the review of income and wealth

Review of Income and Wealth
Series 0, Number 0, 2023
DOI: 10.1111/roiw.12643

SAFE AT LAST? LATE EFFECTS OF A MASS IMMUNIZATION CAMPAIGN ON HOUSEHOLDS' ECONOMIC INSECURITY

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We study the effects of receiving immunization from COVID-19 on households' economic insecurity. To provide causal estimates we use a fuzzy regression discontinuity design which takes advantage of the UK's immunization plan. The plan was primarily based on age, granting differential eligibility to proximate cohorts. Our estimated local average treatment effect indicates that the share of households who declared being economically insecure dropped by 41 percentage points among those who received the vaccine due to the eligibility criteria. Using a difference-in-discontinuity design we next document that immunization was more salient for women as well as for large households and those with children. Our results suggest that the mass immunization campaign against COVID-19 had relevant short-run economic effects, well beyond its expected impact on people's health.

JEL Codes: D14, D90, G51, I38

Keywords: COVID-19 immunization campaign, difference-in-discontinuity, economic insecurity, RDD, United Kingdom

1. Introduction

Economists typically presuppose environments where insurance markets are complete and competitive and where credit markets work perfectly. In such environments, households can respond to any sort of hazards by either insuring against the adverse event in advance or by borrowing to meet the unexpected financial need. In reality, these preconditions are rarely met: insurance contracts are typically unavailable for a large subset of economic risks, while households without house ownership or with weak financial buffers can hardly provide collateral to borrow (e.g., Barr 1992). Unexpected adverse events can therefore leave households without economic protection. The anticipation of the inability to successfully manage an adverse future situation (e.g. a job loss, a large income loss, or a large expenditure) can generate anxiety and welfare losses that are referred to in the literature as the notion of economic insecurity (e.g., Dominitz and Manski 1997; Osberg 1998;

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Bossert and D'Ambrosio 2013; Hacker et al. 2014; Rohde and Tang 2018). See Richiardi and He (2020) for a critical overview of economic insecurity definitions provided in the literature.

Understanding how to reduce economic insecurity in households is particularly important for several reasons. First, and most importantly, economic insecurity has a clear negative impact on individual health (Rohde et al. 2016; Rohde et al. 2017; Watson 2018). People even with a temporary sense of insecurity can develop persistent negative effects, such as obesity and worsening mental health. Second, a number of recent works have documented that economic anxiety can affect economic and political decision making. They show that economic insecurity is robustly associated with populist and right-wing party support (see e.g., Algan et al. 2017; Guiso et al. 2017; Guiso et al. 2019; Bossert et al. 2022; Dippel et al. 2021; Rebechi and Rohde 2022; Watson et al. 2022) as well as with Brexit choice (Colantone and Stanig 2018; Liberini et al. 2019). There is also considerable evidence that indicates that households with insecure property rights systematically invest less, and this has been observed in settings as diverse as rural China (Jacoby et al. 2002), India (A. V. Banerjee et al. 2002), Ghana (Goldstein and Udry 2008) and Argentina (Galiani and Schargrodsky 2010).

In this paper, we provide evidence that immunization from COVID-19 caused a reduction in households' economic insecurity. The COVID-19 pandemic brought forward enduring uncertainty about the future. Especially during the pre-vaccination phase, households were deeply concerned about their capacity (as well as of their country) to return back to their normal life and to respond to an emergency expense. In addition to a health emergency, the pandemic prompted a collective sense of economic anxiety (Binder 2020; Coibion et al. 2020; Clark et al. 2021; Fetzer et al. 2021; Hanspal et al. 2021) and mental distress in households (Proto and Zhang 2021; Vahratian et al. 2021; Staneva et al. 2022). All these concerns could not be fully curtailed by traditional welfare state policies (e.g. Working Tax Credits and Universal Credit in the United Kingdom) but required exceptional measures. Vaccines represented a turning point in the global fight against the virus, and the entire public attention in late 2020/early 2021 was drawn to the practical implementation and efficacy of one of the largest immunization programs in history. See, for example, AP News at https://inyurl.com/wmdmbvp8 and Bloomberg at https://tinyurl.com/399rewk3.

We use high-quality survey data provided by the eighth wave of the Understanding Society COVID-19 Study (University of Essex, Institute for Social and Economic Research 2021a) to examine the effects of vaccination on perceived economic insecurity. The survey was fielded online in the United Kingdom between March 24 and 31, 2021. It crucially asks interviewees whether they have received immunization from COVID-19 and a number of questions concerning their ability to manage the current adverse circumstances financially, as well as their economic prospects in the future that we use as outcomes in our regression framework. Moreover, the survey provides precious information on aspects that the literature identifies as related to the concept of economic security; this means that we can control for the respondent's financial fragility status, such as a weak financial buffer (Lusardi et al. 2011; Hacker et al. 2014); indebtedness or low income (Bossert and D'Ambrosio 2013); for whether the respondent has

experienced a drop in wealth relative to the pre-COVID-19 period (Bossert and D'Ambrosio 2013; Rohde et al. 2014); or whether they have been infected in the recent past.

Empirically, the estimation of the effect of vaccination poses several challenges. For one thing, vaccination was not mandatory, and therefore individuals self-selected into vaccination. For another, the environment strongly influences vaccine compliance. Simply regressing economic insecurity on such a decision will therefore lead to a biased estimation due to the underlying endogeneity problem. To provide causal evidence, we take advantage of the UK's immunization strategy. The plan was deployed on an age basis, granting differential eligibility to proximate cohorts. We exploit this rule in a fuzzy regression discontinuity design (RDD) where the individual decision to get the vaccine is instrumented by the eligibility criteria. During the survey rollout individuals aged 50 and over were eligible for vaccination, while those aged 49 and under were not. We provide compelling evidence that, locally, the vaccine delivery plan is as good as a random instrument, which implies that being 50 years old or just over, rather than 49 or just under, was a matter of chance.

