NBER WORKING PAPER SERIES

VACCINATION POLICY, DELAYED CARE, AND HEALTH EXPENDITURES

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Working Paper 30139
http://www.nber.org/papers/w30139

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2022, Revised August 2022

We have no financial or other relationships to disclose for this paper. We thank Amanda Kowalski, Joshua D. Gottlieb, Maria Polyakova, and Sumedha Gupta for helpful comments and suggestions. We also appreciate comments from seminar and conference participants at the NBER Meeting on COVID-19 and Health Outcomes, Eastern Michigan University, and Applied Microeconomics and Policy Workshop at MEF University. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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JEL No. I1,I12,I18

ABSTRACT

The COVID-19 pandemic has profoundly affected the United States healthcare system, resulting in major disruptions in the delivery of essential care and causing crippling financial losses that threaten the viability of millions of medical practices. There is little empirical evidence on the types of policies or innovations that are effective in shaping healthcare seeking behavior during a public health crisis. This paper evaluates the effect of COVID-19 vaccination on the individual propensity to delay or skip medical care. Our research design exploits the arguably exogenous variation in age-specific vaccine eligibility rollout across states and over time as an instrument for individual vaccination status. We find that receiving a COVID-19 vaccine reduces the likelihood of delaying care for any medical condition by 37 percent. Furthermore, our analysis reveals that children are significantly less likely to delay or skip healthcare as a result of their parents becoming vaccine eligible, indicating the presence of a positive health spillover within households that extends beyond protection against infection. We also find evidence to indicate that vaccination affects healthcare seeking behavior by easing concerns about contracting or spreading COVID-19. Our results highlight the important role that vaccines play in, not only protecting against coronavirus, but also safeguarding against the worsening of health due to delayed or foregone medical care. The decline in delayed or foregone care caused by vaccination is particularly strong among minorities and those with a low socioeconomic background, revealing an important role that vaccination efforts can play in narrowing inequities in health and healthcare. In supplementary analysis, we use novel data on debit and credit card spending to demonstrate that increased vaccine uptake has a positive, albeit statistically insignificant, effect on consumer healthcare spending in the short run. Taken together, our findings imply that advancements in vaccine development coupled with a regulatory process that accelerates the availability of vaccines to public in a safe manner can have the additional benefit of tackling unmet healthcare needs during a public health crisis.

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1. Introduction

More than one-third of adults in the U.S. delayed or skipped at least one type of medical care during the COVID-19 outbreak (Gonzalez et al., 2021). Many individuals report delaying or forgoing routine medical care such as general doctor or specialist visits, checkups, and other preventive health screenings over concerns about coronavirus exposure (Gonzalez, Karpman, and Haley, 2021a).[1] The concern over delayed care is also expressed by healthcare providers as over a third of primary care physicians have indicated in a survey that their patients with chronic conditions are in "noticeably worse health resulting from the pandemic." (Landi, 2020). These are worrisome developments from a public health perspective because "[f]orgoing medical care or avoidance might not only increase morbidity and mortality risk associated with treatable and preventable health conditions and might contribute to reported excess deaths directly or indirectly related to the pandemic" (Czeisler et al., 2020). Delaying or going without medical care due to fear of exposure to coronavirus is particularly concerning for children as such behavior can have negative spillovers to outcomes other than health, including academic performance, labor market productivity, and delinquency (see, e.g., Brown, Kowalski, and Lurie 2020; Arenberg, Neller, and Stripling 2020).[2] In this regard, vaccination efforts have the potential to produce the additional benefit of meeting unmet healthcare, aside from providing protection against the virus.

Reduced patient volumes and consumer healthcare spending caused by delay or avoidance of care have also had a harsh impact on the healthcare sector, threatening the financial viability of millions of providers. Compared to the pre-pandemic period, consumer healthcare spending declined more than 50 percent in the U.S. by March 2020 (Chetty et al., 2020). According to the 2020 Survey of America's Physicians, 70% of physicians indicated reductions in income due to the pandemic, and 8% of physicians closed their practices (Physicians Foundation, 2020). Moreover, physician practices experienced more than 60% decline in average revenues and patient volume since March 2020, and more than 80% have expressed concerns over their financial health.[3] Therefore, medical innovations including the development and accelerated approval of vaccines have the potential to help the broader recovery of economic activities as people may feel safe enough to return to healthcare settings.

In this paper, we use data from the Household Pulse Survey provided by the U.S. Census Bureau, with over 2.2 million individual observations, to estimate the causal effect

[1]According to a nationally representative survey conducted during June 24-30, 2020, 31.5 percent of adults avoided routine care, while 12 percent avoided urgent or emergency care (Czeisler et al., 2020).

[2]Similarly, nearly 20 percent of parents delayed or avoided needed care for their children in the past 12 months because of COVID-19 concerns (Gonzalez, Karpman, and Haley, 2021b).

[3]A detailed summary of the COVID-19 Physician Financial Health Survey can be accessed here: https://bit.ly/37Bjb04.

of vaccination on the likelihood of delaying or avoiding medical care. In supplementary analysis, we use novel consumer credit and debit card data to estimate the impact of vaccination on consumer healthcare spending as well as other expenditure categories. The rapid development and approval of COVID-19 vaccines serve as a unique intervention to explore the effect of vaccination on healthcare seeking behavior. Figure 1 shows a sharp decline in delayed medical care and concerns about getting exposed to or spreading coronavirus after the start of vaccination rollout. Although the descriptive evidence is informative, the relationship between vaccination and delayed care (or concerns about virus exposure) might be confounded by the unobserved characteristics of adults receiving vaccination.

To address non-random selection of adults receiving vaccination, we exploit the arguably exogenous variation in age-specific vaccine eligibility rollout across states and over time as an instrument for individual vaccination status. Our empirical strategy recovers the local average treatment effect (LATE) or the causal effect of getting vaccinated among adults aged 18 and older, whose eligibility status changes with respect to an age cutoff implemented across states and over time. Our results confirm that vaccine eligibility is a strong predictor of vaccination status, but not systematically related to state characteristics and observed characteristics of adults within an age cohort. Moreover, we find no evidence to suggest that outcomes such as delayed medical care influence future vaccine eligibility decisions. We document that vaccine eligibility increases the likelihood of vaccination by about 27 percentage points ($p < 0.01$) for our full sample. Importantly, our analysis also reveals that becoming eligible for a vaccine leads to significant increases in vaccine take-up among vulnerable populations, including minorities, females, and individuals with lower socioeconomic status.

Health seeking behavior encompasses actions undertaken by individuals to promote good health and rectify perceived ill-health (Ward, Mertens, and Thomas, 1997). Attaining good health behavior is critical for the prevention, early diagnosis, and management of illnesses. It is therefore important to understand the causes of delay in or avoidance of health seeking behavior. We first estimate (static) reduced form specifications showing the impact of vaccine eligibility on the likelihood of delaying care. We find that vaccine-eligible adults are 4 percentage points (about 10% of the pre-policy mean, $p < 0.01$) less likely to delay care for any medical condition, whereas they are 2 percentage points (6.7%, $p < 0.01$) less likely to delay care for medical conditions other than COVID-19. In our dynamic specifications, we observe very stable pre-trends and a sharp decline in delayed care after the opening of vaccine eligibility.

Next, we turn to our primary focus of estimating the structural parameter of the effect of vaccination on delayed care using an instrumental variables (IV) approach. Our analysis reveals that vaccination reduces the likelihood of delaying care for any medical condition by 14.6 percentage points (36.6%, $p < 0.01$), and for medical conditions other than COVID-19 by 7.1 percentage points (24%, $p < 0.01$). Consistent with the existing literature (see, e.g., Agrawal et al., 2021), we find the effects of vaccination to be salient among non-White individuals, adults with relatively lower educational attainment and household income, households with children, as well as those that rent their housing. Our IV strategy is robust to a battery of specification checks.[4] We further account for the look-back period in delayed care by using a flexible approach that adjusts the policy assignment date by ten-day increments. We show that our baseline findings are not driven by the choice of policy assignment date.

In our context, it is conceivable that adults with different (unobserved) resistance to receiving a vaccine might also differ in their propensity to delay care. To explore heterogeneity in the LATE, we estimate the marginal treatment effects (MTE) using the "separate" approach following Heckman and Vytlacil (2007) and Brinch, Mogstad, and Wiswall (2017). Consistent with positive selection on unobservable gains, we find that adults with the lowest resistance to receiving vaccination are less likely to delay medical care relative to those with higher resistance. We also estimate average treatment effects (ATEs), and find these to be closer in magnitude to the LATE estimates. Specifically, the ATE implies that delayed care for any medical condition is reduced by 13 percentage points (32%, $p < 0.01$) for a randomly selected adult. On the other hand, the reduction in delayed care for conditions other than COVID-19 is 9 percentage points (29%, $p < 0.01$).

We explore a potential causal mechanism that explains the relationship between vaccination and delayed medical care. This mechanism also sheds light on earlier experimental and observational evidence on the effects of vaccination (Andersson et al., 2021; Furceri, Jimenez, and Kothari, 2021). Specifically, we formalize that vaccination shapes healthcare seeking behavior by altering perceptions of risk. To measure risk perceptions among adults, we exploit a survey question on concerns about contracting or spreading COVID-19. We find that receiving COVID-19 vaccination reduces concerns about virus exposure by 8.5 percentage points ($p < 0.01$). In fact, vaccination has a relatively large impact on concerns over COVID-19 compared to the baseline mean prior to vaccine eligibility (i.e., over 200 percent), suggesting that medical innovations can reduce hesitancy of care by, at least partially, influencing individuals' assessment of risk.

After establishing that vaccination reduces delayed care among adults by lessening concerns about getting or spreading COVID-19, we ask whether there are intrahousehold spillovers to children. Positive spillovers in the form of accessing preventive care may have

[4] As highlighted above, we have a strong first stage for the overall sample and different subsamples, supporting both the relevance and monotonicity assumptions. In the spirit of balancing tests, we show that the instrument is exogenous to observed characteristics, and rule out the possibility of a reverse feedback effect running from the outcome to the instrument. Although the exclusion restriction is not directly testable, we formalize alternative pathways by employing a directed acyclic graph (DAG). We show that the two-stage least squares (TSLS) estimates are remarkably robust to controlling for backdoor paths. Similarly, we observe consistent patterns from reduced form models.

indirect long-term benefits beyond health. The gist of the mechanism we have in mind is that when vaccinated adults feel safer to seek care for themselves (as shown by our analysis), they may also be more likely to seek care for their children as well. Our findings reveal a strong negative relationship between parental vaccination and the likelihood of delaying preventive care for children in the household, indicating that vaccination of parents may exert broader (unanticipated) health benefits for children beyond reducing their exposure to coronavirus.

This paper contributes to four strands of literature. First, our analysis provides the first credible evidence on the causal effect of COVID-19 vaccination on delayed or avoidance of medical care. Earlier experimental studies focus on COVID-19 vaccine anticipation and social distancing behavior (Andersson et al., 2021). Moreover, observational studies explore the effects of COVID-19 vaccination on mobility (Zhong, Wang, and Dai, 2021), mental health (Agrawal et al., 2021), mortality (Gupta et al., 2021; Barro, 2022), as well as hospitalizations and cases (Barro, 2022). We also complement these studies by identifying mechanisms that are likely to explain the changes in these outcomes. On the one hand, risk perceptions might serve as a potential mechanism through which vaccination affects mobility and mental health. On the other hand, reduced delayed care may explain the changes in excess mortality from COVID and non-COVID causes.

Second, our study offers a fresh contribution to the literature on the determinants of vaccine take-up, highlighting the roles of individual characteristics as well as contextual factors such as vaccination rollout (Schmitz and Wübker, 2011; Bronchetti, Huffman, and Magenheim, 2015; Jin and Koch, 2021; Karaivanov et al., 2021). Our findings complement earlier studies that document disparities in vaccination take-up (or hesitancy) with respect to age, gender, race/ethnicity, education, and economic factors (Maurer, 2009; Chang, 2018; Hoffmann, Mosquera, and Chadi, 2019; **?**; Momplaisir et al., 2021).

Third, our paper extends the literature on the impact of medical innovations on health behavior as well as the interplay between these innovations and health inequalities (Lichtenberg, 2014; Cutler and Miller, 2005; Papageorge, 2016; Hamilton et al., 2021). Vaccine development has traditionally been a process measured in many years and even decades. However, the ongoing pandemic, the continuous emergence of new infectious diseases, and new strains of current diseases have significantly increased the pressure to reduce the lifecycle of vaccine development. Achieving accelerated development and approval of vaccines require both technological advancements and a new regulatory and licensure process that operates on a faster track than the existing one. Our analysis implies that policies that would facilitate the availability of vaccines to public rapidly as well as in a safe manner may also provide the additional benefit of reducing disruptions to health seeking behavior during a public health crisis.

Finally, our findings contribute to the recently growing evidence on the effects of the COVID-19 outbreak and potential mitigation strategies (Walker et al., 2020; Anderson et al., 2020; Baker et al., 2020; Cutler and Summers, 2020). Previous work has focused on the role of the pandemic in explaining the changes in healthcare utilization (Ziedan, Simon, and Wing, 2020; Giannouchos et al., 2021; Cantor et al., 2022). Moreover, existing studies found a sharp decline in healthcare expenditures (Chetty et al., 2020). Our paper is unique in terms of linking vaccination to consumer healthcare spending and other spending categories that are broadly linked to economic activities. Overall, our study represents a comprehensive analysis of the causal relationship between vaccination and behaviors related to healthcare access and spending.

2. Data and Descriptive Statistics

2.1. Delayed Care

Our data source for the information on delayed care is the Household Pulse Survey (HPS), deployed by the U.S. Census Bureau, in collaboration with multiple federal agencies, with the purpose of measuring the social and economic implications of the COVID-19 pandemic.[5] The HPS is a 20-minute online survey that provides cross-sectional data on sociodemographic characteristics, self-reported health outcomes, health insurance coverage, access to care, and COVID-19 vaccination status, among other economic and pandemic-related measures in each wave. In this study, we use data from waves 1 through 33, covering the periods between April 23, 2020 and July 5, 2021.[6] We report the start and end date of each survey wave in Table B.1. Our sample includes over 2.2 million observations among adults aged 18 and older.

The primary goal of our analysis is to examine whether the decision of delaying or avoiding medical care is influenced by vaccination status. The data allow us to make a distinction between conditions related to pandemic and non-pandemic factors, specified by the following questions:

> "*At any time in the last 4 weeks, did you DELAY getting medical care because of the coronavirus pandemic?*"

and

> "*At any time in the last 4 weeks, did you need medical care for something other than coronavirus, but DID NOT GET IT because of the coronavirus*

[5] During the initial phase of the survey, five federal agencies partnered with the U.S. Census Bureau. These federal agencies include the Bureau of Labor Statistics, the National Center for Health Statistics, the United States Department of Agriculture's Economic Research Service, the National Center for Education Statistics, and the Department of Housing and Urban Development. In the latest phase of the HPS, as of the writing of this paper, the number of partnering federal agencies has increased to sixteen federal agencies.

[6] The questions about COVID-19 vaccination status have been included since wave 22, which corresponds to January 6, 2021.

pandemic?"

Note that these measures capture actions of the respondents themselves, who include the adult population aged 18 and older. However, we are also able to examine the extent to which children experienced any disruptions in their healthcare due to the pandemic. This information is provided by the following question:

> "*At any time in the last 12 months, did any children in the household miss, delay or skip any PREVENTIVE checkups because of the coronavirus pandemic?*"

Additionally, the HPS provides information on age, gender, race, ethnicity, marital status, educational attainment, household income, health insurance coverage, and the number of children. Accordingly, these characteristics are accounted for in our empirical analysis.[7] We further introduce a survey question related to concerns about exposure to coronavirus assessed by the respondents when we discuss potential mechanisms in Section 4.5.

2.2. Health Expenditures

We obtain health expenditure data from the Opportunity Insights Economic Tracker. It is a publicly available data source that combines anonymized information from leading private companies, including credit card processors and payroll firms, to provide a real-time glimpse of indicators such as employment rates, consumer spending, and job postings in the U.S. (Chetty et al., 2020). The high-frequency, granularity, and the massive size of the data make it ideal to track and study the behavior of individuals in an era where circumstances around the pandemic are rapidly changing.

Our data on consumer expenditures, measured by consumer credit and debit card spending, are provided by Affinity Solutions Inc. Although the Affinity spending series capture 10% of credit and debit card spending in the nation, Chetty et al. (2020) illustrate that the data closely track the historical benchmarks of retail spending and services, which are used in the construction of national accounts. Importantly, the Affinity spending data are representative of total card spending in the United States.

In our analysis, we focus on consumer spending in the healthcare industry.[8] We capture consumer expenditures associated with ambulatory healthcare services, hospitals, as well as nursing and residential care facilities. As a supplementary analysis, we also investigate the impact of COVID-19 vaccination on other spending categories, including overall spending, food services, entertainment, merchandise, grocery, and transportation.

[7] We also use an indicator variable to account for missing observations in household income

[8] The industry classifications in the publicly available data are based on the North American Industry Classification System (NAICS) codes. The healthcare industry is defined using the NAICS code 62, which is the healthcare and social assistance sector. Social assistance includes services provided to individuals with disabilities, emergency and other relief services, vocational rehabilitation services, among others.

Our sample period ranges from January 12, 2021 to July 5, 2021. The frequency of the data is daily.

