstrate the degree of representativeness at the national level, columns 1 and 2 of Table A.1 compare the demographic characteristics of the full sample of women between the ages of 18 and 50 from Optum, and a sample of women in the same age range from the 2019 National Health Interview Survey (NHIS). From columns 1 and 2 we note that Optum's sample of women of ages 18-50 is very similar in terms of race and income, even though there are more Hispanics in Optum and slightly less Asian women. Next, to facilitate an even closer comparison, columns 3 and 4 compares the sample of privately insured mothers from the NHIS and Optum. Again, both seem to be close to each other, although the latter has somewhat fewer Black mothers and more Hispanics.

3.2.2 Alcohol purchase

Our secondary proxy measure of mental health is alcohol purchases, merged with school closures at the county-month level and constructed using the Nielsen Retail Scanner Data. The Nielsen data are collected from 50,000 stores on average, covering over 50% the total sales volume of U.S. grocery and drug stores (food, drug, mass merchandise, convenience, and liquor retail channels). For each month between 2017 and 2020, we measure alcohol demand as the total number of purchases of alcoholic beverages (which are purchased by adults) that we aggregate by the smallest geography available, counties[7]. We use the administrative portion of Nielsen data which contain no demographic data. Nielsen also supplies a self-reported household level dataset with demographics that identify age of children in the household. As alcohol purchases are likely very underreported when relying on self reports (Livingston and Callinan, 2015), we do not use this database. In addition, grocery store data are by nature hard to assign to individuals within a household as typically one household

[7]These include all of the the subcategories within beer, liquor and wine.

member maybe purchasing for consumption by many, thus separating purchases by gender might be not meaningful as it is in prescription claims.

Unlike our measure of parental antidepressant use constructed from patient-level claims records, the disadvantage of using alcohol purchase data is that we lack demographic data such as parental status or gender, and we lack finer geographic identifiers than county. Thus, we are unable to identify purchases by parents, overall, or to distinguish between mothers and fathers, and can only consider county-level changes in alcohol purchases as a secondary proxy measure of mental health changes associated with school closures, after adjusting for other overall pandemic induced economic and health shocks (see description of controls for community pandemic unemployment and COVID-19 morbidity and mortality in the Methods section). On the other hand, one advantage of our analysis of county-level changes in alcohol purchases is that our analysis sheds light on changes in alcohol utilization in response to in-person schooling disruptions for not just the subset of commercially insured individuals, but all individuals in the county, irrespective of their insurance status. To the extent that the commercially insured population is relatively socioeconomically advantaged, and the less advantaged may be less able to privately supplement with services typically provided by schools during closures, we may find larger effects of school closures on mental health captured using county-level alcohol purchases. A second advantage is that our estimates of changes in prescription antidepressant utilization in response to school closures may be biased downwards or upwards, due to concurrent restrictions on access to non-COVID-19 related healthcare, which may limit parents' ability to access a provider for antidepressant prescriptions, or use non-pharmacological mental healthcare substitutes like psychotherapy, which mostly required in-person contact early in the pandemic. In contrast, grocery stores were one of the few businesses that remained open throughout the pandemic shutdowns. Thus, our results would shed light on whether parents dealing with pressures of school closures, and unable to access adequate mental healthcare, resorted to 'self-medication' with alcohol.

4 Methods

4.1 Exposed-treatment and unexposed-control cohorts

The empirical strategy exploits a cohort structure to estimate event studies and summarize the main effects in a difference-in-differences specification. We create two cohorts of exposed and unexposed parents, based on whether they are observed during the pandemic or pre-pandemic. In particular, for the antidepressant analysis the unexposed cohort is followed between September of 2016 until December of 2019, with a pre-period that ends in February of 2018, and the exposed cohort is followed between September of 2018 through December of 2021, with a pre-period that ends in February of 2020. The pre and post period in each cohort is marked by the 19th month since the beginning of each cohort, which is March of 2018 and March of 2020, respectively. This allows us to align both cohorts under a single event time window and compare the demand for antidepressants after the pandemic began to a similar sample of parents but after no interruption to mobility patterns took place.

Both cohorts include identified parents between the ages of 18 and 50 without any antidepressants prescriptions during the look-back months that we define as the first twelve months of the study period. This implies that for the unexposed cohort, we consider women and men without prescriptions before September of 2017 and for the exposed cohort, women and men who are antidepressant-naive before September of 2019. The structure of the dataset is a balanced panel of individuals that we observe each month during the whole period of analysis. The main outcome of interest is the cumulative use of antidepressants, which is an indicator equal to one starting the first month a mother or father is prescribed with an antidepressant. We then collapse the individual panel into cohort $\times$ event time $\times$ zip code cells, which yields the average cumulative rate of antidepressant use for a given cohort c

in month t and zip code z. Comparing monthly new-antidepressant prescription dispensation rates in the pandemic vs prepandemic cohorts (just shifted back in time), provide seasonally adjusted changes in new parental antidepressant use rates associated with the pandemic-induced school closures.

4.2 Identification of mental health effects of school closures

In order to motivate the research design, Figure 2 descriptively shows the relationship between pandemic school closures and increases in maternal prescription antidepressant dispensation during the pandemic. We divided zip codes into quartiles based on the extent of pandemic shortfalls in mobility to schools relative to the prepandemic period, as measured in equation (1), and plot the histogram of antidepressant prescription rate changes. We observe a monotonic relationship between declines in in-person schooling and increase in dispensed prescription antidepressants. In zip codes with the highest declines in in-person schooling, the rate of prescription antidepressant dispensations increased by almost 4.5% after school closures, while prescription antidepressant dispensations increased by only 0.5% in zip codes with the lowest rates of decrease in in-person schooling during the pandemic.

The cohort-level antidepressant data is complemented with the monthly relative changes in mobility to elementary, middle and high schools — which match the same exposed and unexposed months, as explained in section 3 — and with zip code level demographic indicators from the U.S. Census Bureau. We study how the growth rate of antidepressants use changes over time as a result of the pandemic and school closures in a fixed effect Poisson regression model. The event studies we estimate first use the aggregated measure of school mobility as follows (Model 1):

$$y_{zct} = \alpha + \sum_{m=-6, m\neq -1}^{22} \beta_m Exposed_c \times MobilityChange_{zt} + \gamma_z + \gamma_t + \gamma_c + \delta X_{jt} + \varepsilon_{zct} \quad (2)$$

where y_{zct} is the cumulative rate of antidepressants use in zip code z, cohort c and event time t. $Exposed_c$ is a binary variable equal to one for the exposed cohort and $MobilityChange_{zt}$ is the relative change in school mobility by zip code and month. This is the geographic-specific version of panel (b) in Figure 1. γ_z, γ_t and γ_c are zip code, event time and cohort fixed effects, respectively. X_{jt} are covariates at the county level j including the unemployment rate and COVID-19 related deaths[8]. In equation 2, event times $m < -1$ correspond to the pre-treatment periods, while $m \geq 0$ are part of the post period. The reference period, $m = -1$, is February. Overall, the pre-period covers 6 months (September-February) and the post-period 22 months (March-December a year and a half later). The main advantage of such a long post period is that we are able to measure short and medium-term effects, which could shed some light on long-term scarring effects of the pandemic on parental mental health. All specifications are weighted by the size of the geographic-cohort-time cells and standard errors are clustered at the zip code-cohort level.

To test for differential effects of in-person schooling disruptions by age of children, we estimate a second event study that takes the previous $MobilityChange$ variable and breaks it into three parts, as shown in the following model (Model 2):

$$
\begin{aligned}
y_{zct} = \alpha &+ \left(\sum_{m=-6, m\neq -1}^{22} \beta_m Exposed_c \times ElemMobilityChange_{zt} \right) \\
&+ \left(\sum_{m=-6, m\neq -1}^{22} \delta_m Exposed_c \times MiddleMobilityChange_{zt} \right) \\
&+ \left(\sum_{m=-6, m\neq -1}^{22} \tau_m Exposed_c \times HighMobilityChange_{zt} \right) \\
&+ \gamma_z + \gamma_t + \gamma_c + \delta X_{jt} + \epsilon_{zct}
\end{aligned} \tag{3}
$$

The outcome y_{zct}, exposure indicator $Exposed_c$ and fixed effects γ_z, γ_t, γ_c and X_{jt} are

[8] Appendix C demonstrates that removing the county-level controls yields similar estimates for all the main specifications.

just as explained above. The difference is we now include three interactions of the exposed dummy with three relative mobility changes by school type, one for visits to elementary schools, $ElemMobilityChange$, the second one for middle schools, $MiddleMobilityChange$ and the third one for high schools, $HighMobilityChange$. By estimating equation 3, we will be able to assess further which type of school closure affects parental mental health conditions the most.

In order to summarize the main effects presented in the event studies, we estimate the following specification:

$$\begin{aligned} y_{zct} = \alpha + &\beta_1(Exposed_c \times PrePandemic_t) + \beta_2(Exposed_c \times EarlyPandemic_t) \\ &+ \beta_3(Exposed_c \times LaterPandemic_t) + \gamma_z + \gamma_t + \gamma_c + \delta X_{jt} + \nu_{zct} \end{aligned} \tag{4}$$

In practice, equation 4 acts as a shorter event study that groups the set of event times into three periods: a pre-pandemic period, an early pandemic period, covering the first six months of the pandemic (March-August) and a later pandemic period, for the remainder of the post-treatment months. The reference period in this summarized version is still $m = -1$, which is February of 2020 for the exposed cohort and February of 2018 for the unexposed cohort.

In order to explore the possibility that people may engage in self-medication with alcohol in response to mental stress associated with school closures, similar to equation 2, we estimate the following:

$$y_{jct} = \alpha + \sum_{m=-6, m\neq -1}^{10} \beta_m Exposed_c \times MobilityChange_{jt} + \delta X_{jt} + \gamma_j + \gamma_t + \gamma_c + \epsilon_{jct} \tag{5}$$

where y_{jct} is the total units of alcohol purchased in county j by cohort c in month t, $MobilityChange_{jt}$ is the main relative change in mobility between the treated and control cohorts by county and X_{jt} are controls at the county level, which include the unemployment rate and COVID-19 deaths. Equation 5 is also estimated by a fixed effects Poisson regression, therefore, the reported coefficients are obtained by applying the expression $\exp(\hat{\beta}_m) - 1$.

5 Results

5.1 Prescription Antidepressant Use

Figure 3 presents the event study estimates for the aggregate number of schools from equation 2 separately for the sample of mothers (Panel (a)) and fathers (Panel (b)). Since we use a Poisson model, the coefficients displayed are obtained by applying the expression $\exp(\hat{\beta}_m)-1$, where $\hat{\beta}_m$ are the coefficients of interest in event time. As the results indicate, the exposed and unexposed cohorts of mothers and fathers were similar in terms of cumulative prescriptions of antidepressants (statistically no different than zero coefficients before the pandemic started in March of 2020; identifying assumption of parallel trends holds, marked in the Figure with a red vertical line). However, when schools closed and mobility restrictions were imposed, we see an increase in maternal antidepressant prescriptions starting around April of 2020 and decreasing to become a null effect again in October of the same year. Comparing the mobility patterns of Figure 1 with the estimated effects of Figure 3, the first key takeaway is that the demand of antidepressants for mothers between the ages of 18-50 increased significantly when schools closed in March of 2020. Secondly, the effect of school closures on maternal antidepressant prescription dispensations persisted for nearly seven months, starting in Spring 2020 when we observe significant shortfalls in in-person learning and until Fall 2020. Thereafter, as in-person mobility to schools gradually recovered to pre-pandemic levels, we find no significant deviations of pandemic maternal prescription antidepressant dispensations from pre-pandemic levels November 2020 onwards.

Figure 4 presents estimates from Model 2, which consider which type of school closures drive the estimated increase in new maternal antidepressant prescription use in response to school closures[9]. By a simple inspection, it can be seen that elementary school closures for the most part and high school closures just for the initial couple of months following pandemic onset, are the most relevant for the overall demand changes in antidepressants, with an increase in May and a persistent positive effect until September, when schools slowly reopened to 50% or more of their pre-pandemic capacity.

