School's Out! Now What?
Effects of School Closures on Parental Mental Health[*]

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Abstract

Everyday lives of parents of school-age children were substantially altered by school closures that local governments instituted to curb viral spread during the COVID-19 pandemic. Using a national health claims database, we first document a decline in the mental health of parents relative to otherwise similar non-parent adults during the 2020/21 school year, as measured by psychiatric prescription fills among adults without psychiatric treatment in the year preceding the pandemic. Parents of children with pre-existing health conditions shoulder this penalty entirely. Leveraging county-level school closure decisions, using a triple-differences design, we measure the causal effect of school closures on parental mental health. We find that the mental health parenthood penalty is substantially attenuated in counties with greater school closure intensity, especially for parents of children with pre-existing health conditions and for mothers.

Keywords: COVID-19 pandemic, Mental health, School closures, Parents, Healthcare utilization, Gender differences

JEL: D12, I12, I18, I31, J13, J16

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1. Introduction

The COVID-19 pandemic was associated with worsening mental health *en masse* (e.g., Ettman et al., 2020; Grossman et al., 2020) and presented a unique set of circumstances for parents of school-age children. On the one hand, the pandemic may have created additional stressors for parents as caretakers, with more responsibilities to balance and heightened worries about family well-being. On the other hand, increased interactions with children may have reduced feelings of loneliness that reportedly plagued many adults and were associated with declines in physical and mental health during the pandemic (Jabbari et al., 2024). Parental experiences of the pandemic likely depended on (1) the degree of concern parents had about the risks of the pandemic for themselves or their children, in terms of both mental and physical health; and (2) the degree of access that parents had to in-person schooling for their children, as local governments variably instituted cessation of in-person schooling (i.e., school closures) to curb viral spread (Parolin and Lee, 2021).

In this paper, we first document how the mental health of parents of school-age children evolved during the COVID-19 pandemic relative to that of otherwise similar non-parent adults, and how these differences vary by parental gender and by their children's pre-existing health risks. We then estimate the causal effect of K–12 school closures on parental mental health outcomes, holding fixed pandemic severity and contemporaneous economic conditions. Our results provide novel insights into the impact of the pandemic and school closures on the well-being of parents and shed new light on mental health dynamics within families.

We merge two datasets to examine these questions. Optum's de-identified Clinformatics® Data Mart Database (CDM) of commercial claims allows us to determine beneficiary demographics (i.e., age, gender, pre-existing health conditions), identify parent-child relationships, and track monthly prescription fills for medications commonly used to treat anxiety and depression. The restricted-use U.S. School Closure & Distance Learning Database compiled by Parolin and Lee (2021) helps us define county-level school closure indicators based on monthly changes in cellphone GPS pings at each school. We study the mental health, proxied by psychiatric prescription fills, of a cohort of about 1.8 million adult beneficiaries without pre-pandemic psychiatric health exposure, of whom 28.1% are parents of school-age children and 82.0% live in counties with above-median school closure rates.[1]

First, we examine whether the pandemic took a differential toll on parents and non-parents. While survey evidence has drawn attention to worsened mental health across multiple demographic groups during the pandemic, including youth, women, caregivers, people identifying as racial and ethnic minorities, and individuals at higher risk for COVID-19 complications, little attention has been given to parents as a group of interest (e.g., Davis et al., 2021; Hartman-Munick et al., 2022; Jones et al., 2022; Panda et al., 2021; Patrick et al., 2020; Penninx et al., 2022). It is important to examine parents as their own group to understand how policies affect not only individuals but also family dynamics. Early surveys by Patrick et al. (2020), Panda et al. (2021), and Davis et al. (2021) indicated declines in the self-reported well-being of parents during the pandemic. To our knowledge, our work is the first to systematically compare the mental health of parents and non-parents within a causal inference framework. In particular, we employ a difference-in-differences approach to compare within-person changes over time in the mental health of parents versus that of similar non-parent adults of the same gender, age group, and county of residence.

We document a parenthood penalty on mental health during the pandemic. Our results indicate that parents of school-age children experienced a larger increase than similar non-parent adults in their propensity to fill psychiatric prescriptions during the pandemic. This effect is observed predominantly during the 2020/21 school year. Compared to before the pandemic, parents became 0.06 percentage points more likely than similar non-parents to fill psychiatric prescriptions in the Fall of 2020 and Winter of 2021, which translates to an approximately 150% increase over the baseline differential between parents and non-parents prior to the pandemic.

Next, we explore whether this parenthood penalty is differentially pronounced among families in which children with pre-existing health conditions (i.e., at-risk children) have physical conditions that place them at higher risk of complications from COVID-19 infection, mental conditions that may respond to changes in social circumstances brought about by the pandemic, or both. While physical and mental health are distinct, co-occurring physical and mental health diagnoses are common and known to be more severe than physical or mental health conditions alone (Hanna et al., 2024; Holland et al., 2023; Momen et al., 2022). We find that the pandemic takes a larger and more persistent toll on parents of at-risk children. For example, compared to similar non-parent

[1] Supplementary analyses examine the same outcomes for about 0.8 million adults with psychiatric healthcare experience in 2019, of whom 24.6% are parents of school-age children and 78.1% live in counties with above-median school closure rates.

adults, parents of children with pre-existing mental and physical health conditions (i.e., co-occurring risk) became 0.77-1.19 percentage points more likely to fill psychiatric prescriptions, representing increases in parenthood mental health penalties of about 200-300% from baseline differences. In contrast, parents of children not at risk incur a modest parenthood advantage and experience a lower increase in the propensity to fill psychiatric prescriptions compared to similar non-parent adults. We further document that mothers, rather than fathers, are the primary bearers of the observed parenthood effects.

Finally, because parenthood effects are especially prominent during the 2020/21 school year, we study how school closures impact changes in parental mental health. The effect of school closures on parental mental health, beyond the impact of the pandemic, is *ex ante* ambiguous. During the pandemic, parents increased the time spent with their children more than fourfold (Agostinelli et al., 2022), in part due to remote work and options for hybrid school attendance. On the one hand, an increase in time spent with children may have improved the mental well-being of parents compared to non-parents, especially considering that social isolation was a source of psychological distress for many adults during the pandemic. In addition, school closures may have helped mitigate parental concerns about children's mental and/or physical health during the pandemic, especially among parents of at-risk children. For instance, parents with increased proximity to their children may have gained insight into and/or control over circumstances that put their children at greater risk, because school closures keep these children more closely under parental supervision and further from school-based stressors and points of viral contact. Although difficult, parenting at-risk children during the pandemic may have reduced parental anxiety when children are nearby. On the other hand, school closures may have increased parental stressors. Increased parenting responsibilities may have taken an additional toll on time-constrained parents without outlets for childcare assistance, which were in low supply during the pandemic (Gascon and Werner, 2022; Zhang et al., 2023). Increased exposure to childcare responsibilities may have been particularly taxing for parents of at-risk children who may require more attention.[2]

To study how school closures shape parental well-being, we employ a triple-differences design that compares changes in mental health for parents relative to non-parents across counties with above-median versus below-median school closure intensity (hereafter, closure and non-closure coun-

[2]Survey evidence suggests negative associations between children with remote learning difficulties and parental mental health (Davis et al., 2021), as well as between childcare disruptions and parental mood (Gassman-Pines et al., 2022), during the pandemic.

ties). By treating non-parent adults in the same county as a reference group, our approach partials out county-level unobserved factors that affect all adults living in closure and non-closure counties. We find that school closures have a protective effect on parental mental health. Relative to non-parents, parents in closure counties experience a smaller parenthood penalty than parents in non-closure counties. Although this protective effect is present across all parent groups, it is largest among parents facing plausibly greater caregiving responsibilities: parents of at-risk children and mothers. For example, during the first two months of 2021, the increase in filled psychiatric prescriptions among parents of children with co-occurring risk, relative to non-parents, is 1.80 percentage points in non-closure counties but only 0.83 percentage points in closure counties, compared to the same months in 2020. This corresponds to a 55% reduction in the parenthood mental health penalty among this group in closure counties in early 2021, with estimated reductions ranging from 52% to 60% across the 2020/21 school year. More broadly, increases in psychiatric prescription fills among parents are consistently smaller in counties with high rates of school closures than in counties with low rates of school closures. These patterns suggest that school closures mitigated the deterioration of parental mental health, particularly for parents of children with co-occurring risk.

We address alternative interpretations of these data patterns, such as differential changes in healthcare access or parent-specific behavioral responses during the pandemic, through a series of falsification and placebo tests. First, to address the alternative explanation that observed differences in psychiatric prescription fills may reflect differential changes in healthcare access or treatment-seeking behavior across groups during the pandemic, rather than differential mental health outcomes, we conduct a falsification analysis using diabetes prescription fills as a proxy for healthcare utilization for chronic conditions not overtly related to psychiatric conditions. If access for parents were systematically lower than for non-parents, we should expect similar parenthood effects across classes of medication. Contrary to a narrative that parents had differentially higher healthcare access than non-parents, we find that parents are less likely than non-parents to fill diabetes prescriptions during the pandemic, regardless of their children's health risks, and that school closures do not differentially affect diabetes prescription fills. Second, to assess whether differences between closure and non-closure counties capture the effects of school closures rather than other parent-specific trends in these counties, we conduct a placebo test that examines psychiatric prescription fills among parents of children who are too young to be eligible for school, and therefore, unlikely to be directly affected by school closures. If unobservable time-varying local trends affecting parents were driving our results, we would expect to observe similar patterns for this group, since these parents should have exposure to the same factors affecting all parents but not school closures. Instead, we find no evidence that the parenthood penalty for parents of preschool-age children varies with school closure intensity. Taken together, these results suggest that (1) the observed changes in psychiatric prescription fills are unlikely to be driven by differential healthcare access or utilization across groups, and instead, reflect changes in mental health; and (2) the closure effects we isolate are driven by school closures rather than other county-and-time varying factors associated with school closures.

In related work, Gupta et al. (2024) compare cumulative antidepressant prescription fills among a pandemic cohort of mothers living in counties with higher versus lower rates of school closures to a pre-pandemic cohort of parents in the same counties. They find that antidepressant fills increased slightly for mothers living in counties with above-median school closures relative to those in counties with below-median school closures during the pandemic. Notably, because their analysis does not consider non-parent adults and instead relies on the assumption of comparability of potential outcomes in counties with and without school closures, it is unclear whether these patterns are driven by school closures or instead by community-level factors that cause both school closure decisions and the mental health impacts of the pandemic on a community (e.g., viral spread, local economy, healthcare access, other local prevention measures, etc.). Our design isolates the role of school closures by comparing parents of school-age children to otherwise similar non-parent adults living in the same counties, and therefore, exposed to the same pandemic conditions.

Our results highlight a previously overlooked positive externality of school closures for families. While prior work has documented the educational costs of school closures for children (Jack et al., 2023) and mental health benefits for children (Björkegren et al., 2024; Freedman et al., 2024), our evidence suggests that closures also served as a protective factor for parental mental health. This protective effect is especially pronounced for parents who plausibly faced the greatest caregiving demands during the pandemic. Parental mental health is important not only for its own sake but also for the current and future well-being of children. Prior research has associated poor parental mental health during formative years with lower resilience and poorer health outcomes for children during childhood and enduring into adulthood (e.g., Kamis, 2021; Schafer and Ferraro, 2013; Schepman et al., 2011).[3] Our results underscore that policies may have within-family spillovers. Policies that appear costly from one perspective (e.g., school closures for children) may simultaneously yield unanticipated, and perhaps even unrecognized, benefits for other household members.

More broadly, our findings contribute to a growing literature on the interdependence of mental health within families. Effects on mental health are compounded in family settings. Parents and children contribute to each other's current and future well-being. Most existing research has focused on how parental factors affect children's well-being (Case and Paxson, 2002); much less is known about how children's experiences and vulnerabilities feed back into parents' mental health.[4] In documenting that parents of at-risk children experienced a larger mental health burden during the pandemic, our study provides the first large-scale evidence that the parent–child mental health relationship may operate in both directions. Our results further suggest different channels of influence depending on the direction of transmission. Prior research suggests that increased exposure to parents with pre-existing mental health conditions may harm children's well-being. In contrast, our results suggest that increased proximity (in our setting, from school closures) to children with pre-existing health vulnerabilities may be protective for parental mental health that was otherwise strained during the pandemic. We cannot empirically test whether these mental health benefits are driven by direct parental exposure to children, or increased parental control over factors contributing to children's risk, or a combination. In any case, proximity appears to alleviate the strain on parental mental health more than increased responsibility contributes to parental stress.

Understanding these mental health dynamics is essential for designing effective social policy. Our findings suggest that strengthening family supports, especially for households with at-risk children, may promote mental health for parents, for whom policy is rarely tailored. We hope that these results inspire future research into effective interventions that can realize this potential double dividend.

[3] A related literature has also shown that adverse childhood experiences (ACEs) are correlated with neurocognitive decline (Yu et al., 2024) as well as higher likelihoods of felony charges, teenage parenthood, poverty, educational attrition, Medicaid assistance, and employment (Ratcliff et al., 2025).

[4] A nascent literature has begun to document that difficulties faced by children may also be experienced as parental stressors. For example, parents of children with activity limitations (Witt et al., 2009), intellectual and developmental disabilities (Scherer et al., 2019), and complex medical needs (Halsey et al., 2020) are found to be at high risk for depression and anxiety.

2. Data

2.1. Health Claims

We use Optum's de-identified Clinformatics® Data Mart Database (CDM), which is a commercial claims repository. We specifically use CDM's ZIP5 view, which provides beneficiary age, gender, and ZIP code. Using a crosswalk between beneficiary ZIP codes and county FIPS codes, we map beneficiaries to counties to merge individual-level claims with an indicator for school closure county residence (the construction of which is discussed in Section 2.2). Categories of claims available in CDM include prescription fills, diagnoses, outpatient services, and inpatient services, which we observe separately for policyholders and any dependents sharing the same health insurance policy. For our analyses, we need to define a relevant cohort, construct family structures, identify whether children are of an age for their household to be affected by school closures (i.e., school-age), identify psychiatric prescription fills for each family member, and classify prior health history.

Because COVID-19 was declared a pandemic on March 11, 2020 (Centers for Disease Control and Prevention, 2024), we focus on 2020 and 2021 as our study period. We use diagnoses, prescriptions, and services during 2019 to determine pre-pandemic risk factors. Therefore, to define our analysis sample, we begin with the sample of beneficiaries who are continuously enrolled in any CDM plans from January 1, 2019, through December 31, 2021 (8,428,198 beneficiaries and 6,344,234 policies). We focus on adult beneficiaries who are older than 18 and younger than 65 years in 2020 (3,612,590 beneficiaries and 2,574,447 policies). We then exclude beneficiaries who (1) live in ZIP codes that cannot be merged with school closure data, either because the household is associated with multiple or missing ZIP codes or because school closure data is missing for the counties associated with those ZIP codes; (2) are missing gender information; (3) are flagged as dually eligible or receiving Medicare's Low Income Subsidy; or (4) became new parents in 2021. These filters result in a sample of 3,161,452 adult beneficiaries and 2,184,873 policies. The flowchart in Appendix A details these sample restrictions. Our cohort is reflective of the commercially-insured, working-aged population.

Our analyses contrast the mental health outcomes of parents to those of similar non-parent adults living in the same county. We infer family structure from family identifiers provided in the CDM data and beneficiary age in 2020. Dependents share the same family identifier if they belong to the same insurance plan as the policyholder. Dependents often include children and spouses.[5]

[5]Depending on state laws, families may also declare those covered by legal guardianship as adult dependents.

We refer to children between 6 and 17 years old in 2020 as school-age children, because children in this age group are likely to attend elementary, middle, or high school in both 2020 and 2021. We define *parents* as beneficiaries sharing the same family identifier (i.e., the same health plan) who are at least 14 years older than any other beneficiaries younger than 26 years old because children may be dependents until the age of 26.[6] Among the 3,161,452 beneficiaries in the analysis sample, we classify 1,185,115 (37.5%) as parents and 1,976,337 (62.5%) as non-parents. Among parents, 730,537 (61.6%) have at least one school-age child. Because we expect households with school-age children to be most exposed to school closures, primary analyses focus on these parents. Placebo analyses in Section 6 examine the impact of school closures on 110,027 (9.3%) parents who only have children younger than 4 years of age.

We interpret psychiatric prescriptions as revealed measures of underlying mental health needs, following a large literature (e.g., Gaynes et al., 2015; Maguire et al., 2017; Mannering et al., 2024; Sanchez-Ruiz et al., 2025; Tiruye et al., 2024). We construct our main outcome of interest, *Likelihood of Any Psychiatric Prescription Fill*, as a binary variable that captures whether a beneficiary filled a psychiatric prescription in any given month of 2020 or 2021.[7] The literature has also used psychiatric prescriptions without associated psychiatric diagnoses to proxy for mental health concerns, because mental health treatment provided by non-specialist providers (i.e., primary care physicians) is often not associated with formal diagnosis (Rhee and Rosenheck, 2019).

To study changes in psychiatric prescriptions, we consider beneficiaries without prior psychiatric health history (i.e., naive) and beneficiaries with prior psychiatric health history (i.e., experienced) separately because: (1) from a policy-perspective, we are interested in examining the extensive margin (psychiatric prescription incidence) separately from the intensive margin (prevalence); (2) access to psychiatric healthcare may be a function of psychiatric experience; (3) the decision-making process for filling prescriptions for new fills likely differs from that of refills; and (4) the pandemic and school closures may differentially impact these cohorts.

[6]This definition of parenthood is likely an underestimate. Consider a two-parent household with parent A, parent B, and child C. If parent A and parent B are primary policyholders for separate health plans in our data, and child C is a dependent under parent B's plan, then parent A will be considered a non-parent. This measurement error should attenuate our estimates of the differences in mental health outcomes between parents and non-parents because our set of non-parents will include some parents.

[7]In claims data, we do not observe monthly prescription consumption; instead, we observe prescription fill dates and corresponding day supply (e.g., 30 days). We define our dependent variable to take the value of 1 for each month involved between the prescription start (prescription fill date) and prescription end (prescription fill date plus day supply). For example, if a prescription with a 30 day supply were filled on March 20, we would determine the prescription start to be March 20, the prescription end to be April 19, and the dependent variable to take a value of 1 for both March and April.

To determine pre-pandemic psychiatric health history, we check whether adult beneficiaries received any psychiatric prescriptions, diagnoses, or services in 2019 (Appendix B). Beneficiaries without psychiatric experience in 2019 are labeled *naive*, and those with psychiatric experience are labeled *experienced*. Among the 3,161,452 adult beneficiaries in the sample, about 1.8 million are naive and 1.4 million are experienced.

A central focus of our analysis is the extent to which child health risk moderates parental mental health outcomes. Parental mental health is likely affected by children's well-being, and we may expect school closures to play different roles for parents of at-risk children, however perceived, versus other parents. We measure two dimensions of risk using diagnosis codes available to us in claims data (Appendix B). Mental health risk for children is defined using pre-pandemic psychiatric diagnoses. Physical health risk for children is defined using pre-pandemic comorbidities that conferred excess risk from coronavirus, including the following diagnoses: cancer, chronic (non-allergy) respiratory disease, and cardiovascular disease (excluding essential hypertension). From these definitions of child health risk, we construct a categorical variable that categorizes any children younger than 18 years old as (1) not at risk; (2) at risk for mental health only; (3) at risk for physical health only; or (4) at risk for both mental and physical health (i.e., at co-occurring risk).

2.2. School Closures

School closures in the U.S. were nearly universal in the beginning of the pandemic. By early April 2020, primary and secondary schools had shifted to remote learning through a patchwork of state and local government orders for the duration of the academic year. Over the summer of 2020, state educational authorities provided reopening guidance frameworks (e.g., community health metrics) and ceded decisions regarding educational modalities (i.e., online only, in-person only, or hybrid) to school districts for the 2020/21 academic year.