2.3. State Vaccine Eligibility

The information on vaccine eligibility comes from the COVID-19 U.S. State Policy (CUSP) database (Raifman et al., 2020).[9] The CUSP database provides information on age-specific COVID-19 vaccine eligibility across states and over time. Table B.2 shows the policy implementation dates and age thresholds for vaccine eligibility. To address selection into receiving vaccination, we create an instrument that exploits the arguably exogenous variation in age-specific vaccine eligibility rollout over time.[10] Using the vaccine eligibility information from the CUSP, Agrawal et al. (2021) construct a similar instrument to investigate the impact of the COVID-19 vaccine distribution on mental health outcomes. We discuss the plausibility of our instrument and the identifying assumptions below.

Given the structure of survey questions, we observe changes in delayed care for the past few weeks rather than contemporaneous changes in healthcare seeking behavior. In our analysis, we initially account for the look-back period by assigning our policy measure (i.e., vaccine eligibility) to the start date of each wave. However, we later provide a battery of robustness checks by adjusting the policy assignment date, and show that our results are remarkably robust.

2.4. Vaccination Rate

Our analysis is aggregated to the state level in an alternative set of specifications that employ consumer spending as an outcome. Therefore, we use administrative data on vaccination rates at the state level, which also serve as a robustness check for our first stage analysis. The information on state level vaccination rates comes from Our World in

[9]This database also provides information on other mitigation measures or state-level policies implemented during the COVID-19 outbreak. Moreover, there is detailed information on state characteristics (e.g., population density, the number of homeless individuals, percent living under the federal poverty level, etc.). For example, Gupta et al. (2021) use these characteristics in CUSP to explore the relationship between vaccination rates and COVID-19 deaths.

[10]The objective in our instrumental strategy is to recover the treatment effect on compliers, whose eligibility status changed with respect to the age threshold. We note, however, that individuals with pre-existing health conditions or those in high-risk occupation groups (e.g., medical doctors, nurses, etc.) might have received vaccination prior to age-specific eligibility. On the one hand, the vaccination policy may not be binding for many of these groups. For instance, those with serious health conditions may be less likely to delay care regardless of the vaccination policy. On the other hand, not accounting for some of these groups may weaken our first stage. This does not seem to be an issue in our setting, as we show later, that we have a relatively strong first stage, with an F-statistic over 400. Moreover, individuals who received vaccination prior to age-specific eligibility constitute only 3% of our sample. Perhaps not surprisingly, excluding these individuals as a robustness check yields very consistent results. For brevity, we do not report these estimates, but are available upon request.

Data, an open data source reporting daily shares of people vaccinated against COVID-19 in the United States obtained from the Centers for Disease Control and Prevention (CDC). Specifically, we use information on the total number of individuals who received at least one vaccine dose per 100 people in the total population of a given state on a particular date. Note that the vaccination rate measure captures the first dose of a two-dose vaccine, and thus does not vary by the number of doses. We also impute missing (cumulative) vaccination rates when only the daily vaccination rate is available in the data.[11]

2.5. Descriptive Statistics

We provide descriptive statistics on the key variables in Table B.3. Panel A includes measures of delayed medical care, pandemic-related concerns, as well as vaccination status obtained from the HPS. There are few notable observations. First, we observe a sharp decline in delayed medical care after the introduction of COVID-19 vaccines. Specifically, about 40% and 30% report delaying care for any medical condition and conditions other than COVID-19, respectively, during the pre-vaccination period (i.e., prior to January 6, 2021). However, the likelihood of delaying care for any medical condition and conditions other than COVID-19 plummets to 23% and 17%, respectively, after vaccination rollout.[12] In our sample, 57.8% of adults report receiving COVID-19 vaccine. Perhaps more strikingly, we observe a decline in pandemic-related concerns such as getting or spreading COVID-19 during the post-vaccination period. Overall, the picture revealed by the descriptive statistics provides suggestive evidence that vaccines might indeed alter people's health seeking behavior during a pandemic, or times of high-uncertainty in general, perhaps, by influencing their assessment of risk.

The descriptive statistics on sociodemographic attributes are presented in Panel B of Table B.3. A display of these statistics separately between pre- and post-vaccination periods also serves as a balancing test as it would reveal whether the sample composition might have changed over time in a way correlated with the vaccine rollout. We have no reason to think that might be the case, and the figures in Panel B of Table B.3 support this presumption. Specifically, the distribution of individual attributes is fairly similar between the pre- and post-vaccination periods. We note that the average age in our sample is about 54. We have 40% males and 60% females in our sample. About 60% of

[11] For missing data points, we impute the data by adding the daily vaccination rate with its one-day lag or subtracting the daily vaccination rate from its one-day lead. There are a few exceptions on January 12 and 13, during which our imputation strategy is not feasible due to the lack of daily vaccination rates. For these observations, we assume that the vaccination rate is the same as the earliest data point available. However, our results remain the same if we eliminate these observations instead.

[12] Because the survey question on children delaying preventive care was available after wave 28, we do not report it in Table B.3. However, the mean delayed care for children is 0.282, with a standard deviation of 0.450. The number of observations is 104,697.

our sample are married, but they are less likely to have children. Adults in our sample are also more likely to have some college education, relatively high earnings, and health insurance coverage. We also present descriptive statistics of the outcome variables by sociodemographic attributes in Table B.4. We observe declines in delayed care across the board after vaccination rollout. Not surprisingly, vaccination uptake appears to be positively correlated with being White and higher socioeconomic status. Furthermore, despite a slightly lower vaccination uptake, we observe salient reductions in delayed care among non-White adults and those with lower socioeconomic status.

3. Empirical Methodology

Our objective is to evaluate the causal effect of COVID-19 vaccination on healthcare seeking behavior. An important challenge to achieving this goal in a credible manner is the potential endogeneity of individual vaccination status, especially in light of the fact that vaccination decision is voluntary. In other words, individuals who are vaccinated may be different than those who are not in ways that are correlated with their health seeking behavior and unobservable to us. To guard against endogeneity bias, we exploit the plausibly exogenous variation in age-specific state vaccine eligibility rollout as an instrument for vaccination status. Specifically, we formalize our research design using an instrumental variable (IV) specification of the following form:

$$Y_{ist} = \beta_0 + \beta_1 V_{ist} + \beta_2 X_{it} + \delta_s + \gamma_t + \epsilon_{ist}, \tag{1}$$

$$V_{ist} = \alpha_0 + \alpha_1 Z_{ist} + \alpha_2 X_{it} + \delta_s + \gamma_t + \nu_{ist}, \tag{2}$$

where

$$\mathbb{E}[\epsilon_{ist}, \nu_{ist} | X_{it}, \gamma_t, \delta_s] \neq 0, \mathbb{E}[\epsilon_{ist}, Z_{ist} | X_{it}, \gamma_t, \delta_s] = 0, \text{ and } \alpha_1 \neq 0.$$

In this model, Y_{ist} denotes the likelihood of delaying medical care for individual i in state s at survey wave t. V_{ist} measures whether an individual received COVID-19 vaccine. X_{it} is individual observable characteristics (age, gender, race, ethnicity, marital status, educational attainment, household income, health insurance coverage, and the number of children). Z_{ist} identifies individual vaccine eligibility based on the age threshold implemented across states and over time. To capture unobservable time-invariant state-specific factors and national time trends, we include a full set of state and survey wave fixed effects, denoted by δ_s and γ_t, respectively. Later, we also estimate models with region- or state-specific trends, and show that our results are remarkably robust.

In this setting, V_{ist} is endogenous, which is depicted by $\mathbb{E}[\epsilon_{ist}, \nu_{ist} | X_{it}, \gamma_t, \delta_s] \neq 0$. However, the instrument, Z_{ist}, is a strong predictor of vaccination status ($\alpha_1 \neq 0$), and affects healthcare seeking behavior only through its effect on vaccination status

($\mathbb{E}[\epsilon_{ist}, Z_{ist}|X_{it}, \gamma_t, \delta_s] = 0$). Exploiting this plausibly exogenous instrument, we estimate the IV specification above using two-stage least squares (TSLS). Specifically, we predict vaccination status in Equation (6). This predicted measure of $\hat{V}_{ist}$ replaces V_{ist} in Equation (4). To ensure that the estimates derived from our sample is representative of the underlying target population, we use survey weights provided by the HPS. We also cluster the standard errors at the state level.

A common interpretation of the TSLS estimand is that it reflects a weighted average of local average treatment effects (LATEs). However, the LATE interpretation does not necessarily hold when the IV specification includes covariates. Specifically, decomposition of the TSLS estimand with covariates shows that it not only reflects the treatment effects for compliers, but also the (negatively weighted) always-takers (Blandhol et al., 2022). Unless one imposes a correct specification between the instruments and covariates, the IV estimand will depend on the potential outcome levels instead of the treatment effect. A potential remedy is to estimate a fully "saturated" model that non-parametrically controls for covariates. Therefore, we further estimate our model: (i) without state-level covariates and (ii) by saturating the IV specification. To saturate specifications with state-level controls, we replace them with state and survey wave interaction terms. In all specifications, we also control for individual characteristics non-parametrically.

To explore treatment effect heterogeneity along the unobservable dimension, we also estimate the marginal treatment effects (MTE) (Heckman and Vytlacil, 2005; Heckman, Urzua, and Vytlacil, 2006; Brinch, Mogstad, and Wiswall, 2017). As described in detail below, this framework enables us to understand the average effects of vaccination status along different margins of the distribution of the unobserved resistance to receiving vaccination. This strategy further allows us to estimate the full distribution of marginal treatment effects and back out parameters of economic interest such as the average treatment effect (ATE) and average treatment on the treated (ATT) (Andresen, 2018).

3.1. Instrument Assumptions and Validity

Our identification of the causal effects of receiving vaccination on delayed care hinges on a set of assumptions, some of which are mentioned under the equations above. In our analysis, we must rely on four assumptions, including instrument relevance, instrument exogoneity, exclusion restriction, and monotonicity. Below, we discuss the validity of these assumptions and provide evidence in support of them.

3.1.1. Instrument Relevance

The validity of our instrument hinges on the proposition that age-specific vaccine eligibility is a strong predictor of individual vaccination status. We report the first stage estimates in Table 1. Column (1) suggests that we have a strong first stage, supporting the relevance assumption. Specifically, we find that vaccine eligibility increases COVID-19 vaccination by about 27 percentage points ($p < 0.01$). The first stage results are highly robust across specifications, further strengthening our confidence on the validity of the instrument. In Table B.5, we also show that the inclusion of linear trends or state-by-wave fixed effects does not alter our findings.

To further support the validity of the relevance assumption, we conduct a range of tests for underidentification and weak instruments. Specifically, we report the Kleibergen-Paap rank Wald F-statistic (Kleibergen and Paap, 2006) that ranges between 408.4 and 495.2. We also report the Kleibergen-Paap rank LM statistic to test for the rank condition. We do have evidence to reject the null hypothesis of underidentification, suggesting that the instrument is relevant. These first stage results complement existing studies that find substantial increases in vaccination uptake in Canada and Europe (i.e., France, Italy, and Germany) following COVID-19 vaccination mandates (Karaivanov et al., 2021).

3.1.2. Instrument Exogeneity

The independence (or exogeneity) assumption requires our instrument to be as good as random. Put differently, the instrument has to be independent of the outcome variable as well as the potential treatment assignment. We show earlier in Table B.3 that individual characteristics remain stable between the pre- and post-vaccination periods. Moreover, our first stage estimates are remarkably consistent across the board as shown in Table 1 (and Table B.5) as we control for covariates that are likely to be correlated with both vaccination status and healthcare seeking behavior.

To further assess the validity of the exogeneity assumption, we check whether the observable characteristics in each age cohort is correlated with vaccine eligibility. Specifically, we regress our instrument on observable characteristics in each age cohort, and report the estimates in Table B.6. Importantly, we do not observe any systematic correlation between vaccine eligibility and individual characteristics. There are only a few statistically significant estimates spread sporadically over the table, which is perhaps not surprising due to large sample size. Moreover, we do not find any consistent patterns with respect to the sign of the relationship.

As a supplementary analysis, we consider the possibility of our outcome having a (reverse) feedback effect on the policy itself. For instance, if states with high rates of delayed care expand vaccine eligibility as a response, then our instrument would be endogeneous. To see whether our outcome influences future policy decisions, we regress delayed care on current vaccine eligibility as well as the leads of vaccine eligibility (i.e., policy leads), using a flexible approach with the leads increased by ten-day increments up to 30 days. The estimates from this exercise are plotted in Figure B.1. The first estimate reported for day zero shows the contemporaneous effect, which is statistically significant.

We also observe a clear stable pattern for the policy leads. However, all of the policy leads are statistically insignificant at the 5 percent level.

Although the exogeneity assumption is critical for establishing causality in reduced form specifications, it does not ensure that the exclusion restriction is satisfied in an IV framework. In the next section, we discuss the conditions in which exclusion restriction is plausibly satisfied.

3.1.3. Exclusion Restriction

The exclusion restriction requires that age-specific vaccine eligibility affects delayed care only through its impact on vaccination status. This assumption would be violated if there are alternative channels through which vaccine eligibility influences the likelihood of delaying care. We refer to these alternative channels as backdoor paths.

To formally identify backdoor paths, we employ a directed acyclic graph (DAG). Leaving open backdoor paths that do not contain vaccination status would make the exclusion restriction invalid. We provide a simple illustration of backdoor paths in Figure B.2. We identify two potential backdoor paths: $Z_{ist} \leftarrow P \rightarrow Y_{ist}$ and $Z_{ist} \rightarrow S \rightarrow Y_{ist}$. First of all, pre-existing economic conditions, health measures, and policy responses to the pandemic can influence both access to care and consumer spending, as well as states' adoption of vaccination policies, emerging a backdoor path via P. A plausible way to remove the effects of backdoor paths is to condition on them. Therefore, we control for pre-existing COVID-19 death rates and the stringency index to proxy for health measures and states' existing policy responses, respectively.[13] Additionally, we control for the pre-existing unemployment rate to proxy for economic conditions.[14]

We formalize another path via S. There could be positive spillovers of vaccination policies on unvaccinated individuals. Specifically, individuals who are not vaccinated may feel safer to seek care, which would impact both the likelihood of delaying care and consumer healthcare spending. Moreover, there could be spillovers to the healthcare sector. With the onset of the COVID-19 pandemic, most healthcare facilities have rushed to increasing the stringency of clinical guidelines on preventive measures such as phone screening and physical distancing (Aslim and Mungan, 2020). These guidelines were also complemented with the expansion of telehealth services. However, some of these preventive measures were relaxed once vaccines became available. This might have lowered the

[13]We obtain data on COVID-19 death rates compiled by The New York Times (2021) and data on stringency index from Oxford COVID-19 Government Response Tracker (Hale et al., 2021). The death rate and the stringency index are weekly measures. To capture pre-existing conditions, we construct a two-week lagged version of these variables. This also alleviates potential concerns about creating a collider using contemporaneous values of these measures.

[14]We obtain data on unemployment rate from the U.S. Bureau of Labor Statistics. Since the unemployment rate is monthly, we take a one-month lag to account for pre-existing conditions that may influence the adoption of vaccination policies. It is natural to assume policy lags, particularly when policymakers respond to macroeconomic conditions (see, e.g., Aslim, Panovska, and Taş, 2021).

opportunity cost of accessing medical care, and therefore could have reduce the likelihood of delaying or avoiding care and increase health expenditures. Since this behavioral response is mainly driven by unvaccinated individuals, controlling for trends that impact the fraction of unvaccinated would likely close the backdoor path. We rely on a flexible approach that controls for such trends separately at the regional and state levels.

There may be region-specific shocks influencing the decision eligible individuals to get vaccinated, which might in turn shape their behavior about seeking care during the pandemic (e.g., supply-side shocks that impact vaccine distribution and delivery of care or even sociopolitical factors that exhibit regional variation). To account for such spillovers at the regional level, we control for region-by-wave fixed effects (θ_{rt}) in our most parsimonious specification.[15] In alternative specifications, we also control for state-specific linear trends or state-by-wave fixed effects to account for unobserved trends across states that might influence our backdoor paths. Intuitively, this would capture any state-specific responses that affect healthcare seeking behavior among eligible but unvaccinated individuals. For instance, if individuals are less likely to delay care because state vaccination rates increase over time, then this approach would account for such trends.

We also take into account the potential role of social networks that may shape beliefs and behavior during a public health crisis (Bailey et al., 2020). Spillovers within these social networks in the form of positive or negative messages (or posts) about COVID-19 vaccines may create a backdoor path by altering behavior of individuals who are eligible for vaccination. For instance, an eligible individual who is exposed to posts on social media about the potential side effects of COVID-19 vaccine or conceptions about its efficacy in safeguarding against the virus may defer receiving vaccination and seeking healthcare. To mitigate the effects of spillovers within social networks, we construct a measure of friend-exposure to vaccination information using the Social Connectedness Index from Facebook and data on vaccination rates. We describe the construction of this measure in Appendix Section A1. If there are changes in risk perceptions due to unmodeled spillovers within these networks, this would likely be captured by our friend-exposure measure.