Table 1 panel (a) summarizes the main effects for mothers, grouped by early (March through August 2020; significant closure period) and later pandemic periods (September 2020 onwards; reopenings and later), corresponding to periods of significant changes in school mobility during the pandemic, relative to the prepandemic period. Column 1 shows the analogous form of Figure 3. The first row confirms that there are no differential pre-trends before March of 2020 (identifying assumption of parallel trends holds). The coefficient in the second row implies that during the early months of the declared health emergency, the new maternal prescription antidepressant dispensation to the exposed cohort increased by 1.5%. The third row estimate, for the later pandemic period (which covers late 2020 and 2021) displays an statistically significant 0.05% increase. Consistent with the event studies in Figure 4, column 2 shows a statistically significant 0.7% increase in new maternal prescription antidepressant dispensation effect at the 5% percent level from elementary schools, then, as in-person learning returned to schools during the later pandemic period, the incremental pandemic increase becomes zero again. Column 3 shows no effect from middle school closures, while column 4 exhibits a marginally significant effect from high schools, equivalent to 0.9%, only during the early pandemic period of near universal closures [10].

[9]We assign the corresponding school district mobility measure to a zip code if it does not have an elementary, middle or high school within its boundaries. To facilitate comparisons, the fully interacted event studies by type of school restrict the sample to zip codes with presence of at least one elementary, middle and high school, so the sample size including the mobility to all schools combined is restricted to be the same as in the fully interacted model.

[10]As mentioned before, the main results include all the elementary, middle and high schools from the NCES classification. When we restrict the sample to schools without a double or triple category (i.e. classified as elementary-middle, middle-high or elementary-middle-high), as shown in Table E.1, the estimates remain stable across all specifications, even though the sample is somewhat reduced.

The evidence for fathers is somewhat different. In contrast to mothers for whom we find significant increases in new maternal prescription antidepressant use associated with near complete school closures in spring and early fall of 2020, estimates for the sample of fathers presented in Figure 3, panel (b) and Table B.1 show no significant effect of in-person schooling restrictions on paternal prescription antidepressant use throughout the pandemic. Furthermore, Figure B.1, panels (a) through (c) further confirm that none of the elementary, middle and high school closures are associated with significant changes in antidepressant demand from fathers. These findings reiterate earlier evidence on the uneven distribution of childcare within the household, which may create a disproportionate burden and psychological distress for mothers in comparison to fathers (Yamamura and Tsustsui, 2021b; Ahammer et al., 2023; Blanden et al., 2021).

5.2 Alcohol Purchases

Figure 5 presents the main estimates of the event study design in equation 5. It can be seen that the demand for alcoholic beverages increases once schools close after March of 2020, with no noticeable pretrends, which strengthens the comparability of the exposed and unexposed cohorts before the pandemic. Panel (b) of Table 1 summarizes the main estimates into a pre-pandemic and early pandemic period. Model 1, which includes all schools, shows that the demand for alcohol increases by 2% during the first stages of the pandemic. We break the main school mobility measure into three categories for elementary, middle and high schools in Model 2, and the estimates suggest an increase in the units of alcohol purchased of 3.2% once elementary schools close, without any statistically significant effects for the closure of institutions categorized as middle or high.

Even though we cannot identify the individuals who go to the stores to purchase alcohol or their role in the household (i.e. mother or father) in the Nielsen dataset, it is imperative to highlight that the Nielsen results are aligned and support the main findings from the demand of antidepressants in terms of the effects of mobility restrictions to schools during the early pandemic phases. Moreover, both sets of estimates point to the role of elementary schools as the main source of variation, which is linked to the additional burden experienced by mothers of school-age children who may need more attention and care in comparison to older kids.

5.3 Other disparate effects of school closures

In order to explore the heterogeneity of the effects by race, we interact our main coefficients of interest in equation 4 with indicators of race/ethnicity above the state median by zip code. Panel (a) of Table 2 shows that women in predominantly Black and Asian zip codes, in columns 2 and 4, contribute to the overall changes analyzed above during the early phases of the pandemic, with increases in demand equivalent to 2.3% and 1.7%, respectively. Columns 1 and 3, on the other hand, suggest that predominantly White and Hispanic zip codes experience almost no change in their demand of antidepressants during the early pandemic period, with coefficients that are smaller in magnitude and marginally significant, and economically small effects during the post-pandemic period. The detailed summarized estimates and event studies linked to these results are provided in appendix D. Similar to the results for antidepressants, panel (b) of Table 2 shows that counties that are predominantly Black and Asian drive the main estimates of increased alcohol purchases associated with school closures, with increases in alcohol consumption of 2.9% and 1.8% respectively, as schools close during the first months after March of 2020.

5.4 Mothers with prevalent antidepressant use

School closures may not only impact mental health of parents without prior history of mental health conditions, but could result in further deterioration of mental health of parents already suffering from depression. Our main finding this far of significant increases in prescription antidepressant dissemination in response to in-person schooling disruptions has focused on prescription antidepressant-naive mothers with no antidepressant prescription claims in the year preceding the pre-period (September of 2017 for the unexposed cohort and September 2019 for the exposed cohort). To consider implications of school closures for mothers with prevalent prescription antidepressant use, defined as those with antidepressant prescription dispensations during the twelve month period preceding the pre-period, we considered two additional analysis. First, we explored the possibility of worsening maternal mental health due to in-person schooling disruptions by estimating specification 4 for the sample of mothers with prior antidepressant use histories and considering changes in their cumulative antidepressant use. Results presented in Table E.2, column 1 suggest that school closures are not associated with significant changes in prescription antidepressant use by mothers who were already using them. The second set of analysis considers for the sample of mothers with prevalent antidepressant use, changes in cumulative consumption rate of 3 additional classes of mental health related drugs - benzos, Z-drugs or barbiturates - that are frequently co-used to alleviate nervous system activity and treat anxiety, epilepsy and insomnia. Results presented in Table E.2, column 2 show that among the sample of previous antidepressant users, the demand for these frequently co-prescribed mental health drugs also remained unchanged. Despite no evidence of significant worsening of mental health associated with school closures among those with prevalent antidepressant use, these analysis are of importance, as they may reflect that those with better access to healthcare/treatment (established patients already receiving care) may have been better able to cope with mental health stressors during the public health crisis.

5.5 Placebo tests

Under the self-medication hypothesis, sales of alcohol increase as a way to cope with higher psychological burdens, which in this case are originated in the COVID-19 school closures.

Some other products not related to alcoholic beverages, that are instead of everyday need, might not have been purchased at higher rates with the pandemic, and more importantly, not affected by the restrictions to school mobility. With this idea in mind, Table E.3 reports the results of a placebo exercise in which we estimate specification 5 but using as dependent variables the total demand of feminine hygiene products (column 1) and baby care products[11] (column 2) by county and month. The coefficients show no evidence of changes in the demand of these products during the pre- or post-pandemic periods, which strengthens the claim that school closures did affect the demand of certain types of products, but did not modify the acquisition of some products that are thought to be of regular use within households, likely not impacted by restricting the mobility to schools.

6 Discussion & Conclusion

The results presented in this paper for a large sample of commercially insured mothers and fathers in the US, indicate that COVID-19 induced reductions in in-person learning were associated with significant deterioration of parental mental health, reflected in increased prescription antidepressant dispensations, particularly to mothers of elementary school aged-children, during the initial months of the pandemic when in-person instruction declined to less than 50% of typical pre-pandemic levels. Reinforcing our findings of increased antidepressant use associated with schooling disruption, we also find significant increase in alcohol purchases in response to in-person schooling being replaced by hybrid and virtual modalities, which we interpret as evidence of self-medication due to worsening parental mental health. We document that parents, and particularly mothers, of elementary school aged children in racial/ethnic minoritized communities, specially in predominantly Black and Asian geographies, are the most affected by the low levels of school openness, suggesting that they rely

[11] Feminine hygiene products include douches, deodorants and towelettes products. Baby care products include lotions, powder, oils, ointments and bath items.

more heavily on services provided by schools as a form of childcare for younger children.

Similar to the evidence presented in Jo Blanden et al. (2021) for the UK, and Kishida et al. (2021) and Yamamura and Tsustsui (2021b) for Japan, we find that school closures due to the COVID-19 pandemic led to a substantial change in working and schooling conditions that ultimately meant parents had to take care of their children at home, increasing their childcare burden extensively. Also in line with Jo Blanden et al. (2021), we find that mothers living in zip codes where schools opened sooner (in the UK these were children prioritized to return to school) had better mental health. In our study, as schools transition from virtual or hybrid learning modes back to in-person classes, the effects disappear.

Our results point to heterogeneous effects for mothers versus fathers. While the estimates for the former were significant across several school types, they were null for the latter. This is directly related to how parents divide homeschooling and childcare tasks, which leads to marked gaps in mental health inequality amplified by a worldwide health emergency. As Yamamura and Tsustsui (2021b) establishes, fathers might be less involved in childcare responsibilities and were more likely to go to workplaces than mothers during school closures. Moreover, since mothers were more likely to stop working than fathers, the pandemic may have at least temporarily undone some of the gains from increased female labor force participation in the past decades and revived gender-based specialization within households. Fathers focused more on paid work, while mothers took on more responsibility for unpaid work at home (Andrew et al., 2020), and thus were more affected by school closures. Our research results also speak to a potential heterogeneous effect that might exist in higher vs lower SES communities. To some extent, "self-medication" and formal medication may be substitutes or complements (Darden and Papageorge, 2024), thus this is another literature to which our work relates.

Overall, our finding of immediate impacts on proxies for mental health suggests that the school system plays an important role in maintaining population mental health outcomes and in helping families cope with stress, but that detectable impacts appear only short term, as schooling returned to in-person.

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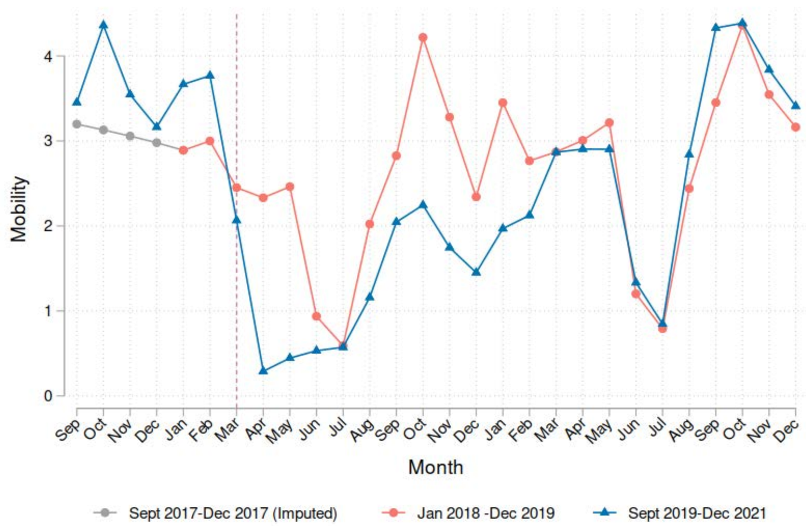
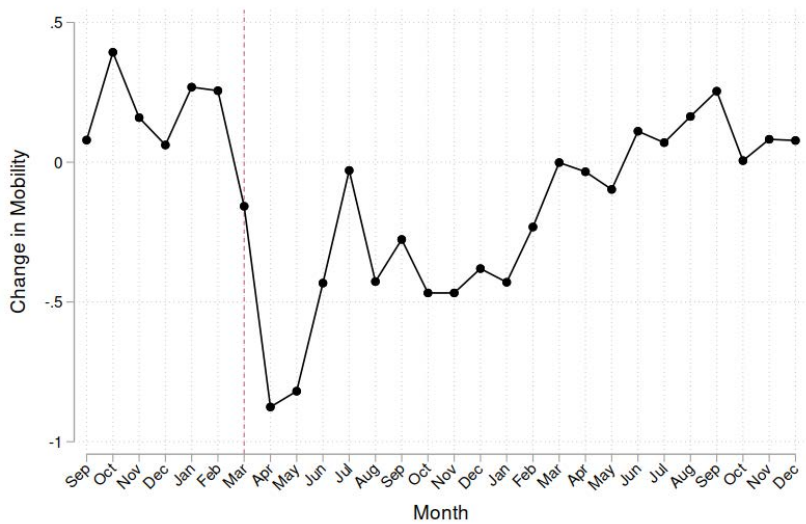
Tables and Figures