To quantify the degree of school closures in a geography, we obtain data on monthly school visit counts based on cellphone GPS pings from the restricted-use U.S. School Closure & Distance Learning Database compiled by **?**. Following **?**, we consider a school to have limited in-person learning (i.e., instituted a *school closure*) in a given month of 2020/21 if the school had at least a 50% decrease in visits relative to the same month in 2019.[8] Figure C.1 plots the percentage of schools with at least a 50% reduction in visits and confirms that while almost no schools experienced

[8]School visits are noisy. For example, if someone with a cellphone uses the school playground, that may count as a visit to the school. Therefore, we consider a school to have closed only when monthly visits are reduced by at least 50% relative to the same month in 2019.

a reduction in visits compared to the earlier year during the pre-pandemic months of January and February of 2020, almost 90% of schools had a 50% or greater reduction in visits in April and May of 2020. However, with the start of the 2020/21 academic year, only about 40% of schools closed from June 2020 onward. This proportion temporarily rises to about 56% of schools in December 2020, coinciding with a new wave of infections from the Delta variant (**?**).

We are interested in exploring differences in mental health outcomes between parents with high versus low levels of exposure to school closures. However, we cannot directly identify which school(s) are attended by a household's children. Children living in the same neighborhood may attend different schools or even travel to different school districts. There may also be policy differences between private and public schools within the same school district. Therefore, we aggregate monthly school closure counts to the county level.[9] This metric probabilistically captures school closure exposure and provides us with a measure of the degree of monthly school closures within driving distance from a household. Any family in a county with a majority (minority) of its schools closed at any time has a higher (lower) propensity for school-age children in that county to be restricted to remote learning modalities. Measurement error due to this measure's probabilistic nature (rather than identifying child-specific school closure status) is considered to be as good as random and is expected to attenuate our estimates of the impact of school closures. Using a crosswalk between beneficiary ZIP codes and county FIPS codes, we map beneficiaries to counties to merge individual-level claims with school closure county residence.

We use a binary classification to separate counties with high versus low exposure to school closures. In particular, we define a county to be a *closure county* if the average percentage of schools in that county with at least a 50% decrease in attendance from September 2020 through March 2021 exceeds the median percentage of schools with at least a 50% decrease in attendance across all counties over the same period. This binary definition separates individuals living in counties with relatively higher school closure intensity (i.e., *closure counties*) from those living in counties with relatively lower school closure intensity (i.e., *non-closure counties*) during the 2020/21 academic year.[10] Regional distributions of closure and non-closure counties differ, as illustrated in Figure C.2. Closure counties tend to be in the West and Northeast, while non-closure counties are more often in the Midwest and South. Because closure and non-closure counties may differ along

[9]School-level visit data are not uniformly available across months. County-level aggregation also helps minimize missingness (Table C.1).

[10]Figure C.1 shows that counties included in CDM are similar in their closure propensities to all counties observed in the U.S. School Closure & Distance Learning Database.

multiple dimensions, our analyses employ county fixed effects to make within-county rather than across-county comparisons.

2.3. Summary Statistics of Primary Cohort

In our main analyzes, we focus on the psychiatric health outcomes of naive adult beneficiaries, specifically contrasting non-parents and parents of school-age children (highlighted in green in Appendix A), because our primary interest is documenting how the incidence of psychiatric prescriptions evolves during the pandemic and responds to school closures. In Section 5.2, we also present results among the experienced cohort.

Among the 1.8 million naive adults, 1,313,164 are non-parents and 514,091 are parents of school-age children (henceforth, *parents*). Table D.1 in Appendix D reports the demographics and baseline psychiatric prescription hazards for parents and non-parents. Naive parents and non-parents differ slightly in their age, gender composition, and geographic dispersion. Parents are slightly older than non-parents (43.7 vs. 42.3 years old, p-value$<$ 0.001), more likely to be female (45.5% vs. 43.4%, p-value$<$ 0.001), and more likely to reside in school closure counties (82.3% vs. 81.9%, p-value$<$ 0.001). Therefore, we control for time-varying gender and age differences across groups in our analyses.

On average, parents are about 4% more likely than non-parents to fill psychiatric prescriptions in January and February of 2020, which constitutes the pre-pandemic baseline period in our analyses (0.98% vs. 0.94%, p-value=0.005). Table D.1 also decomposes these groups by closure county residency. We observe that the difference in the baseline psychiatric prescription hazard between parents and non-parents is statistically indistinguishable across closure and non-closure counties (reported in the last row of Table D.1).

Among parents, approximately 14% have at least one child with mental risk only, 15% with physical risk only, 6% with co-occurring risk, and the remaining 65% have no children with these pre-existing health risks. Table D.2 reports the demographics and baseline psychiatric prescription hazard of non-parents and parents by child health risk category and by closure county residence. The psychiatric prescription hazard in the baseline period is highest among parents of children with co-occurring risk (1.34%), followed by parents of children with mental risk (1.16%) and parents of children with physical risk (1.08%), all of which are significantly higher than that of non-parents (0.94%, all p-values $<$ 0.001). Parents of children not at risk for these conditions have a slightly lower baseline hazard risk compared to non-parents (.89%, p-value $<$ 0.001). These baseline differences may suggest that mental health needs are clustered within families or that additional caregiving responsibilities for children with pre-existing health concerns exert an additional toll on parents. Importantly, these differences in the baseline psychiatric prescription hazards between different parent groups and non-parents are statistically indistinguishable across closure and non-closure counties. The absence of differential baseline gaps between closure and non-closure counties supports the core identifying assumption of our triple-differences design: in the absence of school closures, the gap between parents and non-parents would have followed parallel trends across counties.

3. Changes in Psychiatric Prescription Fills during the Pandemic

3.1. Differences across Parents and Non-parents

We first document how psychiatric prescription fills varied over the course of the pandemic between parents of school-age children and non-parent adults of the same age group and gender. To do so, we estimate:

$$\texttt{Y}_{icm} = \alpha_i + \gamma_{cm} + \beta^t(\texttt{parent}_i) + \delta^t(\texttt{age}_i) + \kappa^t(\texttt{gender}_i) + \epsilon_{icm} \tag{1}$$

where $\texttt{Y}_{icm}$ denotes whether a psychiatric prescription was filled by beneficiary i in county c in time period t and month-year m. Beneficiary fixed effects (α_i) capture time-invariant heterogeneity across individuals. Within-person variation is decomposed into two main components: (1) county-month-year fixed effects (γ_{cm}), and (2) t-specific variation as a function of the parenthood status, age group, and gender of the beneficiary. County-month-year fixed effects (γ_{cm}) account for time-variant unobservables common to all beneficiaries living in the same county. These controls subsume persistent cross-sectional unobservables such as political attitudes, healthcare access, and stigma surrounding the use of mental health resources. They also account for any differential trends in mental health outcomes across counties, which may occur for reasons including variation in pandemic exposure (e.g., viral cases and deaths) or in exposure to other non-pharmaceutical interventions (e.g., stay-at-home orders). Individual departures from these common trends are predicted by period-specific shifters that vary by whether the beneficiary is a parent (to any school-age children), age range (19-34, 35-49, and 50-64) and gender (binary). Because parents and non-parents are observably different along age and gender (Section 2.3), we explicitly control for age group and gender differences in outcomes over time to isolate the differential impact of the pandemic on parents versus non-parents. Remaining individual idiosyncrasies are captured by ϵ_{icm}, which are clustered at the county level. Therefore, our analyses examine within-person changes in psychiatric prescription fills.

We divide the year into five periods over which we summarize the differential impact of the pandemic on parents versus non-parents: (1) January to February; (2) March; (3) April to May; (4) June to August; and (5) September to December.[11] We henceforth refer to period 1 as Winter, period 2 as March, period 3 as Spring, period 4 as Summer, and period 5 as Fall. The subscript t indexes these periods in 2020 and 2021.

The coefficients of interest, β^t, provide estimates of the difference in the within-person change in the likelihood of filling a psychiatric prescription between parents and non-parents of the same age group, gender, and county residence c during period t relative to Winter 2020. These estimates are plotted in Figure 1a. We find that the likelihood of initiating psychiatric prescriptions for parents does not differ from that of similar non-parents in the same county until the 20/21 school year begins. During the school year, naive parents become 0.058 percentage points (pp) more likely to fill psychiatric prescriptions in Fall 2020 (p-value= 0.019) and in Winter 2021 (p-value= 0.069) compared to similar naive non-parents. Considering that the baseline differential between naive parents and non-parents is 0.039 pp in Winter 2020, this increase constitutes a 148.7% relative increase in the propensity of psychiatric prescription use for parents compared to non-parents. The parenthood difference reverts to insignificant levels at the conclusion of the 20/21 academic year.

3.2. Heterogeneity across Children with Pre-existing Comorbidities

Because concerns about the pandemic may be stronger for populations with pre-existing conditions, we also investigate whether these concerns manifested differently for parents of children with and without health risks compared to non-parents, using an interaction model:

$$\mathtt{Y}_{icm} = \alpha_i + \gamma_{cm} + \beta_1^t \mathtt{parent}_i + \sum_{r=1}^{3} [\beta_{2r}^t(\mathtt{parent}_i * \mathtt{H}_i^r)] + \delta^t \mathtt{age}_i + \kappa^t \mathtt{gender}_i + \epsilon_{icm} \tag{2}$$

where $\mathtt{H}_i^r$ denotes a set of indicators for parents of children with pre-existing physical risk only ($r = 1$), parents of children with pre-existing mental risk only ($r = 2$), and parents of children with

[11] We choose these periods to align with a typical school year, including the fall semester (September to December), the spring semester (January to May), and the summer break (June to August). We further divided the spring semester to account for the announcement of the pandemic in March 2020 and the school closure policies that were adopted in April 2020.

co-occurring risk ($r = 3$).[12] Thus, coefficients β_1^t provide estimates comparing parents with children not at risk to non-parents, and coefficients $(\beta_1^t + \sum_{r=1}^{3} \beta_{2r}^t H_i^r)$ provide estimates comparing parents with children at different levels of risk to non-parents.

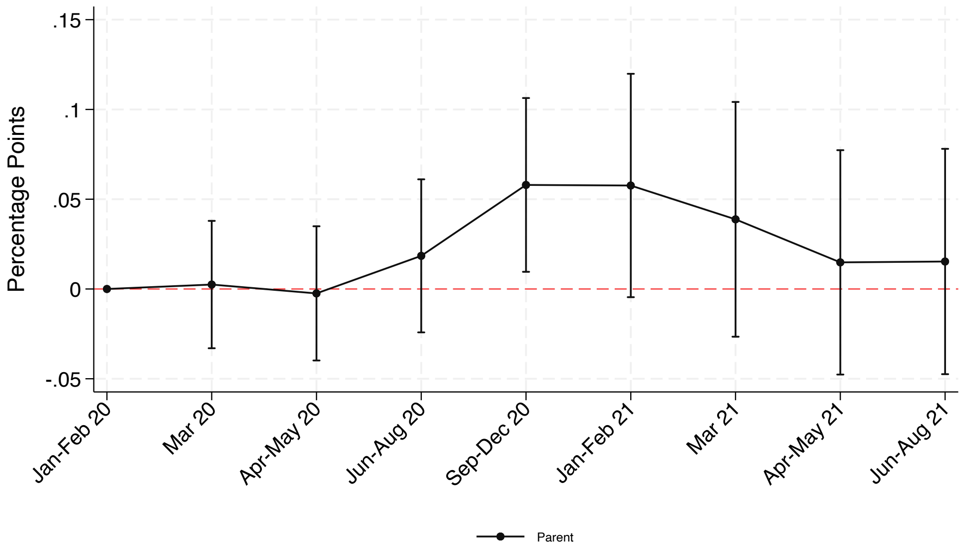
Estimates are plotted in Figure 1b and show that the results obtained in Figure 1a mask important heterogeneity. While differences between non-parents and parents of all types are modest in the early months of the pandemic, as the school year approaches, we observe a stark increase in prescription fills for parents of children with co-occurring risk. Beginning in Summer 2020, these parents become up to 0.477 pp (p-value$<$ 0.001) more likely than non-parents to fill a psychiatric prescription. Coinciding with the school year, this difference persistently increases to 0.771 pp (p-value$<$ 0.001) in Fall 2020, up to 1.189 pp (p-value$<$ 0.001) in Summer 2021, corresponding to a 297.3% increase from the baseline difference between these two groups. These patterns are echoed for parents of children with mental risk only, albeit at slightly lower levels. However, for parents of children with physical risk only, we do not observe significant differences from non-parents, except for a brief period at the start of the school year. In contrast, parents of children not at risk, who account for 65.5% of parents, become increasingly less likely than non-parents to fill a psychiatric prescription over the school year, from -0.079 pp (p-value= 0.002) in Summer 2020 to -0.105 pp (p-value$<$ 0.001) in Fall 2020 to -0.269 pp (p-value$<$ 0.001) in Summer 2021, corresponding to a 489.1% decrease from the baseline difference between these parents and non-parents.

These results highlight the variability in parental experiences during the pandemic. Mental health outcomes for parents of children with the greatest risk (i.e., co-occurring risk) deteriorate sharply compared to similar non-parents in their counties over the course of the 202/21 academic year, followed by parents of children with mental health risks. Parents of children with physical health risks experience small and temporary mental health shocks, while parents of children not at risk fare slightly better than similar non-parents. One possible explanation for the larger relative increases in psychiatric prescription fills among parents of children with mental but not physical health risks is that children with mental health concerns may require more active parental attention during the pandemic school year. As a contrived example, a school-age child with anxiety may require frequent and daily parental support, while a school-age child with asthma may only need access to an inhaler. Regardless of the reason underlying the additional lift in psychiatric prescription fills among parents of children with mental health risks, we observe that these differ-

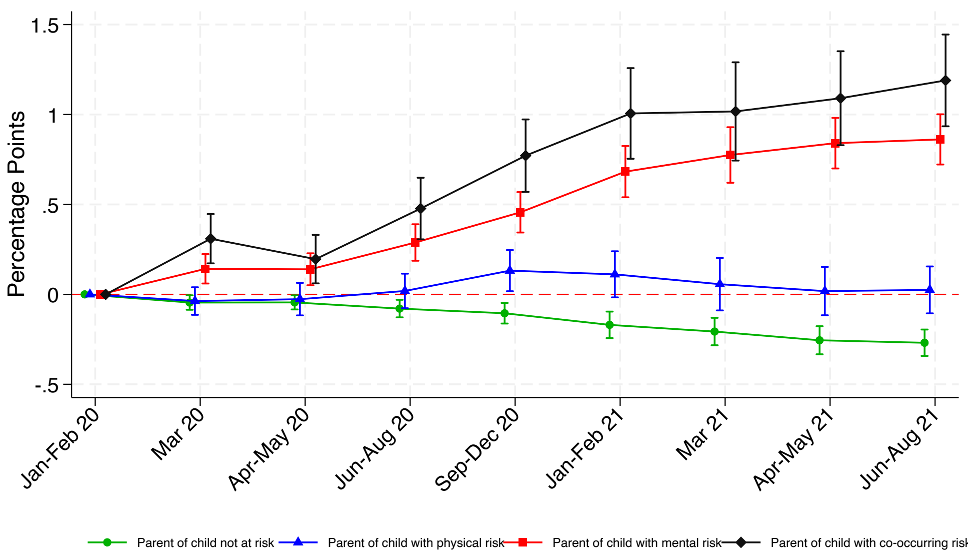
[12] H is defined as 0 for both non-parents and parents with children not at risk.

ences drastically increase with the start of the school year. Therefore, we proceed to study the moderating role of school closures on parental mental health while preserving this dimension of heterogeneity.

Figure 1: Δ Likelihood of Any Psychiatric Prescription Fill for Parents vs. Non-parents



(a) Parenthood



(b) Parenthood by Child Health Risk

Notes: Panel a shows estimates of β^t from Equation 1 comparing the within-person change in the likelihood of filling any psychiatric prescription in time period t (indexed by the x-axis) compared to Winter 2020 for parents of school-age children relative to non-parents. Panel b shows estimates of β_1^t and $(\beta_1^t + \sum_{r=1}^{3} \beta_{2r}^t H_i^r)$ from Equation 2 comparing the within-person change in the likelihood of filling any psychiatric prescription in time period t (indexed by the x-axis) compared to Winter 2020 for parents of children with varying risks relative to non-parents. Controls include age-period, gender-period, county-month-year, and individual fixed effects. Standard errors are clustered at the county-level. Point estimates for Panel a are shown in Table E.1. Point estimates for Panel b are shown in Table E.2.

4. Impact of School Closures on the Parenthood Penalty

We document substantial cross-sectional differences in psychiatric prescription fills between closure and non-closure counties at baseline (Table D.1). Adults in closure counties—both parents and non-parents—are significantly less likely to fill psychiatric prescriptions than their counterparts in non-closure counties (0.90% vs. 1.21% for all adults; 0.89% vs. 1.20% for parents; 0.93% vs. 1.23% for non-parents; all p-values< 0.001). These level differences preclude causal inference from cross-sectional comparisons across counties.

Crucially, however, the parenthood gap in baseline prescription propensity does not differ across closure and non-closure counties (see the last rows of Tables D.1 and D.2). This motivates a triple-differences (DDD) design that compares the evolution of the difference between parents and non-parents within a county over time, across counties with high versus low rates of school closures. Specifically, we estimate:

$$\begin{aligned}\mathtt{Y}_{icm} &= \alpha_i + \gamma_{cm} + \beta_1^t \mathtt{parent}_i + \sum_{r=1}^{3}[\beta_{2r}^t(\mathtt{parent}_i * \mathtt{H}_i^r)] \\ &\quad + \beta_3^t(\mathtt{parent}_i * \mathtt{closure}_c) + \sum_{r=1}^{3}[\beta_{4r}^t(\mathtt{parent}_i * \mathtt{H}_i^r * \mathtt{closure}_c)] \\ &\quad + \delta_1^t \mathtt{age}_i + \delta_2^t(\mathtt{age}_i * \mathtt{closure}_c) + \kappa_1^t \mathtt{gender}_i + \kappa_2^t(\mathtt{gender}_i * \mathtt{closure}_c) + \epsilon_{icm}\end{aligned} \tag{3}$$

where $\mathtt{closure}_c$ takes a value of one if county c has an above-median rate of school closures and takes a value of zero if county c has a below-median rate of school closures. All other variables are defined as before. As in Equations 1 and 2, this specification captures time-invariant individual heterogeneity with α_i, absorbs county-month variation in psychiatric prescription fills with γ_{cm}, and controls for systematic differences across adults of different demographics ($\mathtt{gender}_i$ and $\mathtt{age}_i$). In this specification, we further allow demographic heterogeneity to vary across closure and non-closure counties, as we explore whether parenthood effects vary across closure and non-closure counties by child health risk. Errors are clustered at the county level.

The sum $(\beta_1^t + \sum_{r=1}^{3} \beta_{2r}^t H_i^r)$ captures the change in prescription fills between parents and non-parents in non-closure counties by the health status of children. The sum $(\beta_1^t + \sum_{r=1}^{3} \beta_{2r}^t H_i^r + \beta_3^t + \sum_{r=1}^{3} \beta_{4r}^t H_i^r)$ captures the change in prescription fills between parents and non-parents in closure counties by the health status of children. We interpret $(\beta_3^t + \sum_{r=1}^{3} \beta_{4r}^t)$ to be the causal effect of school closures for parents of children with different risk profiles relative to similar non-parents living in the same counties.

