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FROM SYRINGES TO DISHES:
IMPROVING FOOD SUFFICIENCY THROUGH VACCINATION

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ABSTRACT

This paper examines the impact of COVID-19 vaccination on food insufficiency in the United States, using data from the Household Pulse Survey. Our primary research design exploits variation in vaccine eligibility across states over time as an instrumental variable to address the endogeneity of vaccination decision. We find that vaccination had a substantial impact on food hardship by reducing the likelihood of food insufficiency by 24%, with even stronger effects among minority and financially disadvantaged populations. These results are robust to alternative specifications and the use of regression discontinuity as an alternative identification strategy. We also show that vaccine eligibility had a positive spillover impact on food assistance programs, notably reducing participation in the Supplemental Nutrition Assistance Program and the use of its benefits, suggesting that vaccination policies can help alleviate the government's fiscal burden during public health crises. Our analysis offers detailed insights into the potential mechanisms linking vaccination to food insufficiency. We demonstrate that vaccination yields changes in both material circumstances and financial expectations. Specifically, vaccination increases the use of regular income for spending needs and reduces reports of insufficient food due to unaffordability. Additionally, we find that vaccination improves financial optimism, reflected in expectations for future employment income loss and the ability to meet mortgage and debt obligations. Our findings are consistent with the notion that this optimism, along with labor market recovery, diminished the need for precautionary savings, reduced reliance on government assistance, and encouraged household spending on essential goods like food, ultimately lowering food insufficiency.

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1. Introduction

The COVID-19 pandemic caused significant disruptions across the United States, significantly affecting households' ability to reliably access adequate and nutritious food. Early in the pandemic, food insecurity and food insufficiency surged due to mass unemployment, sharp income losses, and rising food prices (Altman et al., 2021; Parekh et al., 2021).[1] Social distancing, lockdowns, and temporary closure of food retailers further limited access to food (Arndt et al., 2020; Hamadani et al., 2020; Chetty, Friedman, and Stepner, 2024). As a result, millions of Americans, particularly those with lower socioeconomic status or living in regions with inadequate social support, struggled to meet basic nutritional needs (Wolfson and Leung, 2020; Raifman, Bor, and Venkataramani, 2021). For example, Schanzenbach and Pitts (2020) report that the proportion of the population experiencing food insecurity doubled from 10.8% in February 2020 to 23% between April and May 2020. Food insecurity increased across all states, with some experiencing especially severe spikes. During this period, 7% of households nationwide reported receiving free food in the prior week.

The surge in food insecurity persisted throughout the first year of the pandemic despite federal relief efforts. Several programs, including the Coronavirus, Aid, Relief, and Economic Security (CARES) Act and the American Rescue Plan Act (ARPA) helped stabilize conditions. Additionally, the Farmers to Families Food Box Program administered by the U.S. Department of Agriculture (USDA) provided temporary relief by distributing food to Americans and supporting local food distributors and producers affected by pandemic-related disruptions.[2] Despite these efforts, around 18 million adults (8.8%) reported not having enough to eat sometimes or often in the past week of March 2021 - more than double the 8.5 million (3.4%) who reported the same at any point in 2019 (Keith-Jennings, Nchako, and Llobrera, 2021).

Although food access challenges affected all states and populations, the burden fell disproportionately on already vulnerable groups. Black and Latino households and households with children experienced significantly higher rates of food insecurity than others (Wolfson and Leung, 2020).[3] A large body of research links food insecurity to a range of adverse outcomes, including delayed medical care (Bertoldo et al., 2022), chronic illness (Seligman, Schillinger et al., 2010), mental health challenges (Wolfson, Garcia, and

[1] The U.S. government uses two primary metrics to assess and monitor difficulties in accessing food. Food insecurity, as defined by the U.S. Department of Agriculture (USDA), is the condition in which households are, at times, unable to acquire adequate food due to insufficient money or other resources for food. A closely related concept, food insufficiency, represents a more severe form of food insecurity, where households report not having enough to eat either sometimes or often. See https://tinyurl.com/mupdferz for further details.

[2] Launched in May 2020 and ending in May 2021, the Program delivered over 173 million food boxes, including fresh produce, dairy, cooked meats, and seafood, valued at over $5 billion nationwide.

[3] Between 2016 and 2021, food insecurity affected 19.8% of Black households and 16.2% of Hispanic households, compared to 6.6% of White households (Hales and Coleman-Jensen, 2024).

Leung, 2021; Sabião et al., 2022; Whitaker, Phillips, and Orzol, 2006), and impaired child development (Howard, 2011).[4] The financial consequences are substantial. Berkowitz et al. (2019) estimate that food-insecure adults incur $1,834 more in annual healthcare costs than their food-secure counterparts, amounting to $52.9 billion in excess spending in 2016 alone. These figures underscore the importance of addressing food hardship through both economic and public health policy.

This paper examines the causal relationship between COVID-19 vaccination and reductions in food hardship, as measured by food insufficiency, in the United States by using data from the Household Pulse Survey (HPS) and leveraging the variation in the rollout of vaccine eligibility across states over time. In the early stages of the pandemic in the U.S., economic relief packages played an important role in preventing further increases in food insufficiency. However, it was not until early 2021 that the rate of hardship began to decline, which coincided with the commencement of vaccination programs in February.[5] This suggests that vaccination efforts may have also played a role in reducing the number of people struggling with access to sufficient food. However, the extent to which individual decisions to get vaccination impact food insufficiency remains ambiguous, and further research is needed to establish a causal relationship between vaccination and food insufficiency.

We propose that the primary mechanisms linking COVID-19 vaccination to reduced food insufficiency operate through a set of individual-level pathways, particularly improvements in financial stability, labor market outcomes, and economic expectations. Vaccination enables individuals to safely reengage in economic and social activities, which can directly impact their employment prospects, income streams, and household consumption behavior. These effects are most plausibly experienced directly by the vaccinated individual, rather than being driven by broader state- or region-level macroeconomic conditions. This is consistent with the identifying variation in our instrumental variables (IV) strategy, which leverages age-specific eligibility cutoffs.

We identify several distinct channels through which vaccination may reduce food insufficiency at the individual level. First, vaccination can facilitate a return to work by reducing health risks and enabling safer participation in economic and social life.[6] This individual-level reentry into the labor force improves employment prospects and household income, which in turn enhances food access. Consistent with this mechanism, we find that vaccinated individuals are more likely to use regular income to meet spending needs and less likely to apply for unemployment insurance (UI) or report that they cannot

[4]Food insecurity is also associated with higher hospitalization rates during the pandemic and may contribute to long COVID (Ariya et al., 2021).

[5]See, e.g., Figure 2 in the White House research blog by Cecilia Rouse and Brandon Restrepo: https://bit.ly/436iSV3.

[6]Hansen and Mano (2022) show that vaccination plays a critical role in stabilizing the labor market by allowing businesses and public spaces to reopen, resulting in a rise in job opportunities and a decline in unemployment rates.

afford additional food.

Second, receiving a vaccine may directly reduce uncertainty about future economic conditions. Individuals who are vaccinated may feel more confident about their health and job stability, which improves their financial outlook and reduces the perceived need for precautionary saving.[7] In our mechanism analysis, we find that vaccinated individuals report significantly lower expectations of future income loss and greater confidence in meeting upcoming mortgage or rent payments, especially among low-income and minority groups. This increased financial optimism allows households to reallocate resources toward essential consumption, including food.

Third, vaccination also appears to reduce reliance on public assistance. We observe a decline in Supplemental Nutrition Assistance Program (SNAP) participation and in the use of SNAP benefits to meet basic needs following vaccination. This suggests that the individual-level improvements in employment and financial stability also reduce the demand for food assistance.

Finally, vaccinated individuals may also feel more comfortable engaging in everyday public activities such as grocery shopping or dining out. This behavioral shift, driven by perceived health security, can reduce logistical or psychological barriers to food access. Our analysis provides suggestive evidence that individuals were less likely to report receiving grocery or meal deliveries after vaccination. This pattern aligns with earlier studies that documented a rise in online food purchasing during the peak of the pandemic and lockdowns (Wang et al., 2021; Young, Soza-Parra, and Circella, 2022), followed by a likely reversion to pre-pandemic behaviors during the recovery period.

In our research design, we carefully account for potential bias stemming from the likely endogeneity of individual vaccination decisions by using two empirical strategies, each with its own advantages. Specifically, we use an instrumental variables (IV) strategy exploiting the differential rollout of age-specific vaccine eligibility across states and over time as an instrument for vaccine receipt, and a regression discontinuity design (RDD) using a discrete jump in vaccine eligibility around multiple age cutoffs. Using both IV and RDD in a unified framework can provide a more robust analysis as it allows for cross-validation of results. For example, if the results of the IV and RDD are consistent, it can provide more confidence in the validity of our estimates.

Our empirical results show that vaccine eligibility increases the likelihood of vaccination by 26.8 percentage points. Furthermore, the individual decision to get vaccinated reduces the likelihood of food insufficiency by 9.3 percentage points, which corresponds to a 24% decline in food insufficiency relative to the pre-vaccination baseline. Additionally, vaccination reduces the likelihood of severe food insufficiency, a more acute

[7]Studies have shown that when households anticipate economic improvement, they tend to reduce precautionary savings and increase consumption, including spending on essential goods like food (Ren and Zheng, 2023).

level of difficulty in food access, by 6.3 percentage points, corresponding to a 58% reduction relative to the baseline. Our results are not sensitive to different specifications and they are robust to using RDD as an alternative identification strategy. While we observe improvements in food insufficiency across individuals with various demographic and socioeconomic backgrounds, the effects are stronger among minority groups and financially disadvantaged households.

Taken together, our findings highlight the critical role that vaccination can play in reducing food hardship and promoting food sufficiency, particularly among vulnerable populations. By operating through mechanisms, such as labor market reintegration, improved financial stability, and enhanced food access, vaccination serves not only as a public health intervention but also as a powerful tool for addressing preexisting inequities in food access during times of crisis. More broadly, the economic and behavioral benefits of vaccination extend well beyond direct disease prevention, offering improvements in household well-being and resilience in the face of public health emergencies.

2. COVID-19 Vaccine as a Public Health Intervention: Rollout, Context, and Timeline

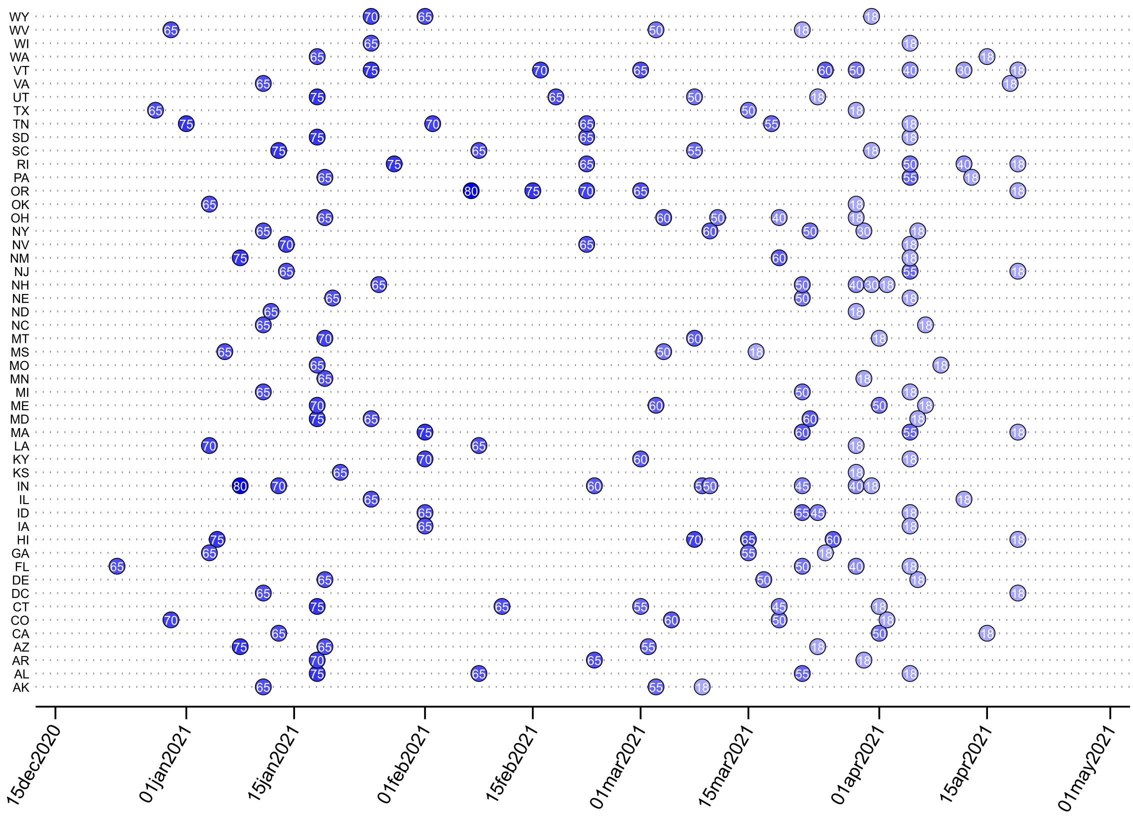
The development and rollout of COVID-19 vaccines have played a critical role in addressing the pandemic. On December 11, 2020, the Pfizer-BioNTech COVID-19 vaccine was granted emergency use authorization by the U.S. Food and Drug Administration for individuals aged 16 and older. The first dose was administered on December 14 to a healthcare professional in New York State three days after the vaccine's approval.[8] Due to limited vaccine supplies, states implemented a tiered system of vaccine distribution that prioritized the most vulnerable populations. Frontline healthcare workers, healthcare service workers, and long-term care residents were among the first groups to receive the vaccine, followed by adults aged 65 years and older and those with underlying medical conditions. The final group to become eligible for the vaccine was the general public aged 18 and older.

Starting in late January 2021, many states began to gradually open up COVID-19 vaccine eligibility to the general public, with different age criteria and timelines across states. Figure 1 displays the variation in age-specific vaccine eligibility across states and over time. Florida, for instance, adopted a four-step approach, starting with those aged 65 and older (65+) on December 23, 2020, followed by those aged 50+ on March 22, 2021, those aged 40+ on March 29, 2021, and finally, all adults aged 18+ on April 5, 2021. In contrast, New York State followed a different timeline, initially making the vaccine available to those aged 65+ on January 11, 2021, followed by those aged 60+ on

[8]See the following article from Washington Post for more details: https://wapo.st/3Yu25GC.

March 10, those aged 50+ on March 23, those aged 30+ on March 30, and finally, all adults aged 18+ on April 6. Florida and New York State are just two examples of the different timelines and age criteria used in the COVID-19 vaccine distribution process across the United States. Throughout March 2021, many states began to expand vaccine eligibility to those below 65, and by mid-April 2021, all states had made the COVID-19 vaccine available to adults aged 18 years and older.

Figure 1. Variation in Age-Specific Vaccine Eligibility



Notes: Older adults became eligible for the COVID-19 vaccine before younger adults, as indicated by the variations in the shading of the scatter plots. Darker colors represent older age groups, while lighter colors correspond to younger age groups.

Data Source: COVID-19 U.S. State Policy (CUSP) Database (Raifman et al., 2020).

3. Data

3.1. Vaccination, Food Insufficiency, and Financial Optimism

Our data on vaccination and food insufficiency come from the Household Pulse Survey (HPS). This survey has been deployed by the U.S. Census Bureau to understand household behaviors from a social and economic perspective during the pandemic and the following recovery period, while also providing a unique opportunity to study the near real-time policy responses and public health interventions at the federal and state levels.[9] The data include a broad range of questions on food sufficiency, vaccination status, employment, housing security, rental assistance, household spending, and concerns about the economy, among many other variables that gauge household experiences during the pandemic.

The HPS data has been released in multiple phases. The first phase, which covered waves 1-12 from April 23, 2020 - July 21, 2020, provided weekly estimates for each wave. It is worth noting that starting from the second phase (wave 13, from August 19, 2020 - October 26, 2020, and onward), new questions were added to the survey. Additionally, the data collection cycle was increased to two weeks.[10] For instance, the questions related to COVID-19 vaccination were included on January 6, 2021 (or wave 22), following the availability of COVID-19 vaccines in the United States. We define an indicator for vaccination uptake based on a "yes" or "no" question about COVID-19 vaccine receipt.[11] To have consistency in the sampling method, our analysis uses data starting from the second phase, covering periods from August 19, 2020 to July 5, 2021, for adults over 19 years of age.[12] In addition, we make weighting adjustments using household weights introduced in the second phase of the HPS to adjust for potential nonresponse bias, a common feature of all statistical surveys (Peterson et al., 2021).

The U.S. Department of Agriculture (USDA) defines food insecurity as "[l]ack of consistent access to enough food to live an active and healthy life" (Rabbitt and Smith,

[9]The survey has been conducted in partnership with various federal agencies, including the Bureau of Labor Statistics (BLS), Centers for Disease Control and Prevention (CDC), and USDA Food and Nutrition Service (FNS), among others.

[10]Moreover, the first phase sampled respondents for up to three weeks, whereas in the subsequent waves (starting from the second phase), samples were independent of each other. In other words, there was no sample rotation across waves.

[11]Existing studies indicate that the accuracy of self-reported COVID-19 vaccine status during the pandemic is relatively high (over 90%) compared to other vaccines, with this high level of accuracy being consistent across various populations and contexts (Stephenson et al., 2022; Archambault et al., 2023). In fact, Nguyen et al. (2021) show that HPS vaccine estimates by age group follow similar patterns to those reported in administrative CDC data. While we do not rule out measurement error in self-reported vaccination, it is unlikely to qualitatively alter our findings. If vaccine uptake in the HPS is overestimated, as suggested by Bradley et al. (2021), our IV estimates would represent a lower bound, since the denominator in the Wald estimator would be larger than the "true" value.

[12]We exclude younger age groups since they are likely to be dependent on their parents. Nonetheless, our baseline results do not change under the sample of adults aged 17+.

2021). Food insufficiency is closely linked to the broader concept of food insecurity, which has been an important indicator of well-being in the United States for over 25 years (USDA ERS, 2024). While both terms address concerns over food access, food insufficiency specifically measures whether a household has adequate food on a regular basis. There is significant overlap between the two, as most households categorized as food secure are also considered food sufficient. Similarly, those facing low food security often experience marginal food sufficiency. Importantly, food insufficiency represents a more severe form of hardship, focusing on whether households generally have enough to eat, making it a more acute measure of food-related distress.

In the HPS, there is a survey item measuring whether households had enough to eat, which relates to the concept of food insufficiency.[13] To measure the severity of food insufficiency at different levels, we leverage the following question:

> "*In the last 7 days, which of these statements best describes the food eaten in your household? Select only one answer.*
> *(1) Enough of the kinds of food (I/we) wanted to eat*
> *(2) Enough, but not always the kinds of food (I/we) wanted to eat*
> *(3) Sometimes not enough to eat*
> *(4) Often not enough to eat* "

The HPS defines this as a measure of food sufficiency over the past seven days. One key advantage of the food insufficiency question is that it is concise, making it both easy to administer and straightforward to interpret (USDA ERS, 2024). According to USDA, categories (3) and (4) are classified as *low* and *very low* food sufficiency, respectively. These levels of hardship indicate disruptions in eating patterns and reduced food intake (Coleman-Jensen et al., 2022). In contrast, category (2) represents a less severe form of food insufficiency, referred to as *marginal* food sufficiency.[14] However, this category may reflect a different type of hardship experienced by households, namely reductions in quality or variety of food. This raises potential concerns about whether households are consuming enough essential nutrients to maintain an active and healthy life. Considering these definitions, we first construct a measure of food insufficiency that covers a broader spectrum of experiences, ranging from marginal sufficiency (having enough but not the desired kinds of food) to low and very low sufficiency (not having enough food sometimes

[13]Because the objective of Census Bureau's HPS is to provide near real-time data on multiple measures of well-being during the pandemic, it does not provide a full module on food insecurity that captures details of food hardship. Moreover, the survey question intends to describe households' food situation in the last 7 days rather than over the course of a year. This is particularly important in estimating the immediate impact of vaccination on food insufficiency. Although there is a full module on food insecurity in the CPS Food Security Supplement (CPS-FSS), the lack of monthly variation in the data prevents researchers from assessing the long-term impact of age-specific vaccine eligibility on food insecurity.

[14]More details about the USDA's measurement of food insufficiency can be found here: https://bit.ly/3K6Fn2S.

or often). This combined outcome captures varying degrees of difficulty in food access, with all levels reflecting some form of insufficiency, whether due to a lack of food variety or quantity. Grouping these experiences under the term food insufficiency reflects the broader issue of insufficient access to food, as well as access to nutritionally adequate food.

In addition, we define a more focused outcome that combines low and very low food sufficiency as *severe food insufficiency*. This distinction highlights households that report sometimes or often not having enough food, marking a more acute level of difficulty in food access.[15] Notably, the HPS includes a follow-up question that explores the reasons behind food insufficiency, which we incorporate to examine underlying mechanisms. The follow-up question specifically asks why households did not have enough to eat or the kinds of food they wanted, emphasizing not just food quantity but also access to preferred types of food.[16] Households could select all applicable reasons. Specifically, the question asks:

> "*Why did you not have enough to eat (or not what you wanted to eat)?*
> *(1) Could not afford to buy more food*
> *(2) Could not get out to buy food (e.g., did not have transportation, or had mobility or health problems that prevented you from getting out)*
> *(3) Afraid to go or did not want to go out to buy food*
> *(4) Could not get groceries or meals delivered to me*
> *(5) The stores did not have the food I wanted* "

The first three reasons consistently appear across waves 13 to 33, which aligns with the sample used in our benchmark analysis. The last two reasons are present only through wave 27. Nevertheless, we incorporate all these potential mechanisms into our empirical framework. These reasons address key mechanisms and offer a novel perspective on the relationship between vaccination and food sufficiency. Specifically, they capture both economic and non-economic factors that collectively influence food access.