Figure 1: School mobility

(a) Visits to schools

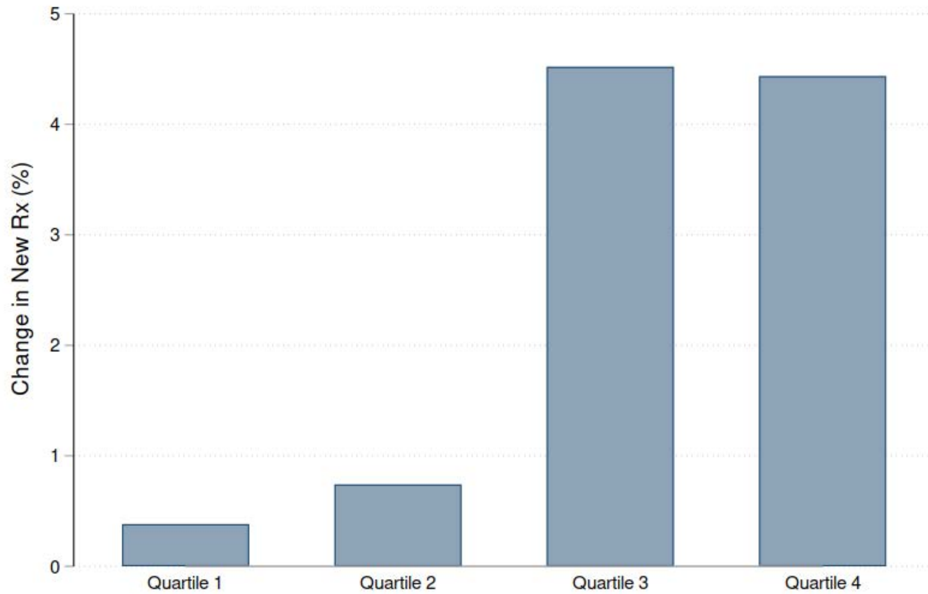


(b) Normalized change



Notes: This figure shows in Panel (a) the normalized measure of school mobility separately for the exposed and unexposed cohorts. The last four months of 2017 were imputed to complete the time series. Panel (b) shows the total monthly change in school mobility between the exposed and unexposed cohorts over time. The red dotted line marks March of 2020 versus March of 2018.

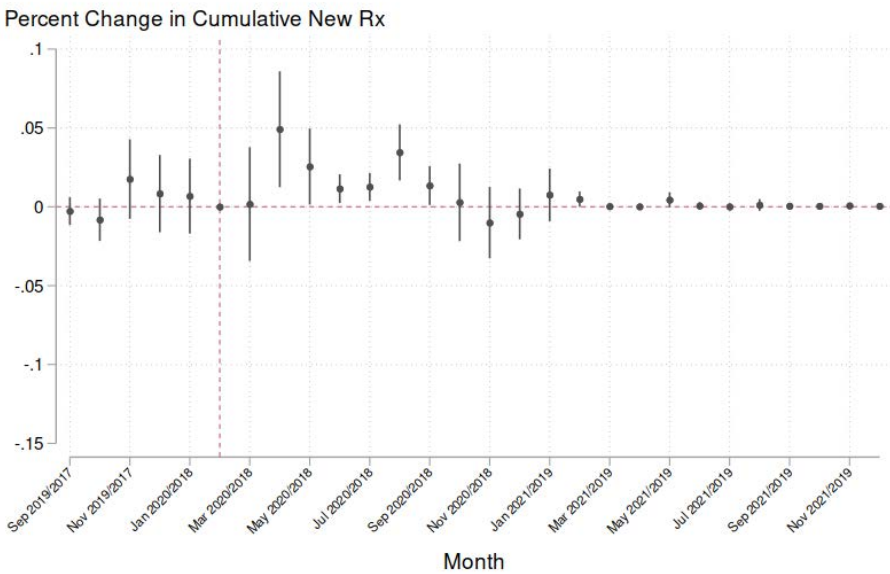
Figure 2: Change in Rx by Quartiles of School Closures



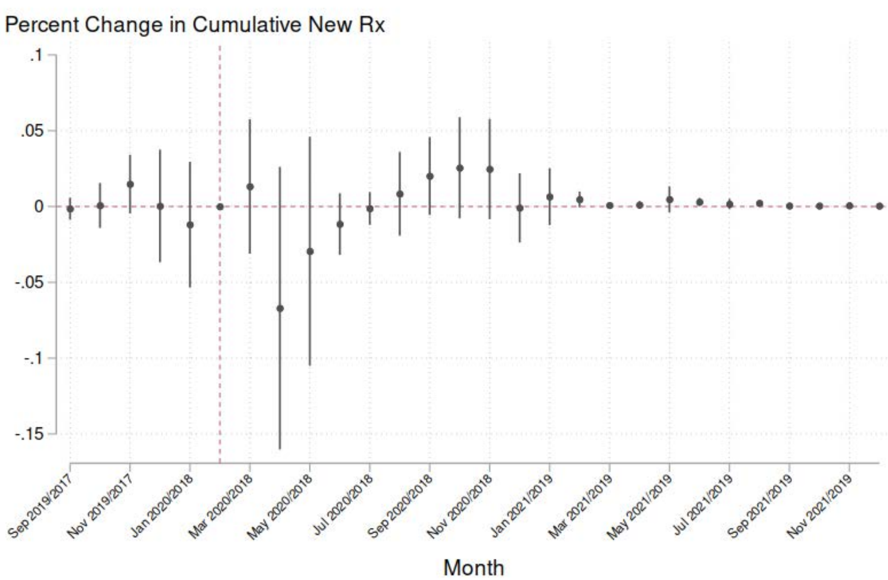
Notes: This figure shows the percent change in antidepressant prescriptions between the exposed and unexposed cohorts of mothers during the post-pandemic period by quartiles of school closure, where Q1 is the less closed and Q4 is the category with the highest level of closures in 2020-2021 with respect to 2018-2019.

Figure 3: Antidepressant event study estimates, all schools

(a) Mothers



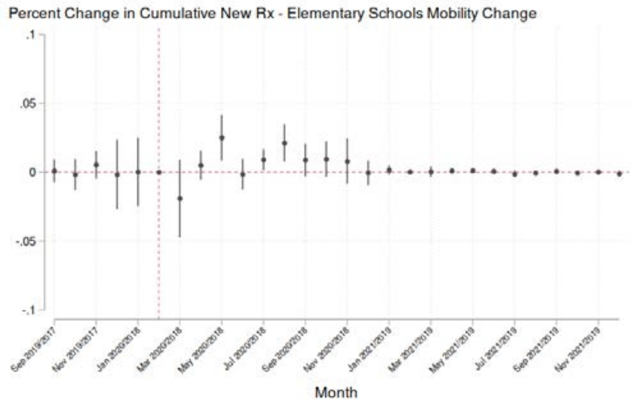
(b) Fathers



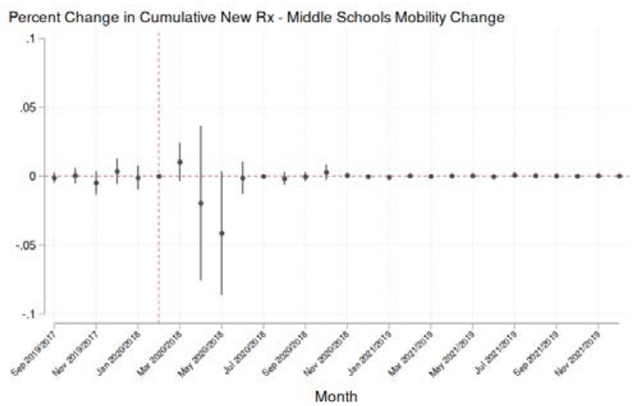
Notes: This figure shows the percent changes in cumulative new antidepressant demand from the event studies by month for mothers (panel a) and fathers (panel b). The unexposed cohort is mothers and fathers followed between September of 2017 and December of 2019. The exposed cohort is mothers and fathers followed between September of 2019 and December of 2021. The reference period for the exposed cohort is February 2020 and for the unexposed cohort is February 2018. Plotted percent changes are the exponentiated event study coefficients minus one. Standard errors are clustered at the zip-cohort level.

Figure 4: Antidepressant event study estimates for mothers, by school type

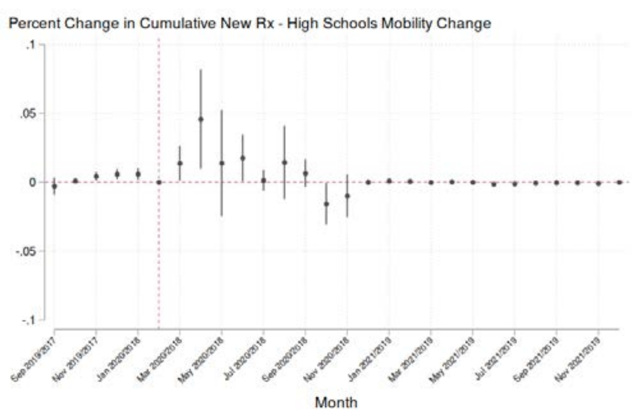
(a) Elementary



(b) Middle

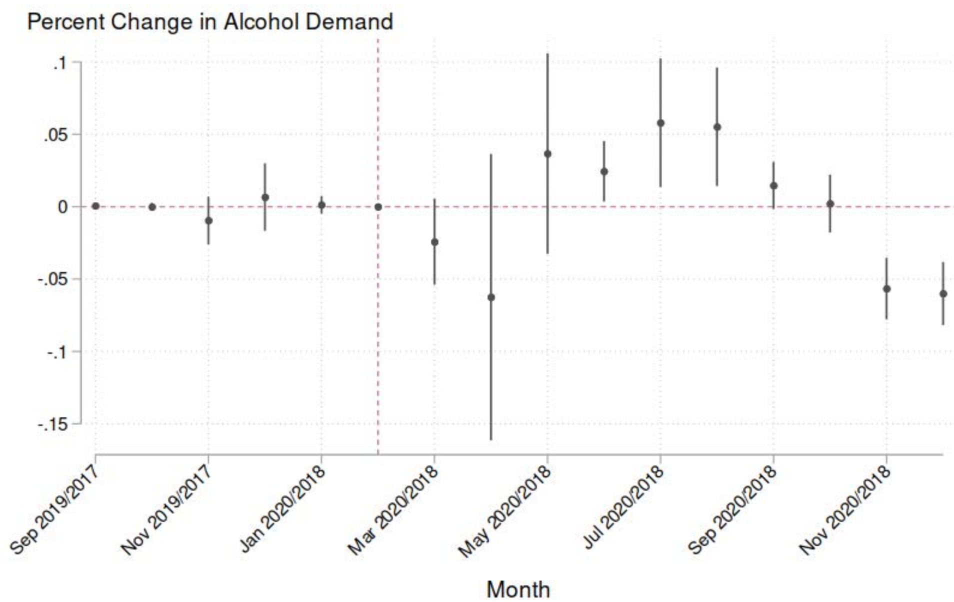


(c) High



Notes: This figure shows the percent changes in cumulative new antidepressant demand from the event studies by month for mothers in zip codes with elementary, middle and high school closures. The unexposed cohort is mothers followed between September of 2017 and December of 2019. The exposed cohort is mothers followed between September of 2019 and December of 2021. The reference period for the exposed cohort is February 2020 and for the unexposed cohort is February 2018. Plotted percent changes are the exponentiated event study coefficients minus one. Standard errors are clustered at the zip-cohort level.