This causal interpretation is afforded by controls for county-month confounders that may drive both prescription fills and school closure decisions (e.g., due to local infection rates or political/economic climates). By comparing mental health outcomes of parents and non-parent adults within the same county, we partial out unobserved county-level factors that affect all adults in closure versus non-closure counties. The identifying assumption is that, absent school closures, the parenthood gap in psychiatric prescription fills would have evolved similarly in closure and non-closure counties. We find empirical support for this assumption in the baseline period. While the pre-period is necessarily limited, because (by construction) individuals in our sample did not fill psychiatric prescriptions in 2019 prior to baseline, the similarity in baseline parenthood gaps across closure and non-closure counties (D.1) supports a causal interpretation of our DDD estimates. In Section 6, we further bolster this interpretation with a placebo test contrasting the mental health outcomes of non-parents and parents of children younger than 4 years old.

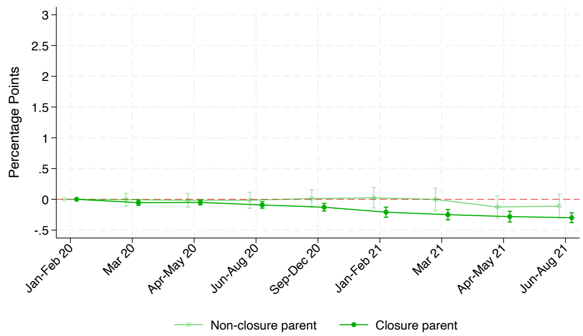
Figures 2a-2d plot estimates of $(\beta_1^t + \sum_{r=1}^{3} \beta_{2r}^t H_i^r + \beta_3^t + \sum_{r=1}^{3} \beta_{4r}^t H_i^r)$ for each category of child health risk (Table E.3). Across these categories, parents (compared to similar non-parents in the same county) in closure counties experience a lower parenthood penalty than their parent counterparts in non-closure counties. These differences, plotted in Figure 2e, are largest among parents of children with co-occurring risk. In Fall 2020, the increase in filled psychiatric prescriptions among parents of children with co-occurring risk, relative to non-parents, is 1.40 pp in non-closure counties but only 0.63 pp in closure counties (difference -0.77 pp, p-value = 0.007). In Winter 2021, this parenthood penalty is 1.80 pp in non-closure counties and 0.83 pp in closure counties (difference -0.96 pp, p-value = 0.006). Across the 2020/21 school year, we see 52-60% reductions in the parenthood mental health penalty among parents of of children with co-occurring risk in closure counties compared to non-closure counties.[13]

For parents of children with physical or mental risk only, directionally aligned estimates suggest that school closures reduced mental health penalties for these parents as well. However, these differences are generally not significant. For parents of children not at risk living in closure counties, beginning in Fall 2020, we observe a decrease in prescription likelihood of -0.14 pp (p-value= 0.073) that climbs to -0.25 pp (p-value= 0.013) in March 2021. Notably, for this group, parents in non-closure counties do not face mental health penalties relative to similar non-parents. Therefore, negative estimates of closure effects may be interpreted as mental health gains among parents.

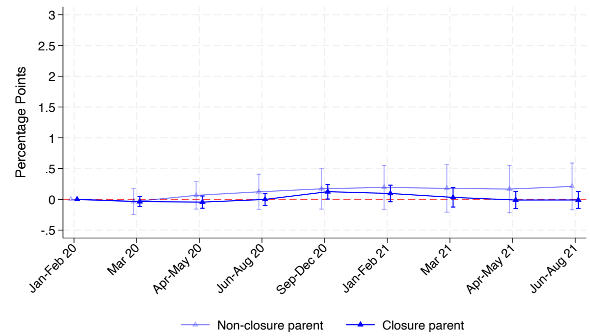
[13]Estimates for main effects without heterogeneity by child health status are plotted in Figure F.1. The reduction in the parenthood mental health penalty among all parents ranges from 81% to 112% in the 2020/21 school year.

In summary, school closures appear to be protective of parental mental health. For parents with children at risk, school closures counteract parenthood penalties on mental health that are otherwise significant during the pandemic. For parents with children not at risk, for whom mental health penalties were not experienced, school closures may benefit parental mental health.

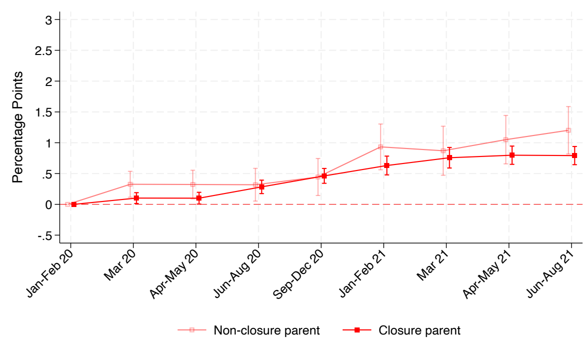
Figure 2: Δ Likelihood of Any Psychiatric Prescription Fill for Naive Parents vs. Non-parents by Closure



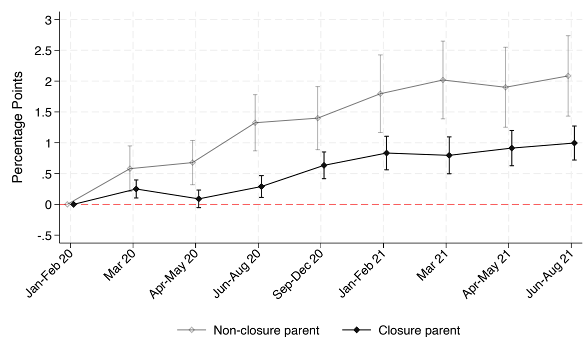
(a) Parents of Children Not at Risk



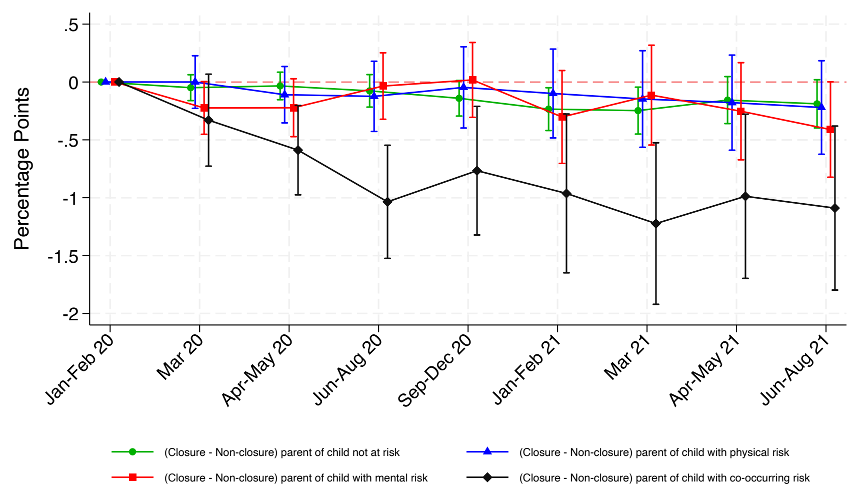
(b) Parents of Children with Physical Risk



(c) Parents of Children with Mental Risk



(d) Parents of Children with Co-occurring Risk



(e) (Closure - Non-closure) Parenthood by Child Health Risk

Notes: Panels a-d show estimates of $(\beta_1^t + \sum_{r=1}^{3} \beta_{2r}^t H_i^r)$ and $(\beta_1^t + \sum_{r=1}^{3} \beta_{2r}^t H_i^r + \beta_3^t + \sum_{r=1}^{3} \beta_{4r}^t H_i^r)$ from Equation 3 comparing the within-person change in the likelihood of filling any psychiatric prescription in time period t (indexed by the x-axis) compared to Winter 2020 for parents of children with varying risks relative to non-parents in closure and non-closure counties. Panel e shows estimates of $(\beta_3^t + \sum_{r=1}^{3} \beta_{4r}^t H_i^r)$, which capture the closure effects for different groups of parents versus non-parent adults. Controls include age-closure-period, gender-closure-period, county-month fixed effects, and individual fixed effects. Standard errors are clustered at the county-level. Point estimates for Panels a-d are shown in Table E.3.

5. Heterogeneity & Subgroup Analyses

In this section, we discuss additional heterogeneity and subgroup analyses that we conducted to help uncover drivers of parenthood effects observed in Sections 3 and 4. We first decompose parenthood effects by parental gender and compare motherhood and fatherhood effects. We also consider parenthood effects for adults with pre-pandemic psychiatric exposure to understand the extent to which the parenthood effects we identify above are unique to naive rather than experienced users of psychiatric healthcare.

5.1. Stronger Parental Mental Health Effects for Mothers than Fathers

In Appendix G, we study differential mental health patterns among mothers and fathers. Several studies suggest that women experienced the pandemic's negative effects more acutely than men (Alonzi et al., 2020; Flor et al., 2022; Martínez et al., 2022). Because women were more likely to shoulder caretaking responsibilities (Flor et al., 2022) and tend to be more affected by their children's well-being (Zahl et al., 2024), we tested whether the parental gap we document in Section 3 was experienced differentially by mothers versus fathers. In particular, we allow the parenthood gap to vary by gender by interacting parenthood definitions by gender in Equation G.1. Figure G.1a reports the difference in the increase in psychiatric prescriptions filled by mothers versus similar non-parent women living in the same county compared to the baseline period. Figure G.1b reports the same difference for fathers versus similar non-parent men. We plot the estimated difference between motherhood and fatherhood effects in Figure G.1c. We observe substantial gender differences within the parental gap. Among parents of children with co-occurring risk, mothers (compared to non-parent women) experience a greater parenthood penalty than fathers (compared to non-parent men) of up to 0.86 pp (p-values < 0.01 from Fall 2020 through Summer 2021). Differences between mothers and fathers are similar in direction and magnitude for parents of children with physical risk only (p-values < 0.05 in March 2020 and from Summer 2020 through Fall 2020, p-value < 0.1 in Winter 2021) and with mental risk only (p-values < 0.05 from Spring 2020 through Summer 2021). Similarly, among parents of children not at risk, mothers experience a greater parenthood advantage than fathers, as large as -0.37 pp (p-value < 0.05 in Fall 2020, p-values < 0.01 from Winter 2021 through Summer 2021).[14] In summary, pandemic parenthood effects are experienced more strongly by mothers than by fathers.

[14] Therefore, it is unlikely that gender-level differences in psychiatric prescription behavior explain the larger motherhood penalty among parents of at-risk children.

Next, we examine the heterogeneous effects of school closures on parental mental health by parental gender and estimate Equation G.2. We find that mothers of children with co-occurring risk experience strong and significant protective effects of school closures at a magnitude nearly twice that of the average effect (comparing Figure G.2e to Figure 2e and Figure G.2d to Figure 2d). Fathers of these children suggestively experience small protective effects from closures too, although these estimates are not significant (Figures G.3d-G.3e). In Figure G.4, we directly compare closure effects for mothers and fathers and observe that mothers of children with physical risk (p-values < 0.05 from Winter 2021 through Summer 2021) and of children with co-occurring risk (p-values < 0.05 from Summer 2020 through Summer 2021) experience significantly stronger closure effects than fathers of these children. We conclude that the protective effects of school closure are experienced more strongly by mothers than by fathers.

5.2. Muted Parental Mental Health Effects for Experienced Adults

In our main analyses, we focus on naive adults. In Appendix H, we provide summary statistics on experienced adults (Tables H.1-H.2)—those with psychiatric healthcare exposure in 2019—and repeat all analyses for this cohort, comparing experienced parents of school-age children (216,446 beneficiaries) to experienced non-parent adults (663,173 beneficiaries). These findings provide insight on the intensive margin. That is, among adults with pre-existing mental health conditions, we estimate the differential likelihood of receiving additional treatment (i.e., filling any psychiatric prescriptions) for parents versus non-parents. Because these adults have had recent, if not continuing, psychiatric treatment (medication management or otherwise), we may expect any psychological effects from the pandemic on mental health to be dampened, relative to naive adults. By a similar logic, we might also expect the mental health of experienced parents to be less reactive to school closures.

The results depicted by Figure H.1a indicate that the likelihood of filling psychiatric prescriptions for experienced parents is slightly higher than that of experienced non-parents throughout the pandemic. However, the magnitude of the parenthood penalty is considerably smaller than that observed among naive adults and is significant only in a few time periods. Coinciding with the start of the school year in Fall 2020, Figure H.1b also reveals that the parenthood penalty is mainly observed for parents of children with co-occurring risk. Again, even among this group of parents, the parenthood penalty is muted compared to estimates obtained from naive adults. For example, in Winter 2021, experienced parents of children with co-occurring risk faced an additional likelihood of filling psychiatric prescriptions of 1.19 pp (p-value < 0.001), which translates to a relative increase of 54% compared to the baseline period. However, in this same period, naive parent counterparts faced an additional likelihood of 1.01 pp (p-value < 0.001), which translates to a much larger relative increase of 252% compared to the baseline period. Parents of children with physical risk and parents of children not at risk do not differ significantly from non-parents. Finally, in contrast to that of naive parents, the mental health of experienced parents does not react to school closures. We estimate null effects across categories of children's health risks (Figure H.2).

These results suggest considerable differences for parents with prior exposure to psychiatric healthcare. In particular, the parenthood penalty is substantially larger among naive adults, and it is only naive parents who benefit from school closures.

6. Falsification and Placebo Tests

6.1. Attribution of Psychiatric Prescription Fills to Mental Health

We interpret psychiatric prescriptions as revealed measures of underlying mental health needs. However, the pandemic may have altered healthcare access and/or treatment-seeking behavior in ways that differ systematically across parents and non-parents. If so, observed differences in prescription fills might reflect changes in access or utilization rather than changes in mental health.

For such an alternative explanation to account for our findings in Section 3, parents—particularly parents of at-risk children—would need to have experienced lower barriers to accessing healthcare for themselves during the pandemic than non-parents or parents of healthier children. Such circumstances could feasibly occur from differentially more points of contact with the healthcare system. This mechanism would also predict parallel gaps among parents and non-parents in the treatment of other chronic conditions requiring continued medical management.

To further assess this possibility, we conduct a falsification test using diabetes prescription fills as a proxy for healthcare utilization unrelated to mental health (Appendix I). Estimating the same specifications as in Equations 1 and 2, we find that parents, especially parents of at-risk children, are less likely than non-parents to fill diabetes prescriptions during the pandemic (Figure I.1). This pattern is inconsistent with an explanation based on differentially higher healthcare access among parents, and instead suggests that our estimates of parenthood mental health penalties may actually understate underlying mental health burdens.

We further examine whether the attenuated parenthood penalty observed in closure counties, which we describe in Section 4, could reflect differential healthcare access across counties. Our triple-differences design compares parents and non-parents within the same county and month, thereby netting out county-level access differences. However, this explanation could still be plausible if school closures disproportionately reduced parents' propensity to seek healthcare relative to non-parents. Re-estimating Equation 3 using diabetes prescriptions as the outcome, we find no evidence that parents of children of any risk type are less likely to fill diabetes prescriptions in closure counties (Figure I.2).[15] These results suggest that differential healthcare access or utilization cannot explain the protective effect of school closures on parental mental health.

Taken together, these findings support an interpretation in which the observed differences in psychiatric prescription fills reflect changes in mental health rather than shifts in access to care or treatment-seeking behavior.

6.2. Attribution to School Closures

We attribute differences in the parenthood penalty across counties with high versus low school closure intensity to the effects of school closures. Factors that vary across counties over time—such as pandemic severity, local policy, or economic disruption—are controlled for through county-month fixed effects. While unlikely, a remaining concern could be that other community-level factors might simultaneously vary systematically with school-closure intensity and differentially affect parents relative to non-parents. For instance, during the pandemic, other non-pharmaceutical interventions are likely to have occurred in counties with high rates of school closures, and some of these regulatory restrictions may have affected childcare availability or socialization outlets for families. Similarly, certain pandemic factors (e.g., worries about contagion among children) may have driven school closures and affected parents more than non-parents. In these cases, we would reasonably expect a negative impact of these factors on the well-being of parents compared to non-parents. Instead, we document that school closures have a protective effect.

To further investigate this concern, we conduct a placebo test focusing on parents of children younger than four years old (Appendix I), since these children are likely ineligible for elementary

[15] We conduct further robustness checks studying adults with (i.e., diabetes-experienced) and without (i.e., diabetes-naive) diabetes exposure in 2019 and arrive at a similar conclusion (Figure I.3).

school in 2020/21.[16] These families are exposed to the same local pandemic conditions and regulatory environments, but their children are not directly affected by school closures.[17] Therefore, we should expect no differences in the evolution of parental mental health for this group across counties with and without school closures.

In the baseline period, the psychiatric prescription hazard among parents of young children is not significantly different from that of non-parents on average (0.91% vs 0.94%, p-value= 0.279), and this difference between parents and non-parents is not significantly different across closure and non-closure counties (-0.06, p-value= 0.445). These parents also experience substantial mental health penalties relative to non-parents during the pandemic (Figure I.4), at magnitudes comparable to those observed for parents of children with co-occurring risk. However, we find no evidence that these penalties vary systematically with school-closure intensity (Figure I.5). The absence of differential effects by closure intensity for this placebo group provides strong support for the identifying assumption underlying our DDD design—namely, in the absence of school closures, the parenthood gap in psychiatric prescription hazard would have evolved similarly in closure and non-closure counties. This finding also mitigates concerns that our main results are driven by broader community-level shocks correlated with school closures that might differentially affect parents and non-parents.

7. Conclusion

A large and growing body of correlational evidence documents substantial and persistent declines in mental health during the COVID-19 pandemic, yet there remains limited causal evidence on how local pandemic policies affected mental health outcomes. In this paper, we leverage variation in K–12 school closures, one of the most salient and widely implemented policy responses, to study mental health spillovers for parents, a vulnerable and particularly affected population. As policymakers continue to develop legislation with children as the focal population, understanding spillovers for parents is particularly important.

We find that during the pandemic, parents—both with and without prior psychiatric experience—

[16] We do not use parents of children older than 18 years of age as a placebo group, because many of these children may have completed secondary education during our study's time period, and we do not know whether these children still reside with their parents. We also do not use parents of children ages 4 or 5 years old, because these children may not have been old enough for exposure to primary school closures during the 2020/21 school year.

[17] We cannot reasonably consider heterogeneity by child health risk, because fewer than $10,000$ of these parents across almost $2,000$ counties have children with any health risks.

were more likely than comparable non-parents to fill psychiatric prescriptions. This parenthood penalty was especially pronounced among mothers and parents of children with pre-existing health conditions, groups likely to shoulder greater caregiving responsibilities. These differences emerged primarily during the school year and were responsive to the intensity of school closures. For parents without a prior history of mental health issues, the parenthood penalty was smaller in counties with higher rates of school closures than in counties with lower closure rates, suggesting that school closures provided at least short-term protection against the onset of formal care needs for mental health. In contrast, among parents with prior psychiatric experience, school closures did not significantly attenuate the parenthood penalty. Taken together, these patterns suggest that school closures primarily affected the incidence of psychiatric care, rather than its prevalence or ongoing treatment among parents facing greater childcare obligations. Because mental health conditions are chronic and generally require maintenance therapy, shocks must be sufficiently large and/or enduring to affect both flow and stock of patients requiring psychiatric care. The pandemic itself seems to have been such a shock, while school closures were perhaps too short-term to reduce the psychiatric care needs among parents already managing their mental health.

Our findings speak to the short-term mental health consequences of K–12 school closures for adults during the COVID-19 pandemic. They do not speak to longer-term adaptation, to children's mental health or educational outcomes, or to the overall optimality of school closure policies in a public health emergency. Rather, our results highlight meaningful heterogeneity in the externalities of local policy and identify parents, particularly mothers and parents of medically vulnerable children, as a distinct stakeholder group bearing disproportionate mental health costs. These insights underscore the importance of accounting for parental well-being when designing household-facing policies and caution against treating families as a homogeneous unit. Social welfare policies that aim to support children—particularly those that normatively seek to help those most in need—may generate uneven spillovers within households. Our conclusions suggest a role for complementary interventions targeted at families facing greater caregiving responsibilities in future crises.