Affordability, on the one hand, is a direct measure that can be influenced by changes in employment, earnings, and broader economic conditions. On the other hand, mobility restrictions may not only reflect economic challenges but also health-related issues, such as morbidities or disabilities caused by contracting the virus. Furthermore, they could represent risk perceptions, where individuals with heightened concerns about contracting the virus may be less likely to leave their homes to purchase food. Finally, changes in

[15] For consistency, we use the terms "food insufficiency" and "severe food insufficiency." The latter corresponds to the Census Bureau's definition of food scarcity.

[16] This question is answered by households who report not having enough to eat (options 3 and 4) or not having the type of food they want (option 2) in the food insufficiency question above. Our measure of food insufficiency aligns with the follow-up questions in the HPS, capturing not only whether households had enough to eat but also changes in food quality and variety, particularly for those facing financial hardship during the pandemic.

the types of food available in stores, particularly in terms of quantity, quality, or variety, could provide broader insights into supply-side disruptions in food production.

Another potential mechanism through which vaccination affects food insufficiency is expectations about future financial and economic conditions. Specifically, if vaccination increases the likelihood of forming optimistic expectations about future finances, it could further smooth out the economic impact of the pandemic. Therefore, we use questions from the HPS regarding the expected loss of employment income and confidence in the ability to pay future mortgage or rent, respectively, to study the changes in optimism about future financial conditions. These questions are listed as follows:

> "*Do you expect that you or anyone in your household will experience a loss of employment income in the next 4 weeks because of the coronavirus pandemic? Select only one answer.*
> *(1) Yes*
> *(2) No* "

and

> "*How confident are you that your household will be able to pay your next rent or mortgage payment on time? Select only one answer.*
> *(1) No confidence*
> *(2) Slight confidence*
> *(3) Moderate confidence*
> *(4) High confidence* "

where we construct a high confidence measure, taking the value 1 for category (4) and 0 otherwise. To account for different levels of financial confidence, we create additional measures of moderate confidence using responses in categories (3)-(4) and any confidence using responses in categories (2)-(4). We further create an indicator variable for the expectation measure, taking the value 1 if individuals expect a loss of employment income and 0 otherwise. Exploiting a set of expectation and confidence measures, we study whether vaccination promotes financial optimism and its impact on the level of confidence.

3.2. Vaccine Eligibility

To obtain random variation in vaccination, we exploit the differential rollout of vaccine eligibility across states and over time. Specifically, states determine vaccine eligibility using plausibly exogenous age criteria. We obtain the eligibility data from the COVID-19 U.S. State Policy (CUSP) database (Raifman et al., 2020; Skinner et al., 2022). The data from CUSP has been used in multiple studies to determine the impact of age-specific vaccine eligibility on the likelihood of delaying or forgoing care (Aslim et al., 2024), mental health problems (Agrawal et al., 2021), risk mitigating behaviors or ex-ante moral hazard (Agrawal, Sood, and Whaley, 2022), as well as hospitalizations and deaths (Smits et al., 2022).

A potential challenge in using age-specific vaccine eligibility to recover causal effects is that certain individuals may be eligible for vaccination prior to the policy implementation. On the one hand, non-compliance may bias our first-stage estimates toward zero. It is likely that those in high-risk occupation groups (e.g., frontline essential workers and K-12 employees) or adults with high-risk medical conditions may have received vaccination before the implementation of age-based criteria. On the other hand, vaccine eligibility may not be binding for many of these groups, especially if they are not likely to experience food insufficiency in the first place.[17] Nonetheless, we address this potential concern in Section 5 using several approaches: (i) showing that the benchmark first-stage estimates are relatively strong, (ii) conducting an event study analysis to investigate potential pre-trend deviations preceding age-specific eligibility, and (iii) creating a bound for the IV estimates by excluding individuals who received vaccination prior to the policy, which only accounts for 5.5% of our working sample.

3.3. Reliance on Food Stamps

An important implication of a reduction in food insufficiency upon vaccination is fiscal externalities, which can reduce the government's net costs (i.e., mechanical costs net of fiscal externalities) of providing or subsidizing vaccines. Specifically, individuals who transition out of food insufficiency may be less likely to rely on welfare programs, such as the food assistance program. To test this hypothesis, we use data on the SNAP from the CPS-FSS for the period August 2020 to June 2021. Although our intention is not to measure costs, we explore the second-order effect of vaccination on SNAP receipt, which is an important piece in the welfare calculation of vaccines.[18] Note that the CPS-FSS has been widely used to study the impact of government interventions on enrollment in SNAP (see, e.g., Rozema and Ziebarth 2017).

The data from the CPS-FSS are cross-sectional and available in December of each year. For each respondent, the survey reports the months for which food stamp benefits were received within a year. Therefore, we are able to pool individuals by person identifiers and months.[19] Similar to the HPS, our sample from the CPS-FSS includes adults over

[17] Moreover, this might not be a serious issue, given that two sources of bias may cancel out in the first stage and the reduced form.

[18] Our estimates could easily be used in future studies that aim to fully explore the welfare effects of vaccination.

[19] Since the information on individual vaccination status is not available in the CPS, we limit our analysis to the reduced-form model, focusing solely on the impact of vaccine eligibility on food stamp receipt. Furthermore, we also replicate our analysis using only the reference person, the person designated as the householder within a household. We find that our findings are not sensitive to this restriction. For brevity, we do not report this result.

19 years of age.

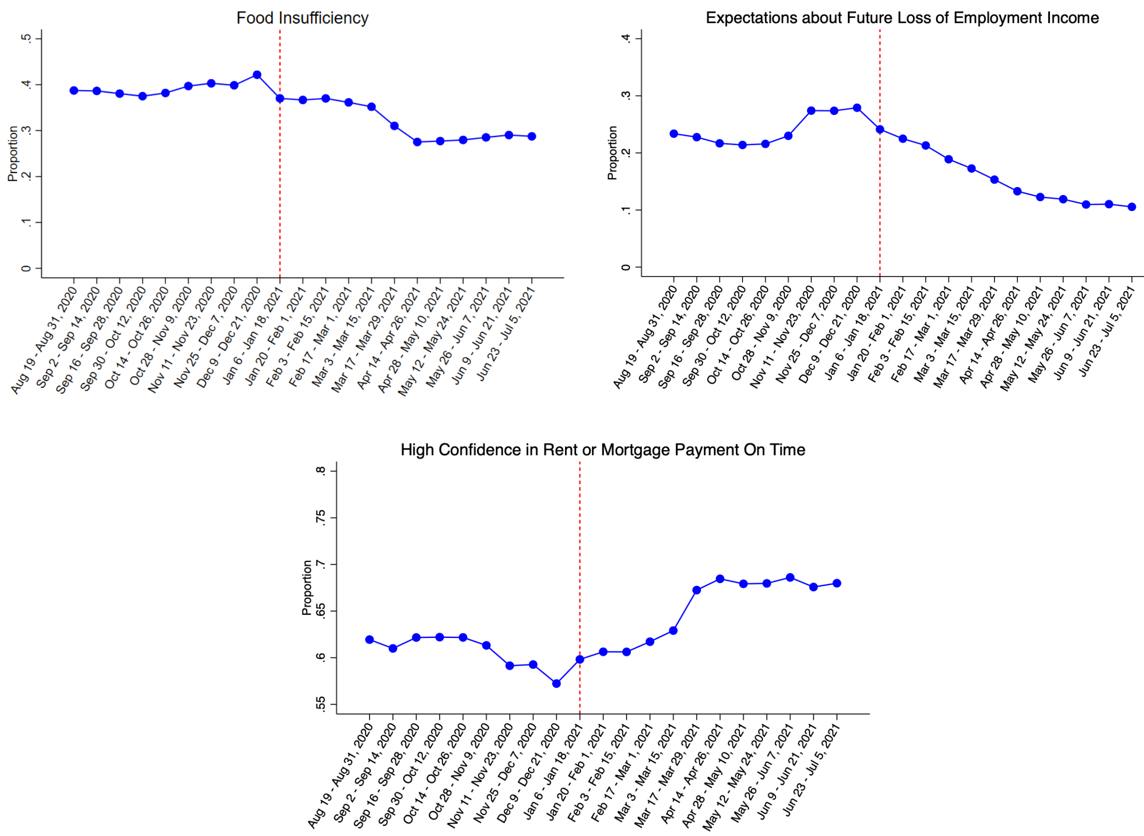
3.4. Descriptive Evidence

Before turning the focus to our two separate identification strategies and the estimation of causal parameters, we provide descriptive evidence on how food insufficiency and measures of financial optimism trend over time. In addition, we provide summary statistics on these outcomes by pre-determined attributes, such as prior household income and race.

3.4.1. Food Insufficiency and Measures of Financial Optimism

The first panel of Figure 2 provides graphical evidence of how food insufficiency in the last 7 days changed over time. Prior to vaccination, we find that close to 40% of households in the United States experienced food insufficiency at the onset of the pandemic.

Figure 2. Food Insufficiency and Measures of Financial Optimism Over Time



Notes: Each observation represents the weighted average of the corresponding outcome within each wave from August 19, 2020 (wave 13) through July 5, 2021 (wave 33). The dashed vertical line indicates the introduction of the COVID-19 vaccine in the United States.

Data Source: Household Pulse Survey, U.S. Census Bureau.

However, we observe a decline in food insufficiency after the vaccine rollout, depicted by the dashed vertical line. The share of households experiencing food insufficiency plummets to 30% over time. This implies an approximately 25% decline in food insufficiency relative to its pre-eligibility level.

As mentioned above, we explore the changes in the expected loss of future employment income and confidence in paying future mortgage or rent payments to study the role of vaccination in forming optimistic financial expectations, which can transition households out of food insufficiency. The second and third panels of Figure 2 provide valuable insights into financial optimism. More than 20% of adults were expecting to lose employment income in the future due to COVID-19 in 2020. However, we observe a sharp decline to about 10% over time, following the vaccine rollout. We further find a surge in people's high confidence levels in their ability to make timely mortgage or rent payments after the vaccine rollout.

Table 1 provides summary statistics for our outcome variables within pre- and post-vaccination waves. On average, 32% of households experience food insufficiency after vaccine eligibility, a decline from 39% prior to vaccine eligibility. Similar to the trends above, individuals are more likely to form optimistic expectations about future financial conditions after vaccine eligibility. However, there are heterogeneous changes in confidence levels for meeting future financial obligations, with the largest increase being in high confidence levels. Therefore, while some individuals may experience greater changes in confidence than others, overall, people are more likely to have higher confidence in their ability to make timely mortgage or rent payments after the vaccine rollout. We also find that about 52.6% of respondents have received a COVID-19 vaccine by July 5, 2021 (wave 33).[20] These findings suggest that the high vaccine uptake rate may have contributed to the overall decrease in food insufficiency and increase in confidence levels observed in our data. In the remainder of the paper, we provide empirical evidence to explore this question further.

3.4.2. Trends by Pre-Determined Attributes

Table A.1 reports summary statistics for pre-determined attributes.[21] An important observation is that both demographic and socioeconomic characteristics are relatively balanced across pre- and post-vaccination waves. This implies that vaccine eligibility

[20] The administrative data from the Centers for Disease Control and Prevention (CDC) show that 56% of the U.S. population has received at least one dose as of July 15, 2021. The estimate of vaccination prevalence from the HPS is close to those from the administrative data.

[21] We refer to demographic or socioeconomic characteristics as pre-determined attributes because they have negligible influence on our point estimates. Note also that household income is reported in 2019, which is prior to the pandemic. This alleviates potential concerns about endogenous changes in income with respect to the pandemic or the vaccination policy in general.

Table 1. Summary Statistics, Outcome Variables

	Pre-Vaccination Waves (Waves 13-21)			Post-Vaccination Waves (Waves 22-33)		
	Mean	SD	Obs.	Mean	SD	Obs.
Lack of Consistent Access to Enough Food						
Food Insufficiency	0.392	0.488	692,603	0.319	0.466	772,101
Severe Food Insufficiency	0.108	0.311	692,603	0.095	0.293	772,101
Measures of Financial Optimism						
Expectations about Future Loss of Employment Income	0.240	0.427	691,138	0.158	0.365	770,230
Levels of Confidence in Paying Future Rent or Mortgage Payments On Time						
– High Confidence	0.607	0.488	460,240	0.650	0.477	485,749
– Moderate Confidence	0.806	0.395	460,240	0.820	0.384	485,749
– Any Confidence	0.914	0.280	460,240	0.923	0.267	485,749
Vaccination Status						
Received Vaccination	0	0	692,603	0.526	0.499	772,101

Notes: The means are weighted using household weights from the HPS. The information on vaccination status is available after wave 21, following age-specific vaccine eligibility.

is independent of these pre-determined attributes. We do further checks on the independence assumption using our regression framework in Section 5.1.

In our sample, the average age of respondents is around 49. In addition, our sample is composed of mostly married, white individuals, as well as those with relatively higher educational attainment and household income. We have information about health insurance coverage as well. On average, 43% of adults have private insurance, while about 28% report having public coverage (i.e., Medicaid and Medicare). Previous studies have shown an increase in Medicaid coverage during the pandemic (Karpman and Zuckerman, 2021). Our descriptive finding for Medicaid, which shows a 0.3 percentage point rise in coverage, is consistent with the existing literature. About 35% of individuals, on average, report the presence of children (aged less than 18) in the household, and the average household size is more likely to be two. A few variables have missing observations, which are controlled for using indicator variables.

The prevalence of food and financial insufficiency is likely to be more salient among low-wage earners and minorities, and the pandemic might have exacerbated preexisting inequities (Dubowitz et al., 2021; Perry, Aronson, and Pescosolido, 2021). For that reason, we also provide descriptive evidence regarding the change in outcomes during the pandemic for low-income households and minority groups. In Figures A.1 and A.2, we find substantial disparities in food insufficiency and expectations about future financial conditions by income and race. Specifically, households with incomes less than $50,000 and minority groups, on average, are more likely to experience food insufficiency, while forming relatively less optimistic expectations about their future financial conditions during the pandemic. Overall, these figures suggest that vaccination may play a more salient role in improving outcomes for disadvantaged populations compared to the baseline. We formally test for heterogeneous effects in Section 5.3.

4. Identification Strategy

4.1. Instrumental Variables

Our objective is to estimate the causal effect of vaccination on the likelihood of food insufficiency. To capture this, we estimate the following regression model:

$$Y_{ist} = \alpha_1 + \psi_t + \kappa_s + \theta I_{ist} + \mathbf{X}'_{ist}\beta + \eta_{ist}, \tag{1}$$

where Y_{ist} is a measure of food insufficiency for individual i in state s at survey wave t, ψ_t are survey wave fixed effects, and κ_s are state fixed effects. The indicator variable I_{ist} switches on upon vaccination and switches off otherwise. The vector of covariates $\mathbf{X}_{ist}$ controls for the following individual characteristics: age, gender, marital status, race, educational attainment, household income, health insurance coverage, presence of children, and household size. In this setting, estimating the parameter of interest θ using OLS is likely to induce selection bias, given that individuals are not randomly assigned to receive the vaccine. To address this concern and recover causal effects, we exploit the differential rollout of age-specific vaccine eligibility across states and over time as an instrument for vaccine receipt. The idea is to obtain random variation in the likelihood of vaccination using exogenous eligibility criteria.

We use two-stage least squares (TSLS) to estimate θ in Equation (1), which is our second stage equation, and further use the following specification for first stage:

$$I_{ist} = \beta_1 + \psi_t + \kappa_s + \gamma E_{ist} + \mathbf{X}'_{ist}\delta + \nu_{ist}, \tag{2}$$

where the instrument E_{ist} denotes whether an individual is eligible for the vaccine based on the age criteria across states and over time. The remaining fixed effects and individual characteristics mirror those in Equation (1). The validity of our instrument hinges on a set of assumptions, including the relevance condition, the exogeneity of the instrument, and monotonicity (in the presence of heterogeneous treatment effects). Under these assumptions, we can identify the causal effect of vaccination among compliers who would not have received vaccination had they not been eligible for the vaccine. In other words, our TSLS estimand would recover the weighted average of local average treatment effects

(LATE).

Alternative specifications and inference. – Although there are no direct tests for instrument exogeneity (since η_{ist} is unobservable, e.g., when considering exclusion restriction), we provide a broad range of alternative specifications to test the validity of this assumption. To assess the independence assumption, we conduct robustness checks in the spirit of covariate balancing tests. Specifically, we progressively include pre-determined variables to show that our estimates are unaltered under the presumption that these variables are uncorrelated with vaccine eligibility. We also estimate event study specifications, which show parallel pre-trends in outcomes prior to the rollout of age-specific vaccine eligibility. Taken together, these exercises imply that the instrument is "as good as random," which is sufficient to draw inference about the causal effects of vaccine eligibility on food insufficiency.

To recover LATE, however, we also need an exclusion restriction. We make sure that our estimates do not change with the inclusion of region-by-wave fixed effects, state-specific linear time trends, as well as state-by-wave fixed effects, respectively. In a few specifications, we also control for lagged economic conditions, health conditions, and policy measures that vary across states and over time, following Aslim et al. (2024).[22] Specifically, we control for the unemployment rate from the U.S. Bureau of Labor Statistics to capture economic conditions. We control for COVID-19 death rates and the stringency index to account for health and state policy measures, respectively.[23] To account for the potential influence of social networks, we further include a measure of friend exposure to vaccine information as part of our state time-varying controls. The idea is that positive or negative messages on social media about vaccination or the availability of food can influence beliefs and behaviors among households. To construct this measure, we use data on social connectedness from Facebook (Bailey et al., 2018) and follow the approach in Bailey et al. (2020).[24] All these different specifications serve as indirect checks for exclusion restriction by closing potential backdoor paths between the instrument and

[22]Note that all these measures are lagged to avoid potential interactions with vaccine eligibility.

[23]We obtain data on death rates from The New York Times (2021) and the stringency index from the Oxford COVID-19 Government Response Tracker (Hale et al., 2021).

[24]We utilize two sets of data: administrative data on vaccination rates from the CDC and data on the Social Connectedness Index from Bailey et al. (2018), which are based on Facebook connections. To calculate friend exposure to vaccination for each state at a given time, we use the following formula:

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

where k represents a friend state and K is the set of all 50 states. The fraction of Facebook connections a user in state s has in state k relative to all other friend states serves as a proxy for the strength of social connection between state s and friend state k. We follow the methodology outlined in Bailey et al. (2020) to construct $FracConnect_{sk}$. The product of $FracConnect_{sk}$ and $VaccinationRate_{kt}$, which is the vaccination rate in state k at time t, reflects the exposure of state s to friend state k's vaccination policy at time t via social media. In short, the summation across all friend states is a weighted measure of exposure to vaccination policy from all friend states. We impose a two-week lag to alleviate any potential interactions with the instrument.

the outcome of interest.

Additionally, we adopt a more conservative approach by saturating the model with age cohort fixed effects and all two-way interactions among age cohort, wave, and state (i.e., state-by-age cohort, age cohort-by-wave, and state-by-wave fixed effects).[25] This constitutes a more demanding identification strategy: the model now relies on residual variation, isolating the component of treatment variation that is orthogonal to all of these two-way combinations. While this approach may enhance credibility by absorbing additional sources of confounding, it does so at the cost of reduced identifying variation. Nonetheless, we assess the robustness of our estimates using these more conservative specifications.

As a final step, we split the sample by observable attributes to show that, despite heterogeneous effects, the first-stage estimates range within a narrow window, supporting the validity of the monotonicity assumption. In Section 5, we provide detailed evidence and discussion for each of these cases.

For inference, we cluster our standard errors at the state level, which is a more conservative choice.[26] As a robustness check, we also explore alternative inference by clustering standard errors at the state-by-age-cohort level, which yields slightly smaller standard errors.

4.2. Regression Discontinuity

To confirm that our estimates are not driven by a specific research design, we employ an alternative identification strategy. Using a regression discontinuity (RD) design, we estimate the average causal effects of vaccine eligibility around the age cutoff of 65. We define the reduced-form specification for our RD design as follows:

$$Y_{ist} = \alpha_2 + \psi_t + \kappa_s + g_l(a-c) + \mathbf{1}[a > c](g_r(a-c) + \pi) + \mathbf{X'}_{ist}\tau + \epsilon_{ist}, \tag{3}$$

where a is age, c is the cutoff at age 65, $\mathbf{1}[\mathrm{a} > \mathrm{c}]$ is an indicator variable taking the value 1 above the cutoff and 0 below the cutoff,[27] and $g_l(.)$ and $g_r(.)$ are unknown functions. Our parameter of interest is π, which identifies the intent-to-treat (ITT) effect of vaccine eligibility on outcomes. Note that we separately estimate the reduced-form differences in mean food insufficiency and vaccination. This approach has the benefit of relying on fewer assumptions for the identification of causal effects. The key assumption is the continuity assumption, which asserts that all observable and unobservable factors should

[25] Note that state-by-age cohort fixed effects subsume age cohort fixed effects, since the latter are nested within the more granular interaction.