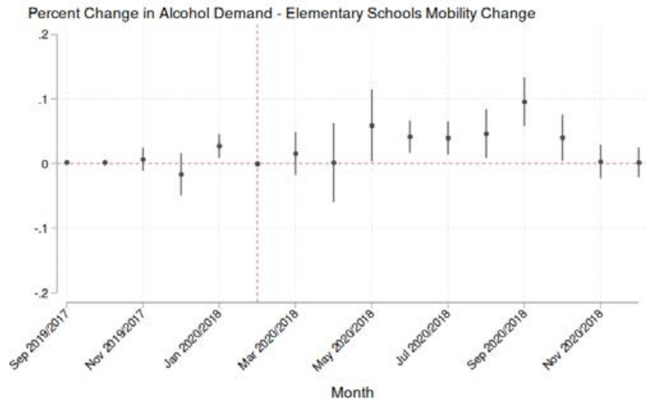
Figure 5: Alcohol demand event study estimates, all schools



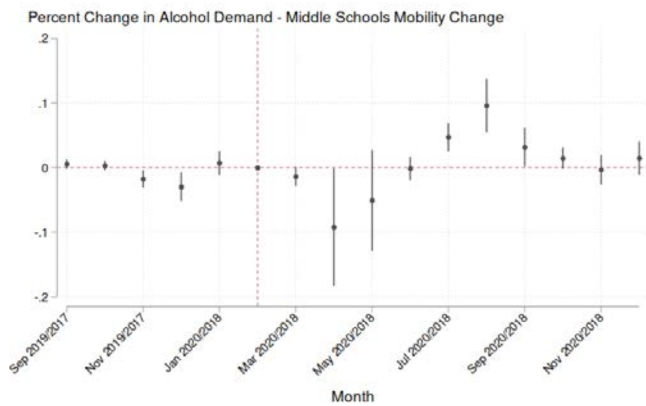
Notes: This figure shows the percent changes in alcohol demand from the event studies by month and county. The unexposed cohort is counties followed between September of 2017 and December of 2018. The exposed cohort is counties followed between September of 2019 and December of 2020. Controls include the monthly unemployment rate and the number of COVID-19 deaths by county. Standard errors are clustered at the county-cohort level.

Figure 6: Alcohol demand event study estimates, by school type

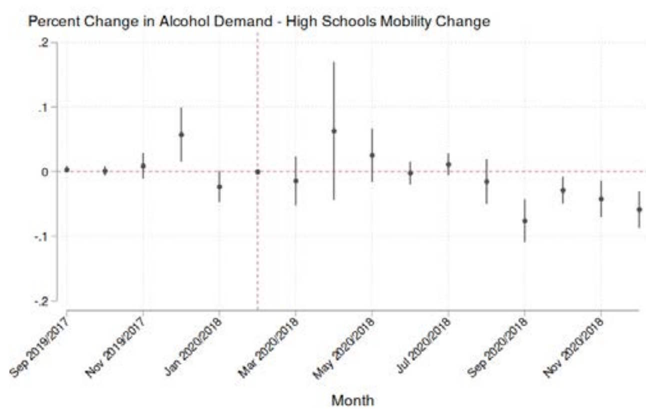
(a) Elementary



(b) Middle



(c) High



Notes: This figure shows the percent changes in alcohol demand from the event studies by month, county and school type. The unexposed cohort is counties followed between September of 2017 and December of 2018. The exposed cohort is counties followed between September of 2019 and December of 2020. Controls include the monthly unemployment rate and the number of COVID-19 deaths by county. Standard errors are clustered at the county-cohort level.

Table 1: Summary of main effects by type of school

	(1)	(2)	(3)	(4)
	Model 1	*Model 2*		
	All Schools	Elementary	Middle	High
	Panel (a): Antidepressants			
Pre-Pandemic Period	-0.0021	0.0007	-0.0006	0.0010
	[0.0042]	[0.0042]	[0.0016]	[0.0008]
Early Pandemic Period	0.0146***	0.0068**	-0.0003	0.0094*
	[0.0039]	[0.0033]	[0.0012]	[0.0048]
Later Pandemic Period	0.0005**	0.0002	0.0003***	-0.0002
	[0.0002]	[0.0006]	[0.0001]	[0.0001]
Observations	738121	738121	738121	738121
	Panel (b): Alcohol			
Pre-Pandemic Period	0.0006	0.0021	0.0042	0.0032
	[0.0005]	[0.0017]	[0.0035]	[0.0026]
Early Pandemic Period	0.0200***	0.0320***	0.0089	-0.0051
	[0.0074]	[0.0113]	[0.0061]	[0.0054]
Observations	78980	78980	78980	78980
Controls	Yes	Yes	Yes	Yes

Notes: This table reports the main effects grouped by event time and type of school. The reference period for the exposed cohort is February 2020 and for the unexposed cohort is February 2018. In panel (a), the dependent variable is the cumulative use of antidepressants and in panel (b) the total demand of alcoholic beverages. The percent changes in each column are the exponentiated coefficients minus one. Controls include the monthly unemployment rate and COVID-19 related deaths at the county level. Standard errors in brackets are clustered at the zip-cohort level in panel (a) and at the county-cohort level in panel (b). *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Summary of main effects by race

	(1) White	(2) Black	(3) Hispanic	(4) Asian
	Panel (a): Antidepressants Model 1			
Pre-Pandemic Period	-0.0052	-0.0026	-0.0051	-0.0073
	[0.0064]	[0.0085]	[0.0096]	[0.0085]
Early Pandemic Period	0.0104*	0.0231***	0.0119*	0.0166***
	[0.0057]	[0.0078]	[0.0062]	[0.0064]
Later Pandemic Period	0.0006**	0.0008	0.0007***	0.0007***
	[0.0002]	[0.0041]	[0.0001]	[0.0001]
Observations	330359	402886	401182	424760
	Panel (b): Alcohol Model 1			
Pre-Pandemic Period	0.0115	0.0004	0.0006	0.0005
	[0.0092]	[0.0003]	[0.0004]	[0.0004]
Early Pandemic Period	0.0260	0.0286***	0.0111	0.0175**
	[0.0240]	[0.0096]	[0.0087]	[0.0084]
Observations	38394	39072	39386	37350
Controls	Yes	Yes	Yes	Yes

Notes: This table reports the main effects grouped by event time and race, from the panel of mothers in zip codes in panel (a) and counties in panel (b) where each race group exceeds the state median. The reference period for the exposed cohort is February 2020 and for the unexposed cohort is February 2018. The percent changes in each column are the exponentiated coefficients minus one. Controls include the monthly unemployment rate and COVID-19 related deaths at the county level. Standard errors in brackets are clustered at the zip-cohort level in panel (a) and at the county-cohort level in panel (b). *** p<0.01, ** p<0.05, * p<0.1.

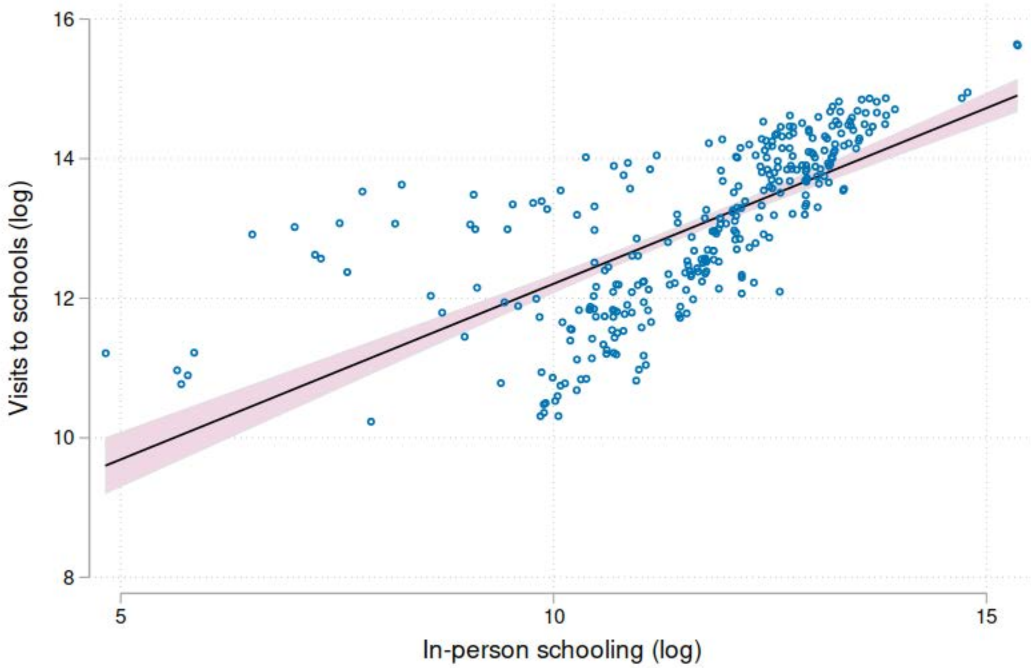
ONLINE APPENDIX

SCHOOL CLOSURES AND PARENTAL MENTAL HEALTH

Sumedha Gupta, Dario Salcedo and Kosali Simon

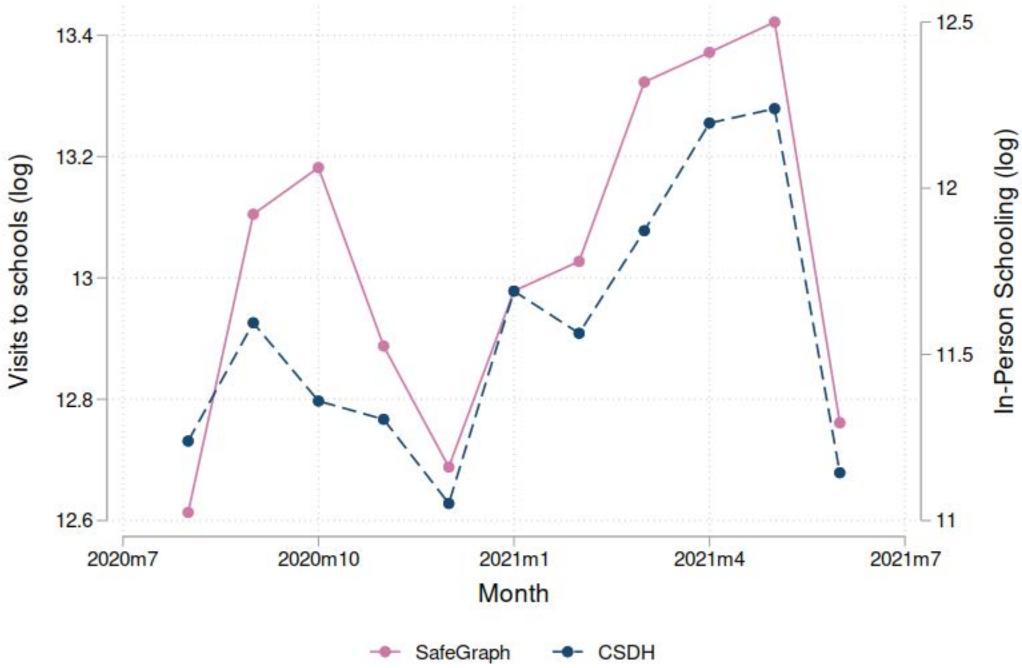
A Comparability to other datasets

Figure A.1: SafeGraph and CSDH state mobility correlation



Notes: This figure shows the scatterplot between the visits to school from SafeGraph and in-person schooling from the CSDH (in logs) at the state level. These were computed using school-level data, which were then collapsed by state and month. The CSDH measure corresponds to total school enrollment weighted by the share of time a school reports being in-person between August of 2020 and June of 2021.