We estimate the local average treatment effect (LATE) of vaccination on households' level of economic insecurity. Specifically, we compare individuals who got the COVID-19 vaccine because they were aged 50, or just over, rather than 49, or just under, and find that the former group is relatively less likely to report that their financial situation in the next 3 months will be worse than now. Our estimated coefficient is statistically significant and negatively signed, which indicates that the share of households who declared being economically insecure fell by 41 percentage points among those who received the vaccine due to the eligibility criteria. The effect is economically relevant and comparable to other studies that examine the effects of vaccination using the same context as ours (e.g., Bagues and Dimitrova 2021).

We further examine other outcomes that capture specific domains of economic insecurity, which were arguably only marginally affected by the pandemic in our UK setting. First, we look at the perceived level of financial fragility by analyzing individual beliefs about the strain they face managing their current financial situation. We find that individuals who got vaccinated, because eligible, perceive a lower level of financial fragility. The estimated effect is, however, less precisely estimated and statistically significant at the 10 percent level. We interpret this difference with our focal measure of economic insecurity as a sign of rational expectations: vaccines give protection against the virus through antibodies that only develop several weeks after the jab; households anticipate that time is required to return to their normal life and update their perceived levels of economic insecurity accordingly, assigning less pessimism to the 3 months ahead and not to the current situation. The second variable captures households' anxiety about the threat of losing their job (in the 3 months ahead). While this is a dimension frequently used in the literature to measure economic insecurity in the post 2008 financial crisis period (Norris and Inglehart 2016; Algan et al. 2017; Guiso et al. 2017; Dustmann et al. 2017; Foster and Frieden 2017), during the COVID-19 pandemic governments promptly acted to protect jobs. This was certainly the case in the United Kingdom where, thanks

to the adoption of the Job Retention Scheme,[1] the unemployment rate only slightly increased during 2020. Finally, we explore a measure of the anxiety produced by a perceived incapacity of meeting ordinary expenses (like usual bills) in the near future. This measure is closely related to the expectation of a large drop in wealth, which fortunately remained low in the United Kingdom thanks to the adoption of further policies that maintained the income level of self-employed individuals.[2]

We next move to the investigation of heterogeneous effects. We examine which groups of households were the most responsive to the immunization by using a difference-in-discontinuity design (e.g., Grembi et al. 2016). Our results indicate that immunization was particularly salient for women, large households, and those with children. These categories were, in fact, the most affected by the COVID-19 pandemic due to a combination of school closures, loss in children's educational skills, and work-from-home option (e.g., Agostinelli et al. 2020; Fuchs-Schündeln et al. 2020; Engzell et al. 2021; Grewenig et al. 2021; Werner and Woessmann 2023) that also impacted upon female labor outcomes, like productivity (e.g., Alon et al. 2021; Campa et al. 2021; Fabrizio et al. 2021).[3] We document that eligibility for vaccination reduced (1) the share of women feeling economically insecure by 6.8 percentage points more relative to men; (2) the share of households with children feeling economically insecure by 7.9 percentage points more relative to households with no child; (3) the share of large households feeling economically insecure by 6.1 percentage points more relative to households with fewer than four members. This finding is consistent with Agrawal et al. (2021) who also document a larger reduction in mental distress to vaccines among American households with children. We further explore several mechanisms linking vaccines to economic security, but we solely find evidence of a positive psychological effect of immunization on UK households (e.g., Agrawal et al. 2021; Bagues and Dimitrova 2021).

Our work is part of a flourishing and fast-moving literature studying the causal impact of vaccines. Almost the entirety of this literature is concerned with the effects of COVID-19 vaccines on physical health outcomes (see Polack et al. 2020; Abu-Raddad et al. 2021; Lopez Bernal et al. 2021, among others). We know little about the effects of vaccines beyond a short period after the act and even less about the effect on recipients' beliefs. The only exceptions we are aware of are the recent studies by Agrawal et al. (2021), Bagues and Dimitrova (2021), and Depetris-Chauvin and González (2021). Bagues and Dimitrova (2021) robustly document that vaccination had a positive causal effect on psychological well-being stemming from a reduction in the perceived likelihood of getting the virus in the

[1]The Coronavirus Job Retention Scheme provided grants to employers to pay 80 percent of a staff wage and employment costs each month, up to a total of £2,500 per person per month. Adopted in March 2020, it remains in force until September 2021.

[2]The Self Employment Income Support Scheme was to a large degree the most important measure in this department. It paid a grant worth 80 percent of profits up to £2,500 each month to self-employed people who were able to document to having suffered from a loss of income. This measure too remained in force until September 2021.

[3]Etheridge and Spantig (2020) also document a larger decline in well-being for women, relative to men, after the first lockdown was imposed in the United Kingdom. Social factors and an overall greater impact of loneliness are among the drivers highlighted by the authors.

future and a higher engagement in social activities. Our results are consistent with their findings in that vaccination reduced pessimism among UK recipients at both the economic and psychological levels. This pattern is also corroborated by Agrawal et al. (2021) who document sizable and economically significant reductions of anxiety and depression symptoms within vaccinated individuals in the United States, especially within households with children. Depetris-Chauvin and González (2021), in addition, show that vaccines had important political consequences in Chile; that is, the authors document an increase in outsiders' support where vaccination rates were higher—a behavior that is consistent with voters being less anxious about the pandemic consequences. Yet, to the best of our knowledge, there is no causal evidence of vaccination on economic insecurity or, more broadly, on economic beliefs and attitudes.