[26] Clustering at the state level guards against overstating precision, as clustering at a higher (coarser) level allows for arbitrary correlation of errors within larger groups.

[27] Because we do not have information about the exact birth month of respondents, we strictly exclude those at the cutoff ($a = c$) to alleviate potential measurement error in the assignment of treatment.

trend smoothly at the age cutoff. If the continuity assumption is plausible, then one can identify the average causal effect at the cutoff as:

$$\lim_{65 \leftarrow a} E[Y^1|a] - \lim_{a \to 65} E[Y^0|a], \tag{4}$$

where Y^1 and Y^0 are potential outcomes for treated and control units, respectively. It is possible to use the first-stage estimates to scale up ITT effects. For instance, using a Wald estimator one can obtain LATE by taking the ratio of the ITT estimate and the first-stage estimate, given a just-identified system. Notice that this approach requires the same identifying assumptions as the IV design to recover LATE.

We use age 65 as our benchmark cutoff for various reasons. First of all, more than half of the states initially provided eligibility to 65+ in earlier waves, and importantly, we are able to sample multiple waves across these states where individuals below 65 were not eligible for vaccination.[28] Moreover, there is evidence that vaccine eligibility among the elderly yields the largest reductions in hospitalizations and COVID-19 cases (Smits et al., 2022). Agrawal, Sood, and Whaley (2022) also find a relatively high take-up of the vaccine among individuals aged over 65. Therefore, if there is higher compliance, the average causal effect of vaccination on food insufficiency could also be larger among this group. These altogether motivate our analysis.

There is, however, an important challenge to the identification of treatment effects around the 65 age cutoff. Specifically, there is a major policy change at this cutoff, which is likely to violate the continuity assumption. Individuals above the 65 age cutoff are eligible for Medicare, a public health insurance program for the elderly in the United States. A potential income effect through Medicare may influence the likelihood of accessing nutritionally adequate food. Moreover, the likelihood of retirement and labor force exits increases among individuals over the age of 65 (Behaghel and Blau, 2012), which in turn could impact food insufficiency, once again through the income effect channel. We alleviate these concerns by (i) conducting a placebo analysis, e.g., using waves where individuals below and above the age of 65 are ineligible and (ii) extending our RD design to include these placebo groups from different waves in a *difference-in-discontinuities* design.[29] In Section 6, we show that our estimates from the difference-in-discontinuities design are remarkably robust and the placebo analysis yields precisely estimated null effects of vaccine eligibility.

In supplementary analyses, we further stack multiple age cutoffs across 41 states within a bandwidth where no other vaccine eligibility policy changes occurred, demonstrating that our findings are robust to the use of alternative age cutoffs.

[28]In our RD analysis, we do not include any states that provided prior eligibility to age groups above 65. Our sample includes 28 states that initially provided eligibility to 65+.

[29]We coin the term "difference-in-discontinuities" to refer to the difference in discontinues at the cutoff between two sample periods.

5. Main Results

5.1. First-Stage and Reduced-Form Estimates

We begin by estimating the impact of vaccine eligibility on the likelihood of vaccination. Table 2 reports the estimates from our first stage in Equation (2). We find that vaccine eligibility increases the likelihood of vaccination by about 24 percentage points ($p < 0.01$) in the most parsimonious specification in column (1). We also show that our point estimates barely change when we progressively control for geographic trends (regional or at the state level). In the most conservative specification in column (5), we non-parametrically control for time-varying factors across states, and find a 26.8 percentage point increase ($p < 0.01$) in the likelihood of vaccination. In Table A.2, we repeat our analysis by progressively including pre-determined attributes, and show that the first-stage estimates are remarkably robust. These altogether suggest that the identifying variation in our data is likely to be exogenous.

Table 2. Vaccine Eligibility and the Likelihood of Receiving Vaccination

	(1)	(2)	(3)	(4)	(5)
Vaccine Eligibility	0.2380***	0.2447***	0.2453***	0.2418***	0.2683***
	(0.012)	(0.014)	(0.013)	(0.012)	(0.013)
Dep. Var. Mean	0.526	0.526	0.526	0.526	0.526
N	1,464,704	1,464,704	1,464,704	1,464,704	1,464,704
State FE	✓	✓	✓	✓	✓
Survey Wave FE	✓	✓	✓	✓	✓
Region × Wave FE		✓	✓		
State time-varying measures			✓	✓	
State-specific linear trends				✓	
State × Wave FE					✓
Kleibergen-Paap rk LM statistic	20.53	20.48	20.55	20.38	20.32
Kleibergen-Paap rk Wald F statistic	427.4	325.3	339.9	420.2	436

Notes: The working sample includes observations from August 19, 2020 (wave 13) through July 5, 2021 (wave 33). All specifications control for individual characteristics, including age, gender, marital status, race, educational attainment, household income, health insurance coverage, the presence of children, and household size. State time-varying measures include two-week lagged COVID-19 death rate and stringency index, one-month lagged unemployment rate, and two-week lagged friend-exposure to vaccination information. The dependent variable mean is based on the sample period during which the vaccines became available. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Our analysis lends validity to the relevance assumption as well. We have highly significant estimates, and the Kleibergen-Paap Wald statistic, the multivariate equivalent of the F-statistic, is over 420 in most specifications (Kleibergen and Paap, 2006). To further assess the strength of our instrument, we conduct the Lagrange Multiplier (LM) test for underidentification using the Kleibergen-Paap rk statistic. This test allows us to explore whether the minimum correlation between the instrument and the endogenous variable is statistically different from zero (Bazzi and Clemens, 2013). The Kleibergen-Paap rk statistic is over 20 in all specifications, suggesting that there is enough independent variation to contribute to the full rank of the correlation matrix between vaccine eligibility and vaccination ($p < 0.01$).

Next, we report the reduced-form estimates in Table 3. We find that individuals who are eligible for the vaccine experience approximately a 6% decline in food insufficiency ($p < 0.01$). Moreover, this reduction is consistent across a broad set of specifications, including those that account for state-by-wave fixed effects.

Importantly, these reduced-form estimates rely on fewer identifying assumptions to establish a causal relationship between vaccine eligibility and food insufficiency. Given the staggered rollout of age-specific vaccine eligibility, it is essential to assess whether trends in vaccination and food insufficiency were parallel prior to eligibility. This also allows us to examine whether early vaccine uptake among high-risk occupational groups led to any significant deviations in pre-trends.

Table 3. Vaccine Eligibility and Food Insufficiency

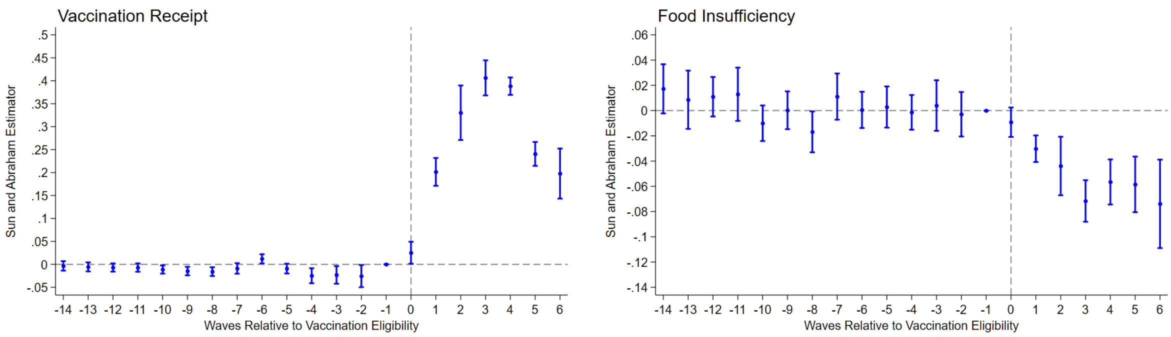
	(1)	(2)	(3)	(4)	(5)
Vaccine Eligibility	-0.0226***	-0.0230***	-0.0231***	-0.0232***	-0.0251***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Pre-Vaccination Mean	0.392	0.392	0.392	0.392	0.392
Effect as a Percent of Mean	-5.77%	-5.87%	-5.89%	-5.92%	-6.40%
N	1,464,704	1,464,704	1,464,704	1,464,704	1,464,704
State FE	√	√	√	√	√
Survey Wave FE	√	√	√	√	√
Region × Wave FE		√	√		
State time-varying measures			√	√	
State-specific linear trends				√	
State × Wave FE					√

Notes: This table reports the reduced-form effects of vaccine eligibility on food insufficiency. The working sample includes observations from August 19, 2020 (wave 13) through July 5, 2021 (wave 33). All specifications control for individual characteristics, including age, gender, marital status, race, educational attainment, household income, health insurance coverage, the presence of children, and household size. State time-varying measures include two-week lagged COVID-19 death rate and stringency index, one-month lagged unemployment rate, and two-week lagged friend-exposure to vaccination information. Pre-vaccination mean is the weighted sample mean of the outcome between waves 13 and 21. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

To assess potential pre-trends in our primary outcomes, we estimate event studies alongside our first-stage and reduced-form specifications. Importantly, we recognize recent methodological advancements in the application of difference-in-differences estimators to settings with staggered treatment adoption. Accordingly, all event-study analyses use the estimator developed by Sun and Abraham (2021).

We estimate event studies not only for food insufficiency but also for all additional outcomes introduced in later sections. Figure 3 displays event-study estimates for both the first stage and food insufficiency. In both cases, we observe relatively flat pre-trends, followed by a sharp increase in vaccine uptake and a subsequent decline in food insufficiency.

Figure 3. Dynamic Effects of Vaccine Eligibility on Vaccination and Food Insufficiency



Notes: The working sample includes observations from August 19, 2020 (wave 13) through April 26, 2021 (wave 28). The figure displays the dynamic reduced-form estimates using the Sun and Abraham (2021) estimator, along with 95% confidence intervals. The last-treated cohort is the control group. All specifications control for individual characteristics, state time-varying measures, state fixed effects, and survey wave fixed effects. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level.

5.2. Vaccination and Food Insufficiency

Table 4 reports our benchmark point estimates from the OLS and TSLS estimations of Equation (1). Using the TSLS method, our objective is to recover the average causal effects of vaccination among compliers. We find that both the OLS and TSLS methods produce estimates that are remarkably consistent across different specifications in columns (1)-(5). In addition, the TSLS estimates suggest a larger decline in food insufficiency than the OLS estimates.[30] Specifically, we show that vaccination reduces the likelihood

[30]Vaccination take-up, on average, is relatively lower among working-age adults (Diesel et al., 2021). In addition to low vaccination rates, working-age adults are also more likely to experience severe food insecurity (Gregory and Coleman-Jensen, 2017). However, vaccine eligibility shifts some of these adults into receiving the vaccine, while increasing the likelihood of transitioning them out of food insufficiency.

of food insufficiency by about 9.3 percentage points ($p < 0.01$) in column (5). This corresponds to a 24% decline in food insufficiency relative to the pre-vaccination baseline.

Table 4. Vaccination and Food Insufficiency

	(1)	(2)	(3)	(4)	(5)
			OLS		
Vaccination	-0.0482***	-0.0481***	-0.0481***	-0.0479***	-0.0479***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Pre-Vaccination Mean	0.392	0.392	0.392	0.392	0.392
Effect as a Percent of Mean	-12.30%	-12.27%	-12.27%	-12.22%	-12.22%
			TSLS		
Vaccination	-0.0951***	-0.0938***	-0.0942***	-0.0959***	-0.0934***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Pre-Vaccination Mean	0.392	0.392	0.392	0.392	0.392
Effect as a Percent of Mean	-24.26%	-23.93%	-24.03%	-24.46%	-23.83%
N	1,464,704	1,464,704	1,464,704	1,464,704	1,464,704
State FE	√	√	√	√	√
Survey Wave FE	√	√	√	√	√
Region × Wave FE		√	√		
State time-varying measures			√	√	
State-specific linear trends				√	
State × Wave FE					√

Notes: The working sample includes observations from August 19, 2020 (wave 13) through July 5, 2021 (wave 33). All specifications control for individual characteristics, including age, gender, marital status, race, educational attainment, household income, health insurance coverage, the presence of children, and household size. State time-varying measures include two-week lagged COVID-19 death rate and stringency index, one-month lagged unemployment rate, and two-week lagged friend-exposure to vaccination information. Pre-vaccination mean is the weighted sample mean of the outcome between waves 13 and 21. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

We replicate our analysis by excluding individuals who report receiving vaccination prior to age-specific vaccine eligibility. Our replication in Table A.3 yields a sizable decline in food insufficiency. Specifically, we find that vaccination reduces the likelihood of food

Therefore, it is plausible to obtain larger average treatment effects for the compliers compared to the whole population.

insufficiency by 6.7 percentage points ($p < 0.01$) in column (5), which is approximately a 17% decline relative to the baseline. These findings altogether suggest that the reduction in food insufficiency ranges between 17% and 24%.

Next, we explore the impact of vaccination on *severe* food insufficiency in Table A.4, which is a more acute form of food insufficiency. We find that vaccination reduces the likelihood of severe food insufficiency by about 6.3 percentage points ($p < 0.01$) in column (5). Given that the pre-vaccination mean is relatively low for severe food insufficiency, our estimates correspond to a larger percent reduction, about 58%, relative to this baseline. These findings altogether suggest that vaccination reduces food hardship at all levels, while substantially curbing disruptions in eating patterns and food intake.

Robustness checks and alternative specifications. – A potential threat to our identification is that economic conditions, health conditions, and state policy measures may create backdoor paths between vaccine eligibility and food insufficiency. Our state-time varying measures, described in Section 4.1, control for these factors. Moreover, trends in vaccination or economic conditions across different geographic levels may influence the behavior of vaccine-eligible adults. We account for such trends using a flexible approach, namely, we progressively control for region-by-wave fixed effects, state-specific linear trends, and state-by-wave fixed effects. In columns (2)-(5) of Table 4, we do not have any evidence that these trends are likely to explain the decline in food insufficiency upon vaccination. Taken together, the robustness of the estimates to alternative specifications is particularly reassuring that the exclusion restriction is likely to hold in our TSLS estimation strategy.

To further assess the validity of the exclusion restriction, we conducted an additional exercise that directly examines whether community-level vaccination rates influence food insufficiency independently of individual vaccination. Specifically, we augmented our model by incorporating administrative data on state-level vaccination rates from the CDC. These rates proxy for others' vaccination behavior and serve to evaluate the potential backdoor path raised by the reviewer. Our hypothesis is that, conditional on individual vaccination status, state vaccination rates should be statistically insignificant in predicting food insufficiency outcomes if the exclusion restriction holds. The results, reported in Tables A.5 and A.6, confirm this hypothesis: the coefficients on state-level vaccination rates are close to zero and statistically insignificant across both OLS and TSLS specifications. Furthermore, the inclusion of this variable leaves our estimates of individual vaccination effects virtually unchanged. These findings suggest that any influence of community vaccination is likely mediated through individual behavior, rather than operating through an independent channel. Consequently, our evidence supports the view that the exclusion restriction is not violated by unaccounted-for spillover effects from others' vaccination decisions. We also formalize this idea in a directed acyclic graph (DAG), presented in Appendix B.

To assess the robustness of our findings, we go a step further by saturating the model with all possible two-way interactions among state, age cohort, and wave – specifically, state-by-age cohort, age cohort-by-wave, and state-by-wave fixed effects.[31] As discussed previously in Section 4.1, this constitutes a more conservative approach. The combination of these fixed effects absorbs most of the systematic, low-frequency, or structural variation, leaving behind high-frequency residual variation that is harder to attribute to confounders, and thus, in some sense, more credible, though less abundant in terms of identifying variation. While this tradeoff reduces the strength of the instrument, we nonetheless report the first-stage and TSLS estimates from these alternative specifications in Tables A.7 and A.8, respectively. Similar to our benchmark results, these alternative specifications reveal a statistically significant increase in vaccine uptake and a substantial reduction in food insufficiency. For inference, we further cluster standard errors at the state-by-age-cohort level as a robustness check. Our results are robust to this alternative clustering, and in fact, column (3) of Tables A.7 and A.8 shows slightly smaller standard errors.

Theoretically, it is plausible that our benchmark estimates capture the combined effect of multiple individuals within a household getting vaccinated. We empirically test this plausible hypothesis. Since the HPS provides information on household size, we restrict our analysis to single-member households to determine whether there is a differential impact of vaccination.

In Figure 4, we present estimates from both reduced-form and IV specifications, comparing the baseline estimates with those obtained when the analysis is restricted to households of one. Importantly, we find that the estimates are not only similar in magnitude, but the baseline estimates are not consistently larger than those from the restricted sample. For instance, both the IV and reduced-form estimates indicate larger effects of vaccination and vaccine eligibility, respectively, on severe food insufficiency. Thus, we do not find evidence supporting the idea that the impact of vaccination on food sufficiency is solely driven by multiple household members becoming eligible for the COVID-19 vaccine at the same time.

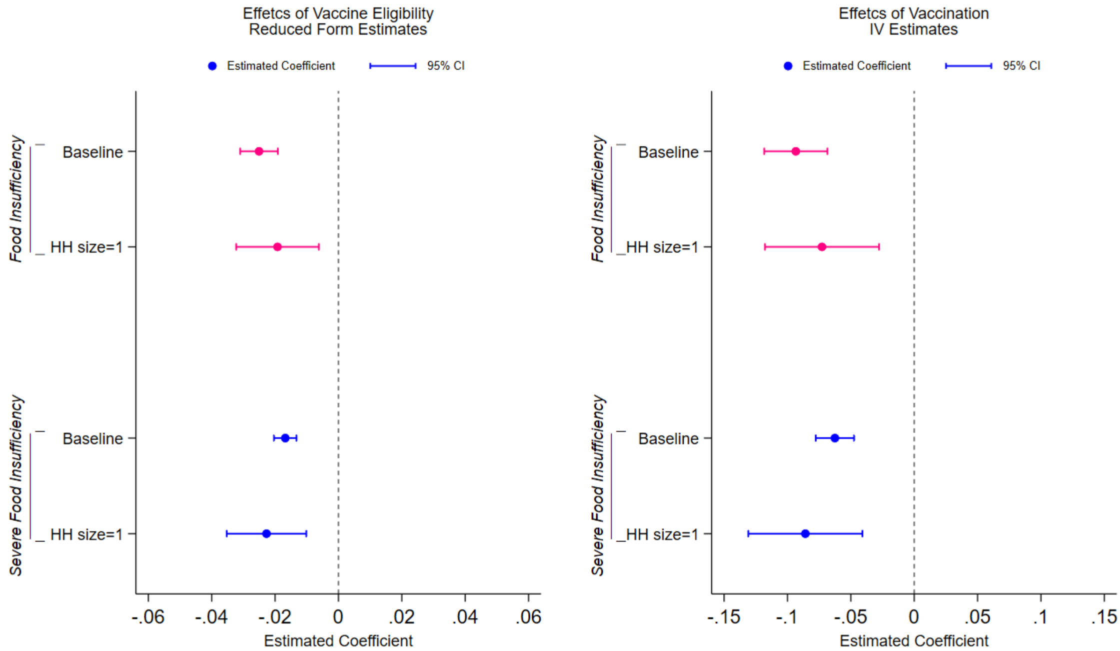
5.3. Heterogeneous Effects on Vaccination and Food Insufficiency

Next, we explore whether the effects on vaccination and food insufficiency are heterogeneous across individuals with different sociodemographic attributes. We now formally analyze our previous descriptive evidence that vaccination improves outcomes for all individuals. We begin by exploring heterogeneous effects in our first stage. The left panel of Figure 5 shows a statistically significant increase in vaccination after individuals

[31] We do not include the full three-way interaction (i.e., *state* $\times$ *age cohort* $\times$ *wave*), as that would absorb all of the identifying variation.

become eligible to receive a vaccine. We observe an increasing gradient in vaccination among non-minority groups and those with higher socioeconomic backgrounds. For instance, the effect size is larger for adults with higher educational attainment, incomes above $100,000, and those who own a house.