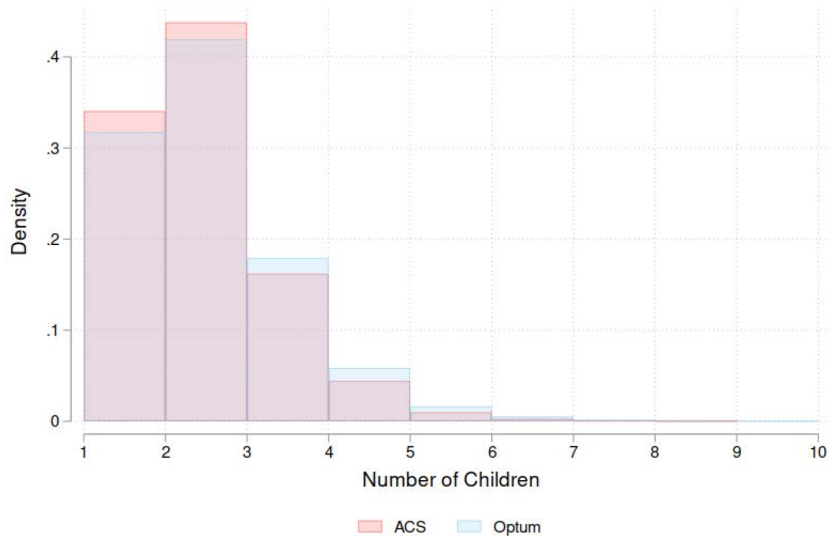
Figure A.2: SafeGraph and CSDH time series



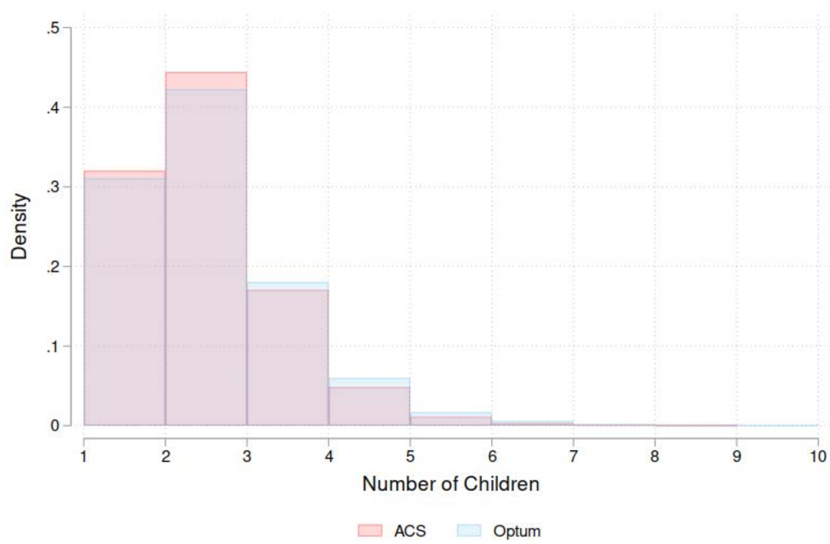
Notes: This figure shows the monthly visits to schools from SafeGraph and in-person schooling from the CSDH (in logs). These were computed using school-level data, which were then collapsed by month. The CSDH measure corresponds to total school enrollment weighted by the share of time a school reports being in-person between August of 2020 and June of 2021.

Figure A.3: Sample of mothers and fathers - ACS and Optum

(a) Mothers



(b) Fathers



Notes: This figure shows the histograms for the number of children under 18 of the ACS 2018-2019 sample of mothers in panel (a) and fathers in panel (b) (red bars) and number of children under 18 of the sample from Optum (blue bars).

Table A.1: Demographic characteristics, Optum and NHIS

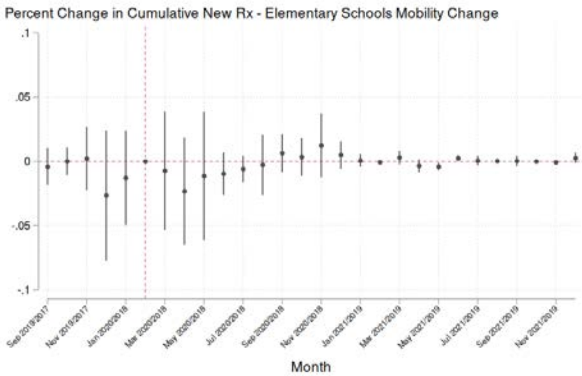
	Optum %	**NHIS % (Private)**	**Optum %**	**NHIS % (Private)**
White	66.250	64.812	66.444	64.278
Black	11.137	12.204	9.341	12.061
Asian	7.080	9.255	8.302	10.136
Hispanic	15.533	10.867	15.913	11.432
HH Income $\leq$ 74K	40.732	40.460	.	.
Identified Mothers	No	No	Yes	Yes

Notes: This table shows several demographic characteristics for the sample of all women between the ages of 18 and 50 in the first two columns, for Optum and the 2019 National Health Interview Survey (NHIS), respectively, and the sample of mothers identified in Optum, in column 3, and the sample of identified mothers from the NHIS, in column 4. Columns 2 and 4 include women covered by private health insurance.

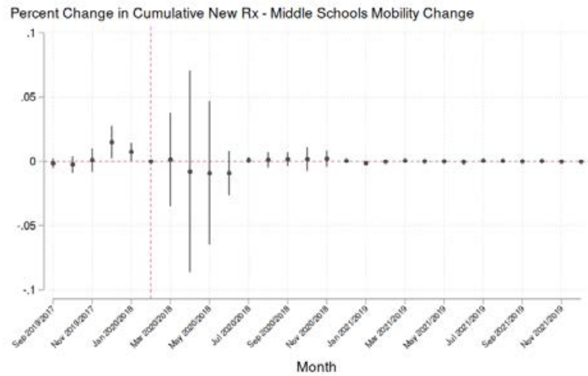
B Antidepressant use for fathers by school type

Figure B.1: Antidepressant event study estimates for fathers by school type

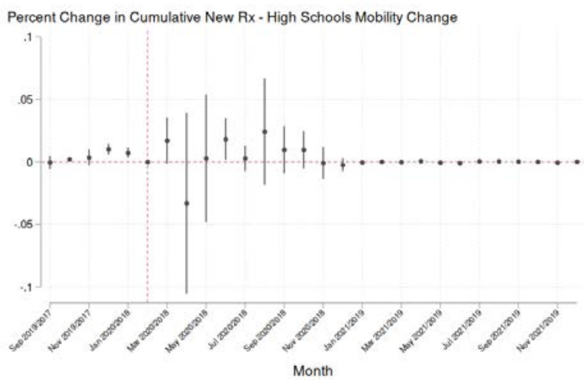
(a) Elementary



(b) Middle



(c) High Schools



Notes: This figure shows the percent changes in cumulative new antidepressant demand from the event studies by month and school type for the sample of fathers. The unexposed cohort is fathers followed between September of 2017 and December of 2019. The exposed cohort is fathers followed between September of 2019 and December of 2021. The reference period for the exposed cohort is February 2020 and for the unexposed cohort is February 2018. Plotted percent changes are the exponentiated event study coefficients minus one. Standard errors are clustered at the zip-cohort level.

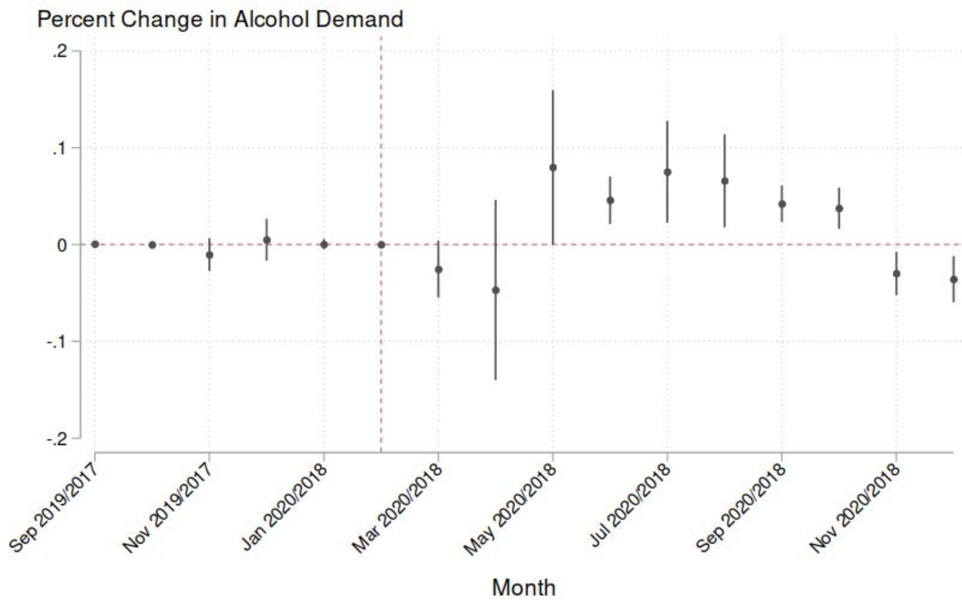
Table B.1: Antidepressant demand, summary of main effects for fathers

	(1) All Schools	(2) Elementary	(3) Middle	(4) High
Pre-Pandemic Period	-0.0003	-0.0023	-0.0004	0.0022**
	[0.0025]	[0.0063]	[0.0018]	[0.0011]
Early Pandemic Period	-0.0010	-0.0061	0.0006	0.0106
	[0.0066]	[0.0055]	[0.0016]	[0.0066]
Later Pandemic Period	0.0008*	-0.0002	0.0001	0.0002
	[0.0004]	[0.0008]	[0.0001]	[0.0002]
Observations	622827	622827	622827	622827

Notes: This table reports the main effects grouped by event time and school type for the sample of fathers. The reference period for the exposed cohort is February 2020 and for the unexposed cohort is February 2018. The percent changes in each column are the exponentiated coefficients minus one. Standard errors in brackets are clustered at the zip-cohort level. *** p<0.01, ** p<0.05, * p<0.1.

C Estimates without controls

Figure C.1: Event study estimates, Alcohol demand



Notes: This figure shows the percent changes in alcohol demand from the event studies by month and county, without controls. The unexposed cohort is counties followed between September of 2017 and December of 2018. The exposed cohort is counties followed between September of 2019 and December of 2020. Standard errors are clustered at the county-cohort level.

Table C.1: Summary of main effects by type of school

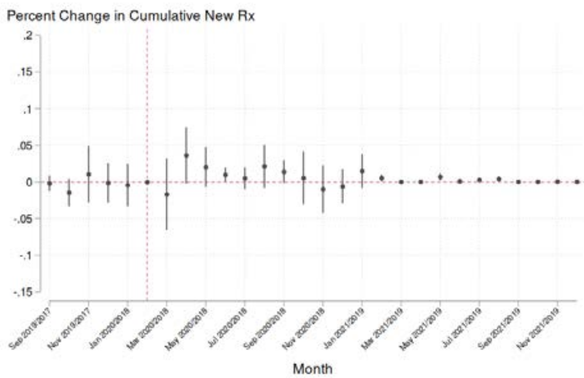
	(1) *Model 1*	(2)	(3) *Model 2*	(4)
	All Schools	Elementary	Middle	High
	Panel (a): Antidepressants (Mothers)			
Pre-Pandemic Period	-0.0020	0.0007	-0.0006	0.0010
	[0.0042]	[0.0042]	[0.0016]	[0.0008]
Early Pandemic Period	0.0133***	0.0062*	-0.0004	0.0084*
	[0.0040]	[0.0033]	[0.0012]	[0.0049]
Later Pandemic Period	0.0005**	0.0001	0.0002***	-0.0002
	[0.0002]	[0.0006]	[0.0001]	[0.0001]
Observations	738121	738121	738121	738121
	Panel (b): Antidepressants (Fathers)			
Pre-Pandemic Period	-0.0003	-0.0023	-0.0004	0.0022**
	[0.0026]	[0.0064]	[0.0018]	[0.0011]
Early Pandemic Period	-0.0011	-0.0063	0.0006	0.0105
	[0.0067]	[0.0055]	[0.0016]	[0.0066]
Later Pandemic Period	0.0008*	-0.0001	0.0001	0.0002
	[0.0005]	[0.0008]	[0.0001]	[0.0002]
Observations	622883	622883	622883	622883
	Panel (c): Alcohol			
Pre-Pandemic Period	0.0006	0.0022	0.0045	0.0037
	[0.0005]	[0.0017]	[0.0037]	[0.0030]
Early Pandemic Period	0.0433***	0.0417***	0.0140**	0.0044
	[0.0093]	[0.0131]	[0.0069]	[0.0064]
Observations	78980	78980	78980	78980
Controls	No	No	No	No

Notes: This table reports the main effects grouped by event time and type of school. The reference period for the exposed cohort is February 2020 and for the unexposed cohort is February 2018. In panels (a) and (b), the dependent variable is the cumulative use of antidepressants and in panel (c) the total demand of alcoholic beverages. The percent changes in each column are the exponentiated coefficients minus one. Standard errors in brackets are clustered at the zip-cohort level in panels (a)-(b) and at the county-cohort level in panel (c). *** p<0.01, ** p<0.05, * p<0.1.