Economic insecurity is typically slow-moving and depends on innate or long-lasting characteristics, like gender, income, or financial health (e.g., Ivlevs 2014; Romaguera-de-la Cruz 2020; Blázquez et al. 2021). However, recent studies have shown that such feelings can change quickly in households with little economic protection or with precarious economic prospects who are hit by large shocks, such as rapid import penetration (e.g., Scheve and Slaughter 2004; D'Ambrosio and Rohde 2014; Rodrik 2018; Guiso et al. 2019) or the unexpected outbreak of a virulent pandemic like COVID-19 (Fetzer et al. 2021). We relate to these works by causally showing that immunization against COVID-19 also had a role in turning the development of the households' level of economic insecurity around, causing reduction in anxiety especially for the most affected individuals. In this sense, our work is also consistent with Binder (2020) who documents that the Federal Reserve's decision to cut the interest rate, to respond to the negative effects of the pandemic, induced households to frame macroeconomic fundamentals in a more optimistic fashion.

The rest of the paper is organized as follows. Section 2 describes the institutional context and the data. Section 3 discusses our empirical strategy. In Section 4, we present estimation results, while in Section 5 we discuss and test the identifying assumptions. Section 6 provides additional evidence for the interpretation of our findings. Section 7 concludes. Further analyses are provided in the Online Appendix.

2. Data and Institutional Context

2.1. *The UK's COVID-19 Vaccination Program*

The fallout from the COVID-19 pandemic in the United Kingdom was one of the most severe in Europe and the world. By November 2021 over 9.8 million cases and 143,000 deaths had been recorded. The economy remained below pre-pandemic levels having initially shrunk by a record 25 percent—twice as much as the previous record annual fall. From the onset of the pandemic, countries relied on non-pharmaceutical interventions (NPIs) (e.g. "lockdowns") to control the spread of the virus. The United Kingdom first entered "lockdown" on the March 23, 2020 and remained in a perpetual state of control for over 12 months. NPIs are inherently restrictive and offered no route back to normal life until an

effective vaccine was found. Research began as early as March 2020 to create such a vaccine.

On December 2, 2020, the Pfizer-BioNTech vaccine was approved for use in a two-dose schedule in the United Kingdom by the Medicines and Healthcare Products Regulatory Agency (MHRA), becoming the first COVID-19 vaccine to be authorized anywhere in the world. Several days later a 90-year-old became the first person to be given the jab. This was shortly followed by the MHRA approving the two-dose use of the Oxford-AstraZeneca vaccine on December 30. A total of 140 million doses of both vaccines were pre-ordered by the Vaccine Taskforce—formed to hasten the creation and manufacturing of a COVID-19 vaccine by collaborating with the government, academia, and industry—allowing the United Kingdom to begin the largest mass vaccination program since the diphtheria vaccine in 1940.

On January 11, 2021, the government published its delivery plan following advice from the Joint Committee on Vaccination and Immunisation (JCVI), an independent medical and scientific expert body that advises the government on prioritization for all vaccines. The full report is available here: https://tinyurl.com/jcvi-rollout. The overarching strategy was to prevent COVID-19 mortality and the protection of health and social care staff and systems. To achieve this, the JCVI set out nine priority cohorts in phase 1 of the rollout: group 1 contained care home residents and residential care workers; group 2 contained all those aged 80+ and health and social care workers; group 3 contained those aged 75–79; group 4 those aged 70–74 and those clinically extremely vulnerable under 70; and group 5 was those aged 65–69. The cohorts proceed on a strict age basis to the final group, group 9, which contained those aged 50–54. In practice, individuals were allocated to the cohorts based on their date of birth, which is identifiable from an individual's NHS number. When using the online booking system to book a slot at a vaccination center, one had to enter their unique NHS number. By doing so, the computer system prevented individuals who were attempting to book a vaccine before their cohort became eligible. A full outline of the rollout by cohort can be found in Table 1. Once one group approached maximal up-take, or vaccine supply allowed, the next cohort became eligible. These nine at-risk groups, at the time, represented 99 percent of all COVID-19 deaths. The JCVI argued that an aged-based approach would allow for rapid deployment and high vaccine uptake compared to a strategy orientated around occupational groups. Following data from clinical trials on the efficacy levels, the JCVI advised that the second-dose of either vaccine should be offered with an interval of up to 12 weeks. In late February, the JCVI published interim advice that the vaccination program should continue along the age-based strategy, with the eventual vaccination of all adults aged 18–49 who were not eligible in phase 1. This guidance was not made final until early April 2021.