3.1.4. Monotonicity

The instrument assumptions above are sufficient for causal interpretation under constant treatment effects. However, we need the monotonicity assumption to recover the LATE under heterogeneous treatment effects. Specifically, this assumption requires that $\mathbb{E}[V_{ist}|X_{it}, \gamma_t, \delta_s, Z_{ist} = 1] \geq \mathbb{E}[V_{ist}|X_{it}, \gamma_t, \delta_s, Z_{ist} = 0]$ or $\mathbb{E}[V_{ist}|X_{it}, \gamma_t, \delta_s, Z_{ist} = 1] \leq \mathbb{E}[V_{ist}|X_{it}, \gamma_t, \delta_s, Z_{ist} = 0]$. Put differently, age-based vaccine eligibility should weakly op-

[15] We use the four U.S. Census regions: Northeast, Midwest, South, and West.

erate in the same direction for all individuals. A violation of the monotonicity assumption would indicate that age-specific vaccine eligibility pushes some people into treatment while pushing others out. The existence of "defiers" would violate this assumption.

While the monotonicity assumption is not directly testable, ex ante this issue does not appear to be prevalent in our setting. The policy is designed to expand vaccine eligibility to all adults within an age cohort. We nonetheless present first stage results for different subsamples. Here, we impose an *average* monotonicity assumption, implying that age-specific vaccine eligibility has a weak positive effect on vaccination uptake in all subsamples. This assumption is sufficient to recover the LATE (Frandsen, Lefgren, and Leslie, 2019). Figure B.3 reports first stage estimates for a large set of subsamples based on: race/ethinicity, educational attainment, household income, gender, whether there are children in the household, and housing status (e.g., rent or own). Consistent with our assumption, we find a positive and statistically significant relationship between vaccine eligibility and vaccination uptake. We also show that all of the estimated coefficients range between 0.2 and 0.3.

4. Results

4.1. Reduced Form and Dynamic Difference-in-Differences Estimates

We begin by showing the reduced form estimates of the impact of vaccine eligibility on the decision to delay medical care in Table 2. To check the sensitivity of our estimates, we report results from a range of specifications beginning with state and survey wave fixed effects in column (1) and successively adding region-by-wave fixed effects in column (2), health and policy response controls in column (3), economic conditions in column (4), and social networks in column (5). As shown in the table, our estimates are remarkably robust across these specifications, indicating that vaccine eligibility reduces the likelihood of delayed medical care by about 4 percentage points in Panel A ($p < 0.01$). This represents a 10% decrease relative to the mean delayed care prior to the policy (see Table B.3 for baseline means). Some of these individuals are likely to delay care for symptoms associated with COVID-19. Therefore, we further explore whether vaccine eligibility reduces the likelihood of delaying medical care for something other than COVID-19. We affirmatively answer this question by obtaining a 2 percentage points decrease in the likelihood of delayed care for conditions other than COVID-19 in Panel B (6.7%, $p < 0.01$).

Next, we use a dynamic difference-in-differences (DID) approach to show that the outcomes in these states trend similarly in the absence of age-specific vaccine eligibility policies. To mitigate contamination bias in leads and lags resulting from using already-treated units as a control, we use the estimator developed by Sun and Abraham (2021). We first check whether our first stage exhibits any pre-trends in Figure 2.[16] We observe very stable pre-trends followed by a sharp increase in vaccination uptake upon vaccine eligibility. In these specifications, we do not control for any observable characteristics to show that our approach does not necessitate parallel trends *conditional* on covariates. Note that our dynamic specifications only control for state and survey wave fixed effects.

Figure 3 presents the dynamic reduced form estimates of the impact of vaccine eligibility on delayed medical care. We again do not condition on any observable characteristics. Following Sun and Abraham (2021), we only trace the dynamic effects until all cohorts are treated because the DID estimator is not well-defined if the control cohort is empty.[17] Similar to our previous analysis, we do not observe any systematic patterns in pre-trends. There is, however, a clear departure from the pre-trends and a sharp decline in the likelihood of delaying care once states began rolling out vaccine eligibility. Taken together, these findings strongly support the notion that vaccine eligibility has a causal effect on the delayed care propensity.

4.2. Vaccination Status and Delayed Care

Table 3 shows the estimates of the impact of COVID-19 vaccination on the propensity of delayed care. Similar to Table 2, we present estimates from a range of models increasing in fixed effects and control variables. The results obtained from the OLS are displayed in columns (1)-(5), and those obtained from the TSLS are shown in columns (6)-(10). Note that the IV specifications in columns (6)-(7) are fully saturated, thus serving as a validity check for the LATE interpretation of our estimates. We begin by using the most broadly defined measure of delayed care in Panel A: the likelihood of delaying medical care for any medical condition. The OLS estimates are extremely robust across specifications, implying that receiving a COVID-19 vaccine reduces the likelihood of delaying care by 3.5 percentage points ($p < 0.01$). This represents a relative reduction of 8.8%.

Turning to the TSLS estimates, the most parsimonious specification in column (6) indicates that individuals are 15 percentage points less likely to delay care, which translates to a 37.6% decrease relative to the pre-treatment mean for individuals who delay care ($p < 0.01$). To close potential backdoor paths that may violate the exclusion restriction, we control for region-by-wave FE, pre-existing health measures and policy responses, as well as pre-existing economic conditions in columns (8) and (9), and finally in column (10), we control for a measure of social media connectedness to guard against bias due to possible spillovers that may influence individual decisions to get vaccinated. Our estimate remains essentially unchanged across specifications, suggesting that state and survey wave fixed

[16] Because individuals did not receive any vaccination prior to wave 22, our pre-period event window goes back to -6.

[17] See Section 4.2 for a detailed discussion in Sun and Abraham (2021).

effects are likely sufficient to capture these additional factors. Importantly, this further supports the proposition that our baseline estimates recover the treatment effects on compliers, rather than the effects on both compliers and always-takers (Blandhol et al., 2022). According to the point estimate in the most comprehensive specification in column (10), vaccination status decreases the propensity to delay medical care for any condition by 14.6 percentage points (36.6%, $p < 0.01$).

Next, we consider whether vaccination has any impact on the likelihood of delaying care for medical conditions other than COVID-19. This is particularly important because delaying care, especially for chronic conditions, may worsen health outcomes, increase healthcare utilization, and surge expenditures in the long run (see, e.g., Richards et al. 2020). Moreover, a precipitous increase in the prevalence of hospitalizations and utilization of acute care services may overburden the healthcare system (Miller et al., 2020). We present results for delayed care for other medical conditions in Panel B of Table 3.

Across OLS estimates in columns (1)-(5), we find a 3 percentage points decline in delayed care for medical conditions other than COVID-19 (10%, $p < 0.01$). The TSLS estimates also indicate a statistically significant, large, and negative effect on delay care for other medical conditions. Specifically, we estimate that COVID-19 vaccination reduces delay care for other medical conditions by 7.5 percentage points without regional or state controls (25%, $p < 0.01$) and 7.1 percentage points with regional or state controls (24%, $p < 0.01$). These findings imply that our estimates are remarkably robust to the inclusion of covariates that close potential backdoor paths between vaccine eligibility and delayed medical care.

4.3. Heterogeneity

4.3.1. Observable attributes

There are reasons to think that both vaccine uptake and its impact on delayed medical care may be salient across certain subgroups. If there are barriers to accessing care associated with sociodemographic characteristics such as education or cognitive correlates, vaccination policies may have heterogeneous effects on shaping individual beliefs and behavior. For instance, previous studies show that COVID-19 vaccine hesitancy is higher among African-American and Hispanic populations, and that gender, education, income, or even medical mistrust and exposure to misinformation are strong predictors of vaccine hesitancy (see, e.g., Momplaisir et al. 2021).

To test for monotonicity, we have already presented our first stage results by sociodemographic groups in Figure B.3. However, we have not interpreted our results in the context of heterogeneity as well as existing studies mentioned above. Consistent with the literature, we have a few notable observations. In terms of magnitude, we find salient effects on vaccine uptake among White individuals, males, as well as those with relatively higher socioeconomic status. Specifically, we find disparities based on educational attainment, household income, the number of children, and housing status. Although these findings stress the importance of mitigating existing barriers, we observe a non-negligible increase in COVID-19 vaccination among minorities, females, and those with relatively lower socioeconomic status.

Figure 4 shows the impact of vaccination on delayed care for any medical condition and for conditions other than COVID-19 by subgroups. We have similar patterns using both measures of delayed care. In contrast to the first stage, we find strong, negative, and statistically significant effects of vaccination on minorities and those with relatively lower educational attainment and household income. Focusing only on delaying care for any medical condition, we further find salient effects on households with children and those that rent instead of own a housing unit. This complements the findings of Agrawal et al. (2021) who show similar reductions among these subgroups for anxiety and depression symptoms upon vaccine eligibility. These findings are important in their own right as they highlight the critical role that effective and equitable distribution of vaccines may play in mitigating some of the existing barriers to medical care during a public health crisis among disadvantaged populations.

4.3.2. Marginal treatment effects

Our benchmark analysis recovers the treatment effect of vaccination on compliers, i.e., the LATE. However, the LATE does not take into account the possibility of treatment effect heterogeneity across different types of adults. Specifically, selection into treatment could be based on (unobserved) treatment gains rather than just levels (Heckman, Urzua, and Vytlacil, 2006). For instance, adults with low resistance to receiving vaccination may have different gains than those with high resistance (Andresen, 2018). The marginal treatment effects (MTE) analysis provides insights into how the impact of vaccination status changes along different margins of the distribution of the unobserved resistance to receiving vaccination. Importantly, estimating the full distribution of treatment effects enables us to recover parameters of economic interest, including the average treatment effect (ATE), the average treatment on the treated (ATT) and untreated (ATUT). Using the potential outcomes framework, we provide the technical details regarding the derivation of the MTE in Appendix Section A2.

Our computation of the MTE closely follows Brinch, Mogstad, and Wiswall (2017), who formulate a separate estimation approach extending the Local Instrumental Variables (LIV) method developed by Heckman and Vytlacil (2005, 2007) to applications with discrete instruments. In the separate approach, the MTE curve is identified by estimating the conditional expectation of Y_1 and Y_0 separately for the treated and the untreated.[18] Moreover, under conditional independence, the separate approach allows for the estimation of a linear approximation of the MTE curve as opposed to the (LIV) method. In other words, the unobserved component for the treated and the untreated can be specified as a linear function of the propensity score. Perhaps more importantly, as demonstrated in Brinch, Mogstad, and Wiswall (2017), invoking the additional assumption of additive separability between observed and unobserved heterogeneity in the treatment effect can relax the parametric restriction on the unobserved component, and this further allows us to estimate a flexible MTE along the common support of the propensity score instead of the propensity score given X.

The left panel of Figure 5 reports the predicted probabilities of receiving a COVID-19 vaccine for vaccinated and unvaccinated adults. We observe a substantial overlap between treated and untreated adults over the unit interval, and this allows us to estimate the MTE curve along this range of common support. Intuitively, the probability of receiving vaccination is higher among the treated adults than the untreated adults, leading to left and right skewed distributions, respectively. The right panel of Figure 5 presents the estimated MTE curves along with 95% confidence intervals.

In panel (a), the outcome of interest is the likelihood of delaying medical care for any medical condition. We observe a clear and meaningful picture. The MTEs increase monotonically as the unobserved resistance to receiving vaccination increases. These findings are consistent with the hypothesis of positive selection on unobservable gains (Andresen, 2018). That is, those with the least resistance experience a larger decline in delayed medical care as opposed to those with higher unobserved resistance. In fact, the heterogeneous treatment effects for those with the highest resistance become statistically insignificant towards the end of the distribution of unobserved propensities.[19] Panel (b) shows a similar but highly significant pattern for delaying care for medical conditions other than COVID-19. Moreover, the test for joint significance of the coefficients for unobservables implies substantial heterogeneity in terms of selection on treatment gains in both panels (a) and (b). The p-values are 0.0001 and 0.037, respectively.

We report the ATE, ATT, and ATUT in the lower right corner of the MTE curves. We find negative and statistically significant ATEs and ATTs in both panels (a) and

[18] A main advantage of this approach is that we only need to identify four averages for the groups including treated and untreated individuals when the instrument is switched on and off (Andresen, 2018; Brinch, Mogstad, and Wiswall, 2017).

[19] An ex-ante claim might be that those with higher resistance to vaccination might be more likely to delay care if, for example, they hear stories of other people contracting COVID-19 upon becoming vaccine eligible, even more dramatically, after getting vaccinated. The idea is that medical mistrust is highly correlated with vaccine hesitancy (Charura, Hill, and Etherson, 2022). If such anecdotes increase the likelihood of medical mistrust (by nurturing the belief that medical innovations or the healthcare system in general do not protect individuals), we may also observe increased delayed care. We do not, however, find any evidence supporting this claim since the estimates are not statistically different from zero when the resistance (U_D) is greater than 0.9.

(b). Specifically, the ATEs suggest that receiving vaccination reduces delayed care by 13 (32%, $p < 0.01$) and 9 percentage points (29%, $p < 0.01$) for a randomly selected adult in panels (a) and (b), respectively. Although the estimated ATEs are closer in magnitude to the LATE estimates, we find ATTs to be slightly larger. Moreover, we also find ATUTs to be negative and statistically significant. Overall, the MTE analysis suggests that there is non-negligible heterogeneity on unobserved treatment gains and that those with low resistance to receiving vaccination experience larger declines in the propensity to delay care.

4.4. Robustness of First Stage and Delayed Care Results

4.4.1. Trend Analysis

In this section, we check the robustness of our benchmark results to alternative specifications. One might worry that there are unobserved trends in delayed medical care that is also correlated with the timing of age-based vaccine eligibility rollout. These unobserved trends could be driven by factors that create an alternative path between vaccine eligibility and delayed medical care, in turn violating the exclusion restriction. Specifically, eligible individuals who are not vaccinated might change their perceptions and behavior associated with seeking care as healthcare facilities attempt to mitigate barriers following states' adoption of vaccination policies.[20] This implies that such trends might influence the likelihood of delaying medical care and does not necessarily require an individual to receive COVID-19 vaccination.

To account for convergence in vaccination behavior and unobserved (regional) shocks that might alter perceptions about COVID-19 vaccine and healthcare access, we include region-by-wave fixed effects in our benchmark analysis. In Table B.8, we extend our analysis to include state-specific (or region-specific) time trends that capture alternative pathways between vaccine eligibility and delayed care. Moreover, we control for unobserved shocks non-parametrically by exploiting the interactions between state and survey wave fixed effects. We provide the OLS estimates in columns (1)-(3) and the TSLS estimates in columns (4)-(6). Given that the decision to get vaccinated is endogenous, we will not dwell on the OLS estimates since it is likely to be biased. We show that adding region-specific and state-specific linear trends yields quantitatively similar TSLS estimates compared to different specifications in our benchmark analysis.

Accounting for state-specific linear trends in column (5) of Panel A, the coefficient indicates a 14.1 percentage points decline in the likelihood of delaying care for any medical condition (35.3%, $p < 0.01$). In our most conservative specification in column (6), we control for state-by-wave fixed effects. Note that this model is fully saturated since it

[20]Note that vaccination policies could also impact COVID-19 cases and hospitalization rates, thereby creating externalities that influence healthcare access.

non-parametrically controls for state characteristics that vary over time. In other words, we are able check whether the LATE interpretation holds in our most comprehensive specification. The effect size on our TSLS estimate is again very similar, suggesting a 13.9 (34.8%, $p < 0.01$) percentage point decline in delay for any medical condition caused by receiving a vaccine.

Panel B presents TSLS estimates for our alternative measure of delaying care for medical conditions other than COVID-19. Our findings are very similar in magnitude across the board compared to the benchmark case. We show that vaccinated individuals are less likely to delay care for other medical conditions by about 7.1 percentage points ($p < 0.01$). This represents a relative reduction of about 23.7%. Note that our baseline estimates in Table 3 range between 24% and 25%.

Taken together, our findings indicate that our benchmark specifications are extremely robust to alternative specifications that aim to close potential backdoor paths running from vaccine eligibility to delayed medical care, lending further support on the validity of the exclusion restriction.

4.4.2. Adjusting Policy Assignment Date

As described earlier, our outcome measures the likelihood of delaying care in the *last 4 weeks*. We might underestimate the effect of vaccine eligibility (and hence vaccination status) on delayed care in the past weeks if the policy is assigned contemporaneously. To test this possibility, we perform a sensitivity analysis that varies policy assignment by ten-day increments before the start of a survey wave. The motivation behind this exercise is to assess whether our benchmark estimates are sensitive to alternative policy assignments given the look-back period for delayed care.

Figure B.4 provides the estimates for delayed care for any medical condition and conditions other than COVID-19, respectively, using: (i) the reduced form specification (top panel); (ii) the IV specification (middle panel); and (iii) the first stage (bottom panel). First of all, we find that the first stage estimates vary around 28 percentage points across different policy assignment dates. These estimates are consistent with our baseline results. Perhaps more importantly, we find that both reduced form and TSLS estimates for delayed care are extremely robust to the adjustment for the look-back period. Overall, these findings highlight that the impact of vaccination status on delayed care in the past weeks is not driven by the choice of policy assignment date.