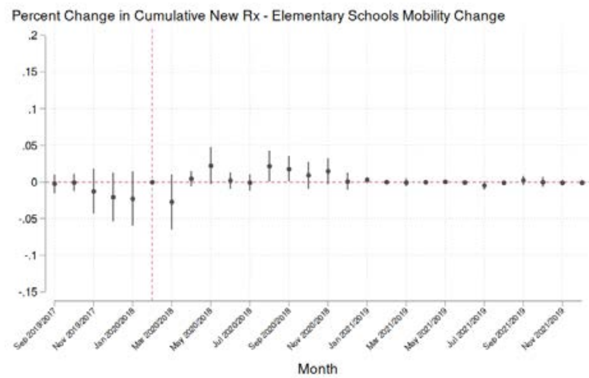
D Heterogeneity Analysis

Figure D.1: Antidepressant event study estimates for mothers in predominantly White zip codes

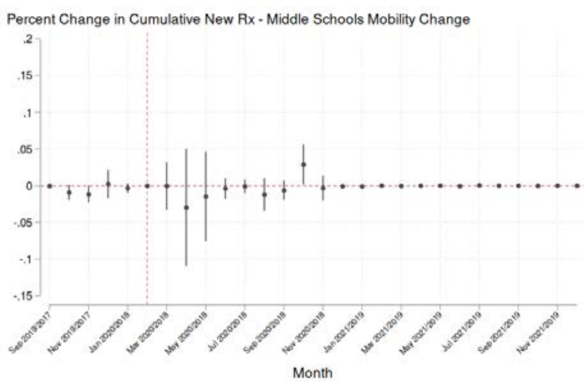
(a) All Schools



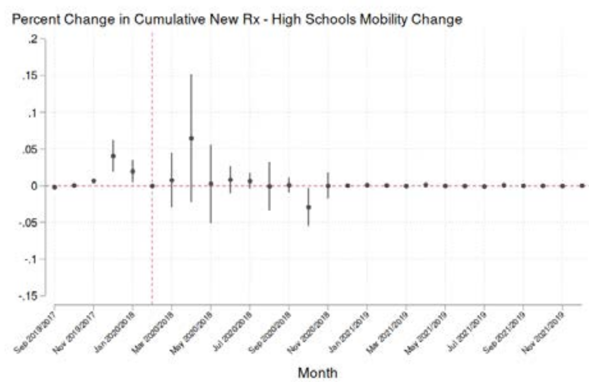
(b) Elementary



(c) Middle



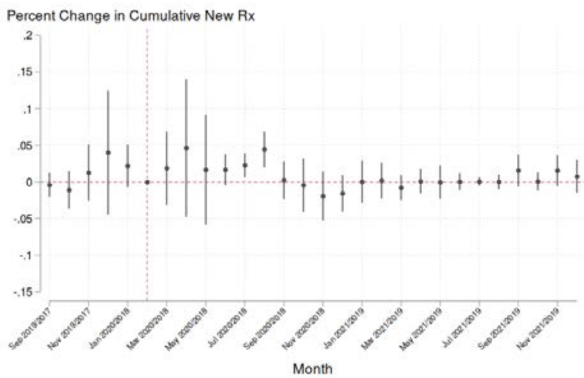
(d) High Schools



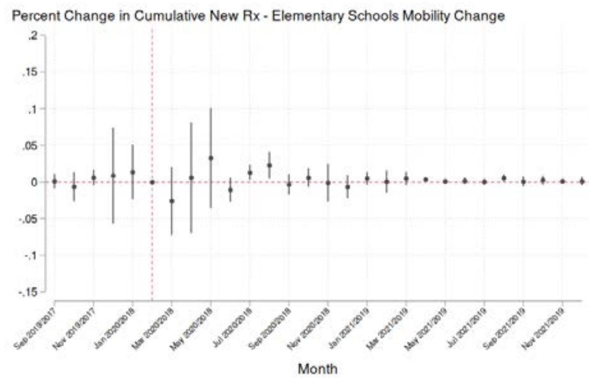
Notes: This figure shows the percent changes in cumulative new antidepressant demand from the event studies by month and school type for mothers in predominantly White zip codes. The unexposed cohort is mothers followed between September of 2017 and December of 2019. The exposed cohort is mothers followed between September of 2019 and December of 2021. The reference period for the exposed cohort is February 2020 and for the unexposed cohort is February 2018. Plotted percent changes are the exponentiated event study coefficients minus one. Standard errors are clustered at the zip-cohort level.

Figure D.2: Antidepressant event study estimates for mothers in predominantly Black zip codes

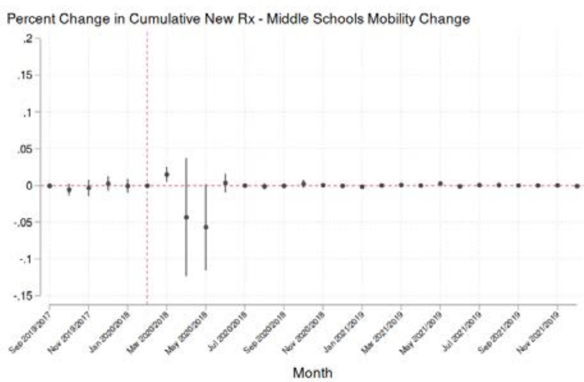
(a) All Schools



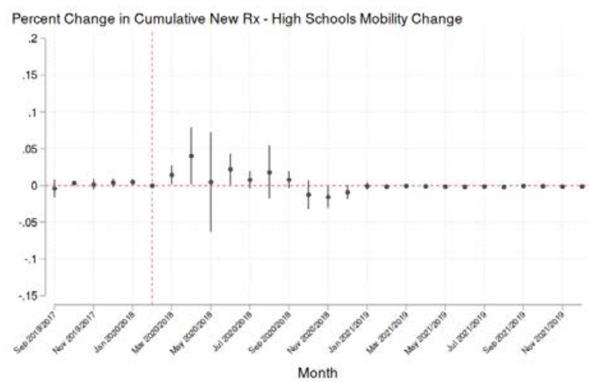
(b) Elementary



(c) Middle



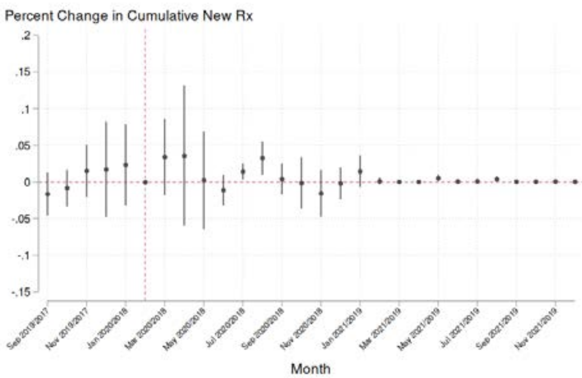
(d) High Schools



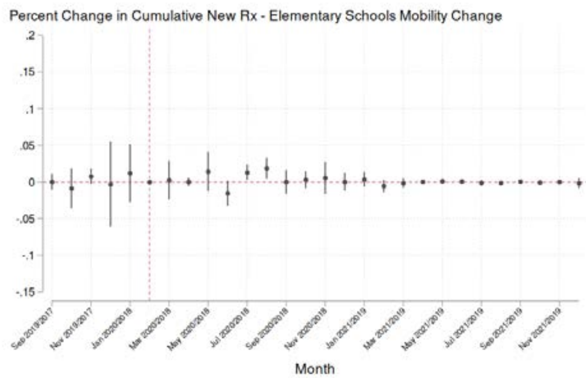
Notes: This figure shows the percent changes in cumulative new antidepressant demand from the event studies by month and school type for mothers in predominantly Black zip codes. The unexposed cohort is mothers followed between September of 2017 and December of 2019. The exposed cohort is mothers followed between September of 2019 and December of 2021. The reference period for the exposed cohort is February 2020 and for the unexposed cohort is February 2018. Plotted percent changes are the exponentiated event study coefficients minus one. Standard errors are clustered at the zip-cohort level.

Figure D.3: Antidepressant event study estimates for mothers in predominantly Hispanic zip codes

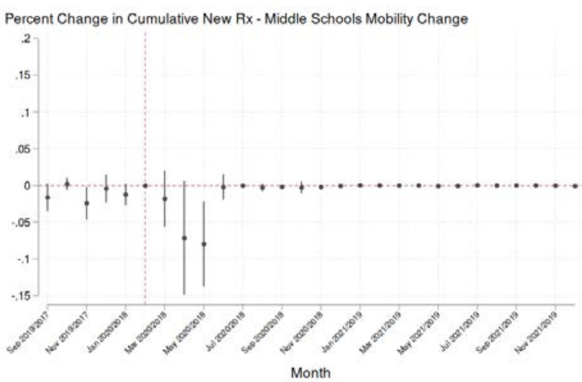
(a) All Schools



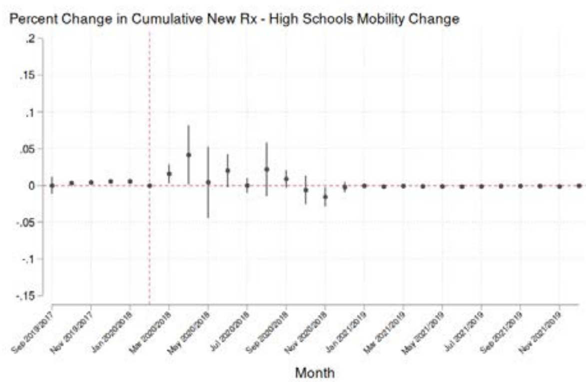
(b) Elementary



(c) Middle



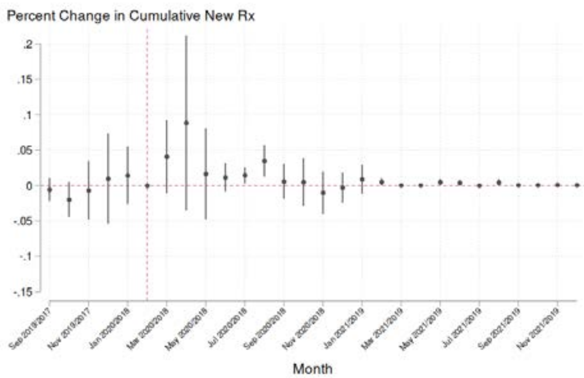
(d) High Schools



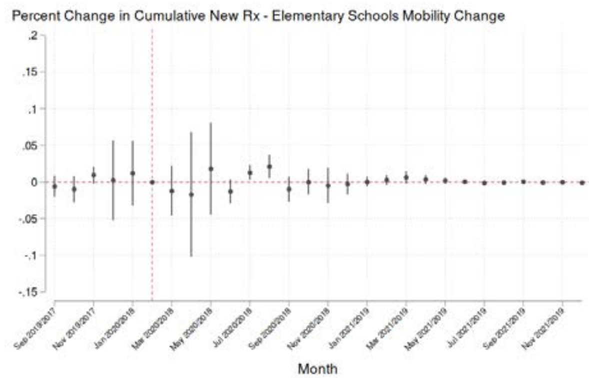
Notes: This figure shows the percent changes in cumulative new antidepressant demand from the event studies by month and school type for mothers in predominantly Hispanic zip codes. The unexposed cohort is mothers followed between September of 2017 and December of 2019. The exposed cohort is mothers followed between September of 2019 and December of 2021. The reference period for the exposed cohort is February 2020 and for the unexposed cohort is February 2018. Plotted percent changes are the exponentiated event study coefficients minus one. Standard errors are clustered at the zip-cohort level.