Declaration of interests

The authors declare no competing interests relevant to the research in this paper. Pursuant to the University of Michigan's data use agreement, CDM reviewed this paper prior to its circulation.

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Data availability

The U.S. School Closure & Distance Learning Database has a public-use version and may be found at https://osf.io/tpwqf/. Optum's de-identified Clinformatics® Data Mart Database (CDM) is confidential and accessible through a data use agreement.

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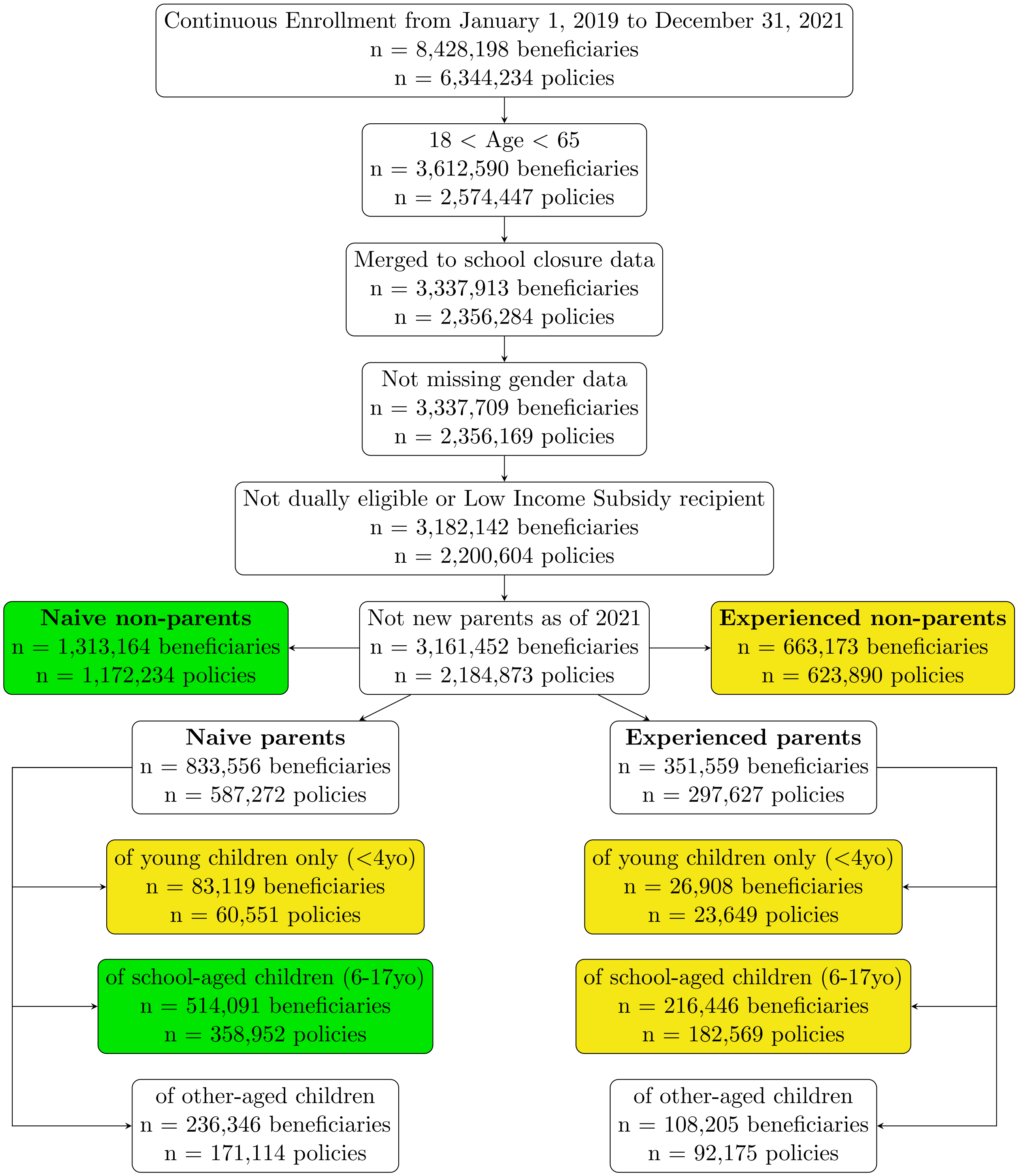
Appendix A. Cohort Selection Flowchart



Notes: Flowchart shows beneficiary and policy counts for each filter. Green boxes indicate main analytic sample. Yellow boxes indicate samples used for supplementary analyses.

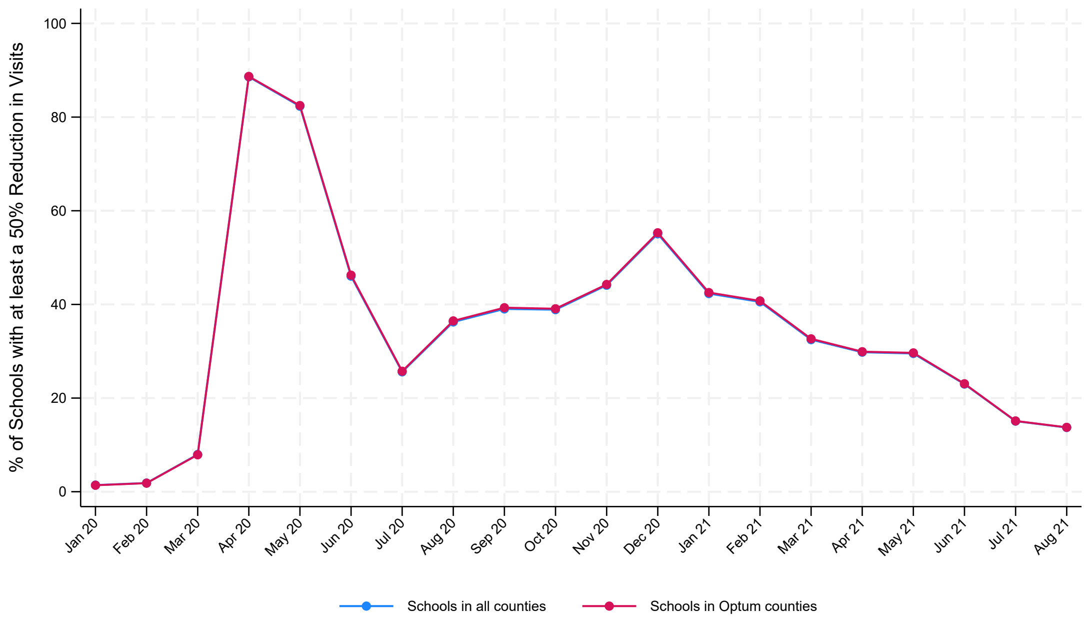
Appendix B. Claims-based Variable Construction

Drug Category	**Drug Names**
Antidepressants	Isocarboxazid; Phenelzine; Tranylcypromine; Rasagiline; Selegiline; Desvenlafaxine; Duloxetine; Levomilnacipran; Venlafaxine; Milnacipran; Citalopram; Escitalopram; Fluoxetine; Fluvoxamine; Paroxetine; Sertraline; Nefazodone; Trazodone; Vilazodone; Vortioxetine; Amitriptyline; Amoxapine; Clomipramine; Desipramine; Doxepin; Imipramine; Maprotiline; Nortriptyline; Protriptyline; Trimipramine; Bupropion; Mirtazapine
Anxiolytics	Amobarbital; Butabarbital; Pentobarbital; Phenobarbital; Secobarbital; Alprazolam; Chlordiazepoxide; Clorazepate; Diazepam; Estazolam; Flurazepam; Lorazepam; Midazolam; Oxazepam; Quazepam; Temazepam; Triazolam; Clobazam; Clonazepam; Prazepam; Halazepam; Buspirone; Chloral Hydrate; Dexmedetomidine; Droperidol; Eszopiclone; Hydroxyzine; Meprobamate; Promethazine; Ramelteon; Suvorexant; Tasimelteon; Zaleplon; Zolpidem; Diphenhydramine; Doxylamine; Propofol
Benzodiazepines	Alprazolam; Chlordiazepoxide; Clorazepate; Diazepam; Estazolam; Flurazepam; Lorazepam; Midazolam; Oxazepam; Quazepam; Temazepam; Triazolam; Clobazam; Clonazepam; Prazepam; Halazepam
Z-Drugs	Eszopiclone; Zaleplon; Zolpidem
Service Category	**CPT Codes**
Psychiatric	90785; 90791; 90792; 90832; 90833; 90834; 90836; 90837; 90838; 90839; 90840; 90845; 90846; 90847; 90849; 90853; 90865; 90885; 90887; 90889; 90899; 90880; 99281; 99282; 99283; 99284; 99285; 99201; 99202; 99203; 99204; 99205; 99211; 99212; 99213; 99214; 99215
Diagnosis Category	**ICD-10-CM Codes**
Psychiatric	F*
— Mood	F3*
— Anxiety	F4*; F5*
— Developmental	F7*; F8*; F9* excluding F99*
Cancer	C*
Respiratory	J* excluding acute conditions (J0*; J1*; J2*) and allergies (J30*)
Cardiovascular	I* excluding essential hypertension (I10*)

Notes: This table provides generic drug names used for identification of psychiatric prescriptions, Current Procedural Terminology (CPT) codes used for determination of psychiatric services, and International Classification of Diseases, Tenth Revision, Clinical Modification (ICD-10-CM) codes used for classification of relevant psychiatric, cancer, respiratory, and cardiovascular diagnoses.

Appendix C. School Closure Descriptive Statistics

Figure C.1: Percent of Schools with Closures



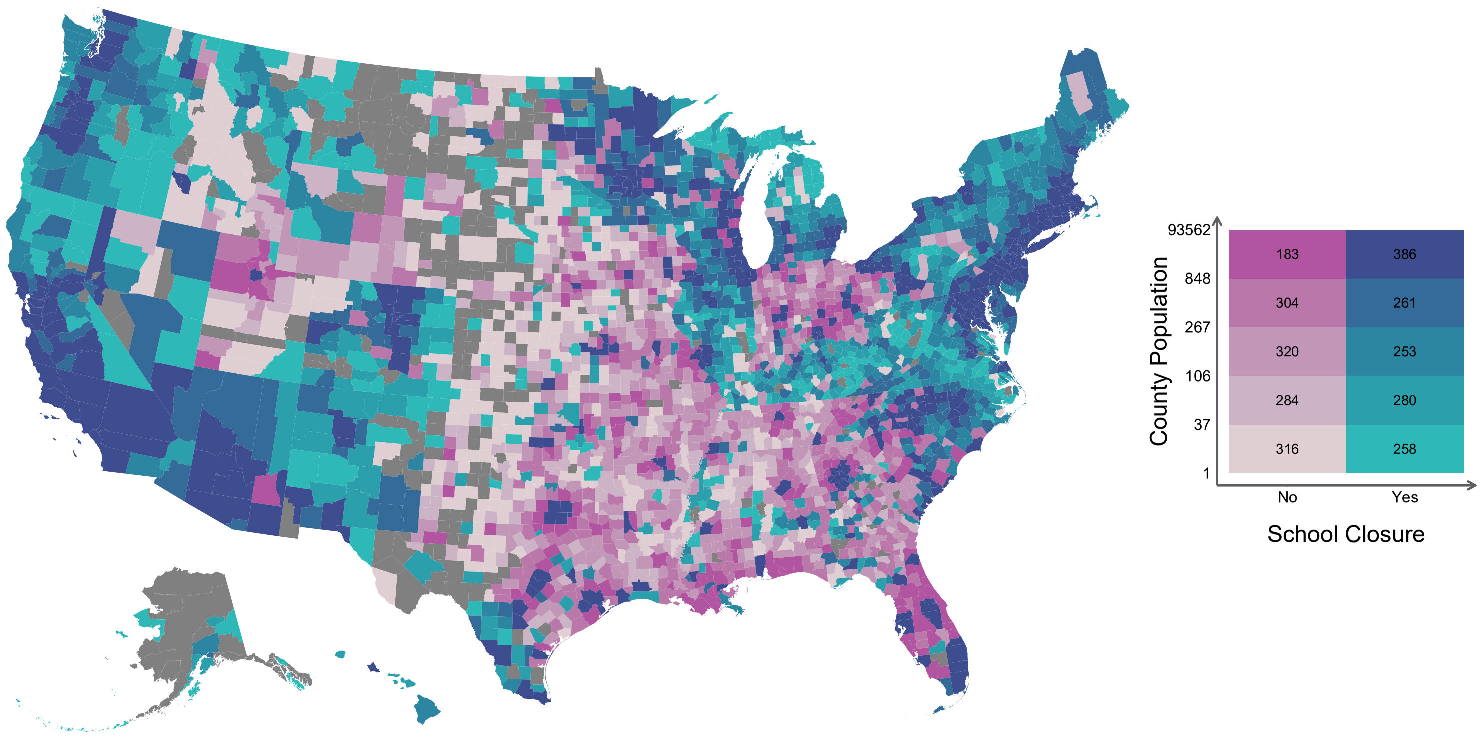
Notes: Figure plots the percentage of schools with at least a 50% reduction in visits across all counties in the U.S. School Closure & Distance Learning Database and across all counties in our CDM cohort.

Table C.1: Monthly Availability of School Visit Data

Time Period	% Schools	% Counties
January 2020	48.862	99.774
February 2020	48.670	99.839
March 2020	48.569	99.742
April 2020	47.730	99.774
May 2020	47.641	99.774
June 2020	47.465	99.774
July 2020	47.070	99.806
August 2020	47.349	99.806
September 2020	47.437	99.806
October 2020	43.893	99.580
November 2020	43.610	99.548
December 2020	43.507	99.515
January 2021	42.387	99.451
February 2021	42.330	99.451
March 2021	42.386	99.451
April 2021	42.256	99.515
May 2021	42.199	99.612
June 2021	42.298	99.515
July 2021	45.808	99.645
August 2021	46.072	99.645

Notes: This table provides monthly availability of school-level visit data relative to the total number of schools (n = 204,625) and total number of counties (n = 3,095) in the data.

Figure C.2: Cohort Geographic Variation



Notes: This map illustrates the county-level population of beneficiaries observed by quintile in closure and non-closure counties. Gray areas indicate no data.

Appendix D. Cohort Demographics

Table D.1: Demographics of Non-parents and Parents

Total	Non-parents (N=1,313,164)	Parents (N=514,091)	Test	Total (N=1,827,255)
Age	42.333 (14.859)	43.659 (7.328)	<0.001	42.706 (13.196)
19-34yo	0.369 (0.482)	0.098 (0.297)	<0.001	0.292 (0.455)
35-49yo	0.226 (0.418)	0.683 (0.465)	<0.001	0.354 (0.478)
50-64yo	0.406 (0.491)	0.219 (0.414)	<0.001	0.353 (0.478)
Female	0.434 (0.496)	0.455 (0.498)	<0.001	0.440 (0.496)
Family Size	1.757 (1.216)	4.186 (1.194)	<0.001	2.441 (1.630)
School Closure	0.819 (0.385)	0.823 (0.382)	<0.001	0.820 (0.384)
Baseline Rx (%)	0.942 (8.351)	0.981 (8.485)	0.005	0.953 (8.389)
Non-Closure	Non-parents (N=237,254)	Parents (N=90,902)	Test	Total (N=328,156)
Age	43.327 (15.184)	42.346 (7.444)	<0.001	43.055 (13.499)
19-34yo	0.341 (0.474)	0.139 (0.346)	<0.001	0.285 (0.451)
35-49yo	0.209 (0.407)	0.687 (0.464)	<0.001	0.342 (0.474)
50-64yo	0.450 (0.498)	0.174 (0.379)	<0.001	0.374 (0.484)
Female	0.416 (0.493)	0.429 (0.495)	<0.001	0.420 (0.494)
Family Size	1.821 (1.275)	4.312 (1.306)	<0.001	2.511 (1.700)
Baseline Rx (%)	1.203 (9.422)	1.225 (9.516)	0.558	1.209 (9.448)
Closure	Non-parents (N=1,075,910)	Parents (N=423,189)	Test	Total (N=1,499,099)
Age	42.114 (14.778)	43.941 (7.272)	<0.001	42.629 (13.128)
19-34yo	0.375 (0.484)	0.089 (0.285)	<0.001	0.294 (0.456)
35-49yo	0.229 (0.420)	0.682 (0.466)	<0.001	0.357 (0.479)
50-64yo	0.396 (0.489)	0.229 (0.420)	<0.001	0.349 (0.477)
Female	0.438 (0.496)	0.460 (0.498)	<0.001	0.445 (0.497)
Family Size	1.743 (1.203)	4.159 (1.167)	<0.001	2.425 (1.614)
Baseline Rx (%)	0.885 (8.095)	0.929 (8.246)	0.003	0.897 (8.138)
Δ Baseline Rx (pp)	reference	0.022 (0.036)	0.537	-0.312 (0.016)

Notes: Table provides means (standard deviations) for naive parents of school-age children and non-parents by whether they reside in a school closure county. Baseline Rx refers to the likelihood of any psychiatric prescription fill in the baseline period of January/February 2020. Tests compare parents to non-parents, and table reports p-values of t-tests. The final row of the table (Δ Baseline Rx) tests the baseline difference between parents and non-parents in closure counties against the baseline difference between parents and non-parents in non-closure counties.