4.5. Mechanisms

Our findings so far indicate that vaccination uptake affects the delay or avoidance of seeking healthcare during the COVID-19 outbreak. Although this finding is novel and informative in and of itself, it is important to explore the potential factors behind the decision to change healthcare seeking behavior upon vaccination. Earlier experimental and observational studies show that the anticipation of COVID-19 vaccines as well as vaccine rollout cause individuals to update their beliefs and behaviors during the pandemic, particularly in the form of increased optimism and reduced worries about the pandemic; decreased social distancing; and increased mobility (Andersson et al., 2021; Zhong, Wang, and Dai, 2021; Furceri, Jimenez, and Kothari, 2021). An important theme that emerges here is that if individuals are vaccinated, then they might have less fears or concerns about contracting or spreading COVID-19. The intuition is simple: vaccines can increase mobility, reduce social distancing behavior, and, perhaps more importantly, shape healthcare seeking behavior by altering risk perceptions about virus exposure. The evidence that we highlight below is consistent with this mechanism.

To formally explore how receiving COVID-19 operates in reducing delayed care, we exploit a survey question in the HPS regarding concerns about contracting or spreading COVID-19.[21] Table 4 presents the estimates for both OLS and IV specifications. The OLS estimates in columns (1)-(5) are close to zero (about 0.8 percentage points). However, the TSLS estimates in columns (6)-(10) suggest that vaccinated individuals are less likely to have concerns about getting or spreading COVID-19. We show that receiving vaccination reduces such concerns by about 8.5 percentage points across specifications ($p < 0.01$). This represent a more than 200 percent reduction in concerns as individuals receive COVID-19 vaccines, suggesting that medical innovations might have a substantial influence on shaping behavior, likely through altering risk perceptions. The first stage estimates in Table B.9 also confirm that vaccine eligibility increases the likelihood of receiving a vaccine by about 34 percentage points. Our findings on reduced concerns or fears related to COVID-19 also explain the potential mechanisms behind the effects of vaccination on mobility, social distancing behavior, or changes in economic activity during the pandemic. Crucially, we explicitly estimate an intermediate step related to how individuals perceive the risk of contracting or spreading the virus.

4.6. Spillovers to Children in the Household

Our main analysis suggests that vaccine eligibility is highly effective in reducing delayed medical care among adults aged 18 and older. We study whether these results extend to children as well as more specific forms of healthcare such as preventive care. Studying the potential spillover to children is important because limited access to healthcare during childhood has been shown translate into poor outcomes in adolescence and adulthood with

[21]The question in the HPS that gauges concerns about coronavirus exposure is the following: *"Main reason for not working for pay or profit – I was concerned about getting or spreading the coronavirus."* Although the question pertains to reasons for not working, those who are concerned about contracting or spreading COVID-19 at work may have similar (if not more) worries or concerns about visiting healthcare facilities.

respect to health, human capital accumulation, labor market productivity, and criminal behavior (Currie, Decker, and Lin, 2008; Levine and Schanzenbach, 2009; Cohodes et al., 2016; Miller and Wherry, 2019; Brown, Kowalski, and Lurie, 2020; Arenberg, Neller, and Stripling, 2020). If vaccinated adults are more likely to seek medical care for themselves during COVID-19, there could be (positive) health spillovers to children in the household. We test the hypothesis that age-based vaccine eligibility increases vaccination uptake among adults, thereby reduces the likelihood of delaying preventive care for any children in the household.[22]

The first stage estimates in column (5) of Table B.9 indicate that age-based vaccine eligibility increases the likelihood of receiving vaccination by 6.6 percentage points ($p < 0.01$). Our weak-identification test reports a Kleibergen-Paap rank Wald F-statistic of 25, which is greater than the conventional thresholds but much lower than our benchmark case. We also note that we have a much smaller sample size since this question was added to the survey after wave 28.

The OLS results shown in columns (1) to (5) of Table 5 indicate a small but positive relationship between vaccination and children delaying preventive checkups during the COVID-19 pandemic. The TSLS estimates displayed in columns (6) to (10), however, reveal a different story. The estimates are negative and large in magnitude across all four specifications. Furthermore, while the estimates remain largely robust as we add more controls, they become more precisely estimated. Children are significantly less likely to miss out on or have their healthcare delayed as a result of the availability of vaccines for their parents. Focusing on the estimate from our most comprehensive specification in column (10), vaccine eligibility for adults appears to cause a 48 percentage point decrease in the propensity of delayed medical care for children ($p < 0.05$). Overall, this result reveals that adult vaccination offers a significant positive health spillover for children regardless of their vaccination status.

4.7. Vaccination and Health Expenditures

Healthcare expenditures have dropped substantially since the beginning of the COVID-19 pandemic (Chetty et al., 2020).[23] According to Chetty et al. (2020), compared to January 2020, there had been a 55% reduction in healthcare spending in the U.S. by the end of March 2020.[24] Previous studies point at a sizeable decline in healthcare utilization over

[22] We check whether children's vaccine eligibility overlaps with our sample period. According to the U.S. Food and Drug Administration (FDA), COVID-19 vaccines for children aged 5-11 were not authorized until October 2021. See the report here: `https://bit.ly/3vEssNW`. However, there is a slight overlap of COVID-19 vaccine approval for teenagers aged 12-15. But note that this approval was on May 2021, and only covered the Pfizer vaccine. Moreover, using data from the CDC, we find relatively low vaccination uptake among this group during our sample period (about 20%).

[23] In fact, Aslim, Chou, and De (2022) show that (real) personal healthcare expenditures decline during economic downturns, including the COVID-19 period.

[24] During the same period, the total spending in the U.S. has decreased by 29%.

the same period (Hartnett et al., 2020; Giannouchos et al., 2021; Cantor et al., 2022). Our analysis confirms that vaccination efforts have a positive impact on healthcare seeking behavior by lowering the likelihood of delayed medical care. A proposition that flows logically from this finding is that the vaccination status may also be linked to an increase in healthcare spending or, more broadly, recovery of economic activities.

In this section, we analyze how COVID-19 vaccination affects health expenditures in the United States. Our empirical strategy is the same as the analysis of delayed care, with slight changes in the structure of data. We exploit daily state-level data on health expenditures and vaccination rates. Specifically, the outcome of interest is the seasonally adjusted health expenditures observed in state s and date t (relative to January 4-31, 2020). The treatment variable is the vaccination rate, which is the total number of individuals who received at least one vaccine dose per 100 people, and the instrument is the proportion of individuals eligible for vaccination based on the age cutoff.

We present the estimated effect of vaccination on health expenditures in the top panel of Table 6. For OLS estimates in columns (1)-(5), we find that the vaccination rate is positively associated with health expenditures across different specifications, but all estimates are close to zero and statistically insignificant.

The TSLS estimates are presented in columns (6)-(10). The estimates suggest that increasing the vaccination rate per 100 by one person increases health expenditures by 0.1 to 0.4 percent relative to January 4-31, 2020. However, none of the estimates are statistically significant at conventional levels. In contrast to the TSLS estimates, the coefficients from the first stage analysis are much more precisely estimated. As shown in the bottom panel of Table 6, we find that one percentage point increase in the proportion of eligible individuals increases the vaccination rate by about 0.06 to 0.08 percent ($p < 0.01$).

Although our instrumental variable is constructed using different age-specific vaccine eligibility cutoffs, the value of the instrument never reaches 100 percent since the vaccination was not authorized for people under 18 years old during our study period. Hence, we test the robustness of our TSLS estimates by using a *normalized* instrument, where we calculate the proportion of eligible individuals based only on the population older than 18. We present the results from the normalized IV in Table B.10. In short, we find very similar estimates compared to those in Table 6.

Overall, we find no robust evidence that the vaccination rate affects healthcare spending, at least, in the short term.[25] Note that we do not observe charges by providers, but rather observe expenditures by consumers. In other words, we are neither able to track

[25] We further check the results using leads of the outcome to test for potential lagged effects of vaccination, and find positive but statistically insignificant effects. We note that the aggregate measure of healthcare spending may mask individual-level effects, especially given that we find a strong impact of individual vaccination status on the likelihood of delaying care. To preserve space, we do not report the results testing for lagged effects, but they are available upon request.

the dollar amount of uncompensated care nor the dollar value of medical debts collected through federal relief funds during COVID-19.[26] Based on suggestive evidence, however, 50 and 34 percent of uninsured adults and those with employer coverage, respectively, had medical bills or debt problems during the first year of the pandemic (Collins, Aboulafia, and Gunja, 2021). Therefore, the amount of uncompensated care or unpaid medical bills during the pandemic may explain, at least partially, as to why there is no significant increase in consumer healthcare spending in the short run.

As a supplementary analysis, we further explore the effect of vaccination on other expenditure categories, including food services, entertainment, merchandise, grocery, transportation, and all expenditures (which aggregates all categories). As shown in Table B.11, we find that vaccination significantly increases expenditures associated with transportation. This result complements earlier studies finding an increase in the demand for public transportation after vaccination (see, e.g., Zhong, Wang, and Dai 2021). It is also suggestive of a potential increase in other expenditure categories in the longer term. Taken together, it may be crucial to track consumer healthcare spending for a longer period to explore the extent to which avoidance of care influences both consumer and provider finances, particularly as federal aides become unavailable.

5. Conclusion

The estimates from the Household Pulse Survey imply that over 40 percent of adults delayed medical care for any medical condition during the first few months of the COVID-19 outbreak. Delaying or avoiding care has negative implications for both the demand and supply side of healthcare sector. Yet we know very little about the policies or innovations that shape healthcare seeking behavior during a pandemic or times of a public health crisis. This paper provides the first causal estimate of the effect of COVID-19 vaccination on the likelihood of delayed or foregone medical care among Americans. To achieve this goal, we exploit the plausibly exogenous variation in age-specific vaccine eligibility rollout across states and over time. We begin by showing that vaccine eligibility substantially increases vaccination take-up across subgroups and in the overall population. Then we recover the structural estimate of the impact of individual vaccination status on delayed or foregone medical care using age-specific vaccine eligibility as an instrument. We document that receiving vaccination reduces delayed care for any medical condition by 37%, while reducing delayed care for conditions other than COVID-19 by 24%.

Our investigation into potential mechanisms for this finding indicates that vaccination affects the likelihood of delaying care by assuaging fears about exposure to coronavirus, suggesting that individuals likely update their risk perceptions upon getting vaccinated.

[26]For instance, the Provider Relief Fund (PRF) under the CARES Act paid providers \$2.5 billion for the treatment of uninsured COVID-19 patients as of September 2021 (Parente and Mortensen, 2022).

Another important finding from our analysis is that children are significantly less likely to delay or skip healthcare delayed as a result of the availability of vaccines for their parents, revealing the presence of a positive health spillover within households. Given that states have started expanding vaccine eligibility to children in late 2021, an interesting avenue of future work is to explore whether children's vaccination had any impact on the likelihood of seeking preventive care among this group. Medical innovations are particularly important as delay or avoidance of medical care for children have negative ramifications beyond health, particularly in the form of worsening human capital, poor labor market productivity, and increased criminality. Policymakers ought to consider such innovations when the objective is to shift health and economic behavior during a pandemic.

Millions of physician practices have experienced sharp reductions in revenues during the pandemic as health services utilization dropped precipitously, threatening the viability of the entire healthcare sector. The government response to the crisis was primarily to infuse significant amount of funds intended to help alleviate the fiscal effect of revenue loss. Just to name a few examples, the Congress authorized over $170 billion through the Provider Relief Fund to compensate hospitals and other health care providers for financial losses and unanticipated costs during the pandemic. Another $7.5 billion were distributed to hospitals and other providers that serve patients living in rural areas through the American Rescue Plan. Congress also established the Paycheck Protection Program that provided health care providers an estimated $100 billion in Paycheck Protection Program loans. Our analysis indicates that continued efforts to reduce COVID-19 transmission through vaccination may produce significant economic benefits by helping the recovery of the healthcare sector, potentially easing the fiscal burden on the government.

In general, our study provides an alternative perspective on whether medical innovations may play a role in increasing demand for healthcare services, especially during times of public health crises. This perspective is especially relevant in the wake of recent advancements in vaccine technology that significantly reduced the time required to develop a vaccine. In this regard, policies that would streamline the regulatory and licensing processes to facilitate the accelerated approval of these newly developed vaccines in a safe manner may yield significant economic and health benefits.

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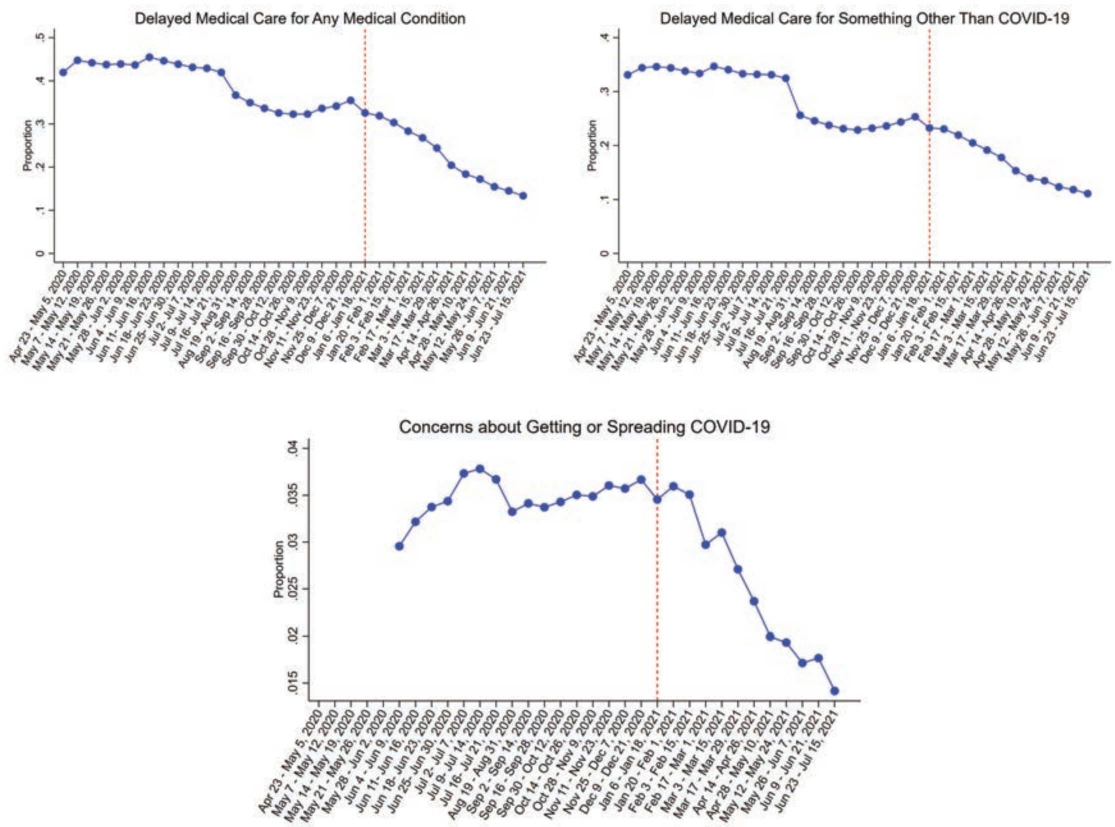
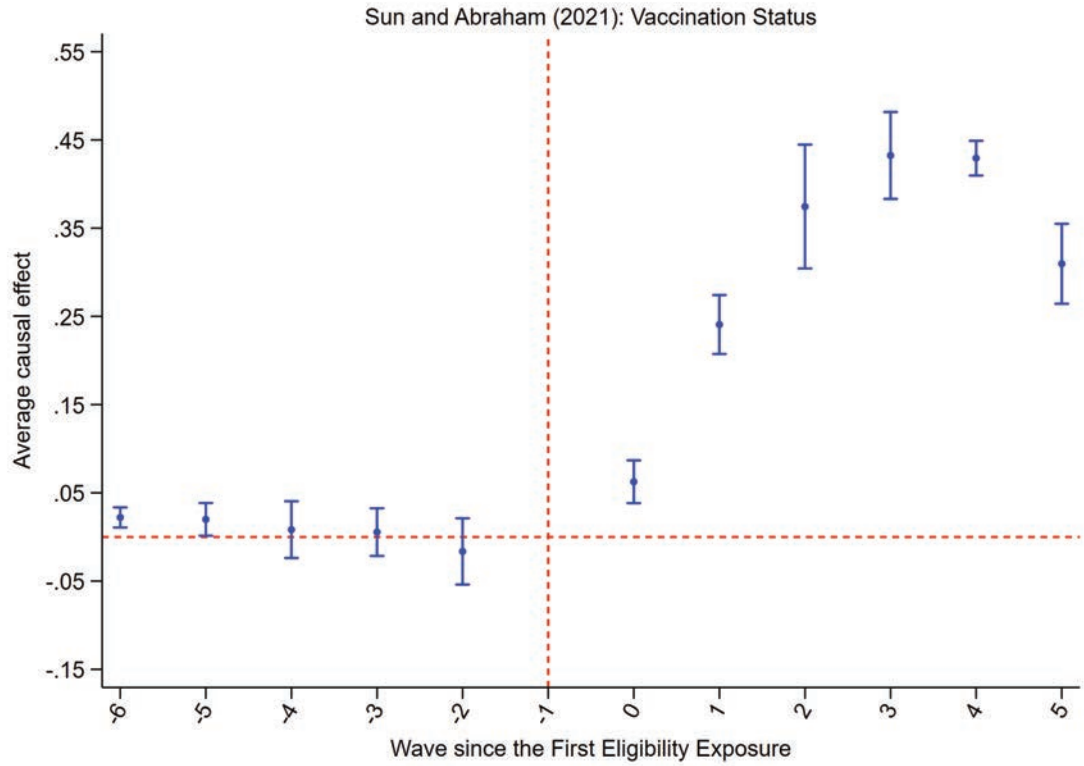
Figure 1. Descriptive Evidence on Delayed Care and Concerns about Getting or Spreading COVID-19