Figure D.4: Antidepressant event study estimates for mothers in predominantly Asian zip codes

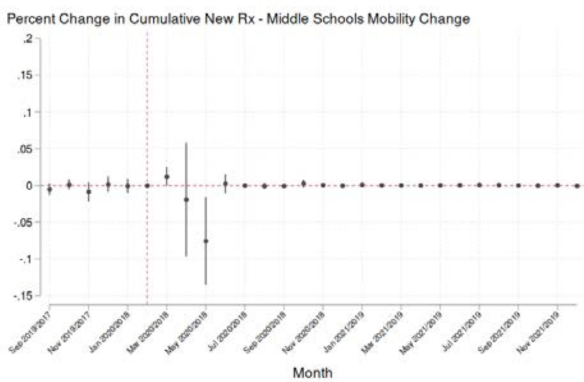
(a) All Schools



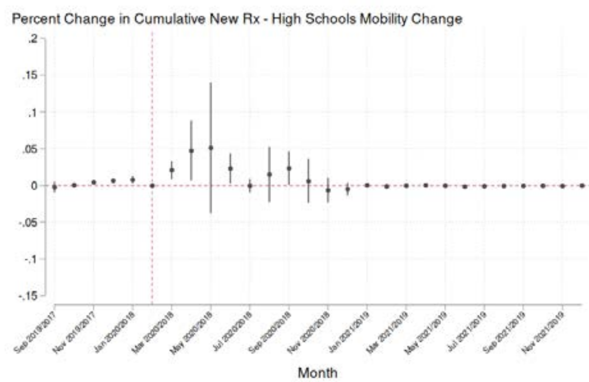
(b) Elementary



(c) Middle



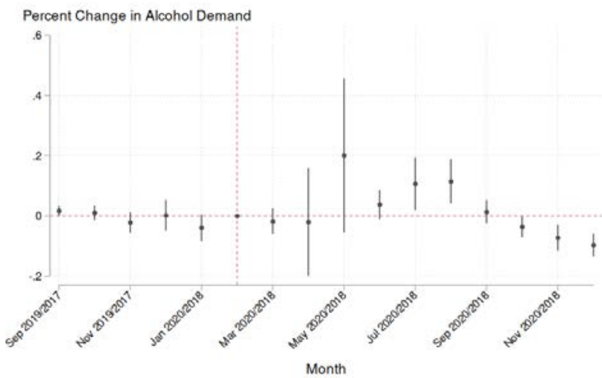
(d) High Schools



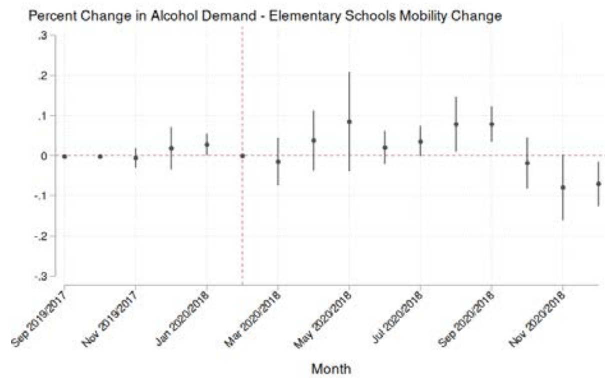
Notes: This figure shows the percent changes in cumulative new antidepressant demand from the event studies by month and school type for mothers in predominantly Asian zip codes. The unexposed cohort is mothers followed between September of 2017 and December of 2019. The exposed cohort is mothers followed between September of 2019 and December of 2021. The reference period for the exposed cohort is February 2020 and for the unexposed cohort is February 2018. Plotted percent changes are the exponentiated event study coefficients minus one. Standard errors are clustered at the zip-cohort level.

Figure D.5: Alcohol demand by school type in predominantly White counties

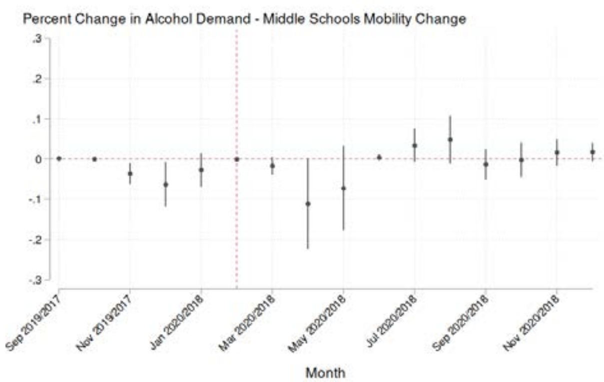
(a) All Schools



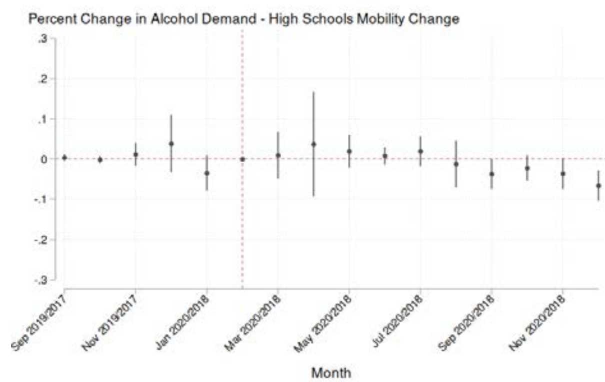
(b) Elementary



(c) Middle



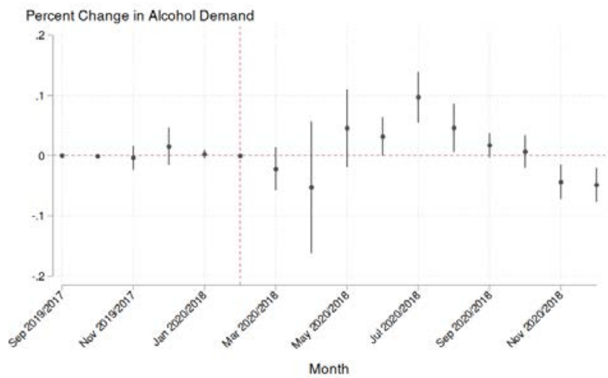
(d) High Schools



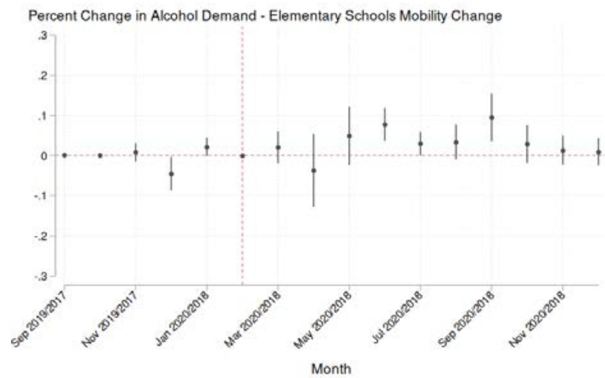
Notes: This figure shows the percent changes in alcohol demand from the event studies by month and school type in predominantly White counties. The unexposed cohort is counties followed between September of 2017 and December of 2018. The exposed cohort is counties followed between September of 2019 and December of 2020. Controls include the monthly unemployment rate and the number of COVID-19 deaths by county. Plotted percent changes are the exponentiated event study coefficients minus one. Standard errors are clustered at the county-cohort level.

Figure D.6: Alcohol demand by school type in predominantly Black counties

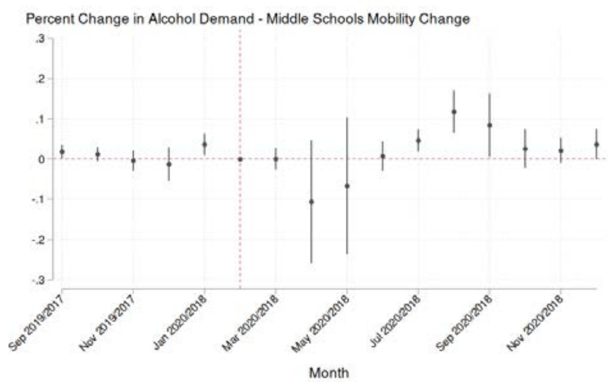
(a) All Schools



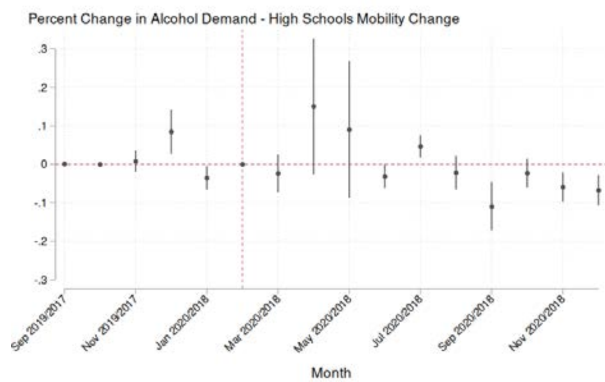
(b) Elementary



(c) Middle



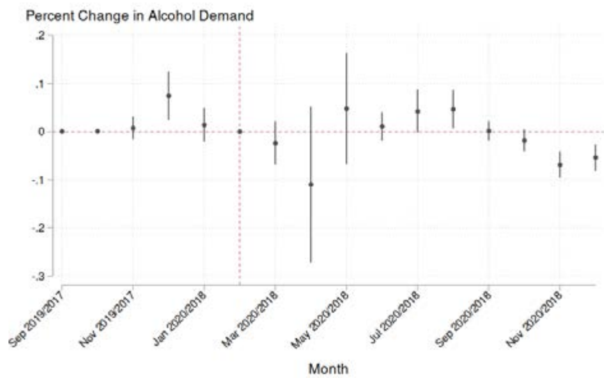
(d) High Schools



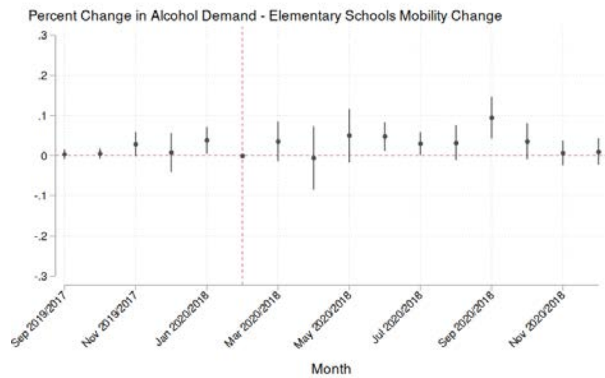
Notes: This figure shows the percent changes in alcohol demand from the event studies by month and school type in predominantly Black counties. The unexposed cohort is counties followed between September of 2017 and December of 2018. The exposed cohort is counties followed between September of 2019 and December of 2020. Controls include the monthly unemployment rate and the number of COVID-19 deaths by county. Plotted percent changes are the exponentiated event study coefficients minus one. Standard errors are clustered at the county-cohort level.

Figure D.7: Alcohol demand by school type in predominantly Hispanic counties

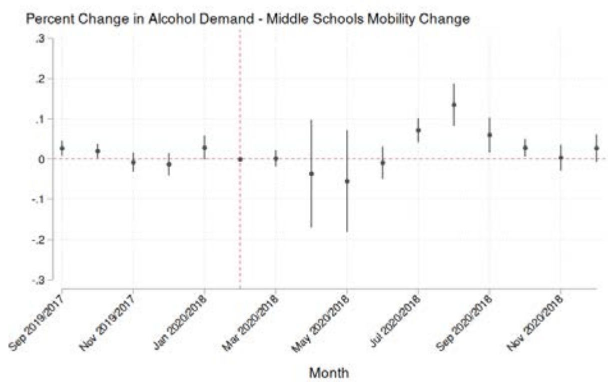
(a) All Schools



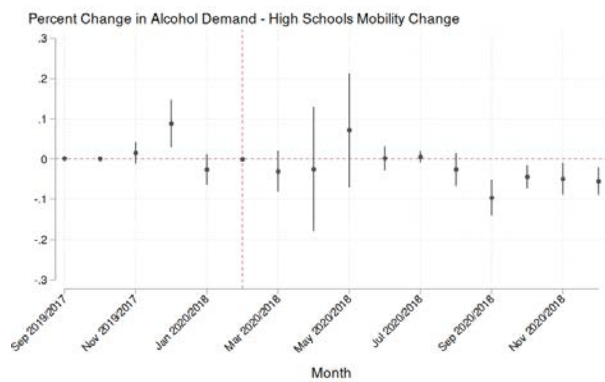
(b) Elementary



(c) Middle



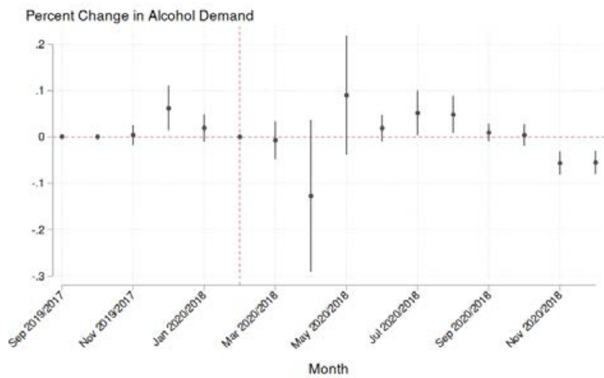
(d) High Schools



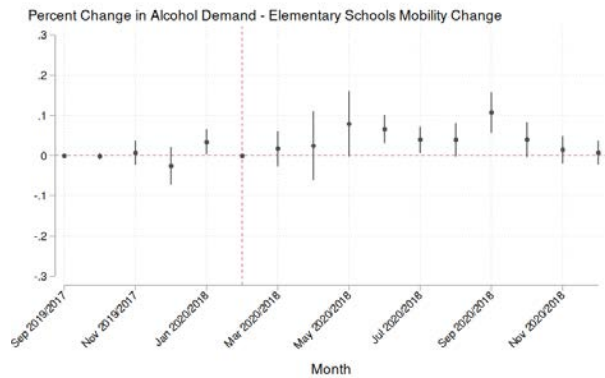
Notes: This figure shows the percent changes in alcohol demand from the event studies by month and school type in predominantly Hispanic counties. The unexposed cohort is counties followed between September of 2017 and December of 2018. The exposed cohort is counties followed between September of 2019 and December of 2020. Controls include the monthly unemployment rate and the number of COVID-19 deaths by county. Plotted percent changes are the exponentiated event study coefficients minus one. Standard errors are clustered at the county-cohort level.