Figure 4. Reduced-Form and IV Estimates by Household Size

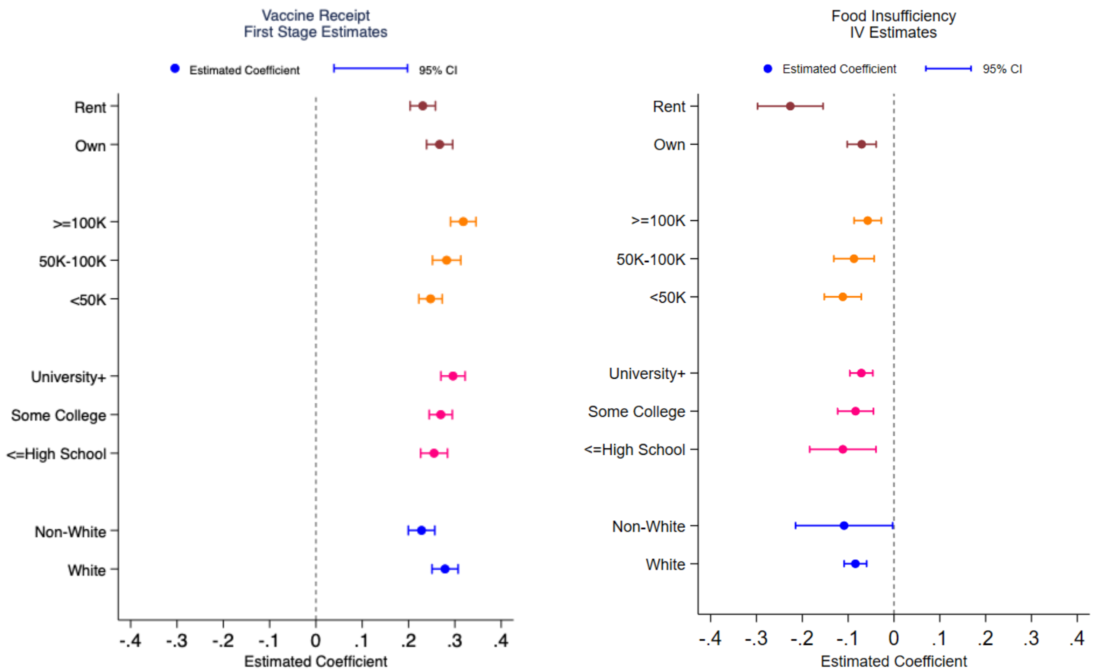


Notes: The food sufficiency outcomes are from the HPS data (waves 13 - 33). These figures show reduced-form or IV estimates for each outcome, along with 95% confidence intervals. For each outcome, two samples are considered, including the baseline sample and a subsample where household size is one. All specifications control for individual characteristics, state fixed effects, survey wave (or survey month) fixed effects, and state-by-wave (or state-by-month) fixed effects. All regressions are weighted, and standard errors are clustered at the state level.

Despite some heterogeneous effects, we find that the effect sizes vary within a relatively narrow window, namely 0.2 to 0.3. Ex-ante, monotonicity is not necessarily a concern in our setting, as individuals receive eligibility regardless of their race or socioeconomic status. Although there is no reason to believe that there could be defiers based on the eligibility rule, we nonetheless provide evidence that this is not the case.

The right panel of Figure 5 reports our TSLS estimates by observable attributes. Supporting our descriptive evidence, we find improvements in food insufficiency across individuals with different sociodemographic backgrounds. Notably, we observe significant reductions among minority groups and financially disadvantaged households relative to the baseline. For instance, food insufficiency declines by approximately 11 percentage points among minority (non-White), less educated (high school degree or below), and financially disadvantaged groups (income below \$50K), a statistically significant effect that remained consistent across these groups.

Figure 5. Heterogeneous Effects on Vaccination and Food Insufficiency



Notes: The working sample includes observations from August 19, 2020 (wave 13) through July 5, 2021 (wave 33). The figure displays separate first-stage and IV estimates for each subsample, along with 95% confidence intervals. All specifications control for remaining individual characteristics, state fixed effects, survey wave fixed effects, and state-by-wave fixed effects. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level.

In contrast, the IV estimates for more affluent and educated groups are smaller than our benchmark: there is a 5.7 percentage point decline in the highest income category (income greater than or equal to \$100K) and a 7.1 percentage point decline among the more highly educated group. The effect sizes vary between 21% and 31% relative to the pre-vaccination mean, indicating heterogeneity in the impact of vaccination on food insufficiency. Although it is clear that all groups experienced improvements, the greatest benefits were particularly evident among minorities and economically disadvantaged individuals.[32] These results highlight the potential of pharmaceutical interventions, such

[32]The heterogeneity result for the renters is consistent with the notion that individuals with considerable socioeconomic challenges are more likely to experience higher rates of food insufficiency. In our data, renters are typically younger, single, less educated, and have lower incomes compared to other demographic groups. These attributes are indicative of greater financial instability and heightened vulnerability to food insecurity, which likely accounts for the more significant impact of vaccination on

as the development and administration of vaccines, to mitigate preexisting disparities in food access during significant public health emergencies.

6. Validity of Food Insufficiency Estimates

Alternative identification strategy. – To test the validity of our benchmark estimates in Section 5, we use an alternative identification strategy. Specifically, we aim to identify the average causal effects of vaccine eligibility around the cutoff at age 65 using an RD design. To do this, we sample waves between January 6, 2021 (wave 22) and April 26, 2021 (wave 28) across 28 states that initially provided eligibility to those above 65, while those below 65 were not eligible.

One of the compelling features of an RD design is the ability to graphically depict the discontinuity around a plausibly exogenous cutoff. This is exactly what we do in Figure 6. We exploit the raw data to plot the unconditional means of vaccination and food insufficiency within each age bin. In the top panel, we use the full sample of eligible adults (going up to age 87) and impose symmetry around the cutoff. Our visual check of the RD plots reveals a clear increase in vaccine take-up and a decrease in food insufficiency. Now, we turn our focus to estimation.

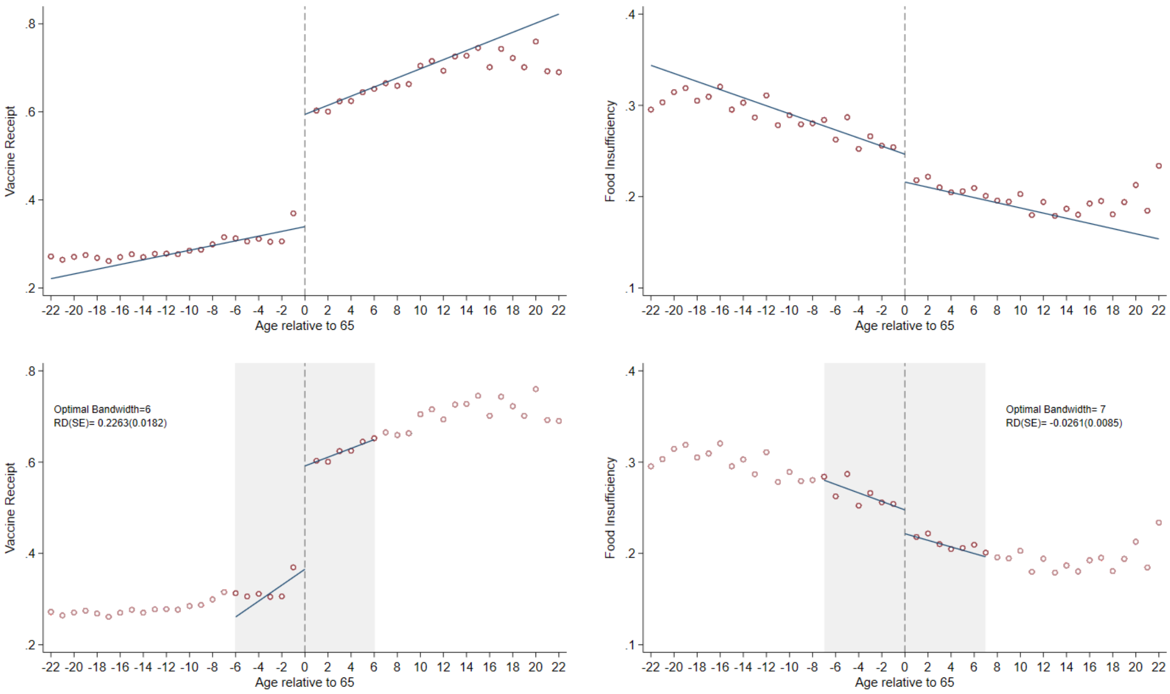
To estimate treatment effects around the cutoff, we use the regression model in Equation 3, with separate local linear polynomials on each side of the age cutoff. In the bottom panel, we report local linear RD estimates and standard errors using a mean squared error (MSE)-optimal bandwidth and triangular kernel. We follow the data-driven bandwidth selection algorithm provided by Calonico et al. (2017) to obtain MSE-optimal bandwidths using the full sample for each outcome.

The figure on the left shows the discontinuity in vaccination, while the figure on the right shows the discontinuity in food insufficiency. Consistent with our benchmark estimates, we have a relatively strong first stage, namely, we find a 23 percentage point increase ($p < 0.01$) in vaccination near the age cutoff. In addition to our first stage, we find a 2.6 percentage point decrease in the likelihood of food insufficiency among adults above the age of 65. This implies approximately a 9.4% reduction in food insufficiency relative to the pre-vaccination baseline.

Next, we ask whether the impact of vaccine eligibility on food insufficiency is heterogeneous around the age cutoff. Our first-stage estimates in Figure A.3 are remarkably consistent with the first-stage estimates in TSLS, ranging between 0.2 and 0.3. Despite the increase in vaccine take-up among all subgroups, the heterogeneity analysis for the elderly reveals interesting disparities in the impact of vaccination on food insufficiency compared to the patterns obtained for the general population of compliers

reducing food insufficiency in this subgroup.

Figure 6. Effects of Vaccine Eligibility Around the Age Cutoff



Notes: The working sample includes observations from 28 states with different time windows between January 6, 2021 (wave 22) and April 26, 2021 (wave 28). Each observation is the average of the corresponding outcome within age bins. The top panel depicts the discontinuity using a linear estimation for the full sample of eligible individuals above the cutoff. The bottom panel reports the local linear RD estimates using the optimal bandwidth (shaded in gray) and triangular kernel function for weighting. Pre-vaccination mean of food insufficiency within the optimal bandwidth is 0.279. Standard errors in parentheses are clustered at the age cohort-state level. All specifications control for gender, marital status, race, educational attainment, household income, state fixed effects, and survey wave fixed effects. Dashed vertical lines denote the eligibility cutoff, which is normalized to zero.

shown in Figure 5. While the effects were more pronounced among minority and economically disadvantaged groups in the general population, there is a more uniform pattern across different demographic and socioeconomic groups among the elderly, with estimates ranging between -0.1 and 0 as illustrated in the right panel of Figure A.3. In addition to the analysis of food insufficiency, we explore the effects of vaccine eligibility on severe food insufficiency. Note that only 4.2 percent of adults around the age cutoff experience a severe form of food insufficiency prior to vaccination. In Figure A.4, we have a precisely estimated null effect for this case. Put differently, we do not have any evidence of a significant reduction in severe food insufficiency among individuals above the age of 65.

Taken together, these findings are consistent with the idea that the benefits of vaccination are more concentrated among younger as well as socially and economically disadvantaged adults. There are multiple reasons supporting this idea in the existing scholarship. First, younger and disadvantaged adults are more likely to experience hardship in accessing food more broadly (Raifman, Bor, and Venkataramani, 2021; Bertoldo et al., 2022). Second, older adults are likely to have access to alternative forms of support such as social security, Medicare, and Meals on Wheels, which alleviate some of the financial distress and health-related challenges they face (Agarwal, Pan, and Qian, 2020; Deshpande, Gross, and Su, 2021; Singleton, 2022). In contrast, the general population relies more heavily on labor market income for food sufficiency. Therefore, since vaccination may have a direct positive impact on labor market engagement, financial optimism or the income effect channel through vaccination may be less binding for the elderly.

Threats to identification. – There are three potential threats to identification in our RD design that may violate the continuity assumption. First, respondents may misreport their age in a systematic way that is correlated with food insufficiency. Although a non-classical measurement error in age may not be common, we nonetheless test for any possible manipulations that can confound our estimates. Following McCrary (2008), we conduct a density test to explore whether there is any sorting near the cutoff. In Figure A.5, we estimate the discontinuity in the density function, measured as the log difference in height, as 0.033, with a standard error of 1.052. Therefore, we do not have any evidence to reject the null hypothesis that the discontinuity in the density of age is zero.

Second, individuals near the cutoff might not be comparable, implying that discontinuities may be driven by other (unobservable) attributes, even in the absence of vaccine eligibility. In Table A.9, we show that pre-determined characteristics are smooth functions of age. Specifically, we do not find statistically significant evidence of a discontinuity in these characteristics around the age cutoff, except for two out of 14 characteristics, which are significant at the 10 percent level.[33] Our findings suggest that the age criteria used in our analysis are exogenous, as we observe balance in observable attributes around the cutoff. This, in turn, alleviates potential concerns about omitted variables, providing greater confidence in our conclusions.

We further explore the sensitivity of our benchmark RD estimates to alternative bandwidths. Figure A.6 reports the first-stage and reduced-form estimates using various bandwidths. To be more flexible and transparent, we incrementally change the bandwidth by 1 year and display the pattern in our estimates. The hollow diamond markers report the benchmark RD estimates using the MSE-optimal bandwidth. We find that our benchmark estimates are exceptionally robust to alternative bandwidths. Specifically,

[33] The race category, *Black*, is only marginally significant at the 10 percent level, with an estimate of 0.0096 and a standard error of 0.006, yielding a t-statistic of approximately 1.6. In contrast, higher education (having a university degree or above) is also positively correlated with vaccine eligibility and is significant at the 10 percent level. Although we control for these characteristics in our model, our estimates remain robust to the exclusion of race and education variables: the first-stage coefficient changes 22.6 pp to 22.8 pp, and the reduced-form estimate shifts from -2.6 pp to -2.9 pp.

the tradeoff between bias and variance seems negligible in our setting.

However, as an extension to the second point, any other policy changes around the cutoff that correlate with potential outcomes may also violate the continuity assumption. This is an important threat to identification, as Medicare eligibility and the likelihood of retirement change at the age of 65, both of which may impact food insufficiency, particularly through the income effect channel. To further assess the validity of the continuity assumption, we conduct two separate placebo analyses. In the first exercise, we restrict our sample to the earlier waves of the pandemic to make sure that all adults are ineligible for vaccines around the age cutoff. The intuition is that, if policies other than vaccine eligibility are effective, we should observe a statistically significant discontinuity in food insufficiency with respect to age. Using an ineligible placebo sample, panel (a) in Figure A.7 shows that food insufficiency is continuous around the age cutoff.

In the second exercise, we do the opposite by restricting our sample to later waves of the pandemic to ensure that all adults around the age cutoff are eligible for vaccines. This exercise allows us to explore the validity of the continuity assumption for the first stage as well. Using an eligible placebo sample, panel (b) in Figure A.7 shows that both vaccine take-up and food insufficiency are continuously related to the running variable at the 5 percent significance level. An additional benefit of conducting these exercises separately for two different time periods is that one can also explore whether there are substantial time-varying changes around the age cutoff. This does not appear to be the case in our setting.

As a final exercise, we combine the placebo samples with our primary sample in a difference-in-discontinuities framework. The idea is to difference out any potential time-invariant changes near the age cutoff. Panel A in Table A.10 reports the estimate for food insufficiency using the ineligible placebo sample, including adults from waves 13-21, as a reference group. This analysis shows that vaccine eligibility reduces food insufficiency by about 11% once we difference out potential changes across waves. Note that this estimate is close in magnitude to the benchmark RD estimate of 9.4%, suggesting that the reduction in food insufficiency is not likely to be driven by changes in other policies.

Panel B in Table A.10 replicates our difference-in-discontinuities analysis using the eligible placebo sample, including adults from waves 30-33, as a reference group. First, we find a 21.6 percentage point increase in the likelihood of vaccination, which is qualitatively the same as the first-stage estimate in Figure 6. In addition, we find an 11.86% reduction in food insufficiency among vaccine-eligible adults after netting out potential changes across waves. Taken together, our alternative identification strategies further confirm that vaccination plays a crucial role in transitioning households out of food insufficiency during the pandemic.

Robustness to multiple age cutoffs. − To align our identification strategy in the RD with that of TSLS, we stack multiple age cutoffs (normalized around zero) to explore the effect of vaccine eligibility on vaccination rates and food insufficiency. The key challenge here is to consistently establish bandwidths for each cutoff, ensuring that no other vaccine eligibility policy changes occurred around these cutoffs. It is important to note that states rolled out vaccine eligibility at different paces, with varying age cutoffs, and some implemented rapid changes, such as adjusting the eligibility cutoff within the same wave. In such cases, an RD design is not feasible, as it becomes complicated to clearly distinguish between treatment and control groups within those specific states and waves. Note that our RD design identifies the LATE for individuals near the age cutoff, rather than for compliers across the entire age distribution. Despite these challenges, we successfully stacked multiple cutoffs at ages 75, 70, 65, and 60, using a sample of individuals across 41 states.

In Figure A.8, we report the RD estimates from both the first-stage and reduced-form specifications. We find approximately a 24.5 percentage point increase ($p < 0.01$) in vaccine uptake around the age cutoff. Notably, this increase was around 23 percentage points when using only the 65 age cutoff. Additionally, we find a 2.7 percentage point decline ($p < 0.01$) in food insufficiency as individuals become eligible for the vaccine. The implied LATE estimate suggests an approximate 11 percentage point decline in food insufficiency. It is important to note that the TSLS yielded a LATE estimate ranging between 9.3 and 9.6 percentage points. This analysis suggests that as we include more cutoffs, the RD estimates and the implied LATE become more similar to our benchmark TSLS estimates.[34]

In Figure A.9, we further demonstrate that our estimates from the multi-cutoff RD approach are robust to bandwidth selection. Specifically, we vary the bandwidths in both the first-stage and reduced-form specifications, yet obtain remarkably similar estimates.

7. Vaccine Eligibility and Reliance on Social Safety Net Programs

Next, we explore whether there are any positive spillovers of vaccine eligibility on food assistance. Specifically, if vaccination helps individuals transition out of food insufficiency by improving their economic conditions, one might expect less reliance on public assistance programs, particularly on food stamps. This would also imply that the net cost of providing vaccines to the government is much lower due to fiscal externalities. The higher the fiscal externalities, the higher the marginal value of public funds devoted toward subsidizing vaccines (see, e.g., Hendren and Sprung-Keyser, 2020). Although our

[34]However, it is important to note that we are still using a triangular kernel around an optimal bandwidth, not the entire age distribution across states.

focus is not directly on measuring the magnitude of fiscal externalities, we explore the impact of vaccine eligibility on the likelihood of receiving food stamp benefits, a key input in the calculation of net costs.

Table 5. Vaccine Eligibility and the Likelihood of Receiving Food Stamp Benefits

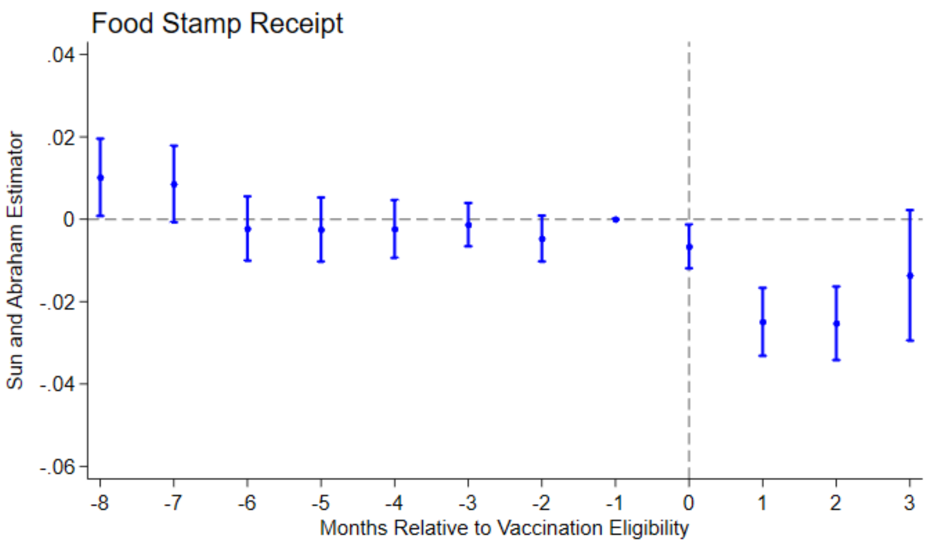
	(1)	(2)	(3)	(4)	(5)
Vaccine Eligibility	-0.0102***	-0.0098***	-0.0098***	-0.0097***	-0.0113***
	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
Pre-Vaccination Mean	0.065	0.065	0.065	0.065	0.065
Effect as a Percent of Mean	-15.69%	-15.08%	-15.08%	-14.92%	-17.38%
N	620,027	620,027	620,027	620,027	620,027
State FE	√	√	√	√	√
Survey Month FE	√	√	√	√	√
Region × Month FE		√	√		
State time-varying measures			√	√	
State-specific linear trends				√	
State × Month FE					√

Notes: The working sample comes from the CPS-FSS and uses observations from August 2020 through June 2021. Each column reports the reduced-form effects of vaccine eligibility on food stamp take-up across different specifications. All specifications control for individual characteristics, including age, gender, marital status, education attainment, race, household income, the number of children, and household size. State time-varying measures include one-month lagged COVID-19 death rate and stringency index, as well as the unemployment rate. Pre-vaccination mean is the weighted sample mean of the outcome between August 2020 and December 2020. All regressions are weighted using the supplement person weight, and standard errors are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 5 reports the estimates pertaining to the reduced-form effects of vaccine eligibility on food stamp receipt. We find a consistent reduction in food stamp receipt across the board. Specifically, in column (5), we find that vaccine eligibility reduces the likelihood of receiving food stamp benefits by 1.13 percentage points ($p < 0.01$). This corresponds to an approximately 17% reduction in food stamp receipt relative to the pre-vaccination baseline. Given that food assistance programs are means-tested, the effects of vaccine eligibility on food stamp receipt are likely to be salient among low-income individuals.