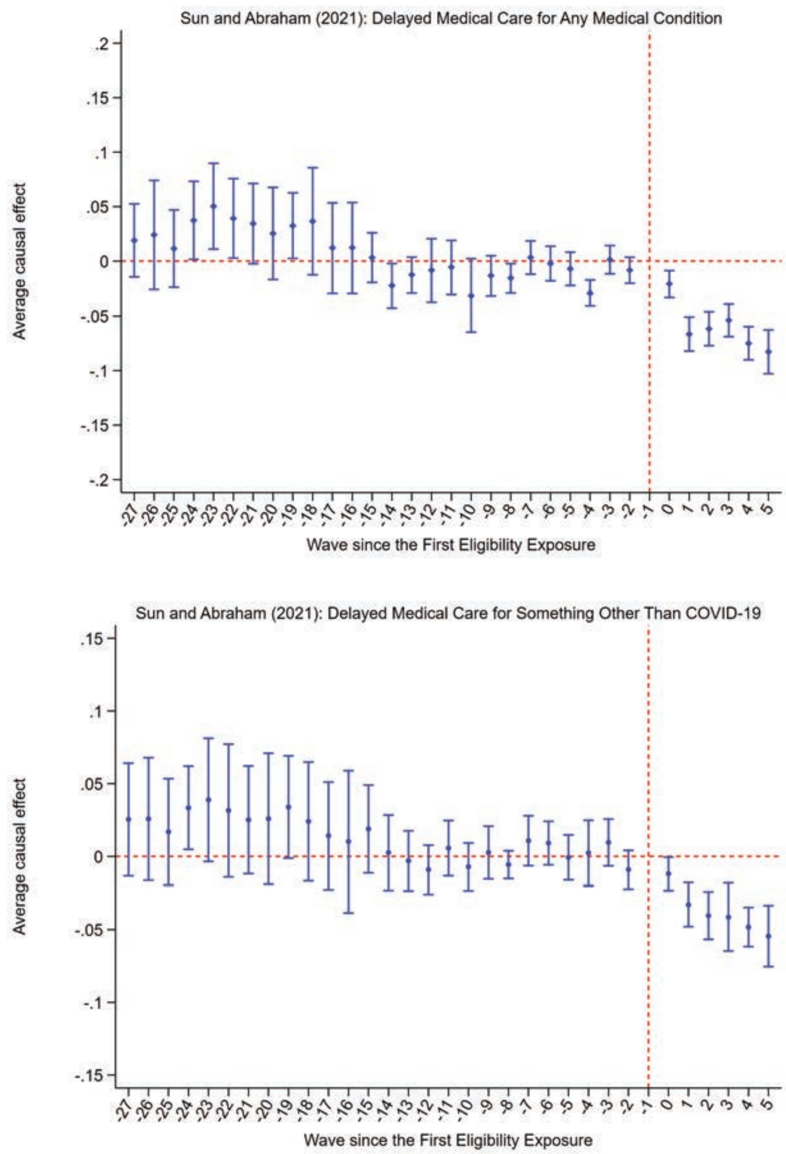
Notes: The top panel displays a time series plot of delayed care for any medical condition as well as conditions other than COVID-19 in the U.S. The bottom panel shows a time series plot of concerns about getting or spreading COVID-19. The sample period for delayed care is from April 23, 2020 through July 5, 2021, which covers survey waves 1 through 33. We have a shorter sample period for concerns over coronavirus exposure since the question was added to the survey in wave 6 (or June 4, 2020). The dashed vertical line indicates the start of COVID-19 vaccination.
Data Source: Household Pulse Survey, U.S. Census Bureau.

Figure 2. First Stage - Dynamic Difference-in-Differences



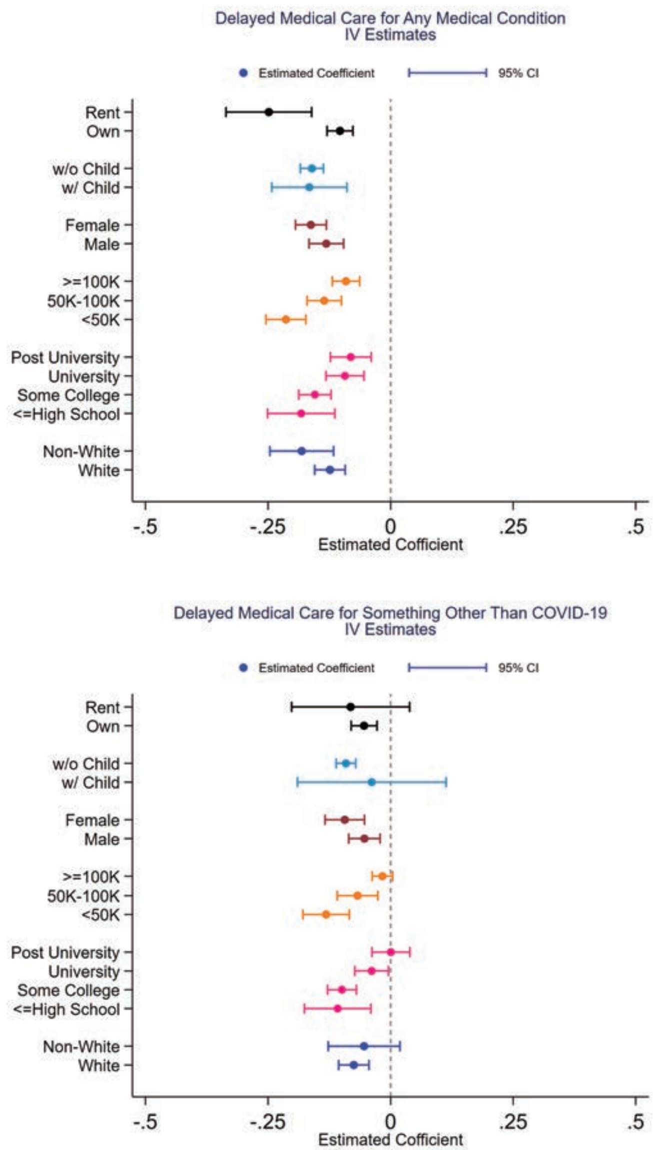
Notes: This figure presents dynamic difference-in-differences estimates showing the effects of vaccine eligibility on vaccination status. The estimates are based on the Sun and Abraham estimator (Sun and Abraham, 2021). Because individuals did not receive any vaccination prior to wave 22 (or January 6, 2021), our pre-period event window goes back to -6, as indicated in the figure. The specification includes state and survey wave FE, and is weighted using the survey weights. We do not condition on any individual attributes or state-level time-varying covariates. Standard errors are clustered at the state level. We also report 95% confidence intervals.

Figure 3. Reduced Form - Dynamic Difference-in-Differences



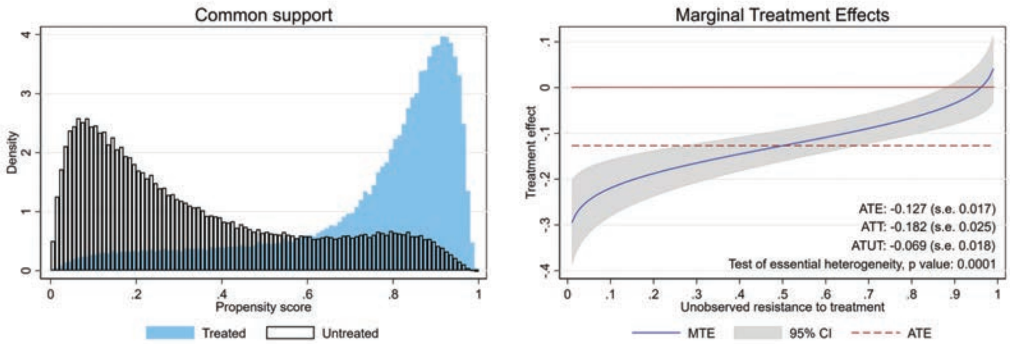
Notes: This figure presents dynamic difference-in-differences estimates showing the reduced form effects of vaccine eligibility on delaying care for any medical condition (top panel) and delaying care for conditions other than COVID-19 (bottom panel). The estimates are based on the Sun and Abraham estimator (Sun and Abraham, 2021). The specification includes state and survey wave FE, and is weighted using the survey weights. We do not condition on any individual attributes or state-level time-varying covariates. Standard errors are clustered at the state level. We also report 95% confidence intervals.

Figure 4. Heterogeneous Effects - IV Estimates

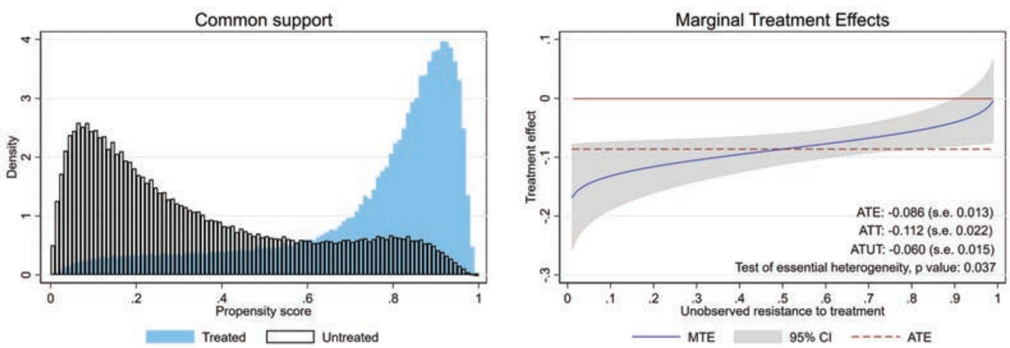


Notes: This figure presents the effects of vaccination status on delaying care for any medical condition (top panel) and delaying care for conditions other than COVID-19 (bottom panel) across subsamples based on different characteristics. Each point indicates the estimated coefficient from a TSLS regression, and each regression includes state FE, survey wave FE, region-by-wave FE (four U.S. Census regions), and covariates in the most inclusive specification in column (5) of Table 1. All regressions are weighted using the survey weights. Standard errors are clustered at the state level. We also report 95% confidence intervals.

Figure 5. Marginal Treatment Effects



(a) Delaying Medical Care for Any Medical Condition



(b) Delaying Medical Care for Something Other Than COVID-19

Notes: This figure presents the marginal treatment effects of vaccination status along different margins. The figures on the left display the common support of the propensity score for marginal treatment effect estimates. Propensity scores are predicted via a probit regression using our most inclusive specification, with individual-level covariates, state-level covariates, state and wave fixed effects, as well as region-by-wave fixed effects. The figures on the right show the MTE curves at different margins of the unobserved resistance to treatment along with 95% confidence intervals. The MTE estimation adopts the separate approach focusing on the sample with common support. The HPS survey weights are applied to calculate the MTEs. The standard errors are clustered at the state level. The lower right corner of MTE curves reports the estimates for the average treatment effect (ATE), average treatment on the treated (ATT), and average treatment on the untreated (ATUT). The *p*-values for the test of essential heterogeneity (i.e., testing the joint significance of the coefficients in $k(u)$) are also reported. The MTE analysis is conducted using `mtefe` in Stata (Andresen, 2018). The details on the derivation of the MTE curve are in Appendix Section A2.

Table 1. First Stage - Vaccine Eligibility and Vaccination Status

	(1)	(2)	(3)	(4)	(5)
Vaccine Eligibility	0.2696***	0.2786***	0.2794***	0.2794***	0.2792***
	(0.012)	(0.014)	(0.014)	(0.014)	(0.014)
N	2,298,654	2,298,654	2,298,654	2,298,654	2,298,654
State FE	√	√	√	√	√
Survey Wave FE	√	√	√	√	√
Region × Wave FE		√	√	√	√
Health measures and policy responses			√	√	√
Economic conditions				√	√
Social networks					√
Kleibergen-Paap rk LM statistic	19.23	18.86	18.91	18.91	18.90
Kleibergen-Paap rk Wald F statistic	495.2	408.4	425.8	425.6	423.5

Notes: This table presents the effects of vaccine eligibility on vaccination status. Each column is a separate regression where we add successively more covariates. Column (1) includes state and survey wave FE. Column (2) adds region-by-wave FE where we use four U.S. Census regions. Column (3) adds two-week lagged COVID-19 death rate and stringency index. Column (4) adds one-month lagged unemployment rate. Column (5) adds two-week lagged friend-exposure to vaccination information. All specifications control for individual attributes: age, gender, race, ethnicity, marital status, educational attainment, household income, health insurance coverage, and the number of children. All regressions are weighted using the survey weights. Standard errors reported in parentheses are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 2. Vaccine Eligibility and Delayed Care

	(1)	(2)	(3)	(4)	(5)
Panel A: Delayed Medical Care for Any Medical Condition					
Vaccine Eligibility	-0.0403***	-0.0405***	-0.0406***	-0.0406***	-0.0406***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Panel B: Delayed Medical Care for Something Other Than COVID-19					
Vaccine Eligibility	-0.0203***	-0.0201***	-0.0199***	-0.0199***	-0.0200***
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
N	2,298,654	2,298,654	2,298,654	2,298,654	2,298,654
State FE	√	√	√	√	√
Survey Wave FE	√	√	√	√	√
Region × Wave FE		√	√	√	√
Health measures and policy responses			√	√	√
Economic conditions				√	√
Social networks					√

Notes: This table presents the reduced form estimates showing the effects of vaccine eligibility on delayed care. Panel A reports the results for delayed care for any medical condition. Panel B reports the results for delayed care for conditions other than COVID-19. Each column is a separate regression where we add successively more covariates. Column (1) includes state and survey wave FE. Column (2) adds region-by-wave FE where we use four U.S. Census regions. Column (3) adds two-week lagged COVID-19 death rate and stringency index. Column (4) adds one-month lagged unemployment rate. Column (5) adds two-week lagged friend-exposure to vaccination information. All specifications control for individual attributes: age, gender, race, ethnicity, marital status, educational attainment, household income, health insurance coverage, and the number of children. All regressions are weighted using the survey weights. Standard errors reported in parentheses are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 3. Vaccination Status and Delayed Care

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
			OLS					IV		
Panel A: Delayed Medical Care for Any Medical Condition										
Received Vaccine	-0.0348***	-0.0350***	-0.0349***	-0.0349***	-0.0350***	-0.1495***	-0.1456***	-0.1452***	-0.1452***	-0.1455***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.011)	(0.014)	(0.013)	(0.013)	(0.014)
Panel B: Delayed Medical Care for Something Other Than COVID-19										
Received Vaccine	-0.0304***	-0.0305***	-0.0306***	-0.0306***	-0.0306***	-0.0752***	-0.0723***	-0.0713***	-0.0713***	-0.0715***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.014)	(0.015)	(0.015)	(0.015)	(0.015)
N	2,298,654	2,298,654	2,298,654	2,298,654	2,298,654	2,298,654	2,298,654	2,298,654	2,298,654	2,298,654
State FE	√	√	√	√	√	√	√	√	√	√
Survey Wave FE	√	√	√	√	√	√	√	√	√	√
Region × Wave FE		√	√	√	√		√	√	√	√
Health measures and policy responses			√	√	√			√	√	√
Economic conditions				√	√				√	√
Social networks					√					√

Notes: This table presents the OLS and TSLS estimates showing the effects of vaccination status on delayed care. Panel A reports the results for delayed care for any medical condition. Panel B reports the results for delayed care for conditions other than COVID-19. Each column is a separate regression where we add successively more covariates. Columns (1) and (6) include state and survey wave FE. Columns (2) and (7) add region-by-wave FE where we use four U.S. Census regions. Columns (3) and (8) add two-week lagged COVID-19 death rate and stringency index. Columns (4) and (9) add one-month lagged unemployment rate. Columns (5) and (10) add two-week lagged friend-exposure to vaccination information. All specifications control for individual attributes: age, gender, race, ethnicity, marital status, educational attainment, household income, health insurance coverage, and the number of children. All regressions are weighted using the survey weights. Standard errors reported in parentheses are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 4. Exploring Mechanisms - Fear of Getting or Spreading COVID-19

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
			OLS					IV		
Panel A: Concerned about Getting or Spreading COVID-19										
Received Vaccine	-0.007950**	-0.008003**	-0.008000**	-0.008007**	-0.008020**	-0.0851***	-0.0846***	-0.0845***	-0.0845***	-0.0845***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
N	770,284	770,284	770,284	770,284	770,284	770,284	770,284	770,284	770,284	770,284
State FE	√	√	√	√	√	√	√	√	√	√
Survey Wave FE	√	√	√	√	√	√	√	√	√	√
Region × Wave FE		√	√	√	√		√	√	√	√
Health measures and policy responses			√	√	√			√	√	√
Economic conditions				√	√				√	√
Social networks					√					√

Notes: This table presents the OLS and TSLS estimates showing the effects of vaccination status on concerns about getting or spreading COVID-19. Each column is a separate regression where we add successively more covariates. Columns (1) and (6) include state and survey wave FE. Columns (2) and (7) add region-by-wave FE where we use four U.S. Census regions. Columns (3) and (8) add two-week lagged COVID-19 death rate and stringency index. Columns (4) and (9) add one-month lagged unemployment rate. Columns (5) and (10) add two-week lagged friend-exposure to vaccination information. All specifications control for individual attributes: age, gender, race, ethnicity, marital status, educational attainment, household income, health insurance coverage, and the number of children. All regressions are weighted using the survey weights. Standard errors reported in parentheses are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 5. Testing for Spillovers to Children in the Household