Figure D.8: Alcohol demand by school type in predominantly Asian counties

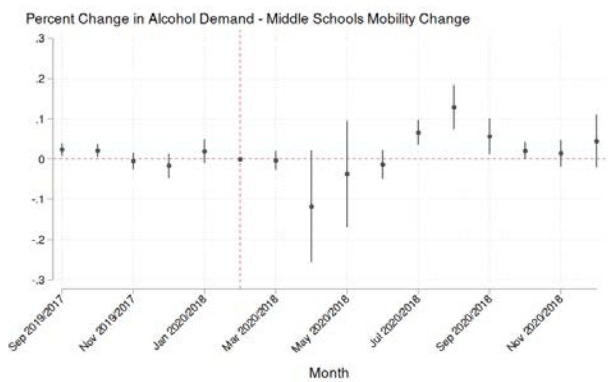
(a) All Schools



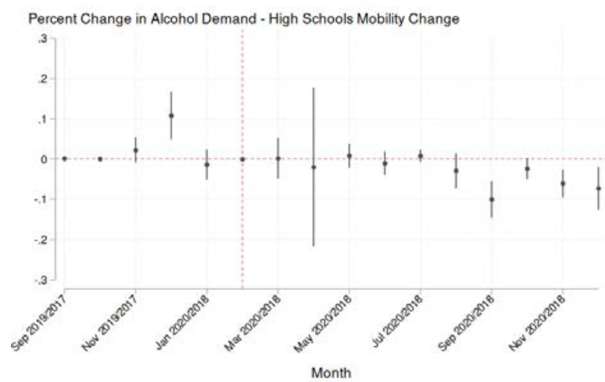
(b) Elementary



(c) Middle



(d) High Schools



Notes: This figure shows the percent changes in alcohol demand from the event studies by month and school type in predominantly Asian counties. The unexposed cohort is counties followed between September of 2017 and December of 2018. The exposed cohort is counties followed between September of 2019 and December of 2020. Controls include the monthly unemployment rate and the number of COVID-19 deaths by county. Plotted percent changes are the exponentiated event study coefficients minus one. Standard errors are clustered at the county-cohort level.

Table D.1: Summary of main effects by school type, predominantly White zip codes/counties

	(1) Elementary	(2) Middle	(3) High
	Panel (a): Antidepressants Model 2		
Pre-Pandemic Period	-0.0036	-0.0026	0.0007
	[0.0067]	[0.0029]	[0.0005]
Early Pandemic Period	0.0040	-0.0027	0.0050
	[0.0045]	[0.0045]	[0.0056]
Later Pandemic Period	-0.0004	0.0002***	0.0002
	[0.0012]	[0.0001]	[0.0003]
Observations	330359	330359	330359
	Panel (b): Alcohol Model 2		
Pre-Pandemic Period	-0.0019	0.0012	0.0023
	[0.0014]	[0.0014]	[0.0038]
Early Pandemic Period	0.0250*	0.0069	-0.0052
	[0.0140]	[0.0054]	[0.0118]
Observations	38394	38394	38394
Controls	Yes	Yes	Yes

Notes: This table reports the main effects grouped by event time and race, from the panel of mothers in zip codes in panel (a) and counties in panel (b) where each race group exceeds the state median. The reference period for the exposed cohort is February 2020 and for the unexposed cohort is February 2018. The percent changes in each column are the exponentiated coefficients minus one. Controls include the monthly unemployment rate and COVID-19 related deaths at the county level. Standard errors in brackets are clustered at the zip-cohort level in panel (a) and at the county-cohort level in panel (b). *** p<0.01, ** p<0.05, * p<0.1.

Table D.2: Summary of main effects by school type, predominantly Black zip codes/counties

	(1) Elementary	(2) Middle	(3) High
	Panel (a): Antidepressants Model 2		
Pre-Pandemic Period	0.0005	-0.0019	0.0018
	[0.0051]	[0.0027]	[0.0020]
Early Pandemic Period	0.0084	0.0004	0.0137**
	[0.0052]	[0.0012]	[0.0069]
Later Pandemic Period	0.0010	0.0003***	-0.0010***
	[0.0013]	[0.0000]	[0.0004]
Observations	402886	402886	402886
	Panel (b): Alcohol Model 2		
Pre-Pandemic Period	0.0015	0.0159*	0.0012
	[0.0028]	[0.0086]	[0.0012]
Early Pandemic Period	0.0331*	0.0261**	-0.0115
	[0.0173]	[0.0116]	[0.0129]
Observations	39072	39072	39072
Controls	Yes	Yes	Yes

Notes: This table reports the main effects grouped by event time and race, from the panel of mothers in zip codes in panel (a) and counties in panel (b) where each race group exceeds the state median. The reference period for the exposed cohort is February 2020 and for the unexposed cohort is February 2018. The percent changes in each column are the exponentiated coefficients minus one. Controls include the monthly unemployment rate and COVID-19 related deaths at the county level. Standard errors in brackets are clustered at the zip-cohort level in panel (a) and at the county-cohort level in panel (b). *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table D.3: Summary of main effects by school type, predominantly Hispanic zip codes/counties

	(1) Elementary	(2) Middle	(3) High
	Panel (a): Antidepressants Model 2		
Pre-Pandemic Period	0.0000	-0.0037	0.0032*
	[0.0059]	[0.0061]	[0.0018]
Early Pandemic Period	0.0038	-0.0010	0.0129*
	[0.0042]	[0.0012]	[0.0068]
Later Pandemic Period	0.0001	0.0002	-0.0004**
	[0.0007]	[0.0003]	[0.0002]
Observations	401182	401182	401182
	Panel (b): Alcohol Model 2		
Pre-Pandemic Period	0.0048	0.0229**	0.0022
	[0.0067]	[0.0094]	[0.0026]
Early Pandemic Period	0.0278*	0.0219**	-0.0079
	[0.0150]	[0.0106]	[0.0063]
Observations	39386	39386	39386
Controls	Yes	Yes	Yes

Notes: This table reports the main effects grouped by event time and race, from the panel of mothers in zip codes in panel (a) and counties in panel (b) where each race group exceeds the state median. The reference period for the exposed cohort is February 2020 and for the unexposed cohort is February 2018. The percent changes in each column are the exponentiated coefficients minus one. Controls include the monthly unemployment rate and COVID-19 related deaths at the county level. Standard errors in brackets are clustered at the zip-cohort level in panel (a) and at the county-cohort level in panel (b). *** p<0.01, ** p<0.05, * p<0.1.

Table D.4: Summary of main effects by school type, predominantly Asian zip codes/counties

	(1) Elementary	(2) Middle	(3) High
	Panel (a): Antidepressants Model 2		
Pre-Pandemic Period	-0.0031	-0.0012	0.0035
	[0.0062]	[0.0023]	[0.0024]
Early Pandemic Period	0.0057	0.0001	0.0139**
	[0.0053]	[0.0012]	[0.0066]
Later Pandemic Period	0.0001	0.0004***	-0.0001
	[0.0007]	[0.0001]	[0.0002]
Observations	424760	424760	424760
	Panel (b): Alcohol Model 2		
Pre-Pandemic Period	0.0004	0.0221***	0.0018
	[0.0023]	[0.0081]	[0.0029]
Early Pandemic Period	0.0357**	0.0169	-0.0076
	[0.0158]	[0.0111]	[0.0064]
Observations	37350	37350	37350
Controls	Yes	Yes	Yes

Notes: This table reports the main effects grouped by event time and race, from the panel of mothers in zip codes in panel (a) and counties in panel (b) where each race group exceeds the state median. The reference period for the exposed cohort is February 2020 and for the unexposed cohort is February 2018. The percent changes in each column are the exponentiated coefficients minus one. Controls include the monthly unemployment rate and COVID-19 related deaths at the county level. Standard errors in brackets are clustered at the zip-cohort level in panel (a) and at the county-cohort level in panel (b). *** p<0.01, ** p<0.05, * p<0.1.

E Robustness and Placebo Tests

Table E.1: Summary of main effects by type of school (restricted sample)

	(1)	(2)	(3)	(4)
	Model 1		*Model 2*	
	All Schools	Elementary	Middle	High
	Panel (a): Antidepressants			
Pre-Pandemic Period	-0.0041	-0.0044	-0.0011	0.0012
	[0.0040]	[0.0053]	[0.0018]	[0.0008]
Early Pandemic Period	0.0134***	0.0071**	0.0004	0.0133***
	[0.0040]	[0.0033]	[0.0013]	[0.0045]
Later Pandemic Period	0.0003	0.0001	0.0002*	-0.0000
	[0.0003]	[0.0004]	[0.0001]	[0.0001]
Observations	736444	736444	736444	736444
	Panel (b): Alcohol			
Pre-Pandemic Period	0.0001	0.0024	0.0008	0.0002
	[0.0013]	[0.0017]	[0.0005]	[0.0003]
Early Pandemic Period	0.0197**	0.0318***	0.0078**	-0.0014
	[0.0079]	[0.0101]	[0.0039]	[0.0021]
Observations	68945	68945	68945	68945
Controls	Yes	Yes	Yes	Yes

Notes: This table reports the main effects grouped by event time and type of school, restricting the sample to cleanly identified elementary, middle and high schools from the NCES. The reference period for the exposed cohort is February 2020 and for the unexposed cohort is February 2018. In panel (a), the dependent variable is the cumulative use of antidepressants and in panel (b) the total demand of alcoholic beverages. The percent changes in each column are the exponentiated coefficients minus one. Controls include the monthly unemployment rate and COVID-19 related deaths at the county level. Standard errors in brackets are clustered at the zip-cohort level in panel (a) and at the county-cohort level in panel (b). *** p<0.01, ** p<0.05, * p<0.1.

Table E.2: Mothers with previous antidepressant use

	(1) Antidepressants	(2) Other New Drugs
Pre-Pandemic Period	0.0003	-0.0023
	[0.0004]	[0.0033]
Early Pandemic Period	0.0002	0.0141
	[0.0016]	[0.0088]
Later Pandemic Period	0.0007	-0.0002
	[0.0005]	[0.0012]
Observations	605561	425957

Notes: This table reports the main effects grouped by event time, where the sample is the group of mothers who are not antidepressant-naive, so they were already using prescriptions for one year during the lookback period. The dependent variable in column 1 is the cumulative use of antidepressants and in column 2 the outcome is the cumulative use of other drugs classified as benzos, Z-drugs and barbiturates. Standard errors in brackets are clustered at the zip-cohort level. *** p<0.01, ** p<0.05, * p<0.1.

Table E.3: Other sales - Nielsen

	(1) Feminine Hygiene	(2) Baby Care Products
Pre-Pandemic Period	0.0005	0.0008
	[0.0004]	[0.0008]
Early Pandemic Period	0.0071	0.0128
	[0.0072]	[0.0087]
Observations	81542	81398
Controls	Yes	Yes

Notes: This table reports the main effects grouped by event time, from the Nielsen retail scanner dataset for all schools combined and alternative outcomes of interest. The dependent variable in column 1 is the total purchases of feminine hygiene products and in column 2 the purchases of baby care products (lotions, powder, oil, ointments and bath) by county and month. The excluded (comparison) period is February for both cohorts. Controls include the monthly unemployment rate and COVID-19 related deaths at the county level. Standard errors in brackets are clustered at the county-cohort level. *** p<0.01, ** p<0.05, * p<0.1.