In Table A.11, we explore the impact of vaccine eligibility on the likelihood of receiving food stamp benefits by family income and educational attainment. A potential caveat of conducting this exercise using family income in the CPS is that income distribution may change endogenously as the pandemic progresses. This may be an issue with educational attainment as well (see, e.g., Meyers and Thomasson, 2017). Nonetheless, we check whether there are shifts in the income distribution between 2019 and 2021 in Figure A.10. Despite slight changes, we do not observe any substantial sorting in the lower and middle quartiles, particularly between 2020 and 2021. In short, our analysis in Table A.11 suggests that the effects of vaccine eligibility on food assistance are concentrated among relatively low-income families, those earning less than $50,000. We find approximately a 13% reduction ($p < 0.05$) in food stamp receipt among low-income families. When we repeat our analysis using educational attainment, we observe reductions in food stamp receipt across the board. However, the estimates become closer to zero as the level of educational attainment increases.

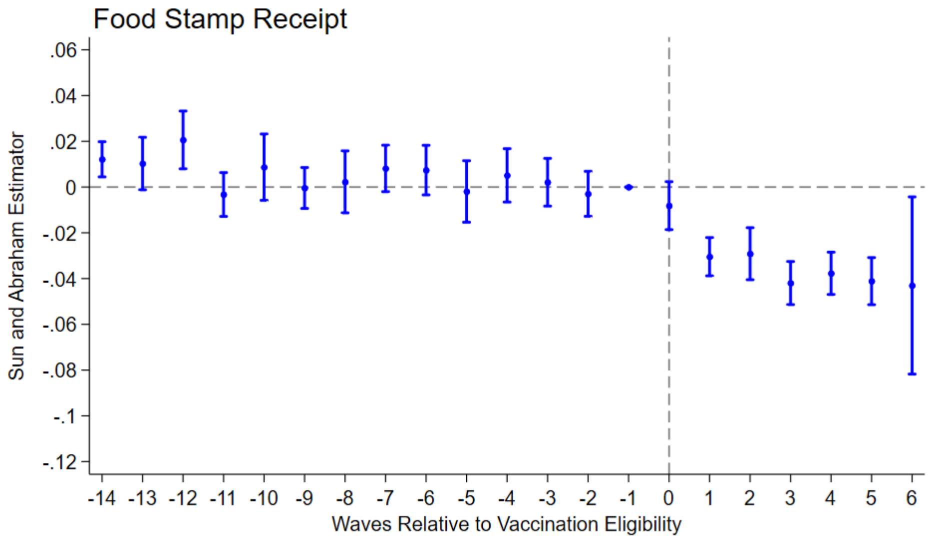
Figure 7. Dynamic Effects on Food Stamp Receipt, CPS-FSS



Notes: The working sample comes from the CPS-FSS and uses observations from August 2020 through April 2021. The figure displays the dynamic reduced-form estimates using the Sun and Abraham (2021) estimator, along with 95% confidence intervals. The last-treated cohort is the control group. All specifications control for individual characteristics, state time-varying measures, state fixed effects, and survey month fixed effects. All regressions are weighted, and standard errors are clustered at the state level.

Note again that we use the CPS-FSS to investigate changes in SNAP receipt following vaccine eligibility. Accordingly, we use the same sample to conduct event studies for SNAP receipt. It is important to note that we have a shorter event window in the post-vaccination period due to the limited sample period available in the CPS during the time of our study. Additionally, we use the cohort eligible in the last period as the control units (i.e., not-yet-treated units). We report our estimates in Figure 7. These estimates support our findings: there is no distinguishable effect on food stamp receipt prior to vaccine eligibility, but there is a significant decline in food stamp receipt immediately following vaccine eligibility.

Figure 8. Dynamic Effects on Food Stamp Receipt, HPS



Notes: The working sample includes observations from August 19, 2020 (wave 13) through April 26, 2021 (wave 28). The figure displays the dynamic reduced-form estimates using the Sun and Abraham (2021) estimator, along with 95% confidence intervals. The last-treated cohort is the control group. All specifications control for remaining individual characteristics, state time-varying measures, state fixed effects, and survey wave fixed effects. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level.

To further strengthen our findings, we exploit the SNAP benefits questionnaire in the HPS to extend our time window and apply the same sample of individuals in both the vaccination and food insufficiency analyses. The event study estimates in Figure 8 further demonstrate a dynamic decline in food stamp receipt following vaccine eligibility. Importantly, the pre-trends are relatively flat, lending support to the validity of the parallel trends assumption.

The HPS also includes a question about whether individuals have applied for UI benefits. While it does not capture the amount of benefits received, it does reflect patterns in the uptake of UI benefits. We explore how vaccine eligibility and vaccination impact UI applications. Table 6 presents the IV estimates, showing a significant decline of approximately 9.6 percentage points ($p < 0.01$, column (5)) in UI applications among individuals who received the COVID-19 vaccine. In Figure A.11, we also provide event-study estimates from our reduced-form specification. As shown in the figure, we observe a decrease in UI applications following vaccine eligibility. These findings align with the mechanisms discussed in the following section.

Table 6. Vaccination and Unemployment Insurance Applications

	(1)	(2)	(3)	(4)	(5)
Vaccination	-0.0936***	-0.0934***	-0.0933***	-0.0934***	-0.0959***
	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)
Pre-Vaccination Mean	0.203	0.203	0.203	0.203	0.203
Effect as a Percent of Mean	-46.11%	-46.01%	-45.96%	-46.01%	-47.24%
N	1,093,625	1,093,625	1,093,625	1,093,625	1,093,625
State FE	√	√	√	√	√
Survey Wave FE	√	√	√	√	√
Region × Wave FE		√	√		
State time-varying measures			√	√	
State-specific linear trends				√	
State × Wave FE					√

Notes: The working sample includes observations from August 19, 2020 (wave 13) through March 29, 2021 (wave 27). The sample period ends at wave 27 due to the modification to the HPS survey questions. All specifications control for individual characteristics, including age, gender, marital status, race, educational attainment, household income, health insurance coverage, the presence of children, and household size. State time-varying measures include two-week lagged COVID-19 death rate and stringency index, one-month lagged unemployment rate, and two-week lagged friend-exposure to vaccination information. Pre-vaccination mean is the weighted sample mean of the outcome between waves 13 and 21. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

8. Mechanisms

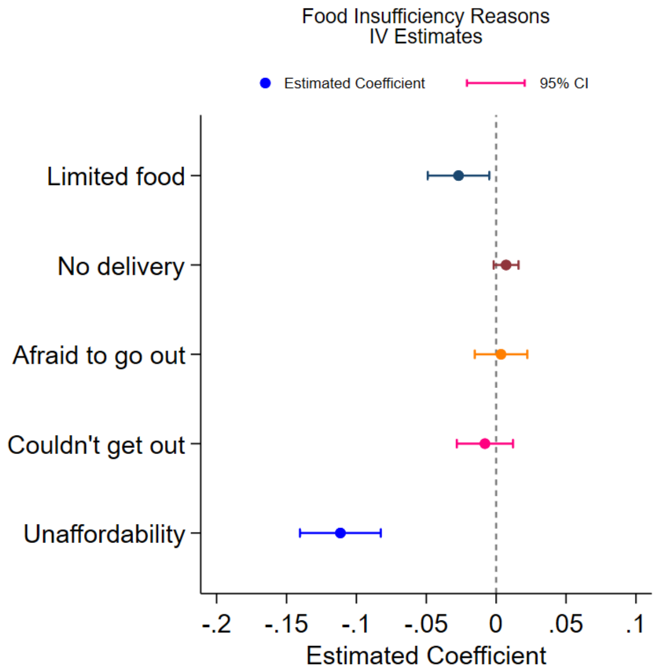
8.1. Material and Non-Material Circumstances

Exploring the mechanisms behind the impact of vaccination on food insufficiency is crucial for understanding the broader effects of COVID-19 vaccination on households' well-being during the pandemic. By disentangling the specific reasons for food insufficiency, we can identify how both demand- and supply-side factors evolved in response to widespread vaccine distribution. Specifically, examining affordability, mobility restrictions, and food availability provides a more nuanced view of how economic and logistical challenges shifted as public health conditions improved.

In Figure 9, we present our IV estimates, using each reason for food insufficiency as a separate outcome in our most comprehensive specification. We observe two notable findings. First, there is a significant reduction in the likelihood of individuals reporting that they could not afford to buy more food, indicating that affordability concerns decreased following vaccination. Second, we find a decline in reports of limited food availability in stores after vaccination. Together, these findings suggest a broader economic recovery post-vaccination, which not only improved labor market outcomes but also alleviated some supply-side bottlenecks.

Additionally, we do not find much evidence that individuals were more likely to increase their mobility simply to buy food after vaccination. For example, there was no significant change in reports of lacking transportation, having mobility or health problems that prevented individuals from going out, or being afraid to go out to buy food. Although marginally insignificant, individuals were less likely to report that groceries or meals were delivered to them post-vaccination. Earlier studies report increased online shopping during the peak of the pandemic and lockdowns (Wang et al., 2021; Young, Soza-Parra, and Circella, 2022), which likely returns to the mean during the recovery period.

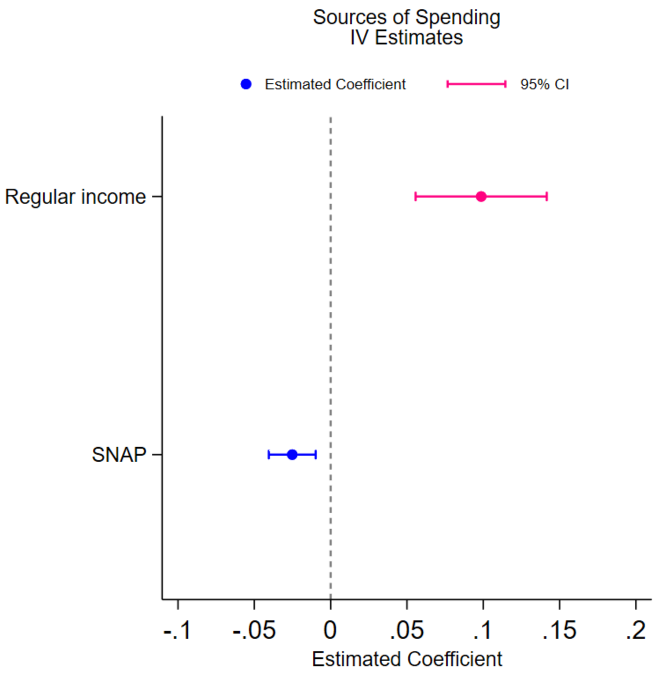
Figure 9. Vaccination and the Reasons for Food Insufficiency



Notes: The working sample includes observations from August 19, 2020 (wave 13) through July 5, 2021 (wave 33) for unaffordability, could not get out, and afraid to go out, and from August 19, 2020 (wave 13) through March 29, 2021 (wave 27) for no delivery and limited food. We restrict the sample period for the latter two outcomes due to the change in survey questions on food insufficiency reasons since wave 28. The figure displays IV estimates for each outcome, along with 95% confidence intervals. All specifications control for individual characteristics, state fixed effects, survey wave fixed effects, and state-by-wave fixed effects. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level.

We further extend our reduced-form specifications by conducting event-study analyses for the outcomes that revealed significant changes in our current analysis. We report our findings in Figure A.12. These event studies reveal relatively flat pre-trends prior to the rollout and confirm our findings that vaccine eligibility improves both the affordability and availability of food.

Figure 10. Vaccination and Sources of Spending



Notes: The working sample includes observations from August 19, 2020 (wave 13) through July 5, 2021 (wave 33). The figure displays IV estimates for each outcome, along with 95% confidence intervals. All specifications control for remaining individual characteristics, state fixed effects, survey wave fixed effects, and state-by-wave fixed effects. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level.

To provide further empirical evidence on mechanisms related to material circumstances, we examine the sources households rely on to meet their spending needs. One key source is regular income, such as the income received prior to the pandemic. In this analysis, we investigate changes in the use of regular income and SNAP benefits to cover household expenses. Our IV estimates, shown in Figure 10, reveal striking findings. Specifically, we observe a decline in the use of SNAP benefits for household spending after individuals became eligible for the vaccine. Moreover, we find an increase in the use of regular income sources following vaccine eligibility.[35] These results not only support our

[35]Next, we explore the dynamic patterns in these potential mechanisms using an event-study analysis, as shown in Figure A.13. Specifically, we investigate the impact of vaccine eligibility on the use of regular income sources versus SNAP benefits to meet household spending needs. The analysis reveals relatively stable pre-trends before individuals become eligible for the vaccine, followed by a clear and

earlier findings on reduced reliance on public assistance after vaccine eligibility, using a different dataset, but also offer new insights into shifting material circumstances during the post-vaccination phase.

8.2. Expectations

A crucial piece to our understanding of how vaccination affects the likelihood of food insufficiency rests on identifying the changes in expectations about future financial and economic conditions. One might expect vaccination to improve future financial and economic conditions through its impact on aggregate activity. For instance, uncertainty shocks, such as those created by pandemics, have contractionary effects on aggregate activity. However, Cakmakli et al. (2021) illustrate that vaccination plays an important role in smoothing out the negative economic impact of the pandemic. In fact, Lhuissier and Tripier (2021) show that optimistic expectations about future financial and economic conditions dampen these negative effects. Additionally, Christelis et al. (2020) find a positive relationship between expected consumption risk and expected consumption growth, suggesting a precautionary motive in savings among households under uncertainty. Given these findings, it is natural to expect improvements in financial expectations and confidence to smooth out cyclical fluctuations in food insufficiency as the strength of precautionary saving declines. Indeed, Ren and Zheng (2023) demonstrate that COVID-19 vaccination is negatively associated with household savings, as proxied by bank deposit flows at the county level. The authors attribute this to an economic recovery channel, where vaccination helps alleviate uncertainties about job losses and salaries, thus boosting confidence among people.

In this section, we complement the existing scholarship by investigating whether individuals who receive vaccination form optimistic expectations about their future financial conditions. In Table 7, we study the relationship between vaccination and expectations about future loss of employment income. We find that vaccination reduces expectations about future loss of employment income by about 10 percentage points ($p < 0.01$) in column (5), a 42% decline relative to the pre-vaccination baseline. Additionally, the event-study estimates in Figure A.14 show relatively flat pre-trends, followed by a significant decline in expectations about future loss of employment income upon vaccine eligibility.

In the top panel of Figure A.15, we also explore whether the changes in expectations are heterogeneous across individuals with different social and economic attributes. We uncover two key findings: (i) vaccination improves financial expectations across all individuals and (ii) financial expectations are more salient among minority groups and

sharp increase in the use of regular income sources and a corresponding decline in reliance on SNAP benefits for household spending.

economically disadvantaged populations in general.

Table 7. Vaccination and Expectations about Future Loss of Employment Income

	(1)	(2)	(3)	(4)	(5)
			OLS		
Vaccination	-0.0295***	-0.0290***	-0.0291***	-0.0287***	-0.0286***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Pre-Vaccination Mean	0.240	0.240	0.240	0.240	0.240
Effect as a Percent of Mean	-12.29%	-12.08%	-12.13%	-11.96%	-11.92%
			TSLS		
Vaccination	-0.0944***	-0.0976***	-0.0974***	-0.0974***	-0.1008***
	(0.013)	(0.014)	(0.014)	(0.012)	(0.013)
Pre-Vaccination Mean	0.240	0.240	0.240	0.240	0.240
Effect as a Percent of Mean	-39.33%	-40.67%	-40.58%	-40.58%	-42.00%
N	1,461,368	1,461,368	1,461,368	1,461,368	1,461,368
State FE	√	√	√	√	√
Survey Wave FE	√	√	√	√	√
Region × Wave FE		√	√		
State time-varying measures			√	√	
State-specific linear trends				√	
State × Wave FE					√

Notes: The working sample includes observations from August 19, 2020 (wave 13) through July 5, 2021 (wave 33). All specifications control for individual characteristics, including age, gender, marital status, race, educational attainment, household income, health insurance coverage, the presence of children, and household size. State time-varying measures include two-week lagged COVID-19 death rate and stringency index, one-month lagged unemployment rate, and two-week lagged friend-exposure to vaccination information. Pre-vaccination mean is the weighted sample mean of the outcome between waves 13 and 21. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

We show in our main analysis that vaccination reduces the likelihood of food insufficiency by 24%. We then combine this parameter with our estimate of the impact of vaccination on financial optimism to obtain the implied elasticity of food sufficiency with respect to financial optimism. Specifically, we estimate that a one percent decrease in future financial optimism as measured by loss of employment income is associated with a 0.57% increase in food insufficiency.

Table 8. Vaccination and Confidence in Paying Future Mortgage or Rent Payments On Time

	High Confidence		Moderate Confidence		Any Confidence	
	(1)	(2)	(3)	(4)	(5)	(6)
			OLS			
Vaccination	0.0391***	0.0392***	0.0319***	0.0318***	0.0188***	0.0187***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
Pre-Vaccination Mean	0.607	0.607	0.806	0.806	0.914	0.914
Effect as a Percent of Mean	6.44%	6.46%	3.96%	3.95%	2.06%	2.05%
			TSLS			
Vaccination	0.1672***	0.1693***	0.1506***	0.1442***	0.0624***	0.0604***
	(0.021)	(0.019)	(0.018)	(0.016)	(0.011)	(0.011)
Pre-Vaccination Mean	0.607	0.607	0.806	0.806	0.914	0.914
Effect as a Percent of Mean	27.55%	27.89%	18.68%	17.89%	6.83%	6.61%
N	945,989	945,989	945,989	945,989	945,989	945,989
State FE	√	√	√	√	√	√
Survey Wave FE	√	√	√	√	√	√
State × Wave FE		√		√		√

Notes: The working sample includes observations from August 19, 2020 (wave 13) through July 5, 2021 (wave 33). All specifications control for individual characteristics, including age, gender, marital status, race, educational attainment, household income, health insurance coverage, the presence of children, and household size. State time-varying measures include two-week lagged COVID-19 death rate and stringency index, one-month lagged unemployment rate, and two-week lagged friend-exposure to vaccination information. Pre-vaccination mean is the weighted sample mean of the outcome between waves 13 and 21. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Next, we study whether vaccination affects the level of confidence in meeting future financial obligations, such as mortgage or rent payments, in Table 8. We find that vaccination leads to an overall increase in financial confidence. However, there are also heterogeneous effects across confidence levels, namely, individuals are more likely to have high financial confidence relative to moderate or any financial confidence. Specifically, we find approximately a 28% increase in the likelihood of having high confidence in paying future mortgage or rent payments on time ($p < 0.01$). The implied elasticity of food sufficiency with respect to financial optimism measured by confidence in paying mortgage and making rent payments is 0.86. This implies that a one percent increase in financial optimism as measured by high confidence in paying mortgage and making rent payments is associated with a 0.86% reduction in food insufficiency. Our event-study analysis in Figure A.16 further shows dynamic increases in financial confidence following vaccine eligibility, specifically measured by improvements in confidence regarding the ability to pay future mortgage or rent payments on time. While increases are observed across all confidence levels - high, moderate, and any confidence - the effect is particularly salient for high confidence.

Echoing our previous findings, the bottom panel in Figure A.15 shows that the increase in high financial confidence is likely to be driven by non-White individuals and those with lower socioeconomic status. Taken together, these findings suggest that vaccination promotes financial optimism, especially among impoverished and minority communities, which may help explain the significant reduction in food insufficiency within these groups.

9. Conclusion

The COVID-19 pandemic caused a surge in food insufficiency, which disproportionately affected vulnerable populations such as Black and Latino households and households with children. This was due to rising unemployment, income loss, and food prices, as well as the closure of many food retailers. Despite the rollout of several economic relief packages by the U.S. government, food insufficiency persisted throughout the first year of the pandemic. However, starting in early 2021, there was a decline in food insufficiency rates.

To examine the relationship between vaccination and food insufficiency among Americans, we use data from the Household Pulse Survey. To address the endogeneity issue of vaccination decisions, we use the variation in vaccine eligibility across states over time as an instrumental variable.

Our findings indicate that vaccination efforts alleviate the effects of the pandemic on food insufficiency. Specifically, vaccine eligibility increases the likelihood of vaccination by 26.8 percentage points, which led to a 9.3 percentage point decline in food insufficiency. While our results demonstrate improvements in food insufficiency across all individuals with various demographic and socioeconomic backgrounds, the effects are stronger among minority groups and financially disadvantaged households. These results highlight the importance of targeted interventions for vulnerable populations, particularly those with lower socioeconomic status or living in regions with inadequate social support, who are at a higher risk of experiencing food insufficiency during health crises. This information can guide policymakers in developing more effective and equitable public health policies and interventions in the future.

Moreover, vaccination played a crucial role in boosting financial optimism among the American population. Specifically, the perception of future loss of employment income and confidence in meeting future mortgage and rent payments both improve significantly with vaccination. This finding provides strong support for the notion that financial optimism represents a crucial pathway for explaining the causal relationship between vaccination and food insufficiency. Our findings also suggest that vaccination has positive spillover effects on food assistance programs by reducing SNAP receipts. As a result, the overall cost of providing vaccines to the government may be lower due to fiscal externalities.