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
			OLS					IV		
Panel A: Children Delayed Preventive Healthcare										
Received Vaccine	0.0127	0.0129	0.0129	0.0129	0.0129	-0.5020	-0.4212	-0.4189	-0.4363*	-0.4791**
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.365)	(0.252)	(0.252)	(0.236)	(0.232)
N	104,697	104,697	104,697	104,697	104,697	104,697	104,697	104,697	104,697	104,697
State FE	√	√	√	√	√	√	√	√	√	√
Survey Wave FE	√	√	√	√	√	√	√	√	√	√
Region × Wave FE		√	√	√	√		√	√	√	√
Health measures and policy responses			√	√	√			√	√	√
Economic conditions				√	√				√	√
Social networks					√					√

Notes: This table presents the OLS and TSLS estimates showing the effects of vaccination status on delaying preventive care for children in the household. Each column is a separate regression where we add successively more covariates. Columns (1) and (6) include state and survey wave FE. Columns (2) and (7) add region-by-wave FE, where we use four U.S. Census regions. Columns (3) and (8) add two-week lagged COVID-19 death rate and stringency index. Columns (4) and (9) add one-month lagged unemployment rate. Columns (5) and (10) add two-week lagged friend-exposure to vaccination information. All specifications control for individual attributes: age, gender, race, ethnicity, marital status, educational attainment, household income, health insurance coverage, and the number of children. All regressions are weighted using the survey weights. Standard errors reported in parentheses are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 6. Vaccination Rate and Health Expenditures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
			OLS					IV		
Panel A: Consumer Healthcare Spending										
Vaccination Rate per 100	0.0002	0.0007	0.0009	0.0006	0.0007	0.0014	0.0036	0.0031	0.0028	0.0044
	(0.0012)	(0.0018)	(0.0017)	(0.0013)	(0.0013)	(0.0034)	(0.0042)	(0.0042)	(0.0043)	(0.0043)
						First Stage Estimates (*Vaccination Rate per 100*)				
Fraction Eligible						0.0849***	0.0566***	0.0564***	0.0539***	0.0553***
						(0.0192)	(0.0132)	(0.0130)	(0.0122)	(0.0121)
N	8,750	8,750	8,750	8,750	8,050	8,750	8,750	8,750	8,750	8,050
State FE	√	√	√	√	√	√	√	√	√	√
Date FE	√	√	√	√	√	√	√	√	√	√
Region × Week FE		√	√	√	√		√	√	√	√
Health measures and policy responses			√	√	√			√	√	√
Economic conditions				√	√				√	√
Social networks					√					√
Kleibergen-Paap rk LM statistic						11.13	9.89	10.09	10.54	10.46
Kleibergen-Paap rk Wald F statistic						19.55	18.40	18.88	19.41	20.90

Notes: This table presents the OLS and TSLS estimates showing the effects of vaccination rate on consumer healthcare spending. Since the outcome is defined at the state level, we create a state-level instrument capturing the proportion of individuals eligible for vaccination based on the age cutoff. We report the first stage estimates under the IV specifications in columns (6)-(10). Each column is a separate regression where we add successively more covariates. Columns (1) and (6) include state and calendar date FE. Columns (2) and (7) add region-by-week FE where we use four U.S. Census regions. Columns (3) and (8) add two-week lagged COVID-19 death rate and stringency index. Columns (4) and (9) add one-month lagged unemployment rate. Columns (5) and (10) add two-week lagged friend-exposure to vaccination information. Standard errors reported in parentheses are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Appendix: For Online Publication

A1. Construction of Social Networks

We account for information spillovers within social networks by constructing a measure of friend exposure to vaccination information, which might affect the behavior of individuals who are eligible for vaccination. We combine data obtained from Bailey et al. (2018) on Social Connectedness Index (measured using Facebook connections) and data on vaccination rates to create a state-level measure of friend exposure to vaccination. Specifically, for each state s in time t, we calculate the friend exposure to vaccination as follows:

$$FriendExpVax_{st} = \sum_{\substack{k \in K \\ k \neq s}} FracConnect_{sk} \times VaccinationRate_{kt}, \tag{3}$$

where k denotes a friend state and K is the collection of all 50 states. We first follow the strategy in Bailey et al. (2020) to construct $FracConnect_{sk}$, which is a fraction of per-user Facebook connections from state k relative to all friend states. This fraction can be considered as an intensity of social connectedness between a state s and its friend state k. We then multiply this fraction with vaccination rate per 100 in state k and sum over all states to create a weighted exposure to vaccination. In our analysis, we include this friend-exposure (lagged by two weeks)[27] as an additional covariate to account for potential changes in healthcare seeking behavior due to spillovers within social networks.

A2. Marginal Treatment Effects

A2.1. The Generalized Roy Model

To understand the logic behind marginal treatment effects (MTEs), we first introduce a generalized Roy model. We closely follow the notation in Andresen (2018) to formalize the model. $Y_{1,i}$ and $Y_{0,i}$ are potential outcomes for individual i receiving ($k = 1$) and not receiving ($k = 0$) vaccination, respectively. Treatment, D_i, in our setting determines vaccination status. X_i includes individual observable characteristics (age, gender, race, ethnicity, marital status, educational attainment, household income, health insurance coverage, and the number of children), state-level covariates (health and policy measures, economic conditions, and social networks), state and survey wave fixed effects, as well as region-by-wave fixed effects.

$$Y_{k,i} = \mu_k(X_i) + U_{k,i}, \qquad k = 0, 1 \tag{4}$$

[27]We include the lagged version of this measure to avoid the interaction between the vaccination rate and the policy measure. Importantly, our results are not sensitive to the inclusion of this measure.

The following latent selection equation shows how patients choose to receive vaccination based on factors observable and unobservable to the econometrician:

$$D_i = \mu_D(\tilde{Z}_i) - v_i, \tag{5}$$
$$D_i = 1\{\mu_D(\tilde{Z}_i) \geq v_i\}, \tag{6}$$

where $\tilde{Z}_i = (X_i, Z_i)$ is a function of all the controls in Equation (4) and the age-specific vaccine eligibility instrument, Z_i. In the selection equation, we interpret v_i as the unobserved resistance to receiving vaccination. Put differently, unobserved v_i is a negative shock to the latent index determining whether individuals receive vaccination. We rewrite Equation (6) as the quantiles of the distribution of v_i:

$$\Phi(\mu_D(\tilde{Z}_i)) \geq \Phi(v_i) \Rightarrow P(\tilde{Z}_i) \geq U_{D,i}, \tag{7}$$

where Φ is the unknown cumulative distribution function of v_i. Therefore, $P(\tilde{Z}_i)$ represents the propensity score: the probability of receiving vaccination conditional on observables $\tilde{Z}_i$. On the other hand, $U_{D,i}$ represents the quantiles of unobserved resistance to receiving vaccination.

To estimate MTEs, we make two assumptions that are not direct testable: conditional independence and separability. The first assumption requires that $(U_{0,i}, U_{1,i}, v_i) \perp Z_i | X_i$. This assumption does not introduce additional restrictions since we impose conditional independence to recover the LATE. The second assumption on separability relates to the functional form. Following the existing literature (see, e.g., Brinch, Mogstad, and Wiswall, 2017), we assume that the MTEs are additively separable into observable (X_i) and unobservable components ($U_{D,i}$). This assumption also allows the MTE to be identified over the common support of $P(\tilde{Z}_i)$ across all values of X_i. Therefore, the MTE is defined as:

$$MTE(x,u) \equiv \mathbb{E}[Y_{1,i} - Y_{0,i} | X_i = x, U_{Di} = u] \tag{8}$$
$$= \underbrace{(\beta_1 - \beta_0)x}_{observables} + \underbrace{\mathbb{E}[U_{1,i} - U_{0,i} | U_{D,i} = u]}_{unobservables}. \tag{9}$$

As seen above, an important implication of separability is that the slope of the MTE is independent of X. We observe that heterogeneity due to observables affects the MTE curve through the intercept.

A2.2. Estimation using the local instrumental variables

The idea behind the local instrumental variables (LIV) estimation strategy is that the MTE can be identified by differentiating the conditional expectation of Y with respect to the propensity score, where there is common support over the unit interval (0, 1). It is common to identify the selection into treatment using a probability model (e.g., probit or logit); however, alternative approaches could also be used (e.g., linear probability model or semiparametric binary choice model).

To formalize this approach, we define the potential outcomes model as:

$$\mathbb{E}[Y|X_i = x, P(\tilde{Z}_i) = p] = x\beta_0 + x(\beta_1 - \beta_0)p + \underbrace{p\mathbb{E}[U_{1,i} - U_{0,i}|U_{D,i} \leq p]}_{K(p)}, \tag{10}$$

where $K(p)$ is a "nonlinear function of the propensity score, and captures heterogeneity along the unobservable resistance to treatment" (Andresen, 2018). An important choice is about the functional form of the unknown $K(p)$. In terms of parametric assumptions, one can assume that the unknown function follows a joint normal distribution.[28] Finally, the MTE curve can be estimated as the derivative of Equation (10) with respect to the propensity score evaluated at u (Heckman, Urzua, and Vytlacil, 2006).

A2.3. Estimation using the separate approach

The main difference in this approach is that the unobserved component is defined separately for the treated and the untreated, while "LIV works directly with the differences in these components across the two groups" (Brinch, Mogstad, and Wiswall, 2017). An important advantage of this approach over LIV is that, with a binary instrument, we only need two values of $P(\tilde{Z}_i)$ for the treated and the untreated, respectively, to estimate the MTE model. Moreover, invoking the second assumption above on additive separability, we can estimate a flexible MTE along the common support of the propensity score.

Taking these together, we identify the MTE by estimating the conditional expectation of the potential outcome using the following regression:

$$Y_j = \mu_j(x) + k_j(p, x) + \epsilon, \tag{11}$$

where $j = 0, 1$. To define MTE, we use the following notation for the conditional expectation:

$$MTE(x, u) \equiv \mathbb{E}[Y_{1,i}|X_i = x, U_{Di} = u] - \mathbb{E}[Y_{0,i}|X_i = x, U_{Di} = u] \tag{12}$$

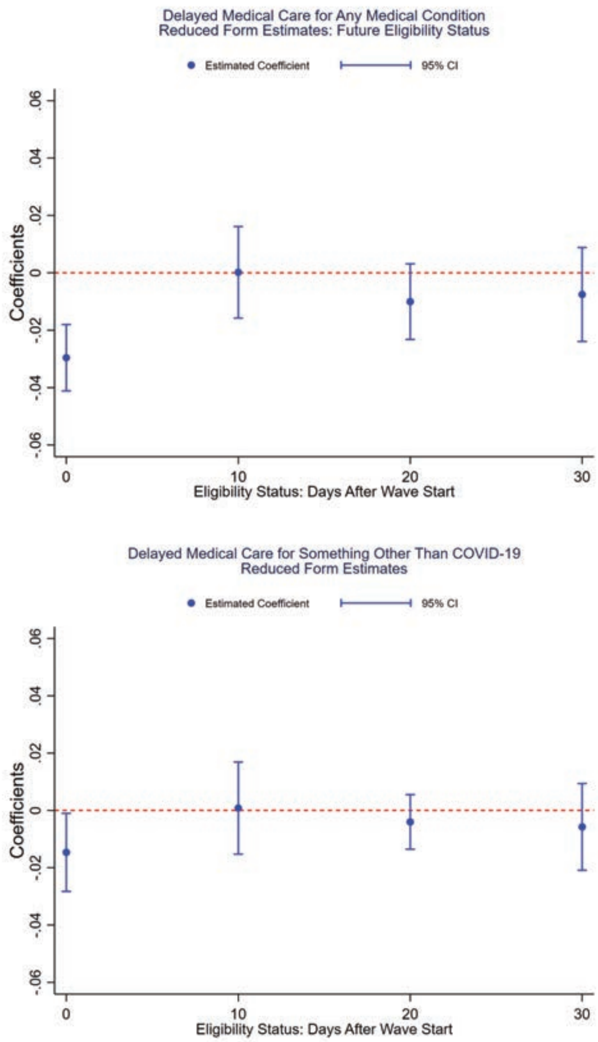
$$= (\beta_1 - \beta_0)x + k_1(u) - k_0(u). \tag{13}$$

Note that the functional form assumptions discussed above regarding the unknown function $k_j(u)$ also applies in this setting.

[28] Alternatively, it is possible to relax the joint normal assumption and estimate the MTE using a semiparametric method.

B. Additional Figures and Tables

Figure B.1. Testing for Reverse Feedback Effects



Notes: This figure displays results from reduced form regressions for delayed care on current vaccine eligibility and the leads of vaccine eligibility by ten-day increments up to 30 days. Each subfigure comes from a separate regression for a different measure of delayed care: delaying care for any medical condition (top panel) and delaying care for conditions other than COVID-19 (bottom panel). Each regression includes state FE, survey wave FE, region-by-wave FE (four U.S. Census regions), and covariates in the most inclusive specification in column (5) of Table 1. All regressions are weighted using the survey weights. Standard errors are clustered at the state level. We also report 95% confidence intervals.

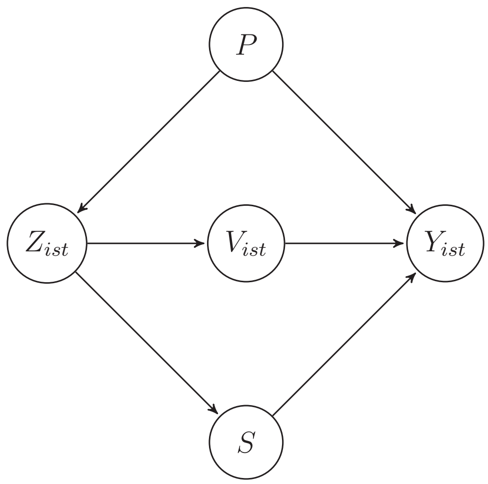
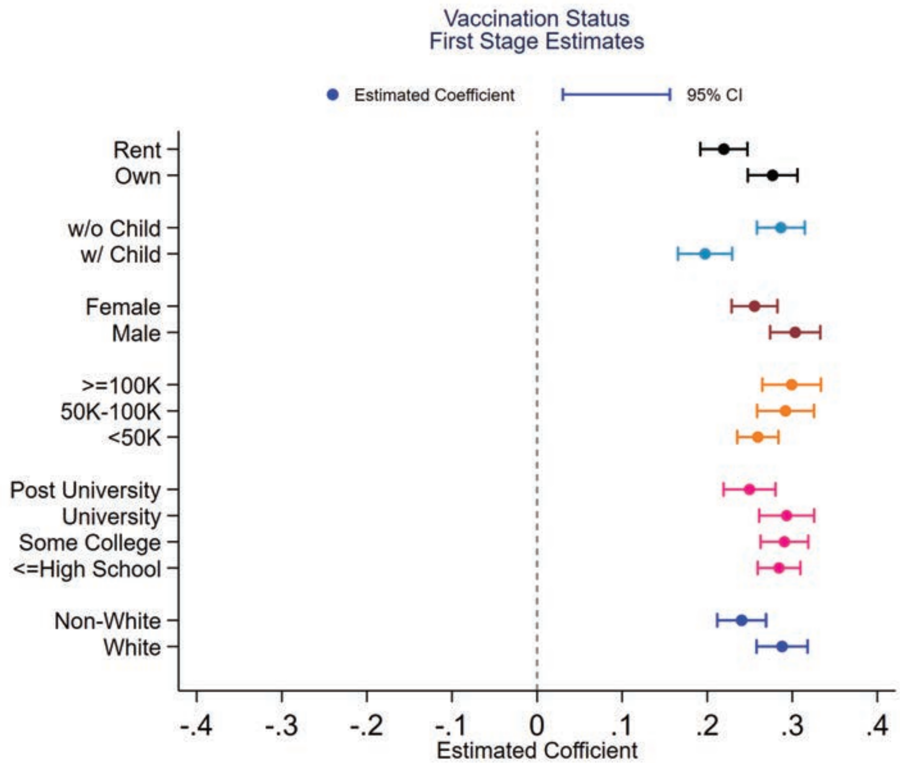