Table D.2: Demographics of Non-parents and Parents by Child Health Risk

Total	Non-parents (N=1,313,164)	Not at risk (N=336,594)	Test	Physical risk (N=75,208)	Test	Mental risk (N=72,109)	Test	Co-occurring risk (N=30,180)	Test
Age	42.333 (14.859)	43.766 (7.381)	<0.001	42.796 (7.105)	<0.001	44.244 (7.261)	<0.001	43.214 (7.233)	<0.001
19-34yo	0.369 (0.482)	0.099 (0.299)	<0.001	0.108 (0.311)	<0.001	0.081 (0.272)	<0.001	0.100 (0.300)	<0.001
35-49yo	0.226 (0.418)	0.674 (0.469)	<0.001	0.716 (0.451)	<0.001	0.680 (0.467)	<0.001	0.709 (0.454)	<0.001
50-64yo	0.406 (0.491)	0.227 (0.419)	<0.001	0.176 (0.381)	<0.001	0.240 (0.427)	<0.001	0.192 (0.394)	<0.001
Female	0.434 (0.496)	0.464 (0.499)	<0.001	0.458 (0.498)	<0.001	0.427 (0.495)	<0.001	0.415 (0.493)	<0.001
Family Size	1.757 (1.216)	4.100 (1.166)	<0.001	4.329 (1.178)	<0.001	4.280 (1.221)	<0.001	4.568 (1.339)	<0.001
School Closure	0.819 (0.385)	0.824 (0.380)	<0.001	0.825 (0.380)	<0.001	0.818 (0.386)	0.403	0.816 (0.387)	0.168
Baseline Rx (%)	0.942 (8.351)	0.887 (8.047)	<0.001	1.084 (8.916)	<0.001	1.160 (9.310)	<0.001	1.342 (9.930)	<0.001
Non-Closure	Non-parents (N=237,254)	Not at risk (N=59,085)	Test	Physical risk (N=13,154)	Test	Mental risk (N=13,117)	Test	Co-occurring risk (N=5,546)	Test
Age	43.327 (15.184)	42.546 (7.482)	<0.001	41.356 (7.235)	<0.001	42.757 (7.397)	<0.001	41.597 (7.381)	<0.001
19-34yo	0.341 (0.474)	0.137 (0.344)	<0.001	0.162 (0.368)	<0.001	0.119 (0.324)	<0.001	0.155 (0.362)	<0.001
35-49yo	0.209 (0.407)	0.680 (0.466)	<0.001	0.701 (0.458)	<0.001	0.697 (0.459)	<0.001	0.702 (0.457)	<0.001
50-64yo	0.450 (0.498)	0.183 (0.386)	<0.001	0.137 (0.344)	<0.001	0.184 (0.387)	<0.001	0.143 (0.350)	<0.001
Female	0.416 (0.493)	0.439 (0.496)	<0.001	0.435 (0.496)	<0.001	0.397 (0.489)	<0.001	0.383 (0.486)	<0.001
Family Size	1.821 (1.275)	4.218 (1.265)	<0.001	4.471 (1.286)	<0.001	4.399 (1.342)	<0.001	4.741 (1.534)	<0.001
Baseline Rx (%)	1.203 (9.422)	1.118 (9.040)	0.047	1.368 (10.170)	0.052	1.433 (10.306)	0.007	1.533 (10.802)	0.010
Closure	Non-parents (N=1,075,910)	Not at risk (N=277,509)	Test	Physical risk (N=62,054)	Test	Mental risk (N=58,992)	Test	Co-occurring risk (N=24,634)	Test
Age	42.114 (14.778)	44.026 (7.333)	<0.001	43.101 (7.039)	<0.001	44.575 (7.189)	<0.001	43.578 (7.149)	<0.001
19-34yo	0.375 (0.484)	0.091 (0.288)	<0.001	0.097 (0.296)	<0.001	0.072 (0.259)	<0.001	0.087 (0.282)	<0.001
35-49yo	0.229 (0.420)	0.673 (0.469)	<0.001	0.719 (0.449)	<0.001	0.676 (0.468)	<0.001	0.710 (0.454)	<0.001
50-64yo	0.396 (0.489)	0.236 (0.425)	<0.001	0.184 (0.387)	<0.001	0.252 (0.434)	<0.001	0.203 (0.402)	<0.001
Female	0.438 (0.496)	0.469 (0.499)	<0.001	0.463 (0.499)	<0.001	0.433 (0.496)	0.022	0.422 (0.494)	<0.001
Family Size	1.743 (1.203)	4.075 (1.143)	<0.001	4.299 (1.151)	<0.001	4.254 (1.191)	<0.001	4.529 (1.288)	<0.001
Baseline Rx (%)	0.885 (8.095)	0.838 (7.818)	0.007	1.024 (8.626)	<0.001	1.099 (9.073)	<0.001	1.299 (9.722)	<0.001
Δ Baseline Rx (pp)	reference	0.039 (0.042)	0.354	-0.026 (0.083)	0.754	-0.016 (0.083)	0.852	0.085 (0.126)	0.501

Notes: Table provides means (standard deviations) for naive adult parents of 6-17 year-old children and non-parents by child health risk and whether they reside in a school closure county. Baseline Rx refers to the likelihood of any psychiatric prescription fill in the baseline period of January/February 2020. Tests compare parents of children with different health risk types to non-parents, and table reports p-values of t-tests. The final row of the table (Δ Baseline Rx) tests the baseline difference between parents and non-parents in closure counties against the baseline difference between parents and non-parents in non-closure counties.

Appendix E. Coefficient Tables

Table E.1: Relevant Estimates from Equation 1

	Estimate	95% CI	P-value
β^t			
1	0.000	(., .)	.
2	0.002	(-0.033, 0.038)	0.891
3	-0.002	(-0.040, 0.035)	0.898
4	0.018	(-0.024, 0.061)	0.396
5	0.058	(0.010, 0.106)	0.019
6	0.058	(-0.005, 0.120)	0.069
7	0.039	(-0.027, 0.104)	0.245
8	0.015	(-0.048, 0.077)	0.642
9	0.015	(-0.047, 0.078)	0.633
N	43853376		
R^2	0.435		

Notes: Table shows linear combinations of estimates (percentage points) and associated 95% confidence intervals and p-values from Equation 1. Numbers $1-9$ correspond to time periods t, where 1 refers to Winter 2020 (baseline), 2 refers to March 2020, 3 refers to Spring 2020, 4 refers to Summer 2020, 5 refers to Fall 2020, 6 refers to Winter 2021, 7 refers to March 2021, 8 refers to Spring 2021, and 9 refers to Summer 2021. These estimates mirror those illustrated in Figure 1a.

Table E.2: Relevant Estimates from Equation 2

	Estimate	95% CI	P-value
β_1^t			
1	0.000	(., .)	.
2	-0.045	(-0.086, -0.005)	0.027
3	-0.045	(-0.084, -0.005)	0.026
4	-0.079	(-0.128, -0.030)	0.002
5	-0.105	(-0.162, -0.048)	0.000
6	-0.169	(-0.243, -0.096)	0.000
7	-0.207	(-0.283, -0.130)	0.000
8	-0.255	(-0.333, -0.177)	0.000
9	-0.269	(-0.342, -0.195)	0.000
$\beta_1^t + \beta_{21}^t$			
1	0.000	(., .)	.
2	-0.037	(-0.114, 0.040)	0.343
3	-0.027	(-0.117, 0.064)	0.564
4	0.019	(-0.077, 0.115)	0.699
5	0.132	(0.018, 0.246)	0.024
6	0.111	(-0.017, 0.240)	0.088
7	0.057	(-0.089, 0.202)	0.446
8	0.018	(-0.116, 0.153)	0.790
9	0.025	(-0.106, 0.155)	0.709
$\beta_1^t + \beta_{22}^t$			
1	0.000	(., .)	.
2	0.142	(0.060, 0.224)	0.001
3	0.139	(0.051, 0.228)	0.002
4	0.288	(0.187, 0.390)	0.000
5	0.456	(0.344, 0.569)	0.000
6	0.682	(0.540, 0.825)	0.000
7	0.775	(0.621, 0.929)	0.000
8	0.841	(0.700, 0.981)	0.000
9	0.862	(0.722, 1.001)	0.000
$\beta_1^t + \beta_{23}^t$			
1	0.000	(., .)	.
2	0.309	(0.172, 0.447)	0.000
3	0.196	(0.061, 0.331)	0.004
4	0.477	(0.306, 0.648)	0.000
5	0.771	(0.570, 0.973)	0.000
6	1.006	(0.754, 1.258)	0.000
7	1.017	(0.744, 1.290)	0.000
8	1.090	(0.829, 1.352)	0.000
9	1.189	(0.934, 1.444)	0.000
N	43853376		
R^2	0.435		

Notes: Table shows linear combinations of estimates (percentage points) and associated 95% confidence intervals and p-values from Equation 2. Numbers $1-9$ correspond to time periods t, where 1 refers to Winter 2020 (baseline), 2 refers to March 2020, 3 refers to Spring 2020, 4 refers to Summer 2020, 5 refers to Fall 2020, 6 refers to Winter 2021, 7 refers to March 2021, 8 refers to Spring 2021, and 9 refers to Summer 2021. These estimates mirror those illustrated in Figure 1b.

Table E.3: Relevant Estimates from Equation 3

	Estimate	95% CI	P-value
β_1^t			
1	0.000	(., .)	.
2	-0.005	(-0.108, 0.098)	0.926
3	-0.016	(-0.128, 0.096)	0.780
4	-0.015	(-0.145, 0.115)	0.822
5	0.012	(-0.128, 0.153)	0.863
6	0.026	(-0.140, 0.191)	0.762
7	-0.002	(-0.186, 0.182)	0.984
8	-0.124	(-0.308, 0.059)	0.184
9	-0.110	(-0.302, 0.082)	0.260
$\beta_1^t + \beta_{21}^t$			
1	0.000	(., .)	.
2	-0.037	(-0.249, 0.176)	0.735
3	0.065	(-0.158, 0.288)	0.569
4	0.122	(-0.164, 0.409)	0.403
5	0.171	(-0.158, 0.501)	0.308
6	0.195	(-0.165, 0.554)	0.288
7	0.178	(-0.208, 0.565)	0.366
8	0.167	(-0.219, 0.553)	0.396
9	0.210	(-0.170, 0.590)	0.279
$\beta_1^t + \beta_{22}^t$			
1	0.000	(., .)	.
2	0.326	(0.116, 0.536)	0.002
3	0.322	(0.091, 0.553)	0.006
4	0.318	(0.053, 0.584)	0.019
5	0.444	(0.144, 0.744)	0.004
6	0.933	(0.561, 1.304)	0.000
7	0.870	(0.472, 1.268)	0.000
8	1.050	(0.657, 1.443)	0.000
9	1.202	(0.817, 1.587)	0.000
$\beta_1^t + \beta_{23}^t$			
1	0.000	(., .)	.
2	0.579	(0.210, 0.949)	0.002
3	0.678	(0.319, 1.037)	0.000
4	1.325	(0.869, 1.780)	0.000
5	1.399	(0.888, 1.911)	0.000
6	1.795	(1.165, 2.425)	0.000
7	2.018	(1.388, 2.649)	0.000
8	1.900	(1.251, 2.549)	0.000
9	2.084	(1.432, 2.737)	0.000
$\beta_1^t + \beta_3^t$			
1	0.000	(., .)	.
2	-0.054	(-0.098, -0.010)	0.016
3	-0.050	(-0.091, -0.009)	0.018
4	-0.091	(-0.144, -0.039)	0.001
5	-0.128	(-0.191, -0.065)	0.000
6	-0.209	(-0.291, -0.126)	0.000
7	-0.249	(-0.333, -0.165)	0.000
8	-0.281	(-0.368, -0.194)	0.000
9	-0.299	(-0.378, -0.219)	0.000
$\beta_1^t + \beta_{21}^t + \beta_3^t + \beta_{41}^t$			

	Estimate	95% CI	P-value
1	0.000	(., .)	.
2	-0.037	(-0.118, 0.044)	0.374
3	-0.045	(-0.143, 0.053)	0.371
4	-0.002	(-0.101, 0.097)	0.973
5	0.125	(0.005, 0.245)	0.041
6	0.095	(-0.040, 0.231)	0.166
7	0.032	(-0.125, 0.188)	0.692
8	-0.011	(-0.152, 0.130)	0.876
9	-0.010	(-0.146, 0.126)	0.882
$\beta_1^t + \beta_{22}^t + \beta_3^t + \beta_{42}^t$			
1	0.000	(., .)	.
2	0.102	(0.014, 0.190)	0.023
3	0.100	(0.005, 0.195)	0.040
4	0.284	(0.174, 0.393)	0.000
5	0.462	(0.341, 0.582)	0.000
6	0.630	(0.478, 0.783)	0.000
7	0.756	(0.590, 0.923)	0.000
8	0.797	(0.649, 0.946)	0.000
9	0.791	(0.644, 0.939)	0.000
$\beta_1^t + \beta_{23}^t + \beta_3^t + \beta_{43}^t$			
1	0.000	(., .)	.
2	0.249	(0.103, 0.396)	0.001
3	0.089	(-0.054, 0.232)	0.221
4	0.289	(0.113, 0.466)	0.001
5	0.633	(0.416, 0.850)	0.000
6	0.833	(0.560, 1.105)	0.000
7	0.795	(0.496, 1.095)	0.000
8	0.913	(0.627, 1.199)	0.000
9	0.995	(0.719, 1.271)	0.000
N	43853376		
R^2	0.435		

Notes: Table shows linear combinations of estimates (percentage points) and associated 95% confidence intervals and p-values from Equation 3. Numbers $1-9$ correspond to time periods t, where 1 refers to Winter 2020 (baseline), 2 refers to March 2020, 3 refers to Spring 2020, 4 refers to Summer 2020, 5 refers to Fall 2020, 6 refers to Winter 2021, 7 refers to March 2021, 8 refers to Spring 2021, and 9 refers to Summer 2021. These estimates mirror those illustrated in Figures 2a-2d

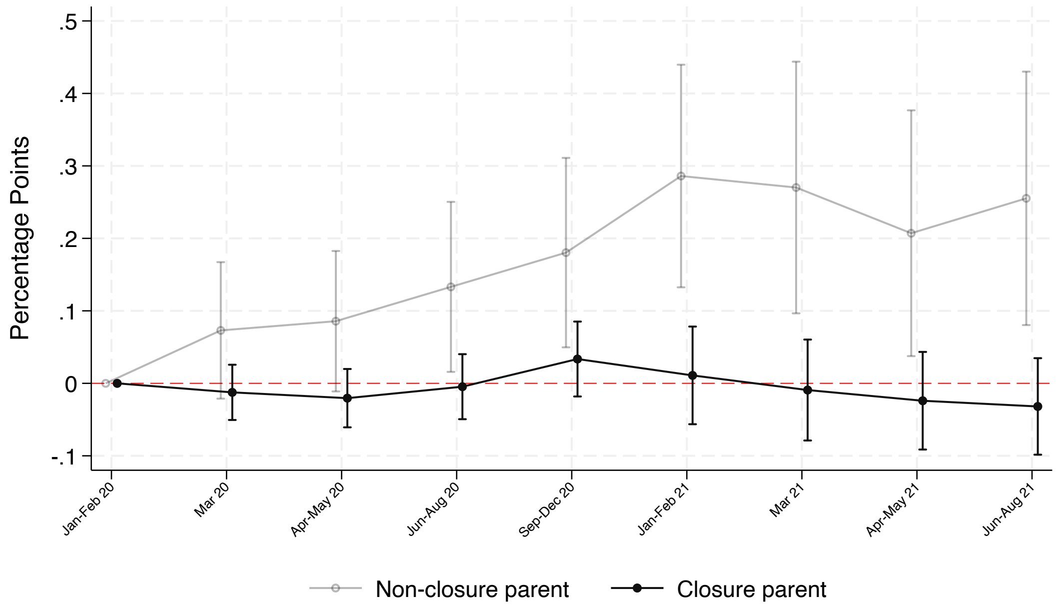
Appendix F. Other Analyses for Naive Adults

We estimate a variant of Equation 3 that excludes heterogeneity in children's health risks when considering how parenthood effects may be moderated by school closures:

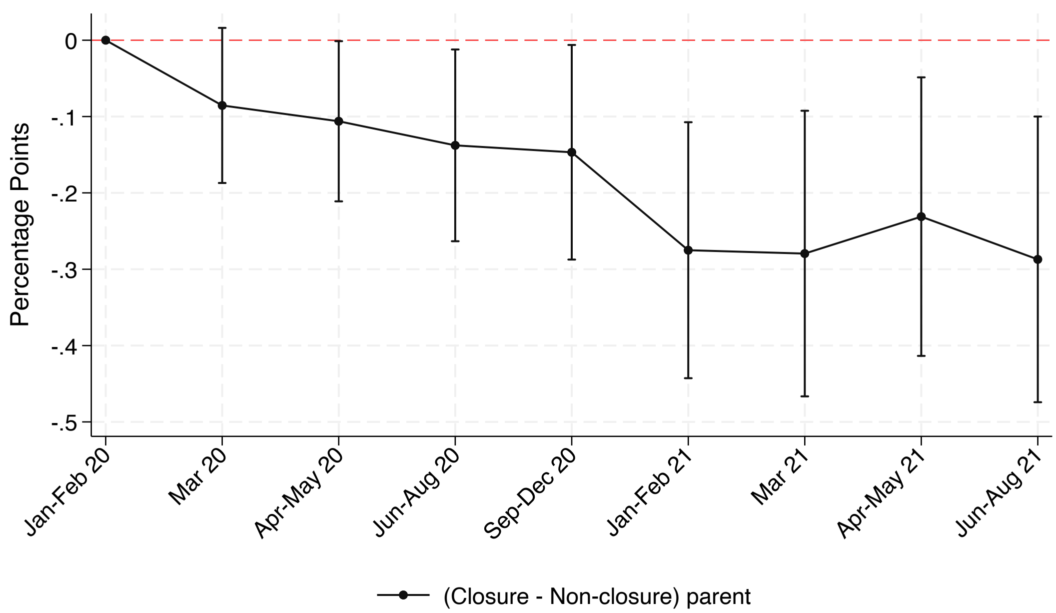
$$\begin{aligned}\texttt{Y}_{icm} = \alpha_i + \gamma_{cm} + \beta_1^t\texttt{parent}_i + \beta_2^t(\texttt{parent}_i * \texttt{closure}_c) + \\ + \delta_1^t\texttt{age}_i + \delta_2^t(\texttt{age}_i * \texttt{closure}_c) + \kappa_1^t\texttt{gender}_i + \kappa_2^t(\texttt{gender}_i * \texttt{closure}_c) + \epsilon_{icm}\end{aligned} \tag{F.1}$$

In this specification, β_1^t captures the change in prescription fills between parents and non-parents in non-closure counties, and the sum $(\beta_1^t + \beta_2^t)$ captures the change in prescription fills between parents and non-parents in closure counties. In this framework, β_2^t may be interpreted as the causal effect of closures for parents relative to non-parents.

Figure F.1: Δ Likelihood of Any Psychiatric Prescription Fill by Closure



(a) Parenthood



(b) (Closure - Non-closure) Parenthood

Notes: Panel a shows estimates of β_1^t and $(\beta_1^t + \beta_2^t)$ from Equation F.1 comparing the within-person change in the likelihood of filling any psychiatric prescription in time period t (indexed by the x-axis) compared to Winter 2020 for parents of school-age children relative to non-parents in closure and non-closure counties. Panel b shows estimates of β_2^t, which capture the closure effects for parents versus non-parent adults. Controls include age-closure-period, gender-closure-period, county-month fixed effects, and individual fixed effects. Standard errors are clustered at the county-level.

Appendix G. Analyses by Gender for Naive Adults

We estimate a variant of Equation 2 that includes interactions by gender when considering how parenthood effects may be moderated by children's health risk:

$$\begin{aligned}\texttt{Y}_{icm} &= \alpha_i + \gamma_{cm} + \beta_1^t \texttt{parent}_i + \sum_{r=1}^{3}[\beta_{2r}^t(\texttt{parent}_i * \texttt{H}_i^r)] + \phi_1^t(\texttt{parent}_i * \texttt{female}_i) \\ &+ \sum_{r=1}^{3}[\phi_{2r}^t(\texttt{parent}_i * \texttt{female}_i * \texttt{H}_i^r)] + \delta^t \texttt{age}_i + \kappa^t \texttt{gender}_i + \epsilon_{icm}\end{aligned} \tag{G.1}$$

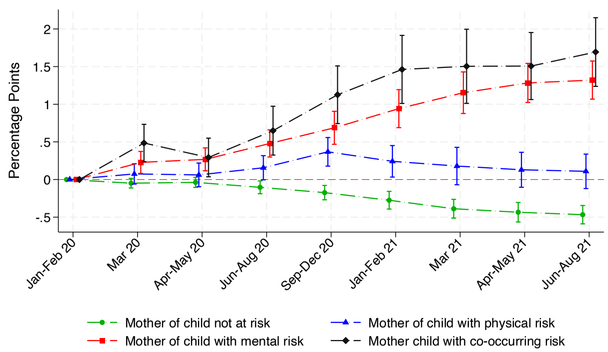
In this specification, β_1^t captures the change in prescription fills between fathers with children not at risk and non-parent men, and $(\beta_1^t + \phi_1^t)$ captures this change between mothers with children not at risk and non-parent women. The sum $(\beta_1^t + \sum_{r=1}^{3} \beta_{2r}^t H_i^r)$ reflects the differences between fathers with risk-type children and non-parent men, and $(\beta_1^t + \sum_{r=1}^{3} \beta_{2r}^t H_i^r + \phi_1^t + \sum_{r=1}^{3} \phi_{2r}^t H_i^r)$ captures the difference between mothers with risk-type children and non-parent women.