Despite previous scholarship on the benefits of vaccination, the potential impact of vaccination programs on far-reaching consequences that extend beyond health outcomes, such as food insufficiency, has not been previously studied. Accordingly, our results suggest that the current valuation of vaccines may be underestimated due to the failure to account for the full spectrum of its benefits. By investing in the development and deployment of effective and timely health interventions, governments can generate economic optimism and stability, leading to improved food sufficiency and overall well-being for their populations. Additionally, a comprehensive approach that considers the broader spillover effects of health interventions beyond direct disease prevention can help address a wide range of social and economic challenges that arise during public health crises, thereby reducing the negative impact on vulnerable populations.

As the COVID-19 pandemic has transitioned into an endemic, the findings of our study remain relevant beyond the pandemic context. The mechanisms identified, particularly the impact of health interventions on economic optimism and subsequent improvements in food sufficiency, extend beyond crisis situations. Medical innovations, including vaccine development, can generate economic optimism (Makris and Toxvaerd, 2020), which may help reduce food hardship during public health emergencies, provided that they are developed efficiently under a regulatory framework ensuring their safe and timely availability to the public. Prior research shows that vaccines not only prevent disease but also promote health equity and stabilize health systems over the life course (Wilder-Smith et al., 2017). Similarly, recent studies document improvements in mental health and future outlook following vaccine rollouts (Agrawal et al., 2021; Aslim et al., 2024). Yet, the influence of vaccination on socioeconomic outcomes such as food insufficiency has received little attention.[36]

More broadly, medical innovations like vaccines can shape labor markets, household spending, and public confidence, even outside of crises. For example, flu vaccination campaigns may reduce absenteeism, stabilize earnings, and ease demands on safety net programs. The financial optimism mechanism we document offers a generalizable framework for understanding how public health measures influence household behavior. By improving expectations about future stability, such interventions may encourage households to allocate more resources toward essentials like food, reducing reliance on programs like SNAP. In turn, this can lessen the long-term fiscal burden of public assistance. These insights can inform policies aimed at strengthening public health infrastructure, alleviating food insufficiency, and enhancing the effectiveness of social

[36]One exception is Bärnighausen et al. (2013), which examines vaccine impacts on tourism and foreign investment in Brazil's dengue context.

safety nets.

Despite the valuable insights our study provides into the relationship between COVID-19 vaccination and food insufficiency, some limitations must be acknowledged. First, the use of self-reported vaccination status and food sufficiency measures may introduce reporting biases that could affect the accuracy of our findings. In particular, shifts in perceived well-being, confidence, and security associated with vaccine eligibility may have influenced how respondents reported their food sufficiency, potentially reflecting changes in outlook in addition to material conditions. Additionally, while we performed various analyses to mitigate threats to our identification strategy and assess robustness, the quasi-experimental nature of our research design cannot fully guarantee the elimination of all confounding factors. Finally, our focus is primarily on short-term effects, and further research is needed to explore the long-term impacts of vaccination on food sufficiency outcomes.

Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work the author(s) used ChatGPT in order to improve the readability and language of the manuscript. After using this tool/service, the author(s) reviewed and edited the content as needed and take full responsibility for the content of the publication.

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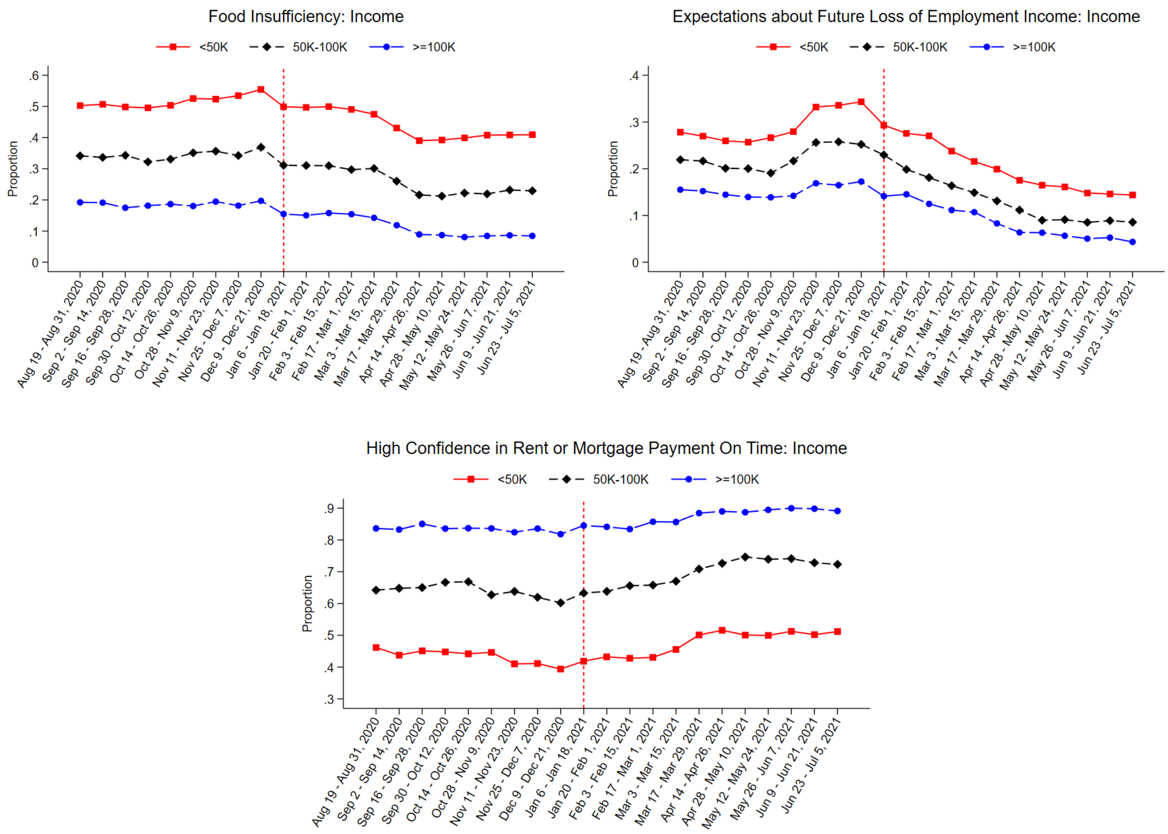
Appendix: For Online Publication

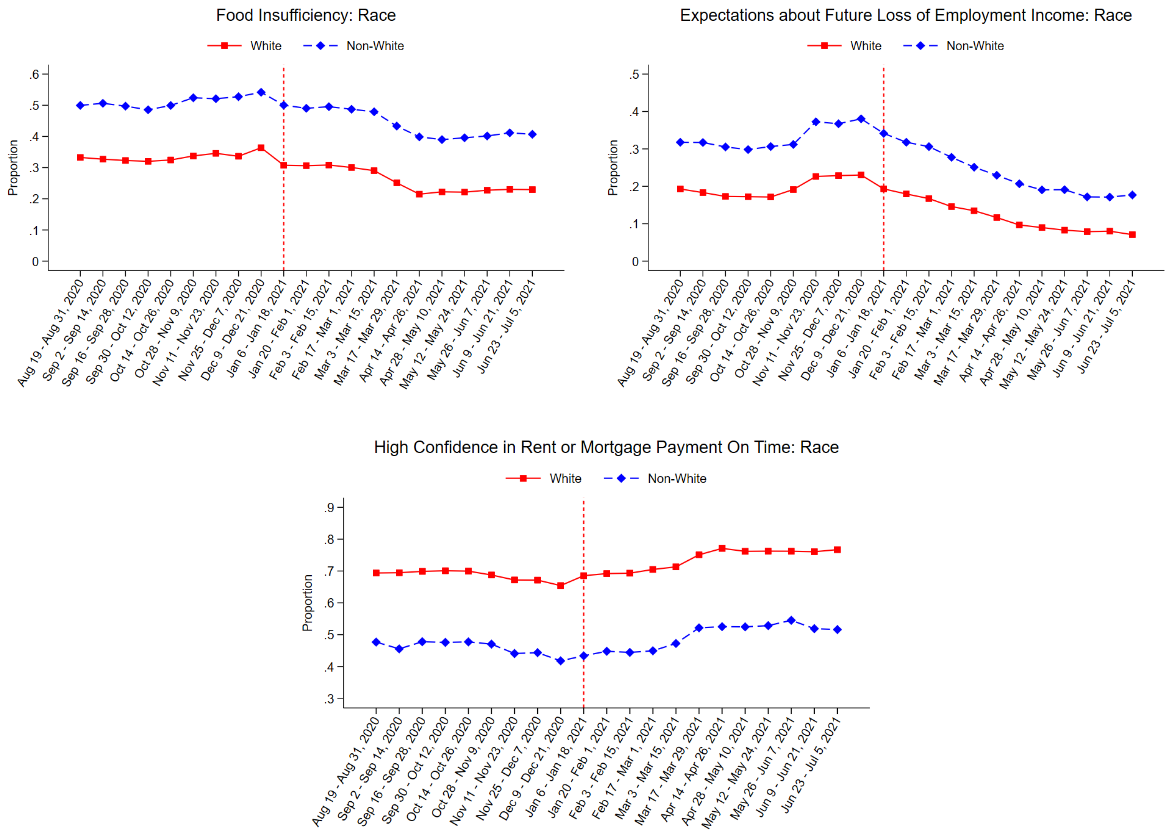
A. Additional Figures and Tables

Figure A.1. Descriptive Evidence by Income



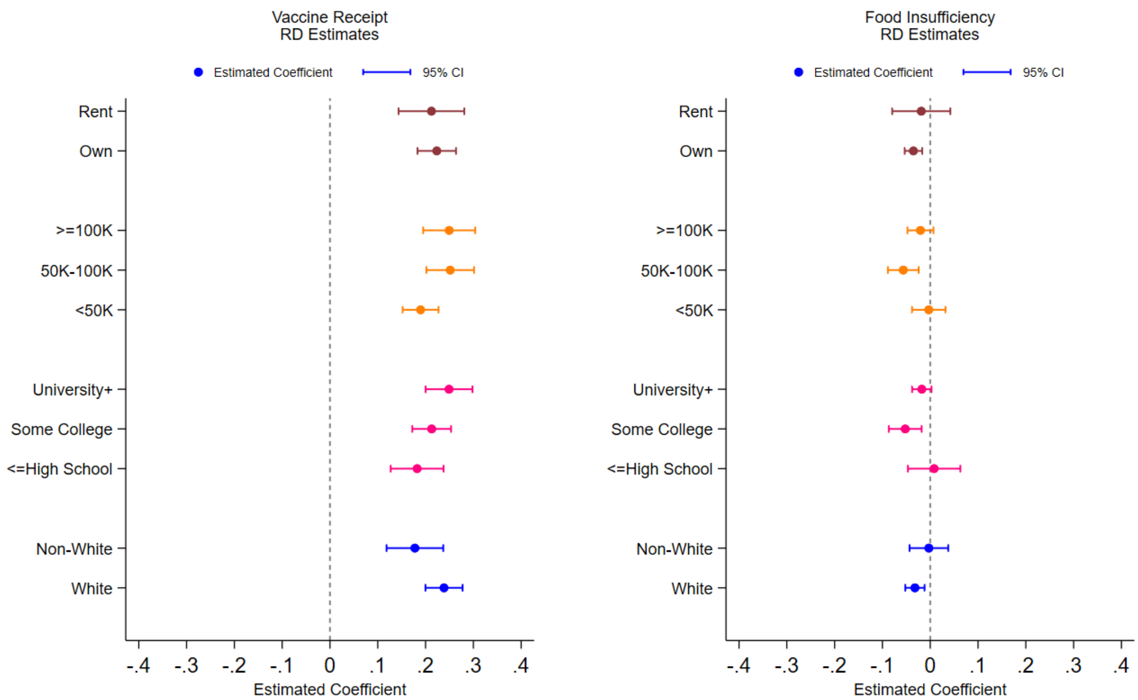
Notes: Each observation represents the weighted average of the corresponding outcome by income within each wave from August 19, 2020 (wave 13) through July 5, 2021 (wave 33). The red vertical line indicates the introduction of the COVID-19 vaccine in the United States.
Data Source: Household Pulse Survey, U.S. Census Bureau.

Figure A.2. Descriptive Evidence by Race



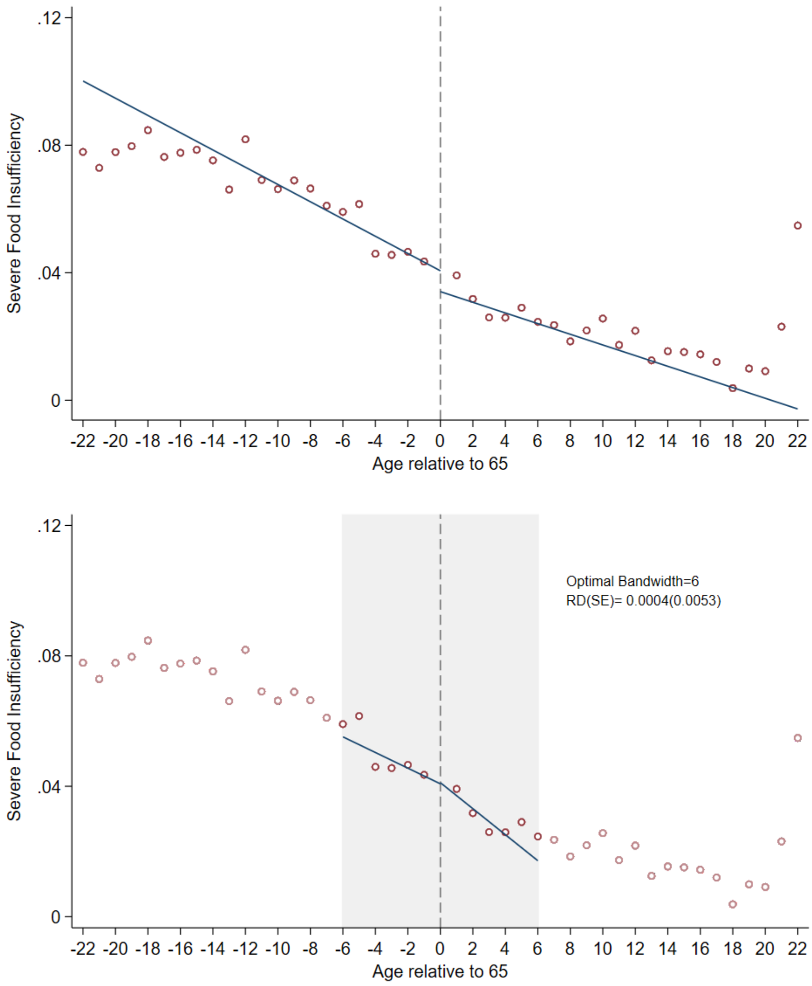
Notes: Each observation represents the weighted average of the corresponding outcome by race within each wave from August 19, 2020 (wave 13) through July 5, 2021 (wave 33). The red vertical line indicates the introduction of the COVID-19 vaccine in the United States.
Data Source: Household Pulse Survey, U.S. Census Bureau.

Figure A.3. Exploring Heterogeneity in the RD Design



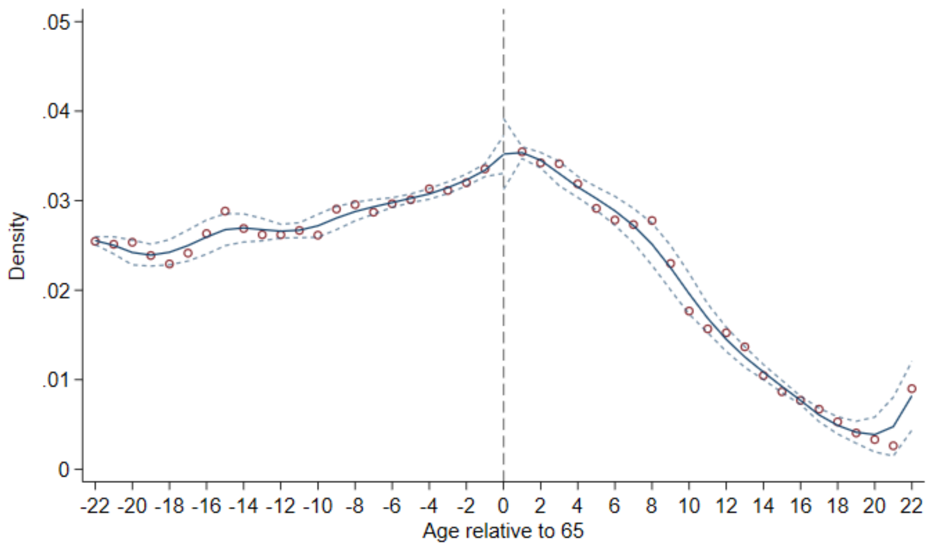
Notes: The working sample includes observations from 28 states with different time windows between January 6, 2021 (wave 22) and April 26, 2021 (wave 28). The figure displays the linear RD estimates for each subsample, along with 95% confidence intervals. All specifications control for remaining individual characteristics, state fixed effects and survey wave fixed effects. The age cutoff mirrors the cutoff in Figure 6, which is at age 65. The bandwidth and kernel are also the same as those in Figure 6.

Figure A.4. Effects of Vaccine Eligibility on Severe Food Insufficiency



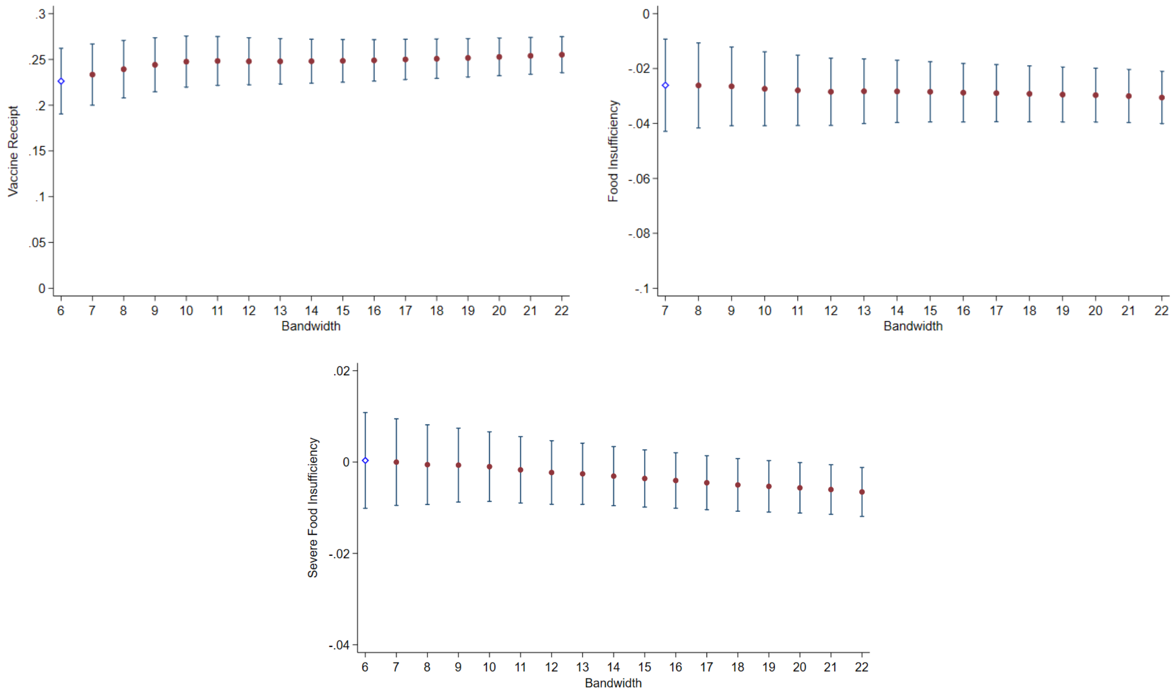
Notes: The top panel depicts the discontinuity using the full eligible sample above the cutoff. The bottom panel reports the linear RD estimates using the optimal bandwidth (shaded in gray). Pre-vaccination mean of the outcome within the optimal bandwidth is 0.042. All regressions use a triangular kernel function for weighting. Standard errors in parentheses are clustered at the age cohort-state level. Control variables mirror those in Figure 6. Dashed vertical lines denote the eligibility cutoff, which is normalized to zero.

Figure A.5. Testing for Discontinuity in the Density of Age



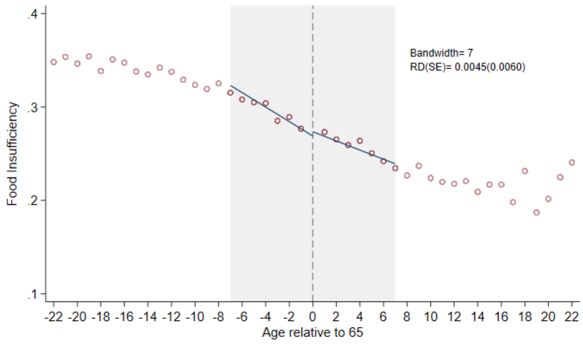
Notes: This figure provides a visual test for potential manipulation in the running variable. The dashed vertical line denotes the eligibility cutoff, which is normalized to zero. Following McCrary (2008), we estimate separate local quadratic regressions on either side of the cutoff, and compute the log difference of the coefficients on the intercepts, with an approximate standard error, to test for the null hypothesis of no manipulation. The log difference in height equals 0.033, and the standard error is 1.052.