Figure B.2. A Simple Illustration of Backdoor Paths

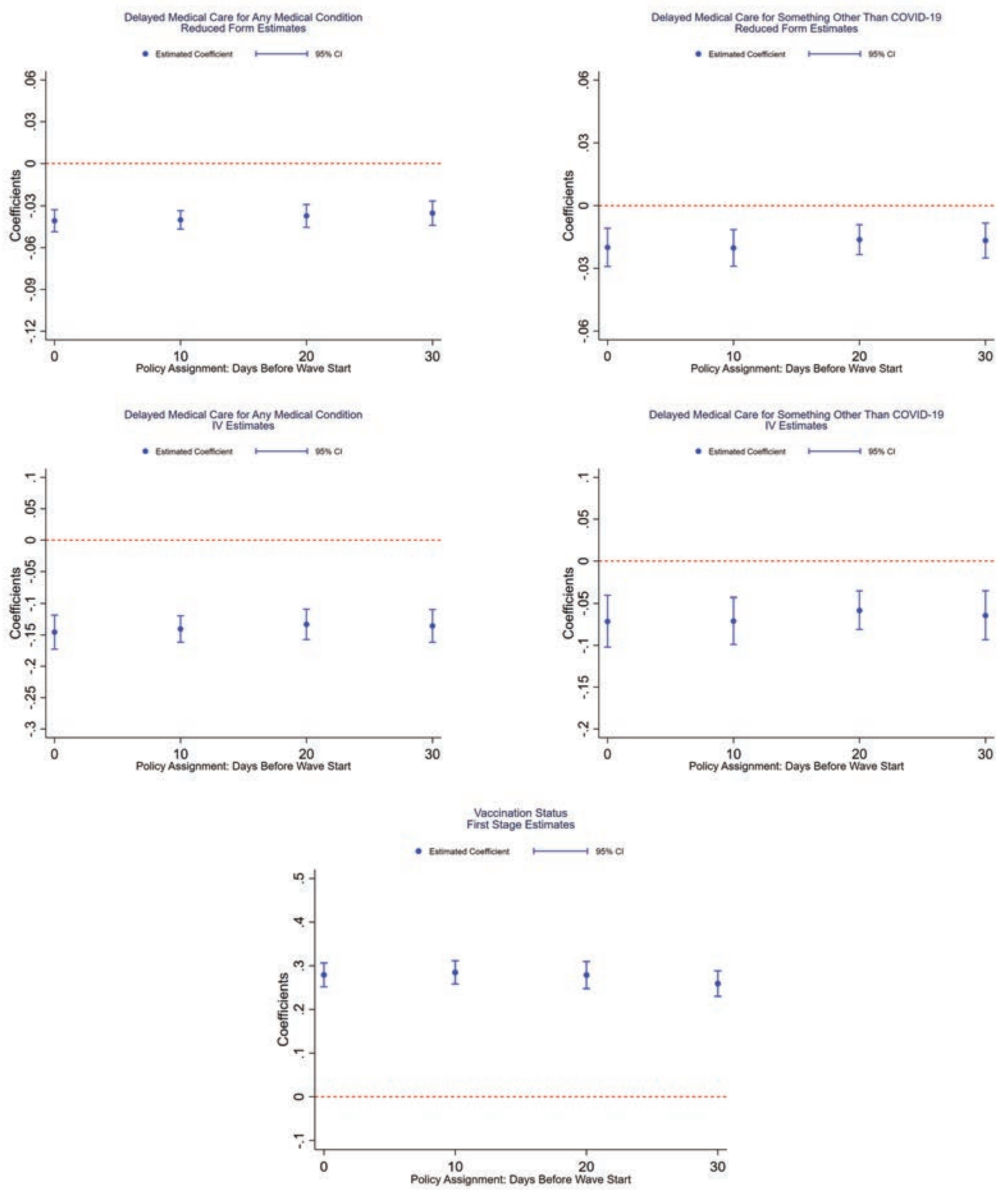
Notes: This figure provides a simple illustration of backdoor paths using a directed acyclic graph (DAG). The notation is defined as follows: Z_{ist} denotes the instrument (vaccine eligibility); V_{ist} is vaccination status; Y_{ist} is the outcome variable (delayed care or consumer healthcare spending); P is pre-existing conditions; and S is spillovers.

Figure B.3. Instrument Monotonicity



Notes: This figure presents the effects of vaccine eligibility on vaccination status across subsamples based on different characteristics. Each point indicates the estimated coefficient from a separate regression, and each regression includes state FE, survey wave FE, region-by-wave FE (four U.S. Census regions), and covariates in the most inclusive specification in column (5) of Table 1. All regressions are weighted using the survey weights. Standard errors are clustered at the state level. We also report 95% confidence intervals.

Figure B.4. Robustness Check: Adjusting Policy Assignment Date



Notes: This figure presents the effects of vaccine eligibility (or vaccination) with varying policy assignment date by ten-day increments before the start of a survey wave. Subfigures denote estimates, respectively, using: (i) the reduced form specification (top panel); (ii) the IV specification (middle panel); and (iii) the first stage (bottom panel). Each point indicates the estimated coefficient from a separate regression, and each regression includes state FE, survey wave FE, region-by-wave FE (four U.S. Census regions), and covariates in the most inclusive specification in column (5) of Table 1. All regressions are weighted using the survey weights. Standard errors are clustered at the state level. We also report 95% confidence intervals.

Table B.1. Household Pulse Survey Waves Used in the Analysis

Survey Wave	Start Date	End Date
1	April 23, 2020	May 5, 2020
2	May 7, 2020	May 12, 2020
3	May 14, 2020	May 19, 2020
4	May 21, 2020	May 26, 2020
5	May 28, 2020	June 2, 2020
6	June 4, 2020	June 9, 2020
7	June 11, 2020	June 16, 2020
8	June 18, 2020	June 23, 2020
9	June 25, 2020	June 30, 2020
10	July 2, 2020	July 7, 2020
11	July 9, 2020	July 14, 2020
12	July 16, 2020	July 21, 2020
13	August 19, 2020	August 31, 2020
14	September 2, 2020	September 14, 2020
15	September 16, 2020	September 28, 2020
16	September 30, 2020	October 12, 2020
17	October 14, 2020	October 26, 2020
18	October 28, 2020	November 9, 2020
19	November 11, 2020	November 23, 2020
20	November 25, 2020	December 7, 2020
21	December 9, 2020	December 21, 2020
22	January 6, 2021	January 18, 2021
23	January 20, 2021	February 1, 2021
24	February 3, 2021	February 15, 2021
25	February 17, 2021	March 1, 2021
26	March 3, 2021	March 15, 2021
27	March 17, 2021	March 29, 2021
28	April 14, 2021	April 26, 2021
29	April 28, 2021	May 10, 2021
30	May 12, 2021	May 24, 2021
31	May 26, 2021	June 7, 2021
32	June 9, 2021	June 21, 2021
33	June 23, 2021	July 5, 2021

Notes: This table reports the start and end date of survey waves used in the analysis. The data come from the Household Pulse Survey provided by the U.S. Census Bureau.

Table B.2. Age-Specific Vaccine Eligibility Rollout by States

States	Dates of COVID-19 vaccine eligibility for adults										
	80+	75+	70+	65+	60+	55+	50+	45+	40+	30+	All Adults
Alabama	1/18/2021	1/18/2021	2/8/2021	2/8/2021	3/22/2021	3/22/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021
Alaska	1/11/2021	1/11/2021	1/11/2021	1/11/2021	3/3/2021	3/3/2021	3/9/2021	3/9/2021	3/9/2021	3/9/2021	3/9/2021
Arizona	1/8/2021	1/8/2021	1/19/2021	1/19/2021	3/2/2021	3/2/2021	3/24/2021	3/24/2021	3/24/2021	3/24/2021	3/24/2021
Arkansas	1/18/2021	1/18/2021	1/18/2021	2/23/2021	3/30/2021	3/30/2021	3/30/2021	3/30/2021	3/30/2021	3/30/2021	3/30/2021
California	1/13/2021	1/13/2021	1/13/2021	1/13/2021	4/1/2021	4/1/2021	4/1/2021	4/15/2021	4/15/2021	4/15/2021	4/15/2021
Colorado	12/30/2020	12/30/2020	12/30/2020	3/5/2021	3/5/2021	3/19/2021	3/19/2021	4/2/2021	4/2/2021	4/2/2021	4/2/2021
Connecticut	1/18/2021	1/18/2021	2/11/2021	2/11/2021	3/1/2021	3/1/2021	3/19/2021	3/19/2021	4/1/2021	4/1/2021	4/1/2021
Delaware	1/19/2021	1/19/2021	1/19/2021	1/19/2021	3/17/2021	3/17/2021	3/17/2021	4/6/2021	4/6/2021	4/6/2021	4/6/2021
District of Columbia	1/11/2021	1/11/2021	1/11/2021	1/11/2021	4/19/2021	4/19/2021	4/19/2021	4/19/2021	4/19/2021	4/19/2021	4/19/2021
Florida	12/23/2020	12/23/2020	12/23/2020	12/23/2020	3/22/2021	3/22/2021	3/22/2021	3/29/2021	3/29/2021	4/5/2021	4/5/2021
Georgia	1/4/2021	1/4/2021	1/4/2021	1/4/2021	3/15/2021	3/15/2021	3/25/2021	3/25/2021	3/25/2021	3/25/2021	3/25/2021
Hawaii	1/5/2021	1/5/2021	3/8/2021	3/15/2021	3/26/2021	4/19/2021	4/19/2021	4/19/2021	4/19/2021	4/19/2021	4/19/2021
Idaho	2/1/2021	2/1/2021	2/1/2021	2/1/2021	3/22/2021	3/22/2021	3/24/2021	3/24/2021	4/5/2021	4/5/2021	4/5/2021
Illinois	1/25/2021	1/25/2021	1/25/2021	1/25/2021	4/12/2021	4/12/2021	4/12/2021	4/12/2021	4/12/2021	4/12/2021	4/12/2021
Indiana	1/8/2021	1/13/2021	1/13/2021	2/23/2021	2/23/2021	3/9/2021	3/10/2021	3/22/2021	3/29/2021	3/31/2021	3/31/2021
Iowa	2/1/2021	2/1/2021	2/1/2021	2/1/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021
Kansas	1/21/2021	1/21/2021	1/21/2021	1/21/2021	3/29/2021	3/29/2021	3/29/2021	3/29/2021	3/29/2021	3/29/2021	3/29/2021
Kentucky	2/1/2021	2/1/2021	2/1/2021	3/1/2021	3/1/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021
Louisiana	1/4/2021	1/4/2021	1/4/2021	2/8/2021	3/29/2021	3/29/2021	3/29/2021	3/29/2021	3/29/2021	3/29/2021	3/29/2021
Maine	1/18/2021	1/18/2021	1/18/2021	3/3/2021	3/3/2021	4/1/2021	4/1/2021	4/7/2021	4/7/2021	4/7/2021	4/7/2021
Maryland	1/18/2021	1/18/2021	1/25/2021	1/25/2021	3/23/2021	4/6/2021	4/6/2021	4/6/2021	4/6/2021	4/6/2021	4/6/2021
Massachusetts	2/1/2021	2/1/2021	3/22/2021	3/22/2021	3/22/2021	4/5/2021	4/19/2021	4/19/2021	4/19/2021	4/19/2021	4/19/2021
Michigan	1/11/2021	1/11/2021	1/11/2021	1/11/2021	3/22/2021	3/22/2021	3/22/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021
Minnesota	1/19/2021	1/19/2021	1/19/2021	1/19/2021	3/30/2021	3/30/2021	3/30/2021	3/30/2021	3/30/2021	3/30/2021	3/30/2021
Mississippi	1/6/2021	1/6/2021	1/6/2021	1/6/2021	3/4/2021	3/4/2021	3/4/2021	3/16/2021	3/16/2021	3/16/2021	3/16/2021
Missouri	1/18/2021	1/18/2021	1/18/2021	1/18/2021	4/9/2021	4/9/2021	4/9/2021	4/9/2021	4/9/2021	4/9/2021	4/9/2021
Montana	1/19/2021	1/19/2021	1/19/2021	3/8/2021	3/8/2021	4/1/2021	4/1/2021	4/1/2021	4/1/2021	4/1/2021	4/1/2021
Nebraska	1/20/2021	1/20/2021	1/20/2021	1/20/2021	3/22/2021	3/22/2021	3/22/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021
Nevada	1/14/2021	1/14/2021	1/14/2021	2/22/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021
New Hampshire	1/26/2021	1/26/2021	1/26/2021	1/26/2021	3/22/2021	3/22/2021	3/22/2021	3/29/2021	3/29/2021	3/31/2021	4/2/2021
New Jersey	1/14/2021	1/14/2021	1/14/2021	1/14/2021	4/5/2021	4/5/2021	4/19/2021	4/19/2021	4/19/2021	4/19/2021	4/19/2021
New Mexico	1/8/2021	1/8/2021	3/19/2021	3/19/2021	3/19/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021
New York	1/11/2021	1/11/2021	1/11/2021	1/11/2021	3/10/2021	3/23/2021	3/23/2021	3/30/2021	3/30/2021	3/30/2021	4/6/2021
North Carolina	1/11/2021	1/11/2021	1/11/2021	1/11/2021	4/7/2021	4/7/2021	4/7/2021	4/7/2021	4/7/2021	4/7/2021	4/7/2021
North Dakota	1/12/2021	1/12/2021	1/12/2021	1/12/2021	3/29/2021	3/29/2021	3/29/2021	3/29/2021	3/29/2021	3/29/2021	3/29/2021
Ohio	1/19/2021	1/19/2021	1/19/2021	1/19/2021	3/4/2021	3/11/2021	3/11/2021	3/19/2021	3/19/2021	3/29/2021	3/29/2021
Oklahoma	1/4/2021	1/4/2021	1/4/2021	1/4/2021	3/29/2021	3/29/2021	3/29/2021	3/29/2021	3/29/2021	3/29/2021	3/29/2021
Oregon	2/7/2021	2/15/2021	2/22/2021	3/1/2021	4/19/2021	4/19/2021	4/19/2021	4/19/2021	4/19/2021	4/19/2021	4/19/2021
Pennsylvania	1/19/2021	1/19/2021	1/19/2021	1/19/2021	4/5/2021	4/5/2021	4/13/2021	4/13/2021	4/13/2021	4/13/2021	4/13/2021
Rhode Island	1/28/2021	1/28/2021	2/22/2021	2/22/2021	4/5/2021	4/5/2021	4/5/2021	4/12/2021	4/12/2021	4/19/2021	4/19/2021
South Carolina	1/13/2021	1/13/2021	2/8/2021	2/8/2021	3/8/2021	3/8/2021	3/31/2021	3/31/2021	3/31/2021	3/31/2021	3/31/2021
South Dakota	1/18/2021	1/18/2021	2/22/2021	2/22/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021
Tennessee	1/1/2021	1/1/2021	2/2/2021	2/22/2021	3/18/2021	3/18/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021
Texas	12/28/2020	12/28/2020	12/28/2020	12/28/2020	3/15/2021	3/15/2021	3/15/2021	3/29/2021	3/29/2021	3/29/2021	3/29/2021
Utah	1/18/2021	1/18/2021	2/18/2021	2/18/2021	3/8/2021	3/8/2021	3/8/2021	3/24/2021	3/24/2021	3/24/2021	3/24/2021
Vermont	1/25/2021	1/25/2021	2/16/2021	3/1/2021	3/25/2021	3/29/2021	3/29/2021	4/5/2021	4/5/2021	4/12/2021	4/19/2021
Virginia	1/11/2021	1/11/2021	1/11/2021	1/11/2021	4/18/2021	4/18/2021	4/18/2021	4/18/2021	4/18/2021	4/18/2021	4/18/2021
Washington	1/18/2021	1/18/2021	1/18/2021	1/18/2021	4/15/2021	4/15/2021	4/15/2021	4/15/2021	4/15/2021	4/15/2021	4/15/2021
West Virginia	12/30/2020	12/30/2020	12/30/2020	12/30/2020	3/3/2021	3/3/2021	3/3/2021	3/22/2021	3/22/2021	3/22/2021	3/22/2021
Wisconsin	1/25/2021	1/25/2021	1/25/2021	1/25/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021	4/5/2021
Wyoming	1/25/2021	1/25/2021	1/25/2021	2/1/2021	3/31/2021	3/31/2021	3/31/2021	3/31/2021	3/31/2021	3/31/2021	3/31/2021

Notes: This table summarizes the age-specific vaccine eligibility rollout across states and over time. The policy implementation dates and age cutoffs are obtained from the U.S. State Policy (CUSP) database (Raifman et al., 2020). In the last column, "all adults" refer to the expansion of vaccine eligibility to individuals aged 18 and older.

Table B.3. Descriptive Statistics

	Pre-Vaccination Waves (Waves 1-21)			Post-Vaccination Waves (Waves 22-33)		
	Mean	SD	Obs.	Mean	SD	Obs.
Panel A: Outcome Variables						
Delayed Medical Care						
Any Medical Condition	0.399	0.490	1,598,584	0.231	0.422	700,070
Something Other Than COVID-19	0.299	0.458	1,598,584	0.172	0.377	700,070
Pandemic-Related Concerns						
Getting or Spreading COVID-19	0.0347	0.183	481,855	0.0260	0.159	288,429
Vaccination Status						
Received COVID-19 Vaccine	–	–	–	0.578	0.494	700,070
Panel B: Sociodemographic Attributes						
Age	52.769	15.593	1,598,584	54.504	15.553	700,070
Male	0.407	0.491	1,598,584	0.404	0.491	700,070
Married	0.588	0.492	1,598,584	0.597	0.491	700,070
Race/Ethnicity						
White	0.770	0.421	1,598,584	0.767	0.423	700,070
Black	0.067	0.25	1,598,584	0.064	0.245	700,070
Asian	0.043	0.204	1,598,584	0.047	0.212	700,070
Hispanic	0.084	0.277	1,598,584	0.087	0.283	700,070
Other Races	0.036	0.187	1,598,584	0.034	0.182	700,070
Educational Attainment						
≤High School	0.130	0.336	1,598,584	0.122	0.328	700,070
Some College	0.318	0.466	1,598,584	0.314	0.464	700,070
University	0.296	0.456	1,598,584	0.297	0.457	700,070
Post-University	0.257	0.437	1,598,584	0.267	0.442	700,070
Household Income						
<50K	0.289	0.453	1,598,584	0.271	0.445	700,070
50K-100K	0.309	0.462	1,598,584	0.299	0.458	700,070
≥100K	0.352	0.478	1,598,584	0.356	0.479	700,070
Missing Income	0.049	0.216	1,598,584	0.074	0.262	700,070
Health Insurance Coverage	0.938	0.24	1,598,584	0.951	0.216	700,070
Number of Children (Age < 18)						
0	0.647	0.478	1,598,584	0.680	0.467	700,070
1	0.152	0.359	1,598,584	0.146	0.353	700,070
2	0.128	0.334	1,598,584	0.114	0.318	700,070
3	0.049	0.215	1,598,584	0.041	0.198	700,070
4	0.016	0.127	1,598,584	0.013	0.114	700,070
5+	0.008	0.09	1,598,584	0.006	0.080	700,070

Notes: This table presents descriptive statistics for outcome variables (Panel A) and individual attributes (Panel B) used in the analysis. Columns (2) and (5) report means for pre- and post-vaccination waves, respectively. The sample includes adults aged 18 and older. The total number of observations for delayed care and individual attributes is 2,298,654. Sample size differs for concerns about getting or spreading COVID-19 and vaccination status. The question on pandemic-related concerns was added to the survey in wave 6 (starting from June 4, 2020), whereas the vaccination question was added in wave 22 (starting from January 6, 2021). Because the question on children delaying preventive care was available after wave 28, we do not report it in the table. However, the mean delayed care for children is 0.282, with a standard deviation of 0.450. Moreover, the number of observations is 104,697.