We also estimate a variant of Equation 3 that includes interactions by gender when considering how parenthood effects may be moderated by school closures:

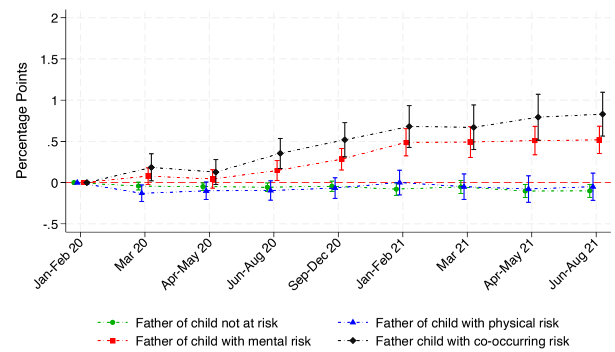
$$\begin{aligned}\texttt{Y}_{icm} &= \alpha_i + \gamma_{cm} + \beta_1^t \texttt{parent}_i + \sum_{r=1}^{3}[\beta_{2r}^t(\texttt{parent}_i * \texttt{H}_i^r)] \\ &+ \phi_1^t(\texttt{parent}_i * \texttt{female}_i) + \sum_{r=1}^{3}[\phi_{2r}^t(\texttt{parent}_i * \texttt{female}_i * \texttt{H}_i^r)] \\ &+ \beta_3^t(\texttt{parent}_i * \texttt{closure}_c) + \sum_{r=1}^{3}[\beta_{4r}^t(\texttt{parent}_i * \texttt{H}_i^r * \texttt{closure}_c)] \\ &+ \phi_3^t(\texttt{parent}_i * \texttt{female}_i * \texttt{closure}_c) + \sum_{r=1}^{3}[\phi_{4r}^t(\texttt{parent}_i * \texttt{female}_i * \texttt{H}_i^r * \texttt{closure}_c)] \\ &+ \delta_1^t \texttt{age}_i + \delta_2^t(\texttt{age}_i * \texttt{closure}_c) + \kappa_1^t \texttt{gender}_i + \kappa_2^t(\texttt{gender}_i * \texttt{closure}_c) + \epsilon_{icm}\end{aligned} \tag{G.2}$$

In this specification, $(\beta_1^t + \sum_{r=1}^{3} \beta_{2r}^t H_i^r)$ captures the difference in prescription fills between fathers and non-parent men in non-closure counties, and $(\beta_1^t + \sum_{r=1}^{3} \beta_{2r}^t H_i^r + \phi_1^t + \sum_{r=1}^{3} \phi_{2r}^t H_i^r)$ captures the difference between mothers and non-parent women. The sum $(\beta_1^t + \sum_{r=1}^{3} \beta_{2r}^t H_i^r + \beta_3^t + \sum_{r=1}^{3} \beta_{4r}^t H_i^r)$ captures the difference in prescription fills between fathers and non-parent men in closure counties by the health status of children, and $(\beta_1^t + \sum_{r=1}^{3} \beta_{2r}^t H_i^r + \beta_3^t + \sum_{r=1}^{3} \beta_{4r}^t H_i^r + \phi_1^t + \sum_{r=1}^{3} \phi_{2r}^t H_i^r + \phi_3^t + \sum_{r=1}^{3} \phi_{4r}^t H_i^r)$ captures this change between mothers and non-parent women.

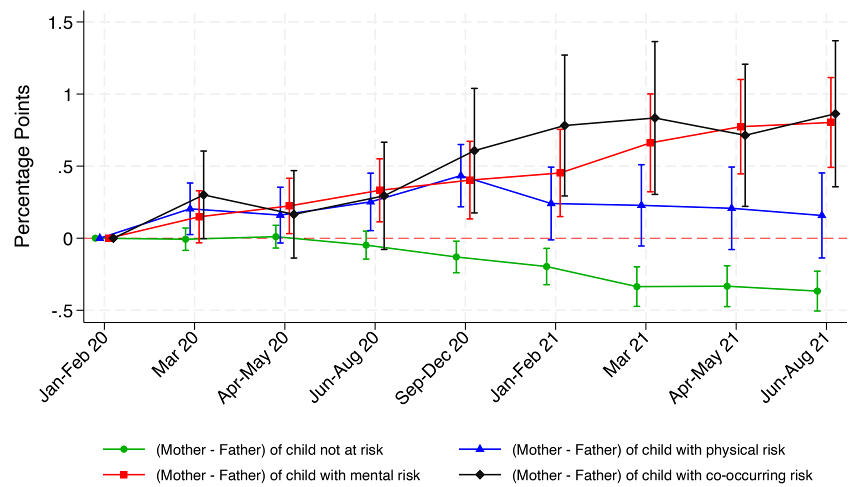
Figure G.1: Δ Likelihood of Any Psychiatric Prescription Fill for Parents vs. Non-parents by Gender



(a) Motherhood by Child Health Risk



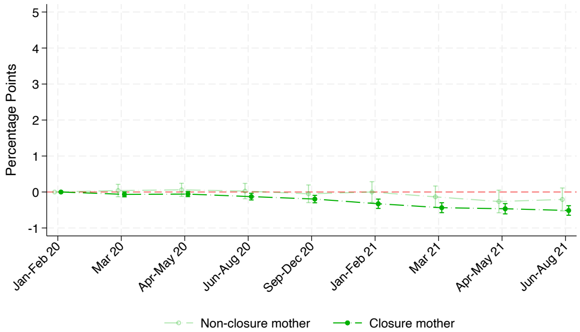
(b) Fatherhood by Child Health Risk



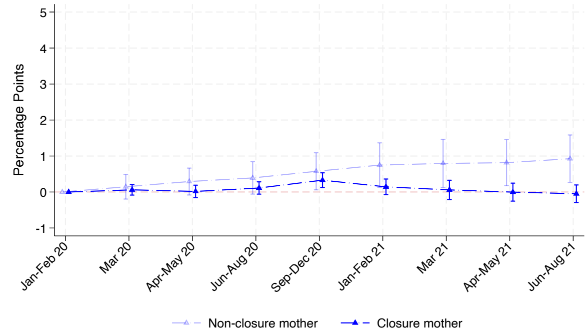
(c) (Motherhood - Fatherhood) by Child Health Risk

Notes: Panels a-b show estimates of $(\beta_1^t + \sum_{r=1}^{3} \beta_{2r}^t H_i^r + \phi_1^t + \sum_{r=1}^{3} \phi_{2r}^t H_i^r)$ and $(\beta_1^t + \sum_{r=1}^{3} \beta_{2r}^t H_i^r)$ from Equation G.1 comparing the within-person change in the likelihood of filling any psychiatric prescription in time period t (indexed by the x-axis) compared to Winter 2020 for mothers of children with varying risk profiles relative to non-parent women and fathers of children with varying risk profiles relative to non-parent men. Panel c shows estimates of $(\phi_1^t + \sum_{r=1}^{3} \phi_{2r}^t H_i^r)$ and illustrates differences in motherhood and fatherhood effects. Controls include age-period, gender-period, county-month-year, and individual fixed effects. Standard errors are clustered at the county-level.

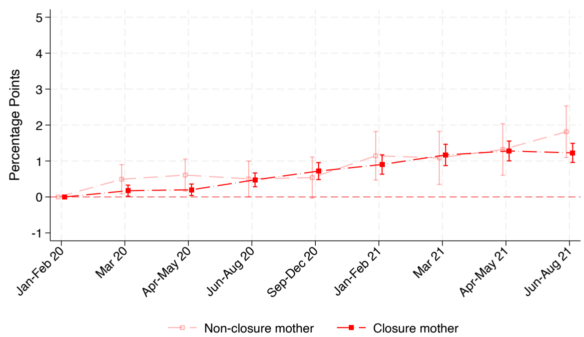
Figure G.2: Δ Likelihood of Any Psychiatric Prescription Fill by Closure for Mothers



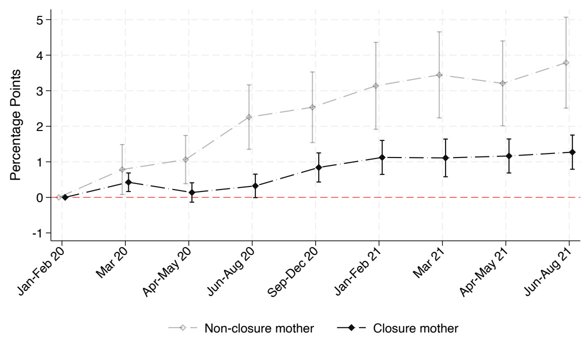
(a) Mothers of Children Not at Risk



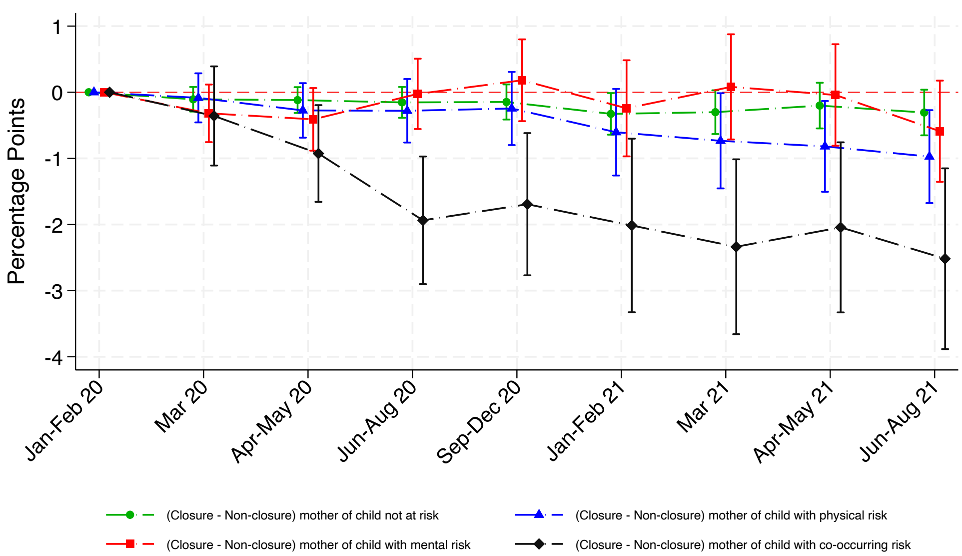
(b) Mothers of Children with Physical Risk



(c) Mothers of Children with Mental Risk



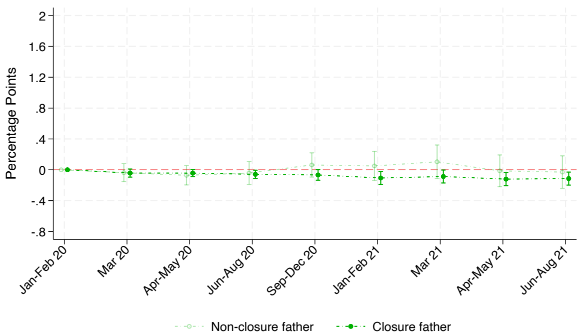
(d) Mothers of Children with Co-occurring Risk



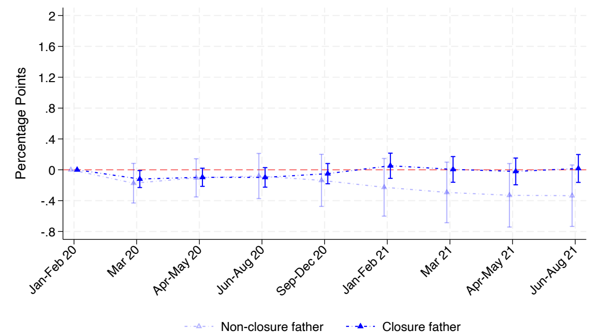
(e) (Closure - Non-closure) Motherhood

Notes: Panels a-d show estimates of $(\beta_1^t+\sum_{r=1}^{3}\beta_{2r}^t H_i^r+\phi_1^t+\sum_{r=1}^{3}\phi_{2r}^t H_i^r)$ and $(\beta_1^t+\sum_{r=1}^{3}\beta_{2r}^t H_i^r+\beta_3^t+\sum_{r=1}^{3}\beta_{4r}^t H_i^r+\phi_1^t+\sum_{r=1}^{3}\phi_{2r}^t H_i^r+\phi_3^t+\sum_{r=1}^{3}\phi_{4r}^t H_i^r)$ from Equation G.2 comparing the within-person change in the likelihood of filling any psychiatric prescription in time period t (indexed by the x-axis) compared to Winter 2020 for mothers of children with varying risks relative to non-parent women in closure and non-closure counties. Panel e shows estimates of $(\beta_3^t+\sum_{r=1}^{3}\beta_{4r}^t H_i^r+\phi_3^t+\sum_{r=1}^{3}\phi_{4r}^t H_i^r)$, which sum captures the difference between closure and non-closure counties. Controls include age-closure-period, gender-closure-period, county-month, and individual fixed effects. Standard errors are clustered at the county-level.

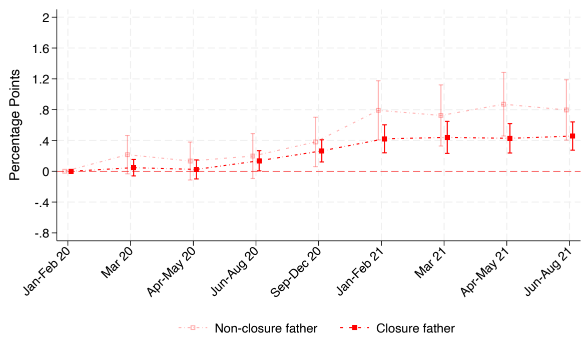
Figure G.3: Δ Likelihood of Any Psychiatric Prescription Fill by Closure for Fathers



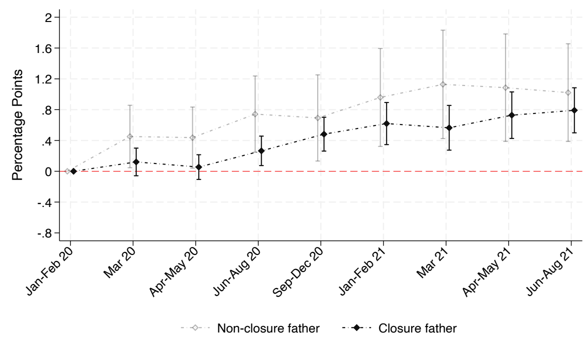
(a) Fathers of Children Not at Risk



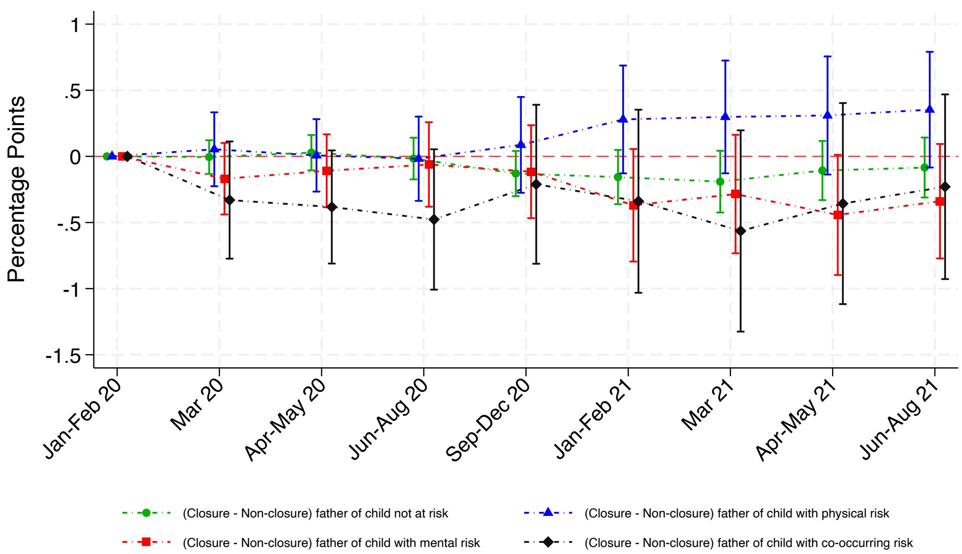
(b) Fathers of Children with Physical Risk



(c) Fathers of Children with Mental Risk



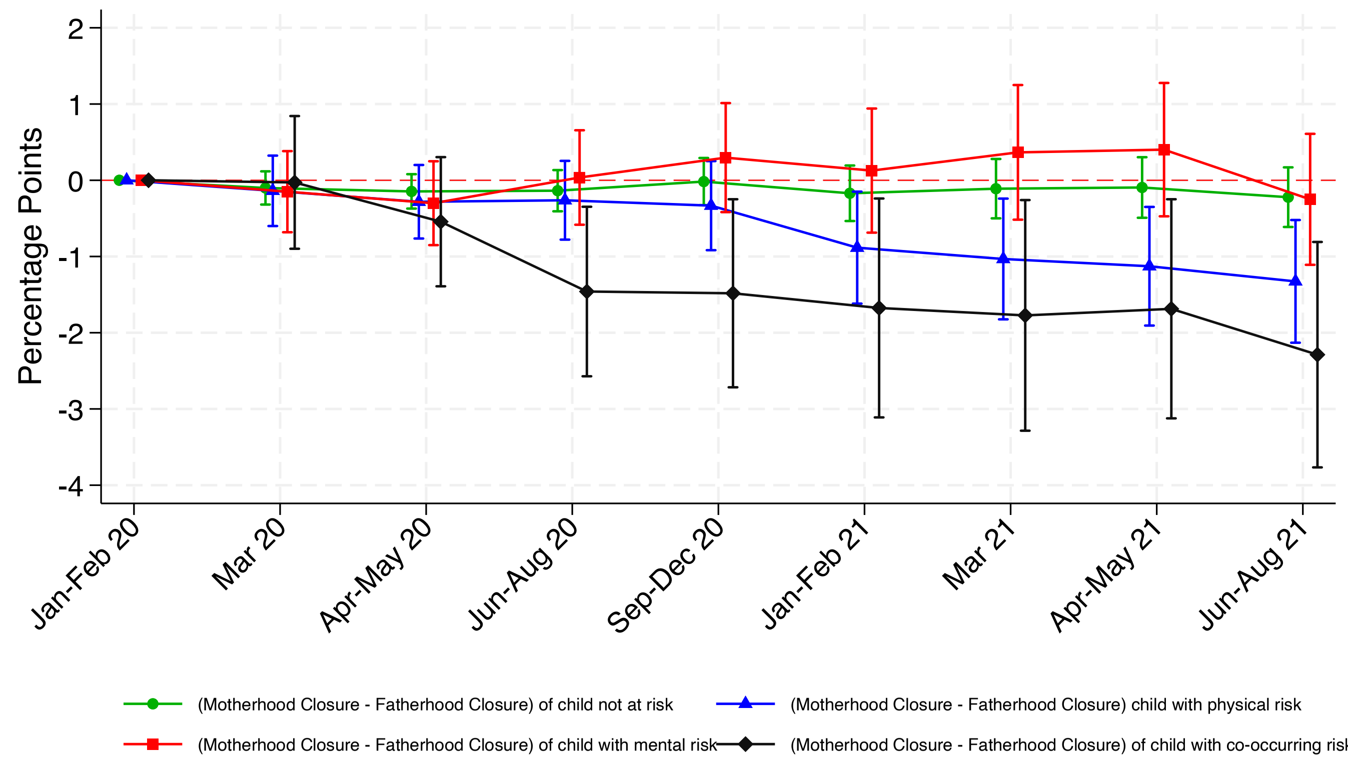
(d) Fathers of Children with Co-occurring Risk



(e) (Closure - Non-closure) Fatherhood

Notes: Panels a-d show estimates of $(\beta_1^t + \sum_{r=1}^{3} \beta_{2r}^t H_i^r)$ and $(\beta_1^t + \sum_{r=1}^{3} \beta_{2r}^t H_i^r + \beta_3^t + \sum_{r=1}^{3} \beta_{4r}^t H_i^r)$ from Equation G.2 comparing the within-person change in the likelihood of filling any psychiatric prescription in time period t (indexed by the x-axis) compared to Winter 2020 for fathers of children with varying risks relative to non-parent men in closure and non-closure counties. Panel e shows estimates of $(\beta_3^t + \sum_{r=1}^{3} \beta_{4r}^t H_i^r)$, which sum captures the difference between closure and non-closure counties. Controls include age-closure-period, gender-closure-period, county-month, and individual fixed effects. Standard errors are clustered at the county-level.