2.2. *The COVID-19 Setting During Survey and Distributional Impacts*

By the time of the eighth survey wave (i.e. March 24–31, 2021), the COVID-19 situation remained bleak. The United Kingdom had just spent the winter months under the most restrictive lockdown the government had imposed over the course of

TABLE 1
PRIORITY GROUPS AND VACCINE ROLLOUT

JCVI Cohort	Priority Group	Population (Millions)	Deaths Attributed to Cohorts (Actual Rollout Date)	
Care home residents	1	0.3	88%	December 8, 2020
Residential care workers	1	0.5		December 8, 2020
80+	2	3.3		December 8, 2020
Healthcare workers	2	2.4		January 9 and 14, 2021
Social care workers	2	1.4		January 9 and 14, 2021
75–79	3	2.3		January 18, 2021
70–74	4	3.2		January 18, 2021
Clinically extremely vulnerable (under 70)	4	1.2		January 18, 2021
65–69	5	2.9	11%	February 15, 2021
At risk (under 65)	6	7.3		February 15, 2021
60–64	7	1.8		March 1, 2021
55–59	8	2.4		March 6, 2021
50–54	9	2.8		March 17, 2021
Total priority group population		32	99%	

Note: The table is a reproduction of Table 2 from the "UK COVID-19 vaccines delivery plan" document (Department for Health and Social Care 2021).

the pandemic, which continued deep into the spring (Hale et al. 2021). This period of lockdown was also the most restrictive in Europe at the time; while those in France and Germany were able to move more freely, UK residents were confined to their homes with severe limits on social interaction. The vaccination campaign itself, however, became a beacon of hope during the winters gloom. The United Kingdom quickly began to average around 300,000 first vaccine doses per day (peaking one day when 1 percent of the population received a vaccine), and both cases and deaths began to fall to lower levels.

While the vaccine followed a regimented rollout, the effects of the pandemic were highly unequally distributed. A consistent finding has been marked differences between the effects on men and women. That is, women had significantly worse labor market outcomes (Adams-Prassl et al. 2020; Forsythe et al. 2020); suffered a higher than average increase in mental distress during the pandemic (Etheridge and Spantig 2022; Quintana-Domeque et al. 2021); and even female-owned businesses experienced a larger drop in activity (Fairlie 2020). Moreover, Adams-Prassl et al. (2020) find evidence that the pandemic replaced working hours with more time dedicated to active childcare or home schooling, which scales with the size of the household. These negative labor market outcomes for larger households are also found in the United States (Montenovo et al. 2020).

Not only are there differential impacts across individual characteristics, but also between regions in the United Kingdom. One example of this is the unequally distributed digital infrastructure and skills, which created frictions in working from home during the pandemic and, thus, worse economic outcomes. In London 75

percent of premises have ultra-fast internet but only 31 percent in Wales (Aitken et al. 2019); just 32 percent and 42 percent of individuals use the internet to find information for work or studying in Northern Ireland and Wales, respectively, compared to 59 percent in London and the South West (Bhattacharjee et al. 2020).

2.3. *Understanding Society COVID-19 Study*

Survey Design

We use data from the Understanding Society COVID-19 Study (University of Essex, Institute for Social and Economic Research 2021a). The COVID-19 Study is built upon Understanding Society and uses monthly online surveys to capture the experiences and behavior of 12,680 participants, from across all regions of the United Kingdom, during the course of the COVID-19 pandemic. Respondents were offered a small financial incentive for each web survey they completed, which had a 7-day fieldwork period with reminders to complete sent out on days 2, 3, and 6. The study covers aspects as diverse as economic insecurity, past experience with the COVID-19 disease, and risk of future infection; it also reports a host of individual characteristics and, crucially, their vaccination status. We opportunistically focus on wave 8. Wave 8 was, in fact, fielded between March 24 and 31, 2021—a period in which two proximate cohorts (50–54 and 45–49) had different eligibility to get the vaccine.

In the module on economic security, owing to the survey design, individuals are randomized at the household level to one of two types of windfall questions. One question refers to an unspecified, hypothetical windfall of £500, and the second refers to a public windfall, in which the government would pay everyone £500. We focus on the sample of individuals who answered the hypothetical windfall survey item since the aspects related to the government intervention arguably confound our focal effect. This means that we examine a sample containing information on 4,682 individuals aged between 25 and 75. We provide evidence in favor of this concern and details on how this choice affects our results in Online Appendix Section C.

Economic Insecurity Items

We use a set of outcomes to examine several dimensions of economic insecurity elicited in wave 8 of the survey. We report the full text of the questions that we use in our study in Online Appendix Section B. Summary statistics and short descriptions of the main variables are provided in Table 2.

Our main measure, which best matches our definition of economic insecurity, is derived from the responses to the question "Looking ahead, how do you think you will be financially 3 months from now?" We construct a dummy variable that takes value 1 if the respondent answered "Worse off than you are now" and 0 otherwise (i.e. "Better off than you are now" or "About the same"). In our sample, about one-tenth of respondents (0.099) perceive the future as being worse financially (see Table 2). However, as expected, we find that this share is smaller in interviewees aged 50, or above, (0.084) and relatively larger in those aged 49, or below, (0.111).

We also examine specific domains of economic insecurity: the perceived level of financial fragility; job insecurity; and the anxiety produced by a perceived incapacity of being able to meet ordinary expenses (like usual bills) in the near future.