Figure A.6. Robustness to Alternative Bandwidths

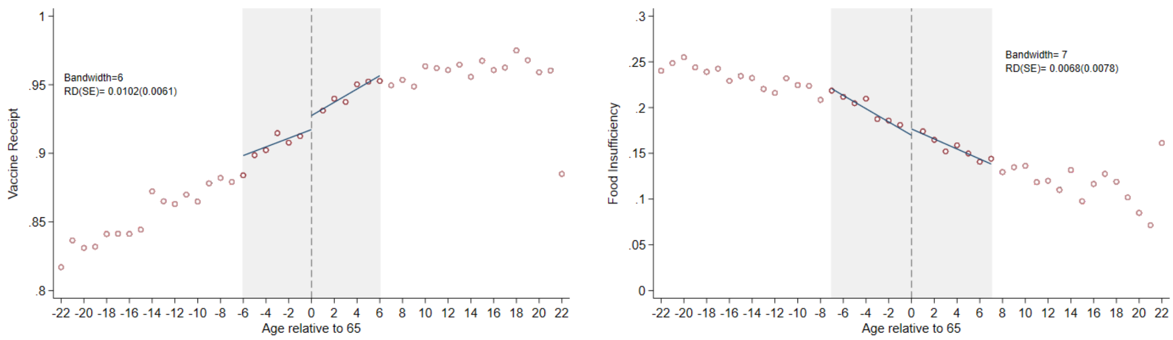


Notes: These figures report a set of linear RD estimates for each outcome, along with 95% confidence intervals, using various bandwidths. The hollow diamond markers report our benchmark RD estimates using the MSE-optimal bandwidth. All regressions use a triangular kernel function for weighting. Standard errors are clustered at the age cohort-state level. Control variables mirror those in Figure 6.

Figure A.7. Assessing the Continuity Assumption Using Placebo Samples



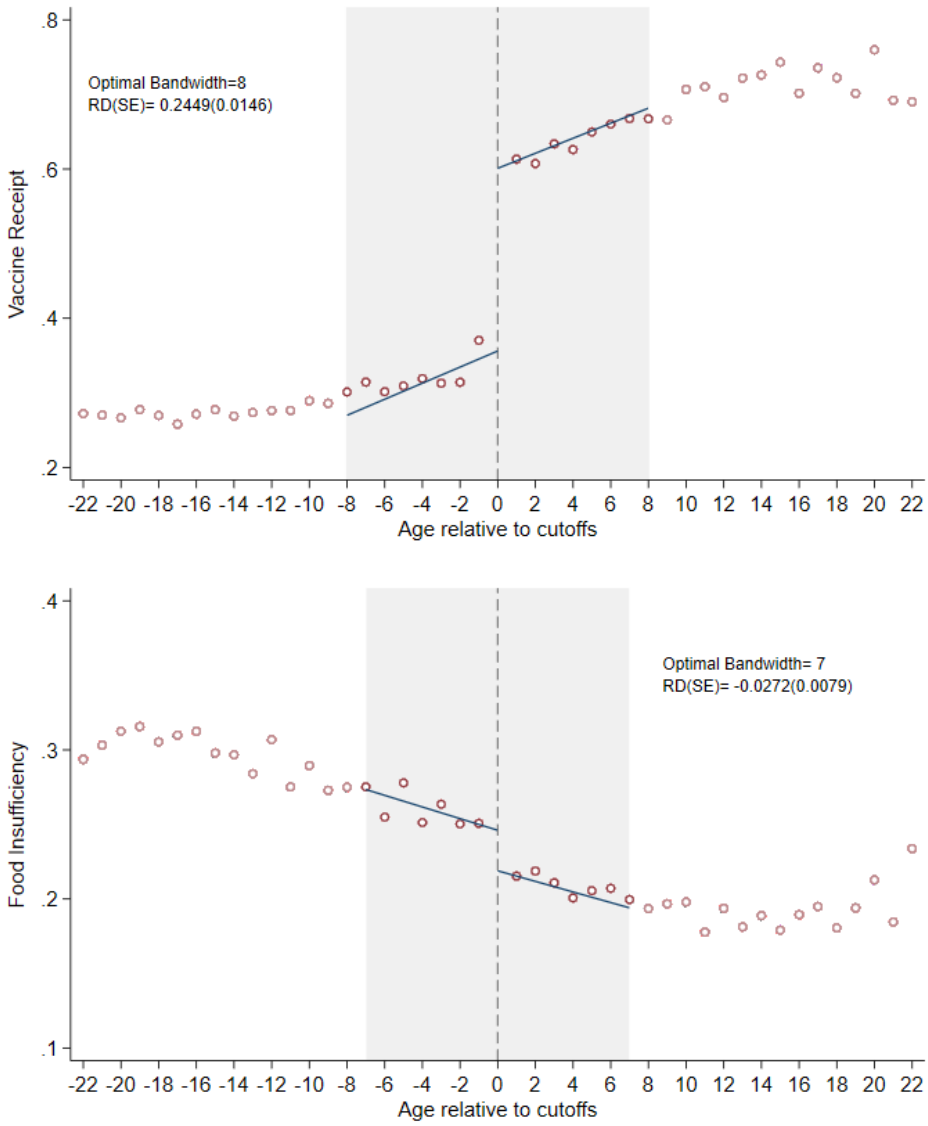
(a) Waves 13-21, Ineligible Sample Around the Cutoff



(b) Waves 30-33, Eligible Sample Around the Cutoff

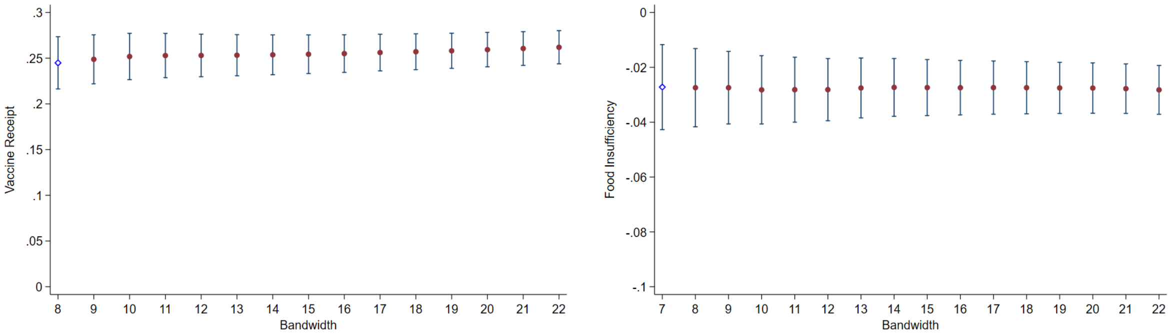
Notes: These figures report the placebo RD estimates using either fully ineligible or fully eligible samples. In Panel (a), individuals are not eligible for vaccines in waves 13-21. For this case, we cannot report first stage estimates since vaccination status is only available after wave 21. In Panel (b), individuals on both sides of the cutoff are eligible for vaccines in waves 30-33. All regressions use a triangular kernel function for weighting. Standard errors are clustered at the age cohort-state level. Control variables and the bandwidth mirror those in Figure 6.

Figure A.8. Effects of Vaccine Eligibility Around Multiple Age Cutoffs



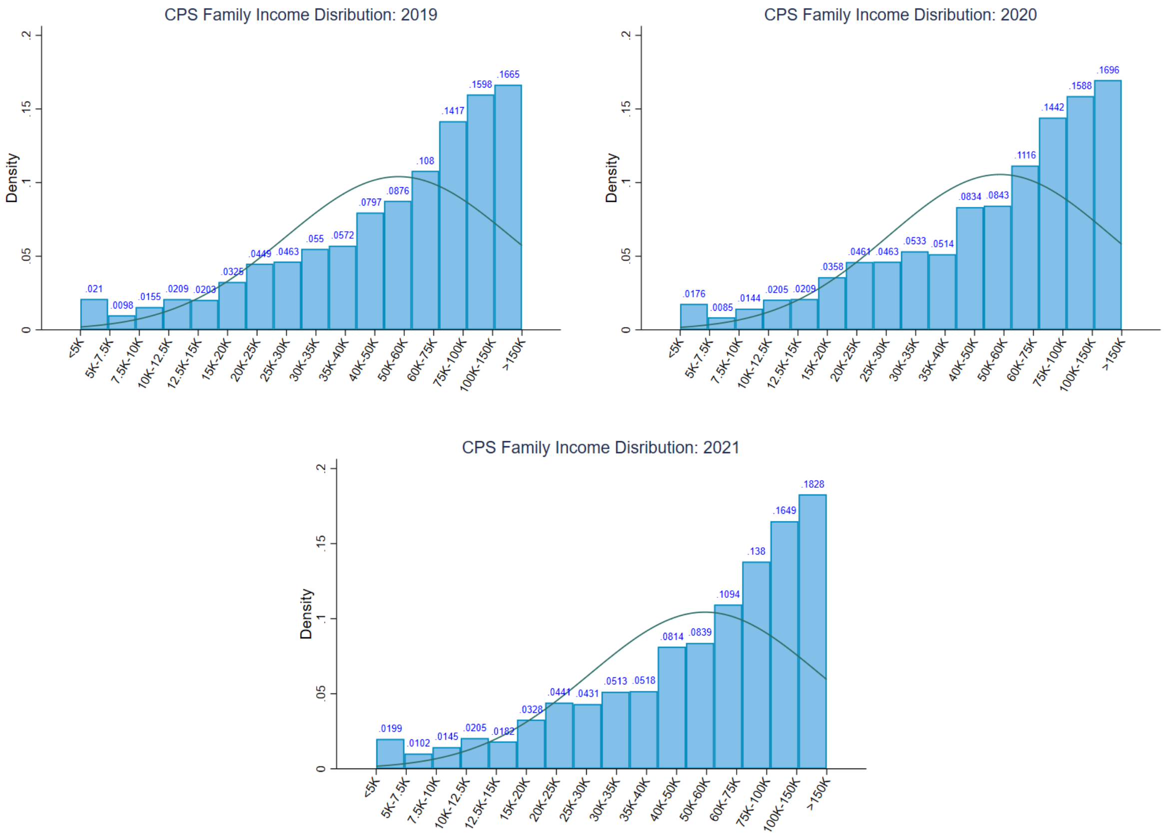
Notes: The working sample includes observations from 41 states with different time windows between January 20, 2021 (wave 23) and March 29 , 2021 (wave 27). Different from the baseline results, multiple cutoffs - including 60, 65, 70, and 75 - are stacked in this RD design. Each observation is the average of the corresponding outcome within age bins. These figures report the local linear RD estimates using the optimal bandwidth (shaded in gray) and triangular kernel function for weighting. Pre-vaccination mean of food insufficiency within the optimal bandwidth is 0.323. Standard errors in parentheses are clustered at the age cohort-state level. All specifications control for gender, marital status, race, educational attainment, household income, state fixed effects, and survey wave fixed effects. Dashed vertical lines denote the eligibility cutoff, which is normalized to zero.

Figure A.9. Robustness to Alternative Bandwidths for Multiple Age Cutoffs



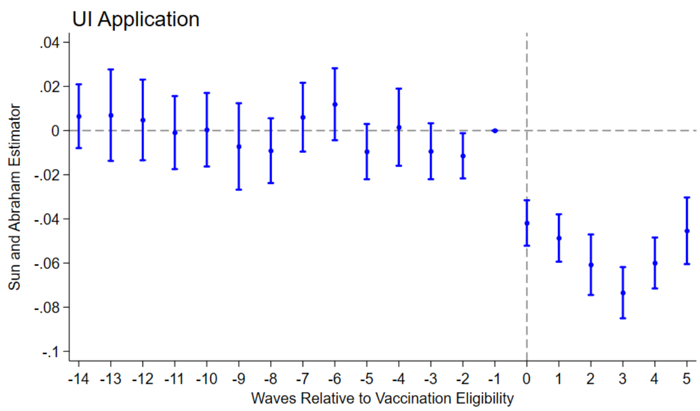
Notes: These figures report a set of linear RD estimates for each outcome, along with 95% confidence intervals, using various bandwidths. The hollow diamond markers report our multi-cutoff RD estimates using the MSE-optimal bandwidth. All regressions use a triangular kernel function for weighting. Standard errors are clustered at the age cohort-state level.

Figure A.10. Changes in Family Income Distribution, 2019-2021



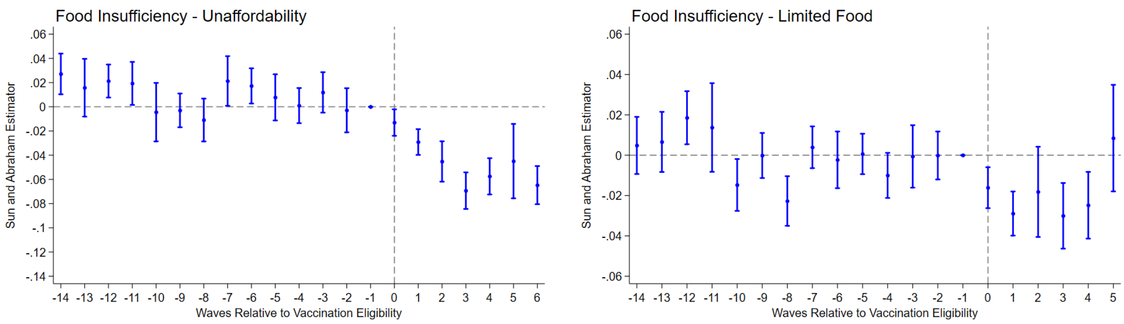
Notes: These figures show the distribution of household income for years 2019 through 2021, respectively. For each distribution, we impose a normal density curve, and use CPS-FSS sampling weights. The number on top of each bin represents the density.
Data Source: Current Population Survey Food Security Supplement (CPS-FSS), U.S. Census Bureau.

Figure A.11. Dynamic Effects on Unemployment Insurance Applications



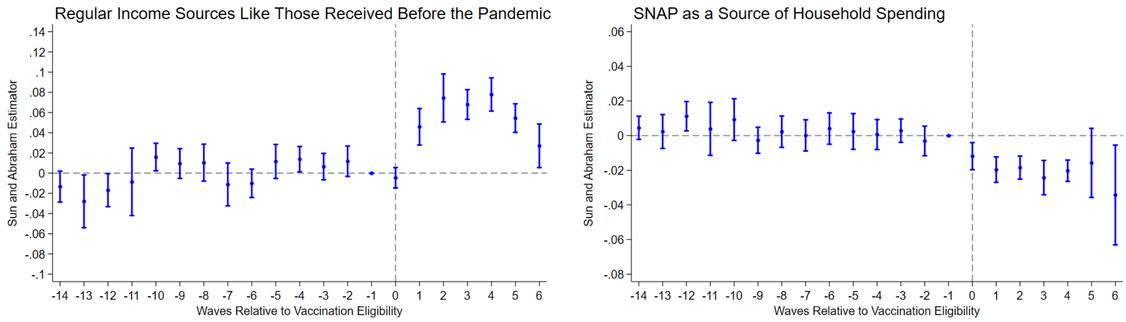
Notes: The working sample includes observations from August 19, 2020 (wave 13) through March 29, 2021 (wave 27). The figure displays the dynamic reduced-form estimates using the Sun and Abraham (2021) estimator, along with 95% confidence intervals. The last-treated cohort is the control group. All specifications control for remaining individual characteristics, state time-varying measures, state fixed effects, and survey wave fixed effects. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level.

Figure A.12. Dynamic Effects on Food Affordability and Availability



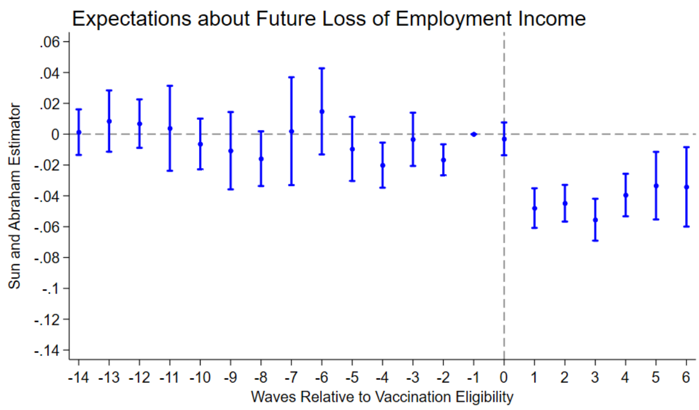
Notes: The working sample includes observations from August 19, 2020 (wave 13) through April 26, 2021 (wave 28) for unaffordability and from August 19, 2020 (wave 13) through March 29, 2021 (wave 27) for limited food. The figure displays the dynamic reduced-form estimates using the Sun and Abraham (2021) estimator, along with 95% confidence intervals. The last-treated cohort is the control group. All specifications control for remaining individual characteristics, state time-varying measures, state fixed effects, and survey wave fixed effects. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level.

Figure A.13. Dynamic Effects on Sources of Spending



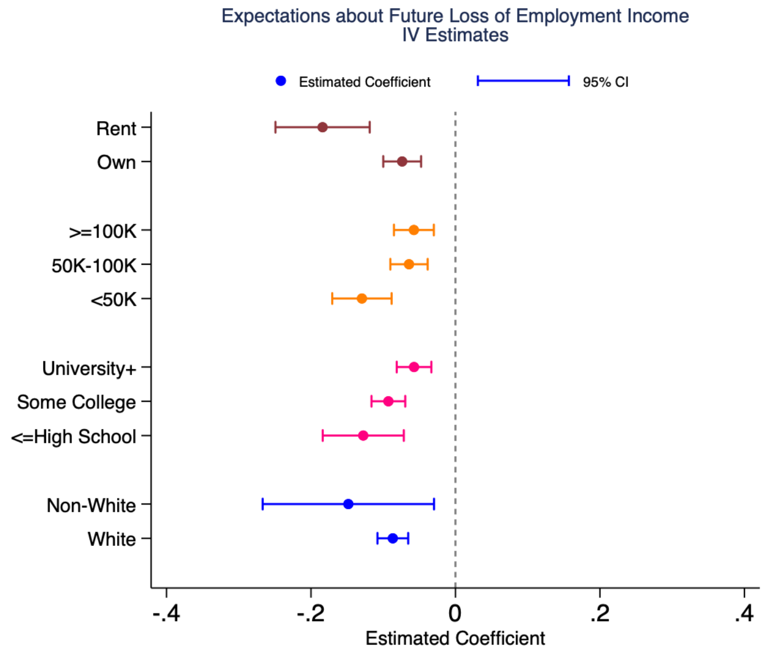
Notes: The working sample includes observations from August 19, 2020 (wave 13) through April 26, 2021 (wave 28). The figure displays the dynamic reduced-form estimates using the Sun and Abraham (2021) estimator, along with 95% confidence intervals. The last-treated cohort is the control group. All specifications control for remaining individual characteristics, state time-varying measures, state fixed effects, and survey wave fixed effects. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level.

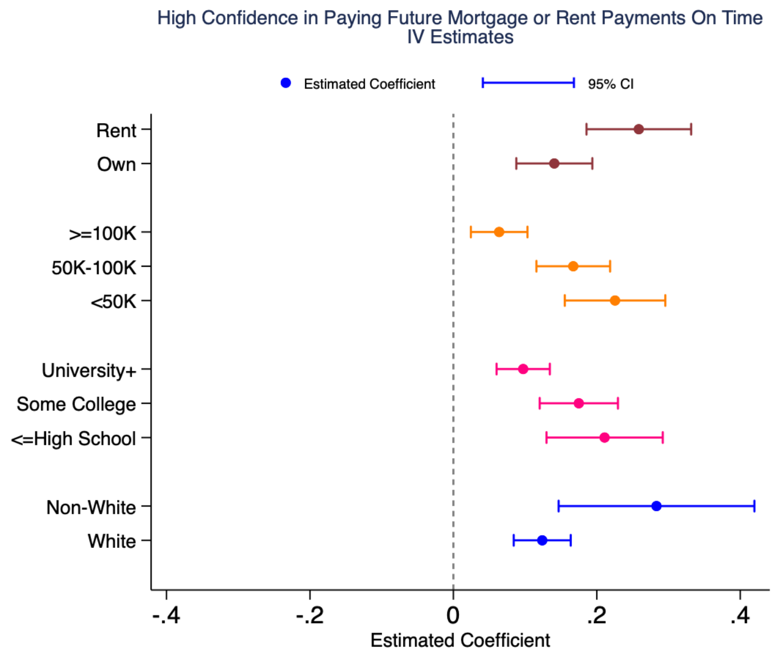
Figure A.14. Dynamic Effects on Expectations about Future Loss of Employment Income



Notes: The working sample includes observations from August 19, 2020 (wave 13) through April 26, 2021 (wave 28). The figure displays the dynamic reduced-form estimates using the Sun and Abraham (2021) estimator, along with 95% confidence intervals. The last-treated cohort is the control group. All specifications control for individual characteristics, state time-varying measures, state fixed effects, and survey wave fixed effects. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level.

Figure A.15. Financial Optimism by Sociodemographic Attributes





Notes: The working sample includes observations from August 19, 2020 (wave 13) through July 5, 2021 (wave 33). The figure displays separate IV estimates for each subsample, along with 95% confidence intervals. All specifications control for remaining individual characteristics, state fixed effects, survey wave fixed effects, and state-by-wave fixed effects. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level.

Figure A.16. Dynamic Effects on Confidence in Paying Future Mortgage or Rent Payments

Notes: The working sample includes observations from August 19, 2020 (wave 13) through April 26, 2021 (wave 28). The figure displays the dynamic reduced-form estimates using the Sun and Abraham (2021) estimator, along with 95% confidence intervals. The last-treated cohort is the control group. All specifications control for individual characteristics, state time-varying measures, state fixed effects, and survey wave fixed effects. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level.