Figure G.4: Δ Likelihood of Any Psychiatric Prescription Fill for Mothers vs. Fathers by Closure



(Motherhood Closure Effect - Fatherhood Closure Effect) by Child Health Risk

Notes: This figure plots the estimates of $(\phi_3^t + \sum_{r=1}^{3} \phi_{4r}^t H_i^r)$ from Equation G.2 comparing the within-person change in the likelihood of filling any psychiatric prescription in time period t (indexed by the x-axis) compared to Winter 2020 for mothers of children with varying risks relative to non-parent women in closure and non-closure counties versus fathers of children with varying risks relative to non-parent men in closure and non-closure counties. Controls include age-closure-period, gender-closure-period, county-month, and individual fixed effects. Standard errors are clustered at the county-level.

Appendix H. Analyses for Experienced Adults

Table H.1: Demographics of Non-parents and Parents

Total	Non-parents (N=663,173)	Parents (N=216,446)	Test	Total (N=879,619)
Age	46.251 (14.549)	43.819 (7.221)	<0.001	45.652 (13.173)
19-34yo	0.264 (0.441)	0.093 (0.291)	<0.001	0.222 (0.416)
35-49yo	0.207 (0.405)	0.683 (0.465)	<0.001	0.324 (0.468)
50-64yo	0.529 (0.499)	0.223 (0.416)	<0.001	0.454 (0.498)
Female	0.587 (0.492)	0.598 (0.490)	<0.001	0.590 (0.492)
Family Size	1.632 (1.078)	4.117 (1.178)	<0.001	2.243 (1.537)
School Closure	0.779 (0.415)	0.788 (0.409)	<0.001	0.781 (0.414)
Baseline Rx (%)	44.925 (48.029)	42.704 (47.638)	<0.001	44.379 (47.943)
Non-Closure	Non-parents (N=146,753)	Parents (N=45,920)	Test	Total (N=192,673)
Age	48.148 (14.043)	42.505 (7.312)	<0.001	46.803 (12.989)
19-34yo	0.211 (0.408)	0.132 (0.339)	<0.001	0.192 (0.394)
35-49yo	0.194 (0.395)	0.693 (0.461)	<0.001	0.313 (0.464)
50-64yo	0.595 (0.491)	0.174 (0.379)	<0.001	0.495 (0.500)
Female	0.587 (0.492)	0.593 (0.491)	0.009	0.588 (0.492)
Family Size	1.604 (1.055)	4.198 (1.245)	<0.001	2.222 (1.562)
Baseline Rx (%)	49.006 (48.236)	45.548 (47.924)	<0.001	48.182 (48.184)
Closure	Non-parents (N=516,420)	Parents (N=170,526)	Test	Total (N=686,946)
Age	45.712 (14.645)	44.172 (7.155)	<0.001	45.330 (13.205)
19-34yo	0.279 (0.448)	0.083 (0.276)	<0.001	0.230 (0.421)
35-49yo	0.211 (0.408)	0.681 (0.466)	<0.001	0.327 (0.469)
50-64yo	0.510 (0.500)	0.236 (0.425)	<0.001	0.442 (0.497)
Female	0.587 (0.492)	0.599 (0.490)	<0.001	0.590 (0.492)
Family Size	1.640 (1.084)	4.095 (1.158)	<0.001	2.249 (1.530)
Baseline Rx (%)	43.766 (47.907)	41.939 (47.532)	<0.001	43.312 (47.821)
Δ Baseline Rx (pp)	reference	1.631 (0.289)	<0.001	-4.869 (0.124)

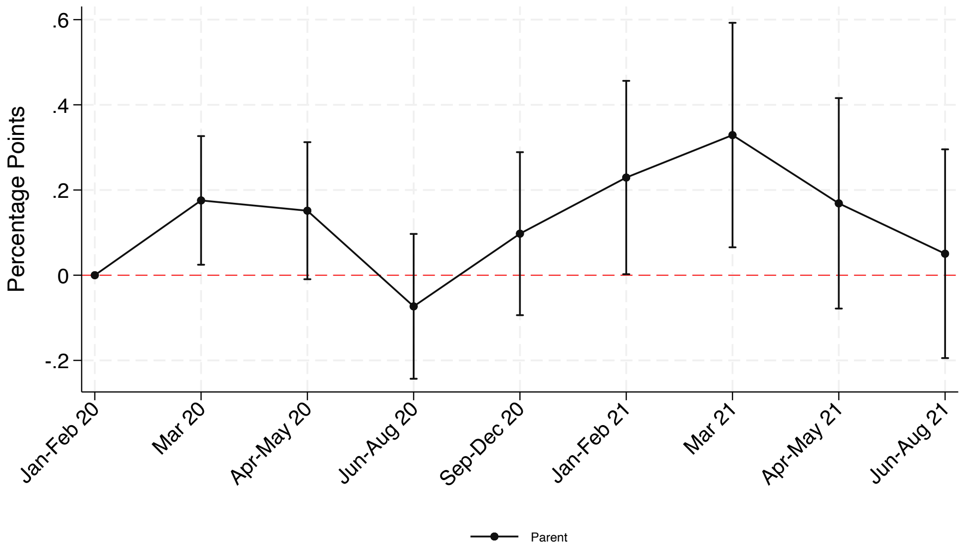
Notes: Table provides means (standard deviations) for experienced parents of school-age children and non-parents by whether they reside in a school closure county. Baseline Rx refers to the likelihood of any psychiatric prescription fill in the baseline period of January/February 2020. Tests compare parents to non-parents, and table reports p-values of t-tests. The final row of the table (Δ Baseline Rx) tests the baseline difference between parents and non-parents in closure counties against the baseline difference between parents and non-parents in non-closure counties.

Table H.2: Demographics of Non-parents and Parents by Child Health Risk

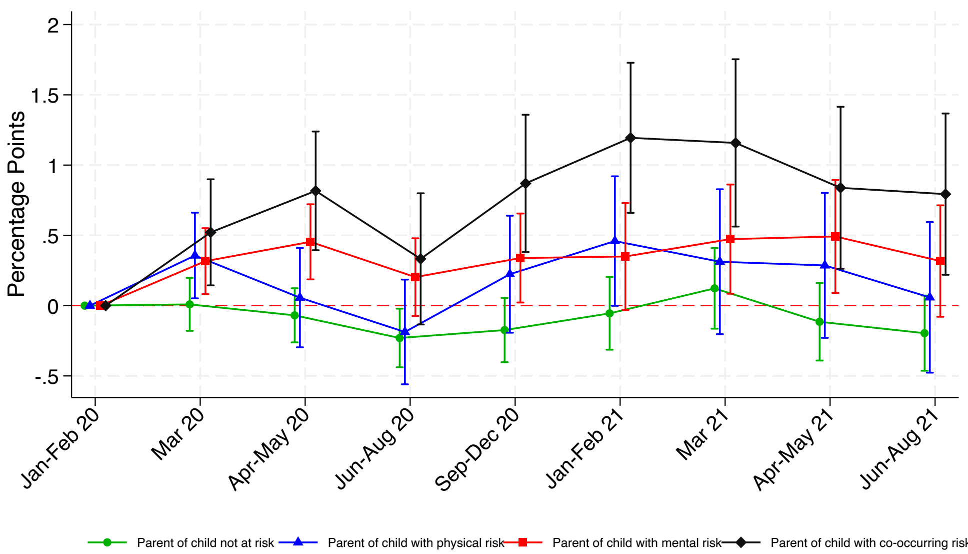
Total	Non-parents (N=663,173)	Not at risk (N=116,186)	Test	Physical risk (N=30,545)	Test	Mental risk (N=47,676)	Test	Co-occurring risk (N=22,039)	Test
Age	46.251 (14.549)	43.860 (7.338)	<0.001	42.849 (7.076)	<0.001	44.499 (7.044)	<0.001	43.475 (7.005)	<0.001
19-34yo	0.264 (0.441)	0.099 (0.298)	<0.001	0.107 (0.310)	<0.001	0.072 (0.259)	<0.001	0.092 (0.289)	<0.001
35-49yo	0.207 (0.405)	0.671 (0.470)	<0.001	0.714 (0.452)	<0.001	0.682 (0.466)	<0.001	0.709 (0.454)	<0.001
50-64yo	0.529 (0.499)	0.231 (0.421)	<0.001	0.179 (0.383)	<0.001	0.246 (0.430)	<0.001	0.199 (0.399)	<0.001
Female	0.587 (0.492)	0.593 (0.491)	<0.001	0.599 (0.490)	<0.001	0.606 (0.489)	<0.001	0.607 (0.488)	<0.001
Family Size	1.632 (1.078)	3.999 (1.144)	<0.001	4.224 (1.152)	<0.001	4.186 (1.199)	<0.001	4.445 (1.254)	<0.001
School Closure	0.779 (0.415)	0.784 (0.412)	<0.001	0.792 (0.406)	<0.001	0.795 (0.404)	<0.001	0.791 (0.407)	<0.001
Baseline Rx (%)	44.925 (48.029)	40.408 (47.247)	<0.001	40.249 (47.115)	<0.001	47.822 (48.182)	<0.001	47.144 (48.051)	<0.001
Non-Closure	Non-parents (N=146,753)	Not at risk (N=25,152)	Test	Physical risk (N=6,367)	Test	Mental risk (N=9,787)	Test	Co-occurring risk (N=4,614)	Test
Age	48.148 (14.043)	42.682 (7.432)	<0.001	41.441 (7.153)	<0.001	43.056 (7.119)	<0.001	41.842 (7.080)	<0.001
19-34yo	0.211 (0.408)	0.135 (0.342)	<0.001	0.155 (0.362)	<0.001	0.106 (0.308)	<0.001	0.141 (0.348)	<0.001
35-49yo	0.194 (0.395)	0.678 (0.467)	<0.001	0.714 (0.452)	<0.001	0.709 (0.454)	<0.001	0.713 (0.452)	<0.001
50-64yo	0.595 (0.491)	0.187 (0.390)	<0.001	0.131 (0.337)	<0.001	0.185 (0.388)	<0.001	0.146 (0.353)	<0.001
Female	0.587 (0.492)	0.591 (0.492)	0.168	0.591 (0.492)	0.528	0.599 (0.490)	0.018	0.598 (0.490)	0.108
Family Size	1.604 (1.055)	4.073 (1.201)	<0.001	4.305 (1.214)	<0.001	4.281 (1.287)	<0.001	4.555 (1.329)	<0.001
Baseline Rx (%)	49.006 (48.236)	43.438 (47.695)	<0.001	43.859 (47.659)	<0.001	50.163 (48.215)	0.022	49.588 (47.991)	0.419
Closure	Non-parents (N=516,420)	Not at risk (N=91,034)	Test	Physical risk (N=24,178)	Test	Mental risk (N=37,889)	Test	Co-occurring risk (N=17,425)	Test
Age	45.712 (14.645)	44.185 (7.278)	<0.001	43.220 (7.008)	<0.001	44.872 (6.976)	<0.001	43.907 (6.920)	<0.001
19-34yo	0.279 (0.448)	0.089 (0.284)	<0.001	0.095 (0.293)	<0.001	0.064 (0.244)	<0.001	0.079 (0.269)	<0.001
35-49yo	0.211 (0.408)	0.669 (0.471)	<0.001	0.714 (0.452)	<0.001	0.675 (0.468)	<0.001	0.709 (0.454)	<0.001
50-64yo	0.510 (0.500)	0.243 (0.429)	<0.001	0.191 (0.393)	<0.001	0.261 (0.439)	<0.001	0.213 (0.409)	<0.001
Female	0.587 (0.492)	0.593 (0.491)	<0.001	0.601 (0.490)	<0.001	0.607 (0.488)	<0.001	0.609 (0.488)	<0.001
Family Size	1.640 (1.084)	3.978 (1.126)	<0.001	4.202 (1.134)	<0.001	4.161 (1.174)	<0.001	4.416 (1.232)	<0.001
Baseline Rx (%)	43.766 (47.907)	39.571 (47.089)	<0.001	39.298 (46.926)	<0.001	47.217 (48.155)	<0.001	46.496 (48.047)	<0.001
Δ Baseline Rx (pp)	reference	1.373 (0.369)	<0.001	0.679 (0.690)	0.325	2.294 (0.562)	<0.001	2.148 (0.807)	0.008

Notes: Table provides means (standard deviations) for experienced adult parents of 6-17 year-old children and non-parents by child health risk and whether they reside in a school closure county. Baseline Rx refers to the likelihood of any psychiatric prescription fill in the baseline period of January/February 2020. Tests compare parents of children with different health risk types to non-parents, and table reports p-values of t-tests. The final row of the table (Δ Baseline Rx) tests the baseline difference between parents and non-parents in closure counties against the baseline difference between parents and non-parents in non-closure counties.

Figure H.1: Δ Likelihood of Any Psychiatric Prescription Fill



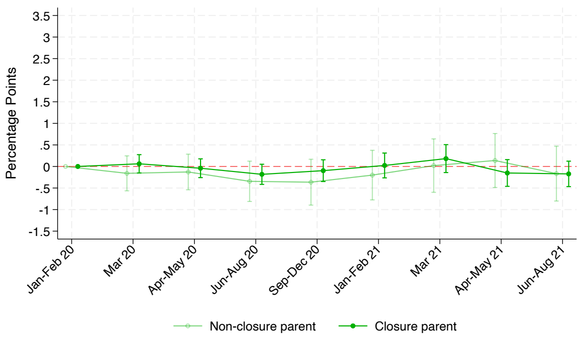
(a) Parenthood



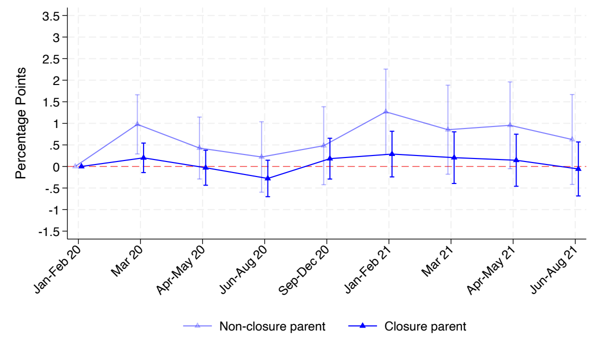
(b) Parenthood by Child Health Risk

Notes: Panel a shows estimates of β^t from Equation 1 comparing the within-person change in the likelihood of filling any psychiatric prescription in time period t (indexed by the x-axis) compared to Winter 2020 for parents of school-age children relative to non-parents. Panel b shows estimates of β_1^t and $(\beta_1^t + \sum_{r=1}^{3} \beta_{2r}^t H_i^r)$ from Equation 2 comparing the within-person change in the likelihood of filling any psychiatric prescription in time period t (indexed by the x-axis) compared to Winter 2020 for parents of children with varying risks relative to non-parents. Controls include age-period, gender-period, county-month-year, and individual fixed effects. Standard errors are clustered at the county-level.

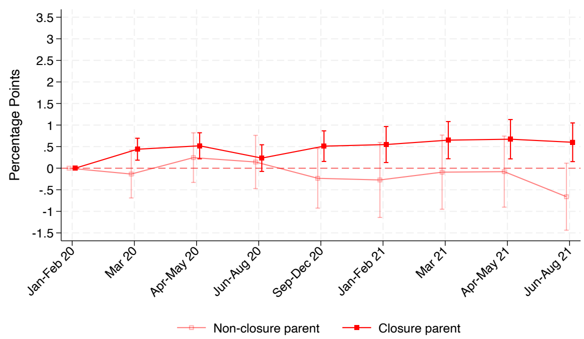
Figure H.2: Δ Likelihood of Any Psychiatric Prescription Fill by Closure



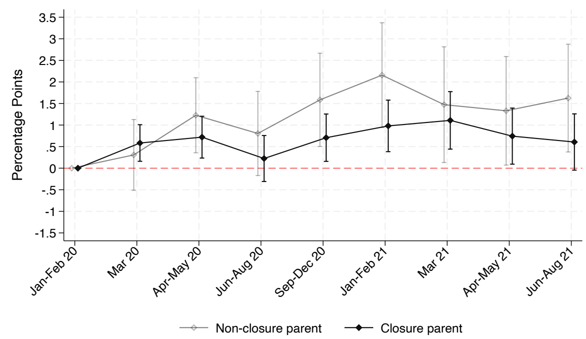
(a) Parents of Children Not at Risk



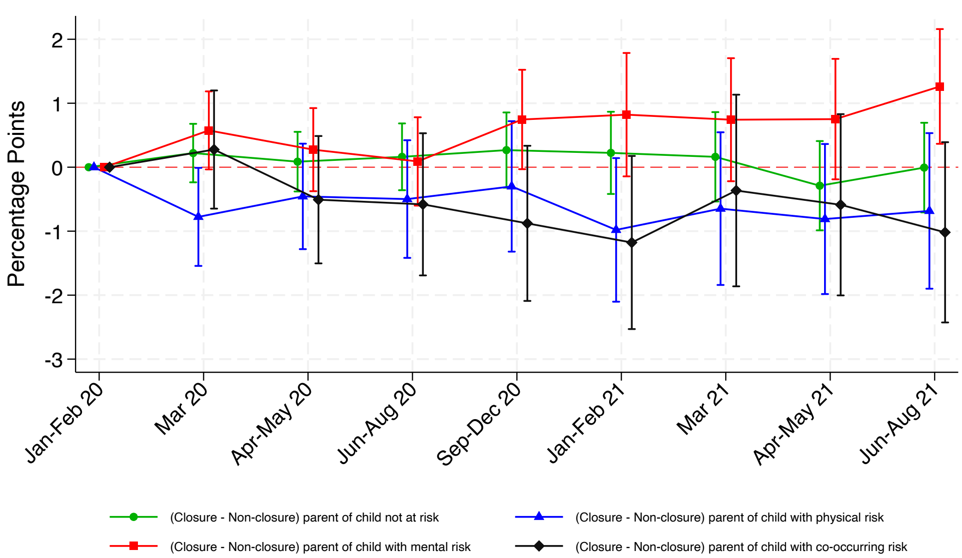
(b) Parents of Children with Physical Risk