Table B.4. Descriptive Statistics by Subgroups - Delayed Medical Care & Vaccination Status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9	(10)	(11)
		Race/Ethnicity		Educational Attainment				Household Income			
	Total	White	Non-White	≤High School	Some College	University	Post-University	<50K	50K-100K	>100K	Missing Income
Waves 1-21 (Pre-Vaccination)											
Panel A: Delayed Medical Care for Any Medical Condition											
Mean	0.399	0.397	0.402	0.339	0.408	0.396	0.420	0.420	0.401	0.384	0.353
SD	0.490	0.489	0.490	0.473	0.491	0.489	0.493	0.494	0.490	0.486	0.478
Panel B: Delayed Medical Care for Something Other Than COVID-19											
Mean	0.299	0.292	0.322	0.284	0.330	0.280	0.290	0.347	0.301	0.263	0.260
SD	0.458	0.455	0.467	0.451	0.470	0.449	0.454	0.476	0.459	0.440	0.439
N	1,598,584	1,230,151	368,433	207,256	508,622	472,645	410,061	462,183	494,562	563,069	78,770
Waves 22-33 (Post-Vaccination)											
Panel C: Delayed Medical Care for Any Medical Condition											
Mean	0.231	0.220	0.269	0.206	0.245	0.226	0.233	0.275	0.229	0.205	0.209
SD	0.422	0.414	0.443	0.405	0.430	0.418	0.423	0.446	0.420	(0.404)	0.407
Panel D: Delayed Medical Care for Something Other Than COVID-19											
Mean	0.172	0.161	0.209	0.177	0.200	0.155	0.155	0.227	0.171	0.134	0.156
SD	0.377	0.367	0.406	0.381	0.400	0.362	0.362	0.419	0.376	0.341	0.363
Panel E: Vaccination Status (Received Vaccine)											
Mean	0.578	0.589	0.543	0.458	0.524	0.595	0.677	0.499	0.581	0.629	0.611
SD	0.494	0.492	0.498	0.498	0.499	0.491	0.468	0.500	0.493	0.483	0.488
N	700,070	536,820	163,250	85,746	219,559	207,707	187,058	189,943	209,140	248,919	52,068

Notes: This table presents descriptive statistics for outcome variables by selected subgroups. The subgroups are defined by race/ethnicity, educational attainment, and household income. Panels A and B report means for pre-vaccination waves (i.e., waves 1-21). Panels C-E report means for post-vaccination waves (i.e., waves 22-33). The sample includes adults aged 18 and older. The total number of observations for delayed care is 2,298,654. Sample size differs for vaccination status. The question on vaccination status was added in wave 22 (starting from January 6, 2021).

Table B.5. First Stage - Alternative Specifications

	(1)	(2)	(3)
Vaccine Eligibility	0.2719***	0.2754***	0.3043***
	(0.013)	(0.012)	(0.013)
N	2,298,654	2,298,654	2,298,654
State FE	√	√	√
Survey Wave FE	√	√	√
Region Wave Linear Trend	√		
State Wave Linear Trend		√	
State × Wave FE			√
Kleibergen-Paap rk LM statistic	19.01	19.14	18.74
Kleibergen-Paap rk Wald F statistic	470.6	504.5	558.4

Notes: This table presents the effects of vaccine eligibility on vaccination status using alternative specifications. Each column is a separate regression. Column (1) includes state FE, survey wave FE, and region-specific linear trend, where we use four U.S. Census regions. Column (2) includes state FE, survey wave FE, and state-specific linear trend. Column (3) includes state FE, survey wave FE, and state-by-wave FE. All specifications control for individual attributes: age, gender, race, ethnicity, marital status, educational attainment, household income, health insurance coverage, and the number of children. All regressions are weighted using the survey weights. Standard errors reported in parentheses are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table B.6. Balancing Test - Vaccine Eligibility and Individual Characteristics

	Age Cohort								
	18-29	30-39	40-49	50-54	55-59	60-64	65-69	70-74	75+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Male	0.000278	-0.000240	-0.000125	-0.000183	0.000041	0.000033	0.000806	-0.002532**	-0.000428
	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Married	0.000491	0.000163	-0.000532	0.000723	0.000980	-0.000425	0.000189	0.000110	0.001158
	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Race/Ethnicity (Reference Group: Non-Hispanic White)									
Black	0.000129	-0.000623	0.000221	0.001154	-0.001349	0.001313	0.000742	0.000998	-0.002615
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)	(0.004)
Asian	-0.000058	0.000087	-0.000019	-0.000431	-0.001821*	-0.001358	-0.000982	0.004823	0.005497
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	(0.005)
Hispanic	0.000542	0.000517	0.001075*	0.000407	-0.001134	-0.002826**	-0.000180	0.000884	0.000370
	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.002)
Other Races	-0.003722**	-0.000057	-0.001262	0.004168*	0.000893	-0.003673***	0.001760	0.001341	-0.010691**
	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.003)	(0.004)	(0.005)
Education (Reference Group: ≤High School)									
Some College	-0.000317	0.000660	0.000013	0.000246	-0.000594	0.000220	-0.001380	0.001478	0.000299
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
University	0.000079	0.000115	-0.000066	-0.000361	0.000289	0.000792	-0.001468	0.001031	-0.000119
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Post-University	-0.000986	-0.000629	-0.000130	-0.001623**	-0.000236	-0.000077	-0.001121	0.001461	0.000367
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Total household income (before taxes) (Reference Group: <50K)									
50K-100K	0.001228*	0.000130	0.000801	0.001600	0.000390	0.000510	-0.002421**	0.001322	-0.000818
	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
>100K	-0.000603	0.000586	0.000830	0.001294	-0.000310	0.000365	-0.001391	0.000768	-0.000223
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Income missing	-0.000559	-0.000921	0.002305*	0.002979	-0.000289	0.003378	-0.003488*	0.002118	0.001753
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Health Insurance Coverage	0.001200*	0.000478	-0.000410	0.000871	-0.002253	0.001083	0.006285*	-0.000431	0.002940
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	(0.005)	(0.004)
Number of Child under 18 (Reference Group: 0)									
1	-0.002026	-0.000384	0.000128	-0.000181	0.001680	0.000384	0.000740	0.002233	-0.001639
	(0.002)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)
2	0.001237	0.000205	-0.000325	-0.000334	-0.001743	0.002054	-0.001204	-0.003967	-0.000521
	(0.001)	(0.001)	(0.000)	(0.001)	(0.002)	(0.002)	(0.003)	(0.005)	(0.003)
3	0.002465*	0.000159	0.000673	-0.001091	-0.002349	-0.003597	0.005879	-0.018186**	0.001545
	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.002)	(0.005)	(0.009)	(0.004)
4	0.001781	-0.001650	0.000146	-0.002742	-0.006816*	-0.005981	0.018396	0.032322	0.065385
	(0.003)	(0.001)	(0.001)	(0.004)	(0.003)	(0.005)	(0.014)	(0.026)	(0.053)
5+	-0.003847	0.002286	0.001325	-0.004873*	-0.013983**	-0.001589	-0.006558	-0.000598	0.014423
	(0.002)	(0.002)	(0.002)	(0.003)	(0.006)	(0.008)	(0.009)	(0.011)	(0.009)
N	144,410	382,828	438,843	219,500	223,067	242,875	248,406	202,956	195,76
State FE	√	√	√	√	√	√	√	√	√
Survey Wave FE	√	√	√	√	√	√	√	√	√

Notes: This table presents the relationship between the instrument and observable characteristics. To obtain the estimates, we regress vaccine eligibility on all individual characteristics reported in the table. Each column is a separate regression using a different age cohort, and includes state and survey wave FE. All regressions are weighted using the survey weights. Standard errors reported in parentheses are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table B.7. Vaccine Eligibility and Delayed Care - Alternative Specifications

	(1)	(2)	(3)
Panel A: Delayed Medical Care for Any Medical Condition			
Vaccine Eligibility	-0.0391***	-0.0388***	-0.0424***
	(0.004)	(0.004)	(0.004)
Panel B: Delayed Medical Care for Something Other Than COVID-19			
Vaccine Eligibility	-0.0195***	-0.0195***	-0.0215***
	(0.004)	(0.004)	(0.005)
N	2,298,654	2,298,654	2,298,654
State FE	$\checkmark$	$\checkmark$	$\checkmark$
Survey Wave FE	$\checkmark$	$\checkmark$	$\checkmark$
Region Wave Linear Trend	$\checkmark$		
State Wave Linear Trend		$\checkmark$	
State $\times$ Wave FE			$\checkmark$

Notes: This table presents the reduced form estimates showing the effects of vaccine eligibility on delayed care using alternative specifications. Panel A reports the results for delayed care for any medical condition. Panel B reports the results for delayed care for conditions other than COVID-19. Each column is a separate regression. Column (1) includes state FE, survey wave FE, and region-specific linear trend, where we use four U.S. Census regions. Column (2) includes state FE, survey wave FE, and state-specific linear trend. Column (3) includes state FE, survey wave FE, and state-by-wave FE. All specifications control for individual attributes: age, gender, race, ethnicity, marital status, educational attainment, household income, health insurance coverage, and the number of children. All regressions are weighted using the survey weights. Standard errors reported in parentheses are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table B.8. Vaccination Status and Delayed Care - Alternative Specifications

	(1)	(2)	(3)	(4)	(5)	(6)
		OLS			IV	
Panel A: Delayed Medical Care for Any Medical Condition						
Received Vaccine	-0.0350***	-0.0359***	-0.0358***	-0.1439***	-0.1411***	-0.1394***
	(0.002)	(0.002)	(0.002)	(0.013)	(0.013)	(0.014)
Panel B: Delayed Medical Care for Something Other Than COVID-19						
Received Vaccine	-0.0304***	-0.0310***	-0.0311***	-0.0719***	-0.0707***	-0.0707***
	(0.002)	(0.002)	(0.002)	(0.015)	(0.015)	(0.016)
N	2,298,654	2,298,654	2,298,654	2,298,654	2,298,654	2,298,654
State FE	√	√	√	√	√	√
Survey Wave FE	√	√	√	√	√	√
Region Wave Linear Trend	√			√		
State Wave Linear Trend		√			√	
State × Wave FE			√			√

Notes: This table presents the OLS and TSLS estimates showing the effects of vaccination status on delayed care using alternative specifications. Panel A reports the results for delayed care for any medical condition. Panel B reports the results for delayed care for conditions other than COVID-19. Each column is a separate regression. Columns (1) and (4) include state FE, survey wave FE, and region-specific linear trend, where we use four U.S. Census regions. Columns (2) and (5) include state FE, survey wave FE, and state-specific linear trend. Columns (3) and (6) include state FE, survey wave FE, and state-by-wave FE. All specifications control for individual attributes: age, gender, race, ethnicity, marital status, educational attainment, household income, health insurance coverage, and the number of children. All regressions are weighted using the survey weights. Standard errors reported in parentheses are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table B.9. First Stage - Samples for Other Outcomes

	(1)	(2)	(3)	(4)	(5)
Sample A: Concerns about Getting or Spreading COVID-19					
Vaccine Eligibility	0.3349***	0.3406***	0.3410***	0.3410***	0.3409***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Kleibergen-Paap rk LM statistic	18.45	18.55	18.57	18.58	18.56
Kleibergen-Paap rk Wald F statistic	837.5	766.2	788.3	788.5	785.2
N	770,284	770,284	770,284	770,284	770,284
Sample B: Children Delayed Preventive Healthcare					
Vaccine Eligibility	0.0516***	0.0624***	0.0622***	0.0668***	0.0664***
	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)
Kleibergen-Paap rk LM statistic	3.884	4.976	5.011	5.253	5.066
Kleibergen-Paap rk Wald F statistic	12.95	28.42	26.80	25.80	25.07
N	104,697	104,697	104,697	104,697	104,697
State FE	√	√	√	√	√
Survey Wave FE	√	√	√	√	√
Region × Wave FE		√	√	√	√
Health measures and policy responses			√	√	√
Economic conditions				√	√
Social networks					√

Notes: This table presents the effects of vaccine eligibility on vaccination status using samples in Table 4 (Sample A) and Table 5 (Sample B), respectively. Each column is a separate regression where we add successively more covariates. Column (1) includes state and survey wave FE. Column (2) adds region-by-wave FE, where we use four U.S. Census regions. Column (3) adds two-week lagged COVID-19 death rate and stringency index. Column (4) adds one-month lagged unemployment rate. Column (5) adds two-week lagged friend-exposure to vaccination information. All specifications control for individual attributes: age, gender, race, ethnicity, marital status, educational attainment, household income, health insurance coverage, and the number of children. All regressions are weighted using the survey weights. Standard errors reported in parentheses are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table B.10. Vaccination Rate and Health Expenditures - Normalized IV

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
			OLS					IV		
Panel A: Consumer Healthcare Spending										
Vaccination Rate per 100	0.0002	0.0007	0.0009	0.0006	0.0007	0.0012	0.0041	0.0035	0.0031	0.0049
	(0.0012)	(0.0018)	(0.0017)	(0.0013)	(0.0013)	(0.0045)	(0.0048)	(0.0048)	(0.0050)	(0.0048)
						First Stage Estimates (*Vaccination Rate per 100*)				
Fraction Eligible						0.0497***	0.0373***	0.0372***	0.0357***	0.0382***
						(0.0140)	(0.0100)	(0.0099)	(0.0096)	(0.0094)
N	8,750	8,750	8,750	8,750	8,050	8,750	8,750	8,750	8,750	8,050
State FE	√	√	√	√	√	√	√	√	√	√
Date FE	√	√	√	√	√	√	√	√	√	√
Region × Week FE		√	√	√	√		√	√	√	√
Health measures and policy responses			√	√	√			√	√	√
Economic conditions				√	√				√	√
Social networks					√					√
Kleibergen-Paap rk LM statistic						7.97	8.17	8.29	8.42	8.94
Kleibergen-Paap rk Wald F statistic						12.67	13.88	14.04	13.95	16.50

Notes: This table presents the effects of vaccination rate on consumer healthcare spending using an alternative instrument, where we calculate the proportion of eligible individuals based only on the population older than 18. We report the first stage estimates under the IV specifications in columns (6)-(10). Each column is a separate regression where we add successively more covariates. Columns (1) and (6) include state and calendar date FE. Columns (2) and (7) add region-by-week FE, where we use four U.S. Census regions. Columns (3) and (8) add two-week lagged COVID-19 death rate and stringency index. Columns (4) and (9) add one-month lagged unemployment rate. Columns (5) and (10) add two-week lagged friend-exposure to vaccination information. Standard errors reported in parentheses are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table B.11. Vaccination Rate and Expenditures of Other Categories

	(1)	(2)	(3)	(4)	(5)	(6)
	IV					
	Food Service	Entertainment	Merchandise	Grocery	Transportation	All Spending
Vaccination Rate per 100	0.0004	0.0020	0.0057	-0.0014	0.0093***	0.0032*
	(0.0020)	(0.0050)	(0.0044)	(0.0026)	(0.0034)	(0.0018)
N	8,050	8,050	8,050	8,050	8,050	8,050
State FE	√	√	√	√	√	√
Date FE	√	√	√	√	√	√
Region × Week FE	√	√	√	√	√	√
Health measures and policy responses	√	√	√	√	√	√
Economic conditions	√	√	√	√	√	√
Social networks	√	√	√	√	√	√
Kleibergen-Paap rk LM statistic	10.46	10.46	10.46	10.46	10.46	10.46
Kleibergen-Paap rk Wald F statistic	20.90	20.90	20.90	20.90	20.90	20.90

Notes: This table presents the effects of vaccination rate on consumer spending in other categories. In column (6), "all spending" refers to all expenditure categories. Each column is a separate regression, and includes state and calendar date FE, region-by-week FE, two-week lagged COVID-19 death rate and stringency index, one-month lagged unemployment rate, and two-week lagged friend-exposure to vaccination information. Standard errors reported in parentheses are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.