Table A.1. Summary Statistics for Pre-Determined Attributes

	Pre-Vaccination Waves (Waves 13-21)			Post-Vaccination Waves (Waves 22-33)		
	Mean	SD	Obs.	Mean	SD	Obs.
Age	49.471	16.704	692,603	49.926	16.756	772,101
Male	0.472	0.499	692,603	0.473	0.499	772,101
Married	0.533	0.499	692,603	0.535	0.499	772,101
Race/Ethnicity						
White	0.672	0.469	692,603	0.671	0.470	772,101
Black	0.112	0.316	692,603	0.113	0.316	772,101
Asian	0.043	0.204	692,603	0.046	0.209	772,101
Hispanic	0.136	0.343	692,603	0.136	0.343	772,101
Other Races	0.036	0.187	692,603	0.034	0.181	772,101
Educational Attainment						
≤High School	0.365	0.481	692,603	0.357	0.479	772,101
Some College	0.297	0.457	692,603	0.298	0.458	772,101
≥University	0.338	0.473	692,603	0.345	0.475	772,101
Prior Household Income						
<50K	0.330	0.470	692,603	0.327	0.469	772,101
50K-100K	0.261	0.439	692,603	0.254	0.435	772,101
≥100K	0.239	0.426	692,603	0.235	0.424	772,101
Missing Income	0.170	0.376	692,603	0.184	0.388	772,101
Health Insurance Coverage						
Not Insured	0.073	0.261	692,603	0.066	0.249	772,101
Private	0.430	0.495	692,603	0.441	0.497	772,101
Medicare	0.170	0.376	692,603	0.165	0.372	772,101
Medicaid	0.113	0.317	692,603	0.116	0.320	772,101
Other	0.121	0.326	692,603	0.113	0.317	772,101
Missing Insurance	0.092	0.289	692,603	0.098	0.297	772,101
Presence of Children (Age < 18)	0.355	0.479	692,603	0.349	0.477	772,101
Household Size						
1	0.178	0.382	692,603	0.173	0.378	772,101
2	0.369	0.483	692,603	0.372	0.483	772,101
3	0.175	0.380	692,603	0.176	0.381	772,101
4	0.150	0.358	692,603	0.152	0.359	772,101
5	0.074	0.261	692,603	0.073	0.260	772,101
6+	0.055	0.227	692,603	0.054	0.225	772,101

Notes: The means are weighted using household weights from the HPS.

Table A.2. Progressively Including Pre-Determined Attributes in the First Stage

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Vaccine Eligibility	0.2383***	0.2381***	0.2396***	0.2390***	0.2387***	0.2403***	0.2390***	0.2389***	0.2380***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.011)	(0.011)	(0.012)	(0.012)
Dep. Var. Mean	0.526	0.526	0.526	0.526	0.526	0.526	0.526	0.526	0.526
N	1,464,704	1,464,704	1,464,704	1,464,704	1,464,704	1,464,704	1,464,704	1,464,704	1,464,704
State FE	√	√	√	√	√	√	√	√	√
Survey Wave FE	√	√	√	√	√	√	√	√	√
Pre-Determined Attributes									
Age	√	√	√	√	√	√	√	√	√
Male		√	√	√	√	√	√	√	√
Married			√	√	√	√	√	√	√
Education				√	√	√	√	√	√
Race/Ethnicity					√	√	√	√	√
Prior Household Income						√	√	√	√
Presence of Children							√	√	√
Household Size								√	√
Health Insurance									√

Notes: The working sample includes observations from August 19, 2020 (wave 13) through July 5, 2021 (wave 33). All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.3. Vaccination and Food Insufficiency (Excluding Prior Vaccination)

	(1)	(2)	(3)	(4)	(5)
			OLS		
Vaccination	-0.0558***	-0.0556***	-0.0556***	-0.0557***	-0.0553***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Pre-Vaccination Mean	0.392	0.392	0.392	0.392	0.392
Effect as a Percent of Mean	-14.23%	-14.18%	-14.18%	-14.21%	-14.11%
			TSLS		
Vaccination	-0.0663***	-0.0658***	-0.0659***	-0.0668***	-0.0669***
	(0.007)	(0.006)	(0.006)	(0.007)	(0.007)
Pre-Vaccination Mean	0.392	0.392	0.392	0.392	0.392
Effect as a Percent of Mean	-16.91%	-16.79%	-16.81%	-17.04%	-17.07%
N	1,384,200	1,384,200	1,384,200	1,384,200	1,384,200
State FE	√	√	√	√	√
Survey Wave FE	√	√	√	√	√
Region × Wave FE		√	√		
State time-varying measures			√	√	
State-specific linear trends				√	
State × Wave FE					√

Notes: The working sample includes observations from August 19, 2020 (wave 13) through July 5, 2021 (wave 33). All specifications control for individual characteristics, including age, gender, marital status, race, educational attainment, household income, health insurance coverage, the presence of children, and household size. State time-varying measures include two-week lagged COVID-19 death rate and stringency index, one-month lagged unemployment rate, and two-week lagged friend-exposure to vaccination information. Pre-vaccination mean is the weighted sample mean of the outcome between waves 13 and 21. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.4. Vaccination and Severe Food Insufficiency

	(1)	(2)	(3)	(4)	(5)
			OLS		
Vaccination	-0.0292***	-0.0293***	-0.0293***	-0.0293***	-0.0293***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Pre-Vaccination Mean	0.108	0.108	0.108	0.108	0.108
Effect as a Percent of Mean	-27.04%	-27.13%	-27.13%	-27.13%	-27.13%
			TSLS		
Vaccination	-0.0647***	-0.0641***	-0.0644***	-0.0637***	-0.0625***
	(0.007)	(0.008)	(0.008)	(0.007)	(0.008)
Pre-Vaccination Mean	0.108	0.108	0.108	0.108	0.108
Effect as a Percent of Mean	-59.91%	-59.35%	-59.63%	-58.98%	-57.87%
N	1,464,704	1,464,704	1,464,704	1,464,704	1,464,704
State FE	√	√	√	√	√
Survey Wave FE	√	√	√	√	√
Region × Wave FE		√	√		
State time-varying measures			√	√	
State-specific linear trends				√	
State × Wave FE					√

Notes: The working sample includes observations from August 19, 2020 (wave 13) through July 5, 2021 (wave 33). See Section 3.1 for the definition of *severe* food insufficiency. All specifications control for individual characteristics, including age, gender, marital status, race, educational attainment, household income, health insurance coverage, the presence of children, and household size. State time-varying measures include two-week lagged COVID-19 death rate and stringency index, one-month lagged unemployment rate, and two-week lagged friend-exposure to vaccination information. Pre-vaccination mean is the weighted sample mean of the outcome between waves 13 and 21. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.5. Vaccination and Food Insufficiency, Controlling for State-Level Vaccination

	(1)	(2)	(3)	(4)
	OLS			
Vaccination	-0.0482***	-0.0481***	-0.0481***	-0.0481***
	(0.003)	(0.002)	(0.002)	(0.002)
State Vaccination Rate (per 100)		-0.0069	-0.0000	0.0029
		(0.007)	(0.007)	(0.009)
Pre-Vaccination Mean	0.392	0.392	0.392	0.392
Effect as a Percent of Mean	-12.30%	-12.27%	-12.27%	-12.27%
	TSLS			
Vaccination	-0.0951***	-0.0951***	-0.0938***	-0.0942***
	(0.012)	(0.012)	(0.012)	(0.012)
State Vaccination Rate (per 100)		0.0001	0.0024	0.0052
		(0.006)	(0.006)	(0.007)
Pre-Vaccination Mean	0.392	0.392	0.392	0.392
Effect as a Percent of Mean	-24.26%	-24.26%	-23.93%	-24.03%
N	1,464,704	1,464,704	1,464,704	1,464,704
State FE	√	√	√	√
Survey Wave FE	√	√	√	√
Region × Wave FE			√	√
State time-varying measures				√

Notes: The working sample includes observations from August 19, 2020 (wave 13) through July 5, 2021 (wave 33). All specifications control for individual characteristics, including age, gender, marital status, race, educational attainment, household income, health insurance coverage, the presence of children, and household size. State time-varying measures include two-week lagged COVID-19 death rate and stringency index, one-month lagged unemployment rate, and two-week lagged friend-exposure to vaccination information. Pre-vaccination mean is the weighted sample mean of the outcome between waves 13 and 21. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.6. Vaccination and Severe Food Insufficiency, Controlling for State-Level Vaccination

	(1)	(2)	(3)	(4)
		OLS		
Vaccination	-0.0292***	-0.0293***	-0.0293***	-0.0293***
	(0.002)	(0.002)	(0.002)	(0.002)
State Vaccination Rate (per 100)		-0.0065*	-0.0091	-0.0079
		(0.004)	(0.006)	(0.007)
Pre-Vaccination Mean	0.108	0.108	0.108	0.108
Effect as a Percent of Mean	-27.04%	-27.13%	-27.13%	-27.13%
		TSLS		
Vaccination	-0.0647***	-0.0647***	-0.0641***	-0.0644***
	(0.007)	(0.007)	(0.008)	(0.008)
State Vaccination Rate (per 100)		-0.0012	-0.0073	-0.0062
		(0.005)	(0.007)	(0.007)
Pre-Vaccination Mean	0.108	0.108	0.108	0.108
Effect as a Percent of Mean	-59.91%	-59.91%	-59.35%	-59.63%
N	1,464,704	1,464,704	1,464,704	1,464,704
State FE	√	√	√	√
Survey Wave FE	√	√	√	√
Region × Wave FE			√	√
State time-varying measures				√

Notes: The working sample includes observations from August 19, 2020 (wave 13) through July 5, 2021 (wave 33). See Section 3.1 for the definition of *severe* food insufficiency. All specifications control for individual characteristics, including age, gender, marital status, race, educational attainment, household income, health insurance coverage, the presence of children, and household size. State time-varying measures include two-week lagged COVID-19 death rate and stringency index, one-month lagged unemployment rate, and two-week lagged friend-exposure to vaccination information. Pre-vaccination mean is the weighted sample mean of the outcome between waves 13 and 21. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.7. Vaccine Eligibility and the Likelihood of Receiving Vaccination, Alternative Specifications and Inference

	(1)	(2)	(3)
Vaccine Eligibility	0.0696***	0.1081***	0.1081***
	(0.015)	(0.016)	(0.012)
Dep. Var. Mean	0.526	0.526	0.526
N	1,464,704	1,464,704	1,464,704
State Time-Varying Measures	√		
State FE	√	√	√
Survey Wave FE	√	√	√
State × Age Cohort FE	√	√	√
Wave × Age Cohort FE	√	√	√
State × Wave FE		√	√
Cluster of Std. Error	State	State	State-Age Cohort
Kleibergen-Paap rk LM statistic	17.35	20.50	29.37
Kleibergen-Paap rk Wald F statistic	21.47	48.31	80.17

Notes: The working sample includes observations from August 19, 2020 (wave 13) through July 5, 2021 (wave 33). The age cohort is categorized as 20-24, 25-29, 30-34, 35-39, 40-44, 45-49, 50-54, 55-59, 60-64, and 65-87. All specifications control for individual characteristics, including dummies for age cohort, gender, marital status, race, educational attainment, household income, health insurance coverage, the presence of children, and household size. State time-varying measures include two-week lagged COVID-19 death rate and stringency index, one-month lagged unemployment rate, and two-week lagged friend-exposure to vaccination information. The dependent variable mean is based on the sample period during which the vaccines became available. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.8. Vaccination and Food Insufficiency, Alternative Specifications and Inference

	(1)	(2)	(3)
Vaccination	-0.1689**	-0.1586***	-0.1586***
	(0.082)	(0.057)	(0.054)
Pre-Vaccination Mean	0.392	0.392	0.392
Effect as a Percent of Mean	-43.09%	-40.46%	-40.46%
N	1,464,704	1,464,704	1,464,704
State Time-Varying Measures	√		
State FE	√	√	√
Survey Wave FE	√	√	√
State × Age Cohort FE	√	√	√
Wave × Age Cohort FE	√	√	√
State × Wave FE		√	√
Cluster of Std. Error	State	State	State-Age Cohort

Notes: The working sample includes observations from August 19, 2020 (wave 13) through July 5, 2021 (wave 33). The age cohort is categorized as 20-24, 25-29, 30-34, 35-39, 40-44, 45-49, 50-54, 55-59, 60-64, and 65-87. All specifications control for individual characteristics, including dummies for age cohort, gender, marital status, race, educational attainment, household income, health insurance coverage, the presence of children, and household size. State time-varying measures include two-week lagged COVID-19 death rate and stringency index, one-month lagged unemployment rate, and two-week lagged friend-exposure to vaccination information. Pre-vaccination mean is the weighted sample mean of the outcome between waves 13 and 21. All regressions are weighted using household weights from the HPS, and standard errors are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.9. Covariate Smoothness Around the Cutoff

	Male	Married	White	Black	Hispanic	Non-White	≤ High School
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Vaccine Eligibility	0.0060	-0.0035	0.0085	0.0096*	-0.0029	-0.0085	-0.0098
	(0.011)	(0.011)	(0.007)	(0.006)	(0.005)	(0.007)	(0.008)
Dep. Var. Mean	0.428	0.624	0.803	0.072	0.065	0.197	0.127
N	37,784	37,784	37,784	37,784	37,784	37,784	37,784
	Some College	University +	< 50K	50K-100K	≥ 100K	< 50K, Missing†	Own a House
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Vaccine Eligibility	-0.0105	0.0202*	0.0050	-0.0091	-0.0009	0.0100	0.0079
	(0.011)	(0.011)	(0.010)	(0.009)	(0.011)	(0.011)	(0.007)
Dep. Var. Mean	0.332	0.541	0.261	0.292	0.318	0.390	0.855
N	37,784	37,784	37,784	37,784	37,784	37,784	34,533
Bandwidth	7	7	7	7	7	7	7
State FE	√	√	√	√	√	√	√
Survey Wave FE	√	√	√	√	√	√	√

Notes: Each column replaces the outcome variable with a pre-determined attribute and reports the linear RD estimate using the regression in Equation 3. Our objective is to assess the smoothness of these attributes near the cutoff. †In addition to column (10), the indicator variable for income <50K in column (13) includes missing values. All specifications only control for state and survey wave fixed effects. We use the largest optimal bandwidth in Figure 6. All regressions use a triangular kernel function for weighting. Standard errors are clustered at the age cohort-state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.10. Difference-in-Discontinuities: Primary Waves vs. Placebo Waves

	Vaccine Receipt	Food Insufficiency
	(1)	(2)
Panel A: Reference Placebo Waves 13-21, Ineligible Sample Around the Cutoff		
Treated (Age $>$ 65) $\times$ Primary Waves	–	-0.0307***
	–	(0.011)
Pre-Vaccination Mean†	–	0.279
Effect as a Percent of Mean	–	-11.00%
Bandwidth	–	7
N	–	135,038
Panel B: Reference Placebo Waves 30-33, Eligible Sample Around the Cutoff		
Treated (Age $>$ 65) $\times$ Primary Waves	0.2163***	-0.0331***
	(0.018)	(0.012)
Pre-Vaccination Mean†	–	0.279
Effect as a Percent of Mean	–	-11.86%
Bandwidth	6	7
N	65,187	76,767

Notes: This table reports the difference in linear RD estimates between the primary and placebo samples, which is coined as difference-in-discontinuities. The primary sample refers to the benchmark RD sample between waves 22 and 28. The placebo samples in Panels A and B include waves 13-21 and 30-33, respectively. †Pre-vaccination mean is based on the primary sample of 28 states and age groups within the bandwidth. All regressions use a triangular kernel function for weighting. Standard errors are clustered at the age cohort-state level. Control variables and the bandwidth mirror those in Figure 6. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.11. Reliance on Food Assistance by Family Income and Educational Attainment

	$<$ 50K	50K-100K	$\geq$ 100K	$\leq$ High School	Some College	University +
	(1)	(2)	(3)	(4)	(5)	(6)
Vaccine Eligibility	-0.0186**	-0.0006	-0.0009	-0.0193***	-0.0144***	-0.0036*
	(0.008)	(0.002)	(0.001)	(0.007)	(0.004)	(0.002)
Pre-Vaccination Mean	0.148	0.024	0.005	0.114	0.064	0.016
Effect as a Percent of Mean	-12.57%	-2.50%	-18.00%	-16.93%	-22.50%	-22.50%
N	224,598	199,894	195,535	219,907	168,858	231,262
State FE	✓	✓	✓	✓	✓	✓
Survey Month FE	✓	✓	✓	✓	✓	✓
State time-varying measures	✓	✓	✓	✓	✓	✓
State $\times$ Month FE	✓	✓	✓	✓	✓	✓

Notes: This table reports the heterogeneous effects of vaccine eligibility on food stamp take-up using data from the CPS-FSS. The working sample uses observations from August 2020 through June 2021. Each column is a separate regression stratified by either family income (<50K, 50K-100K, and ≥100K) or education attainment (≤ high school, some college, and university +). All specifications control for remaining individual characteristics, including age, gender, marital status, education attainment, race, household income, the number of children, and household size. State time-varying measures include one-month lagged COVID-19 death rate and stringency index, as well as the unemployment rate. All regressions are weighted using the supplement person weight, and standard errors are clustered at the state level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

B. Illustration of a Potential Backdoor Path

We included community vaccination rates in specifications that do not absorb their variation through state-specific trends or state-by-wave fixed effects, allowing us to assess both the magnitude and statistical significance of this variable and to explore whether it represents a salient backdoor path. We report our estimates in Tables A.5 and A.6. We find that not only are the estimates very close in magnitude to our benchmark results, but also that, conditional on individuals' vaccination status, the impact of state-level vaccination rates is close to zero and statistically insignificant. This suggests that community-level effects are unlikely to operate independently of individual vaccination status in influencing food insufficiency. We emphasize, however, that this result does not imply community-level vaccination rates have no effect on economic recovery or financial expectations. On the contrary, we hypothesize that they influence our proposed mechanism. Rather, our analysis shows that these effects do not operate through an independent (backdoor) path that bypasses individual vaccination status.

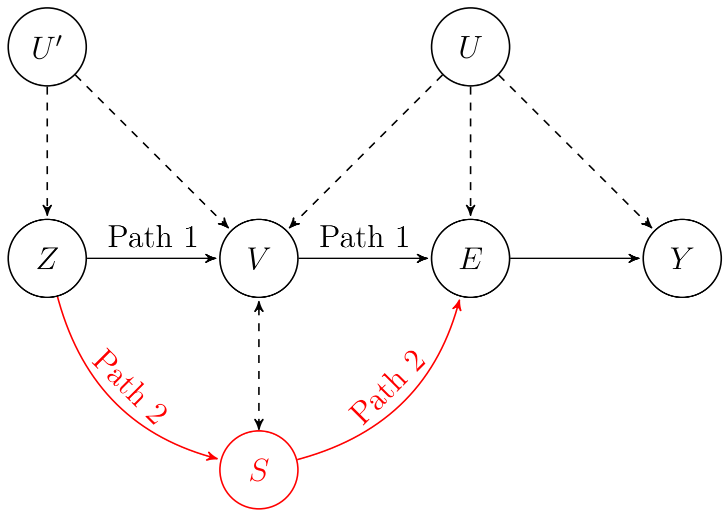


Figure B.1. A Simple Illustration of A Potential Backdoor Path

Notes: This figure provides a simple illustration of backdoor paths using a directed acyclic graph (DAG). The notation is defined as follows: Z is vaccine eligibility, V is own vaccination status, E is our proposed mechanisms (e.g., financial expectations), Y is food insufficiency, and S is the state-level vaccination. Path 1 is the ideal path in an IV model ($Z \to V \to E \to Y$), where we have no exclusion restriction violation. The presence of both U and U' does not result in an exclusion restriction violation. U represents the typical source of endogeneity in vaccination decisions, while the path involving $U^{'}$ contains V. Path 2 ($Z \to S \to E \to Y$) indicates an exclusion restriction violation if the path does not involve V represented by the dashed line.

To formalize our argument, we present a directed acyclic graph (DAG) in Figure B.1, illustrating the potential backdoor path. Path 1, $Z \to V \to E \to Y$, represents the ideal pathway in an IV model that does not violate the exclusion restriction. In contrast, Path 2, $Z \to S \to E \to Y$, illustrates a potential violation of the exclusion restriction. In this scenario, age-specific vaccine eligibility (Z) affects community-level vaccination rates (S), which in turn influence the proposed mechanisms (e.g., financial expectations), ultimately affecting food insufficiency (Y) - all without involving individual vaccination status (V).

However, our empirical results suggest that this backdoor path does not operate independently of V. When there is a bidirectional relationship between V and S (i.e., $V \leftrightarrow S$), the mechanisms are shaped by the joint dynamics of individual and community-level vaccination. As a result, all relevant paths, such as $Z \to V \to S \to E \to Y$ and $Z \to S \to V \to E \to Y$, include V. Thus, the exclusion restriction is not violated in the presence of V along all operative paths. Our objective is not to estimate all potential pathways that satisfy the exclusion restriction, but rather to provide evidence that S likely does not create an independent backdoor path.