(c) Parents of Children with Mental Risk



(d) Parents of Children with Co-occurring Risk

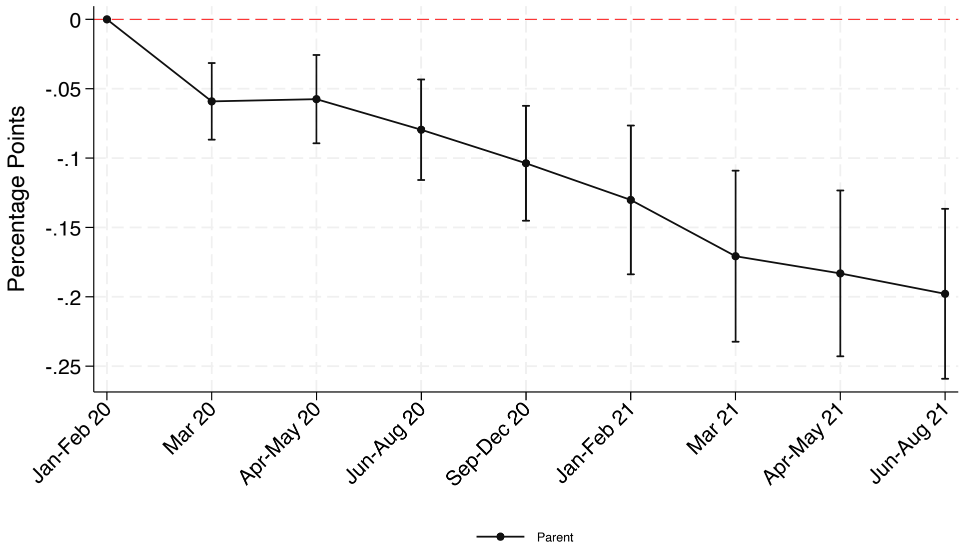


(e) (Closure - Non-closure) Parenthood

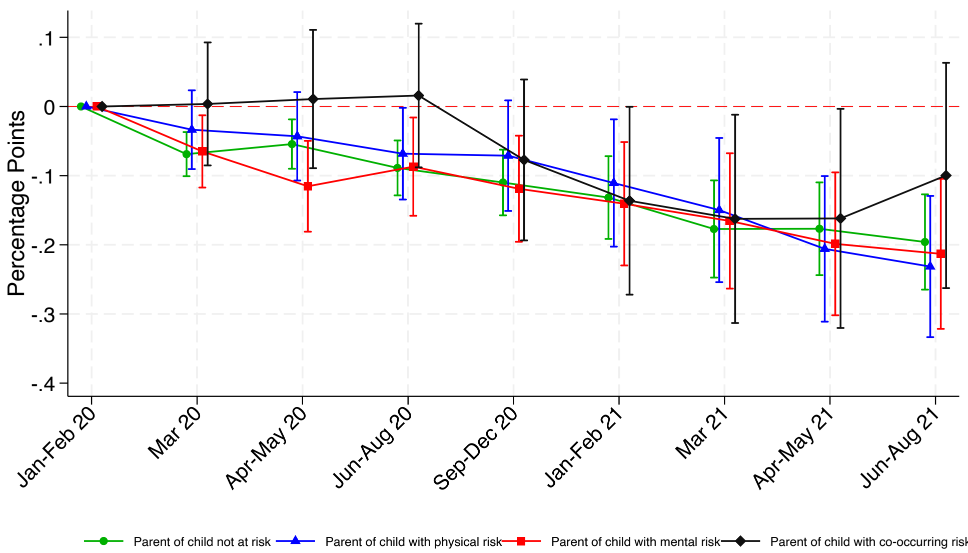
Notes: Panels a-d show estimates of $(\beta_1^t + \sum_{r=1}^{3} \beta_{2r}^t H_i^r)$ and $(\beta_1^t + \sum_{r=1}^{3} \beta_{2r}^t H_i^r + \beta_3^t + \sum_{r=1}^{3} \beta_{4r}^t H_i^r)$ from Equation 3 comparing the within-person change in the likelihood of filling any psychiatric prescription in time period t (indexed by the x-axis) compared to Winter 2020 for parents of children with varying risks relative to non-parents in closure and non-closure counties. Panel e shows estimates of $(\beta_3^t + \sum_{r=1}^{3} \beta_{4r}^t H_i^r)$, which sum captures the difference between closure and non-closure counties. Controls include age-closure-period, gender-closure-period, county-month, and individual fixed effects. Standard errors are clustered at the county-level.

Appendix I. Placebo & Falsification Tests

Figure I.1: Δ Likelihood of Any Diabetes Prescription Fill for Naive Adults



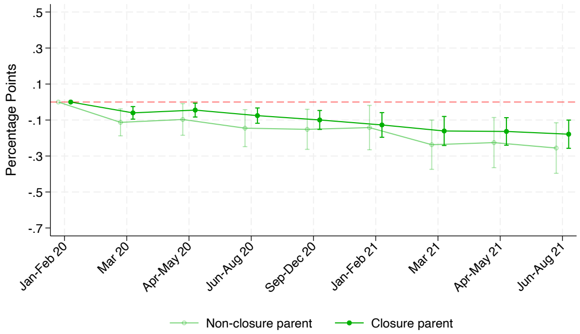
(a) Parenthood



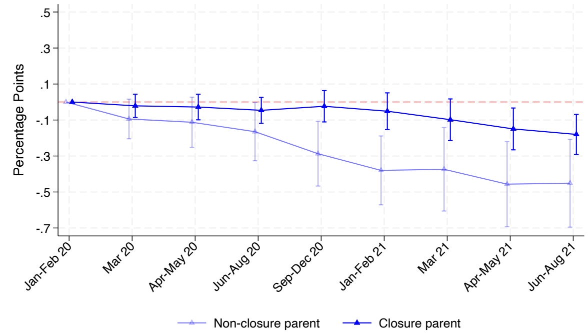
(b) Parenthood by Child Health Risk

Notes: Panel a shows estimates of β^t from Equation 1 comparing the within-person change in the likelihood of filling any diabetes prescription in time period t (indexed by the x-axis) compared to Winter 2020 for parents of children with varying risks relative to non-parents. Panel b shows estimates of β_1^t and $(\beta_1^t + \sum_{r=1}^{3} \beta_{2r}^t H_i^r)$ from Equation 2 comparing the within-person change in the likelihood of filling any diabetes prescription in time period t (indexed by the x-axis) compared to Winter 2020 for parents of children with varying risks relative to non-parents. Controls include age-period, gender-period, county-month-year, and individual fixed effects. Standard errors are clustered at the county-level.

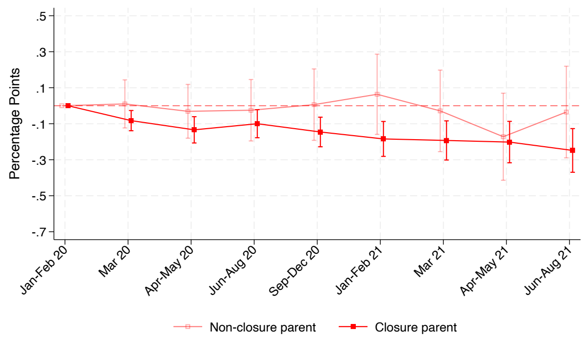
Figure I.2: Δ Likelihood of Any Diabetes Prescription Fill by Closure for Naive Adults



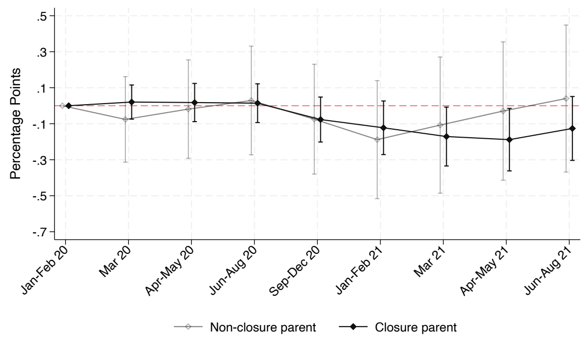
(a) Parents of Children Not at Risk



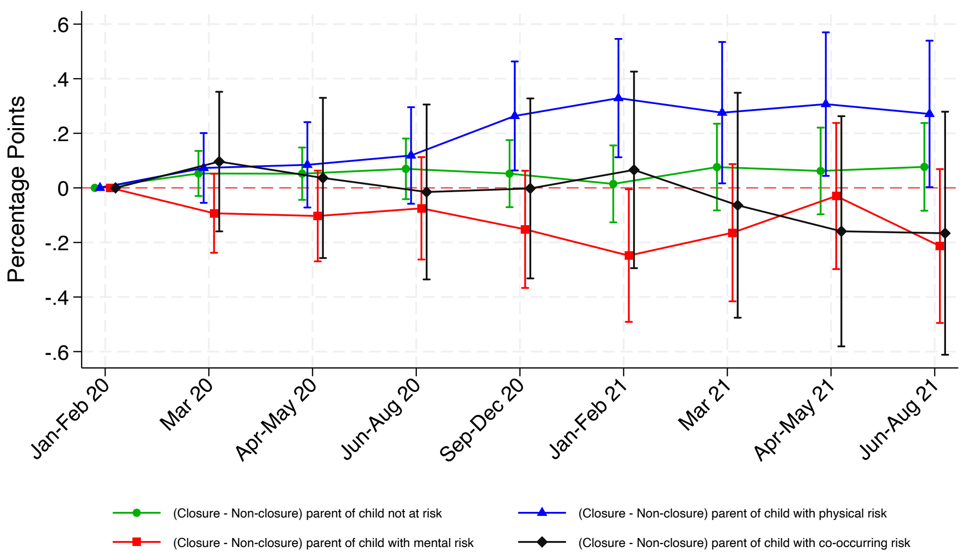
(b) Parents of Children with Physical Risk



(c) Parents of Children with Mental Risk



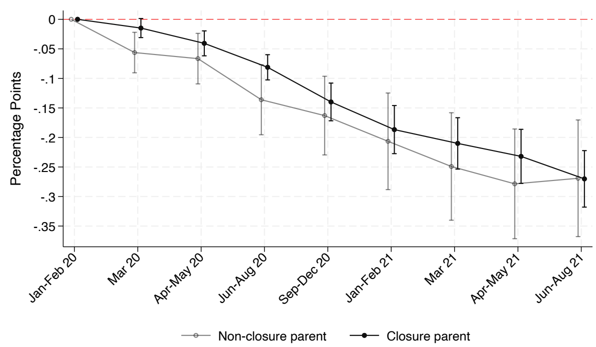
(d) Parents of Children with Co-occurring Risk



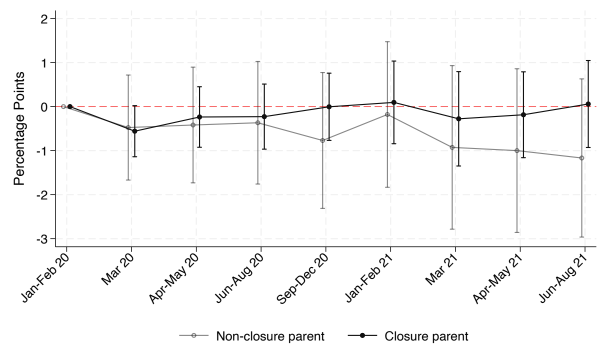
(e) (Closure - Non-closure) Parenthood

Notes: Panels a-d show estimates of $(\beta_1^t + \sum_{r=1}^3 \beta_{2r}^t H_i^r)$ and $(\beta_1^t + \sum_{r=1}^3 \beta_{2r}^t H_i^r + \beta_3^t + \sum_{r=1}^3 \beta_{4r}^t H_i^r)$ from Equation 3 comparing the within-person change in the likelihood of filling any diabetes prescription in time period t (indexed by the x-axis) compared to Winter 2020 for parents of children with varying risks relative to non-parents in closure and non-closure counties. Panel e shows estimates of $(\beta_3^t + \sum_{r=1}^3 \beta_{4r}^t H_i^r)$, which sum captures the difference between closure and non-closure counties. Controls include age-closure-period, gender-closure-period, county-month, and individual fixed effects. Standard errors are clustered at the county-level.

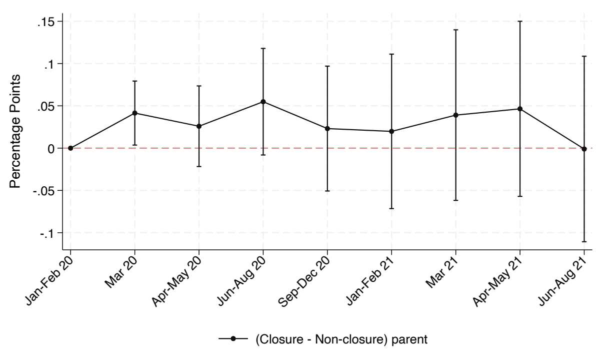
Figure I.3: Δ Likelihood of Any Diabetes Prescription Fill by Closure for Naive Adults



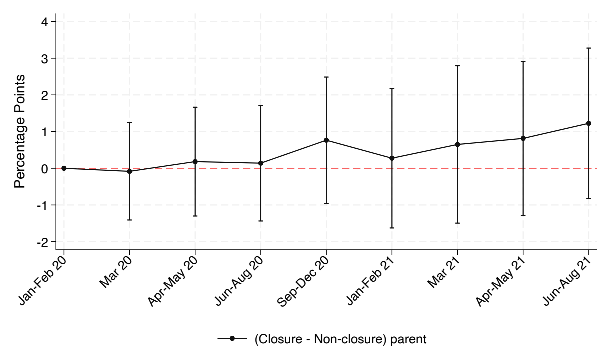
(a) Diabetes-Naive: Parenthood



(b) Diabetes-Experienced: Parenthood



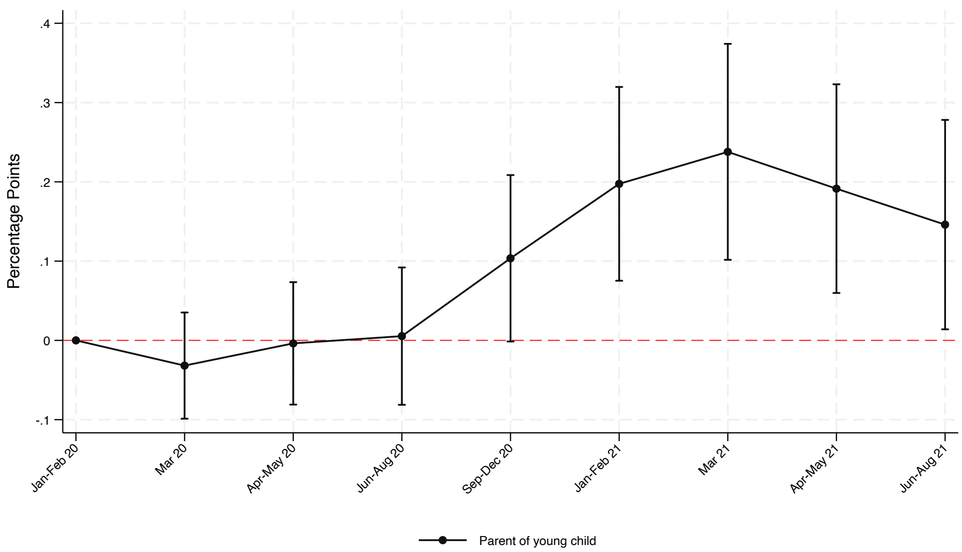
(c) Diabetes-Naive: (Closure - Non-closure) Parenthood



(d) Diabetes-Experienced: (Closure - Non-closure) Parenthood

Notes: Panels a and c show estimates for adults without psychiatric exposure and without diabetes exposure in 2019. Panels b and d show estimates for adults without psychiatric exposure but with diabetes exposure in 2019. Panels a-b show estimates of β_1^t and $(\beta_1^t + \beta_2^t)$ from Equation F.1 comparing the within-person change in the likelihood of filling any diabetes prescription in time period t (indexed by the x-axis) compared to Winter 2020 for parents of school-age children relative to non-parents in closure and non-closure counties. Panels c-d show estimates of β_2^t, which capture the closure effects for parents versus non-parent adults. Controls include age-closure-period, gender-closure-period, county-month, and individual fixed effects. Standard errors are clustered at the county-level.

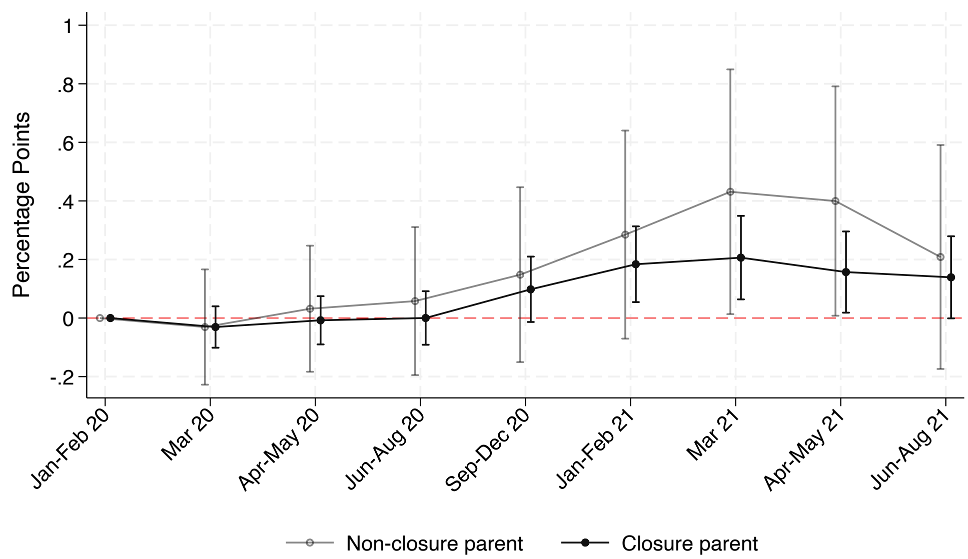
Figure I.4: Δ Likelihood of Any Psychiatric Prescription Fill for Naive Parents of Children $<$ 4 Years



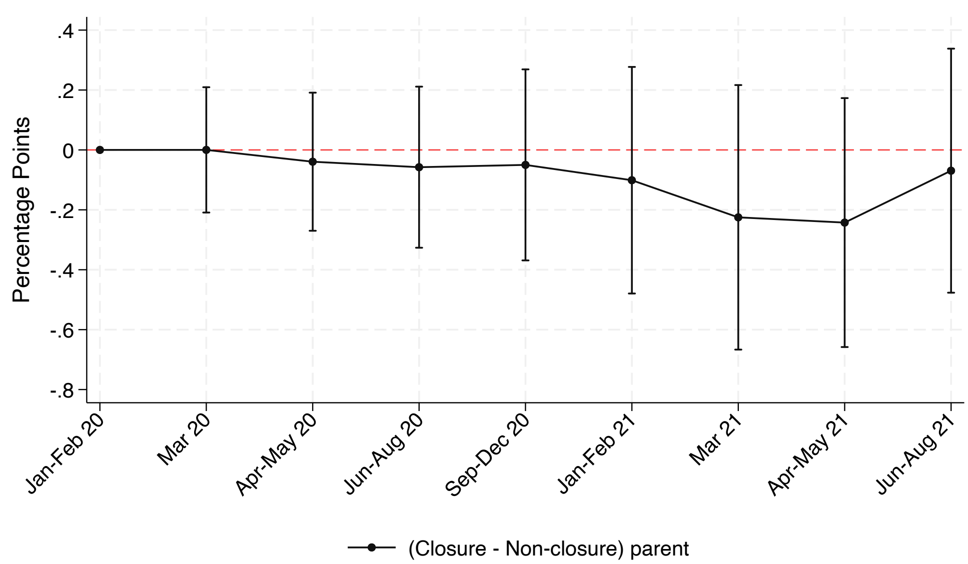
Parenthood

Notes: Panel I.4 shows estimates of β^t from Equation 1 comparing the within-person change in the likelihood of filling any psychiatric prescription in time period t (indexed by the x-axis) compared to Winter 2020 for parents of younger than school-age children relative to non-parents.

Figure I.5: Δ Likelihood of Any Psychiatric Prescription Fill by Closure for Naive Parents of Children < 4 Years



(a) Parenthood



(b) (Closure - Non-closure) Parenthood

Notes: Panel a shows estimates of β_1^t and $(\beta_1^t + \beta_2^t)$ from Equation F.1 comparing the within-person change in the likelihood of filling any psychiatric prescription in time period t (indexed by the x-axis) compared to Winter 2020 for parents of younger than school-age children relative to non-parents in closure and non-closure counties. Panel b shows estimates of β_2^t from Equation F.1, which capture the closure effects for parents versus non-parent adults. Controls include age-closure-period, gender-closure-period, county-month fixed effects, and individual fixed effects. Standard errors are clustered at the county-level.