TABLE 2
SUMMARY STATISTICS AND VARIABLE DEFINITIONS

	Mean	Standard Deviation	Minimum	Maximum	Observations	Description
Panel A: Economic Insecurity Outcomes						
Economic insecurity	0.099	—	0	1	959	=1 if the respondent answered "Worse off than you are now" to the question "Looking ahead, how do you think you will be financially 3 months from now, will you be …", 0 otherwise
Perceived financial fragility	0.269	—	0	1	959	=1 if the respondent answered "Finding it quite difficult" or "Finding it very difficult" to the question "How well would you say you yourself are managing financially these days? Would you say you are …", and 0 otherwise
Expected trouble from ordinary expenses (standardized)	0	1	−0.456	4.832	948	[O]n a scale of 0 to 100 percent how likely do you think it is that you will have difficult paying your usual bills and expenses in the next 3 months?
Job insecurity (standardized)	0	1	−0.460	5.094	801	[O]n a scale of 0 to 100 percent how likely do you think it is that you will lose your job
Panel B: Other Outcomes						
Risk of getting COVID-19 (standardized)	0	1	−1.238	4.048	958	Derived from the question "In your view, how likely is it that you will contract COVID-19 in the next month?", where 4 is "Very likely" and 1 is "Very unlikely"
People in this neighborhood can be trusted	0.684	—	0	1	958	=1 if the respondent answered "Strongly agree" or "Agree" to the statement "People in this neighborhood can be trusted", and 0 otherwise
Am similar to others in neighborhood	0.555	—	0	1	957	=1 if the respondent answered "Strongly agree" or "Agree" to the statement "I think of myself as similar to the people that live in this neighborhood", and 0 otherwise
People willing to help their neighbors	0.737	—	0	1	956	=1 if the respondent answered "Strongly agree" or "Agree" to the statement "People around here are willing to help their neighbors", and 0 otherwise
GHQ-12 (standardized)	0	1	−1.917	3.886	955	GHQ-12 Likert scale of mental distress
Panel C: Explanatory Variables						
Age ≥ 50 (running variable)	0.236	2.838	−5	5	959	The age of the respondent relative to age 50, that is, the eligibility cut-off
Vaccinated	0.589	—	0	1	959	=1 if the respondent has received at least 1 dose of a COVID-19 vaccination at the time of interview, 0 otherwise
Eligible	0.535	—	0	1	959	=1 if the respondent is eligible for a vaccination, that is, aged 50 or above

In our sample, we observe that around one-fourth of respondents (0.269) declare that they find it difficult to manage their immediate financial situation. The other two dimensions we explore are closely related to the expectation of job loss or of a large drop in wealth. Arguably, these two expectations remained low in the United Kingdom thanks to the implementation of policies that curtailed the loss of jobs and maintained self-employed people's income level near to normal. Accordingly, we observe low average scores, within the 0–100 domain, respectively, of 8.39 and 8.65.

Individual and Household-Level Characteristics

Importantly, the COVID-19 Study allows us to identify those individuals who have received a vaccine against COVID-19. We define vaccination status as whether a respondent has had at least one dose of the COVID-19 vaccine.[4] In our sample, around 70 percent of respondents (0.695) are inoculated with substantial variation by age groups. Using information on the respondent's age we are able to determine the cohorts' eligibility for vaccination during the survey rollout. As expected, vaccine uptake is larger in eligible cohorts (the mean share is 0.913).

To assess the identifying assumptions of our identification strategy and to explore heterogeneous treatment effects, we define a broad set of variables that capture individual and household-level characteristics. Specifically: gender; whether the respondent has a partner; whether they have children aged 5–18; household size; their gross income; whether the respondent has any savings; experienced a decline in net wealth during the pandemic; any non-mortgage debt; whether they have had COVID-19; and whether they are clinically vulnerable. We are also able to construct variables that capture one's belief about the risk of catching COVID-19 and pro-social attitudes—these are discussed in more detail later. Full summary statistics and definitions for these variables can be found in Online Appendix Table A1. We note that continuous variables, such as the household size and gross income, are standardized to have mean of 0 and standard deviation equal to 1.

3. Empirical Strategy

Assessing the effects of the COVID-19 vaccination campaign on people's beliefs is empirically challenging. For one thing, vaccine uptake was not made mandatory by the UK authority, but the decision on whether to get the jab was left to the individual. Arguably, such a decision is likely to be influenced by predetermined individual characteristics and tastes (Argote et al. 2021; Dabla-Norris et al. 2021; Borga et al. 2022). For another thing, vaccine compliance is strongly influenced by the environment, that is, where and when this individual decision is

[4]During the survey rollout, cohorts aged 70 and over were eligible for a second dose. Unfortunately, we observe only a tiny fraction of inoculated respondents aged 70 or over. We, therefore, lack statistical power to estimate the impact of the second jab.

taken. For example, Archibong and Annan (2021), A. Banerjee et al. (2021), Hansen and Schmidtblaicher (2021), Lowes and Montero (2021), and Martinez-Bravo and Stegmann (2021) document that propaganda, the authority's miscommunication, medical malpractice, or the campaign design have strong effects on vaccine compliance. Simply regressing economic insecurity on the individual decision of getting the COVID-19 vaccine will therefore lead to a biased estimation due to an underlying endogeneity problem.

In this paper, we exploit plausibly exogenous variation in vaccine compliance offered by a quasi-natural experiment provided by the rules behind the UK's vaccination campaign. As explained in Section 2.1, the campaign proceeded according to a tight schedule primarily based on age, granting differential eligibility to proximate cohorts within a given time frame. Specifically, we exploit the fact that during the Understanding Society COVID-19 Study interviews individuals aged 50 and over were eligible for vaccination, while those aged 49 and under were not. We illustrate our empirical strategy in Figure 1. The horizontal spikes depict the eligibility period by age cohort across the first 5 months of 2021. The vertical shaded area displays the survey rollout period (March 24–31, 2021). During the survey administration there were five cohorts, older than 50 years, eligible for vaccination (in red); younger ones were ineligible (in blue). We therefore use this design to instrument the individual decision to get one dose of the vaccine (described by the dummy *Vaccinated*) with eligibility to vaccination (described by the dummy *Eligible*). Formally, our Two Stage Least Square (2SLS) approach can be written as follows:

$$y_i = f(Age^*) + \beta\ Vaccinated_i + X_i'\gamma + \varepsilon_i, \tag{1}$$

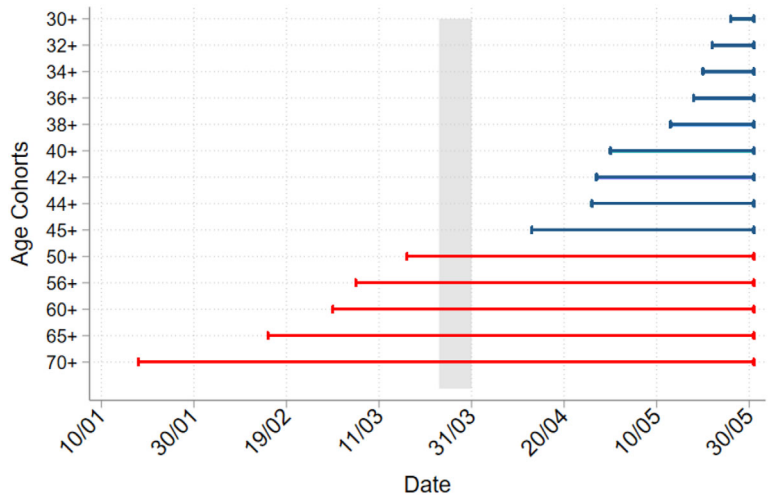

Figure 1. Eligibility by Cohort and Date.
Notes: The shaded area indicates the Understanding Society COVID-19 Study schedule of interviews (i.e. March 24–31, 2021), while horizontal bands depict the period of eligibility of each cohort. Cohorts highlighted in red are the ones eligible for vaccination during the survey period [Colour figure can be viewed at wileyonlinelibrary.com].

where exogenous variation in vaccination is obtained through the estimation of the following first-stage equation:

$$Vaccinated_i = g(Age^*) + \theta\ Eligible_i + X_i'\gamma + u_i. \tag{2}$$

In both regressions, i indicates interviewees; y_i is the outcome of interest (either of the four measures of economic insecurity); $f(\cdot)$ and $g(\cdot)$ are polynomial functions of the assignment variable, Age^*, which is one where Age is above or equal to 50. ε_i and u_i are the error terms.

The identification of β requires that eligibility for immunization only affects vaccination. In Section 5.2, we inspect a set of 11 individual characteristics and check their behavior around the cutoff of 50. While we do not find evidence of any jump for 10 out of 11 variables, we obtain a relatively larger share of households who experienced a drop in wealth, since the start of the pandemic, in the cohorts that are 50 or just older. In Online Appendix Section D, we provide considerable evidence that this discontinuity is likely to be a false positive. However, to be conservative in our regressions we control for whether the respondent has experienced a drop in wealth. In Online Appendix Table A2, we also show regressions where we omit this variable and our results are qualitatively unchanged. In the vector X_i we also include country fixed effects (England, Northern Ireland, Scotland, and Wales), which captures the potential differences in vaccine deployment and uptake rate.

Figure 2 illustrates the first-stage relationship by plotting the proportion of respondents who got inoculated across age cohorts. The blue line is a third-order polynomial fit of the individual-level data for the cohorts aged 49 and under (i.e. those ineligible at the time of the survey rollout). The red line is the fit for the cohorts aged 50 and over (i.e. those eligible at the time of the survey rollout). The shaded area displays the 95 percent confidence level of the fit. Even graphically, it is easy to appreciate how eligibility for vaccination translates into a significant difference in the proportion of people who had at least one jab.[5]

One issue we potentially face with the above-described instrumental variable is that eligibility might correlate with the error term—an issue that would violate our exclusion restriction. Eligible individuals are in fact also older and typically shielded by a higher level of economic security.[6] It would be therefore difficult to identify β as the causal effect of vaccination on economic insecurity by using equation (1). To overcome this issue, we therefore zoom into the neighborhood around the cutoff and turn to a fuzzy RDD, which compares individuals with approximately same age but differential eligibility. In our main specification we use a 5-year symmetric

[5]In Online Appendix Table A3, we estimate a statistically significant difference in the share of vaccinated individuals, between cohorts aged 50–54 and 45–49, which is equal to around 22 percentage points (column 1). When we analyze the full sample (column 2), comparing the cohorts aged 50–75 and 25–49, we obtain a difference of 0.29.

[6]Based on an index of economic insecurity that captures both large economic losses and weak financial buffers, Hacker et al. (2014) document that Americans became more economic insecure during the 2008–2010 period, with the households headed by young adults (18–34) being by far the most insecure age group. Financial fragility and a larger exposure to anxiety by the youngest has also been documented during the COVID-19 pandemic in the United States (Clark et al. 2021), in Catalonia, Italy, and the United Kingdom (Quintana-Domeque et al. 2021), as well as globally (Varma et al. 2021).

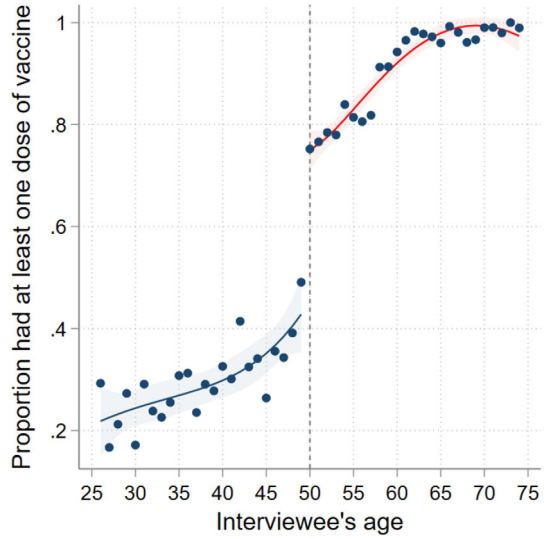

Figure 2. Share of People Who Received the COVID-19 Vaccine by Age.
Notes: The figure scatters the share of people who received the COVID-19 vaccine by age between 25 and 75. A third-order polynomial fit of the individual data is also reported with the surrounding area indicating the 95 percent level of confidence.

bandwidth, around the cutoff of 50, which is the typical length of the eligible cohort set out for vaccine rollout (see Table 1). This means that we estimate β by comparing the cohort aged 50–54 with the cohort aged 45–49. Our identifying assumption is therefore that the vaccine delivery plan is *locally* (i.e. in the proximity of the cutoff) as good as a random instrument, which implies that being 50 years old or just over, rather than 49 or just under, between March 24 and 31, 2021, is a matter of chance. In Section 5, we will discuss and provide evidence in favor of our identifying assumption and show estimations of β varying the bandwidth around the cutoff or using an optimal bandwidth that trades off accuracy and consistency of the estimate (Calonico et al. 2014). We note, at this stage, that there are no age-specific welfare policy changes that occur at this cutoff.

3.1. *Interpreting the RDD Coefficient*

Before we explore the results of our analysis it is worth discussing the interpretation of β. There are three important aspects that are worth remarking upon. First, we note that eligibility does not switch on the treatment status deterministically: some individuals aged 50 or over may still decide to not get inoculated; likewise, others who are younger than 49 could get the jab because they are clinically vulnerable or medical staff. Imperfect compliance thus turns our regression design into a fuzzy one, where the discontinuity in eligibility serves as an instrument for the probability of getting one dose of the vaccine. Our coefficient β has, therefore, to be interpreted as a LATE—that is, the causal effect of being eligible for vaccination on the level of economic insecurity of individuals who actually got the jab (*compliers*).

Our results are therefore not informative about individuals who did get vaccinated despite not being eligible.[7]

Second, we note that effects estimated through a regression discontinuity design are local because they are estimated in the proximity of the cutoff (Angrist and Pischke 2008 p. 263). Comparing cohorts that have almost the same age has the merit of improving the internal validity of our study. However, this is likely to come at the cost of being able to generalize our results. For this reason, in the Online Appendix, we provide estimates based on the full sample as they are arguably externally valid for the entire UK population at a cost of some internal validity.

Third, it could have been that households yet not eligible for vaccination (i.e. in the cohorts 45–49) developed an expectation of protection because they knew they were soon to be protected. Anticipating protection means that they had, at least partially, updated their beliefs about their level of economic insecurity despite not being inoculated. This anticipatory effect is likely to generate a bias *toward zero* in our estimates. Therefore, our coefficient has to be interpreted as a lower bound of the true effect of immunization on economic insecurity.

4. Results

We begin by inspecting graphically the relationship between the level of economic insecurity and the running variable, age—that is, the reduced form relationship. Figure 3 illustrates the share of respondents who perceive their financial situation in the next 3 months as worse than now across age cohorts, from 25 to 75. Dots mark shares of respondents, within a given age, who feel anxious about their future; the blue curve is a third-order polynomial fit of the mean shares for the cohorts aged 49 or under; the red one is a third-order polynomial fit of the mean shares for the cohorts aged 50 or over. We illustrate RDD graphs by fitting data using a third-order polynomial fit that well describes the data without providing complex patterns. For consistency, we also control for a third-order polynomial in age in regressions that use the whole sample. The vertical dashed line marks the cutoff at the age of 50. The gray shaded areas denote the 5-year bandwidth around the threshold. As one can see, in a neighborhood of the cutoff, the red curve lies below the blue one in both cases, suggesting that eligibility for vaccination locally generates a reduction in economic insecurity. We test rigorously this in a regression framework that is used to estimate, in a neighborhood of the cutoff, the effect of eligibility on economic insecurity. In column 1 of Table 3, we therefore set a 5-year

[7]Put it differently, our analysis does not provide an estimate of the average treatment effect (ATE) of vaccination on economic insecurity, which is the weighted average of effects on *compliers* (i.e. the LATE) and on *always-takers* (Angrist and Pischke 2008, p. 159). The latter group is comprised of households who got inoculated despite being younger than 50. In our context, they include medical staff as well as vulnerable people. To make an idea of their proportion, we count those who get a jab in the non-eligible age cohorts 45–49. They account for 37 percent of them, 29 percent of whom are vulnerable. However, the average shares of individuals who declare being economically insecure in these cohorts, between vulnerable and non-vulnerable, are not considerably different as they are 0.116 and 0.104, respectively. Therefore, while the estimated LATE could be moderately smaller than the ATE, we do not expect this difference to be large.

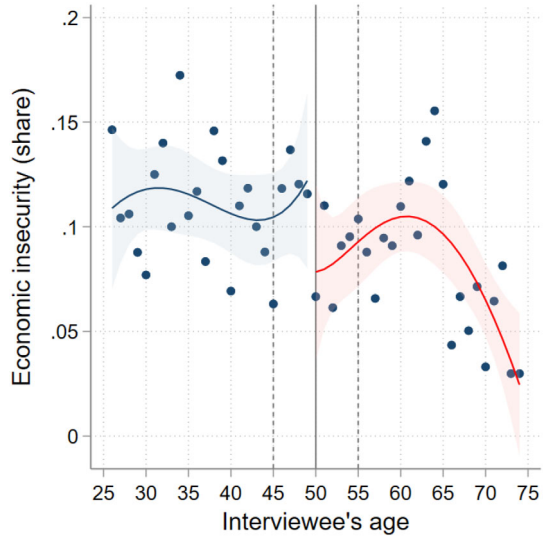

Figure 3. Perceived Economic Insecurity Levels by Age.
Notes: The figure scatters the share of individuals who feel economically insecure by age cohort. The blue and red curves are third-order polynomial fits of the mean shares on either side of the threshold, 50 years of age. The shaded areas indicate the 95 percent confidence interval. The 5-year bandwidth around the threshold is marked by the two dashed vertical bars.

bandwidth and, following Gelman and Imbens (2019), we control for a linear trend in age. In addition to that, we include the dummy that is 1 if the respondent has experienced a drop in wealth during the pandemic and a set of country fixed effects. The estimated coefficient is −0.091, and it is statistically significant at the 5 percent level of confidence. It indicates that the share of respondents feeling economically anxious about their future finance is 9.1 percentage points less in the cohorts aged 50–54, relative to those aged 45–49.

In columns 2 and 3 of Table 3, we move to the examination of the effect of vaccine intake on perceived economic insecurity. We first estimate the effect through ordinary least squares (OLS) (column 2). The estimated coefficient is negative (−0.015), but it is not statistically different from zero (standard error = 0.023). We warn against using OLS as this estimate is likely affected by a selection bias. As we argued earlier, vaccine uptake was not made mandatory, and individuals self-selected into treatment. We argue that those who did not self-select have specific characteristics that make them the most likely to react the most had they self-selected into treatment. For example, individuals with a higher level of economic insecurity are shown to have lower levels of trust in institutions and are therefore less likely to get vaccinated (Argote et al. 2021). Also, vaccine hesitancy is found to be larger in younger individuals (Dabla-Norris et al. 2021) who are the cohorts who exhibit higher levels of economic insecurity.

We therefore move on by instrumenting the decision to get the vaccine using eligibility in a fuzzy RDD described in equation (1). Our 2SLS estimate of β is −0.414, and it is, as expected, larger than the OLS estimate. Importantly, it is now

TABLE 3
ECONOMIC INSECURITY AND COVID-19 IMMUNIZATION: MAIN ESTIMATES

	Economic Insecurity		
	Reduced Form (1)	OLS (2)	2SLS (3)
Eligible	−0.091**		
	(0.039)		
Vaccinated		−0.015	−0.414**
		(0.023)	(0.206)
Polynomial	1	1	1
Bandwidth	5	5	5
Observations	959	959	959

Notes: The dependent variable takes the value 1 if the respondent answered "Finding it quite difficult" or "Finding it very difficult" to the question "How well would you say you yourself are managing financially these days? Would you say you are … ", and 0 otherwise. 2SLS estimates use eligibility as an instrument for having been vaccinated. All columns control for country fixed effects as well as for whether the respondent has experienced a drop in wealth during the pandemic. Heteroskedastic robust standard errors are reported in parentheses.

*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

statistically significant at the 5 percent level (column 3); The estimated coefficient is also economically significant indicating that, because of immunization, the share of respondents feeling economically anxious about their future finance dropped by two-fifths in the cohorts aged 50–54, relative to those aged 45–49. We show the robustness of our 2SLS results in Online Appendix Section E, by relaxing the exogeneity assumption using the test provided by Conley et al. (2012).

In Online Appendix Table A4, we replicate our analysis on the full sample of respondents. As one can see, the results are qualitatively unchanged, but the magnitudes are smaller. This is likely due to the fact that our baseline analysis (which uses the restricted sample) excludes the eldest cohorts that, as explained in Section 3, are the most financially secure and, because of that, the ones who were likely to react less to the immunization.

We additionally inspect variation in specific domains of economic insecurity, such as perceived financial fragility, anxiety from failing to meet ordinary expenses, and the fear of losing the job. The three illustrations in Online Appendix Figure A1 show the binned scatter plots as in Figure 3. This time, however, there is no visible discontinuity in both panels. In Online Appendix Table A5, we document that vaccination has a persistent negative sign on economic insecurity; however, the estimated effect is imprecise for these specific dimensions. The fact that the effect of vaccination on the respondents' perceived level of financial fragility is not robust is something we interpret as households holding rational expectations; households anticipated that physical protection against COVID-19 requires the time necessary for the antibodies to develop, typically several weeks after the jab, and accordingly assigned less pessimism to 3 months ahead rather than today. The non-impact of the vaccines on the domains of managing ordinary bills and job insecurity instead suggests that vaccination was not sufficiently salient for those dimensions of economic insecurity that were only marginally affected by the pandemic in the context of the United Kingdom thanks to the prompt government intervention.

5. Validation Analysis

As explained in Section 3, our identifying assumption is that the vaccine delivery plan is as good as a random instrument, in the proximity of the cutoff, which implies that being 50 years old or just over, rather than 49 or just under, is a matter of chance. Therefore, the plan should affect the interviewees' level of economic security only through their reception of the COVID-19 vaccine. While this exclusion restriction is typically difficult to test, in this section we offer a battery of additional analyses to validate our results. In the Online Appendix, we present two further checks that show robustness to the relaxation of the instrumental variable (IV) exclusion restriction using Conley et al. (2012) test (Section E) and that our estimates are not sensitive to the specific way we cluster standard errors (Section F).

5.1. *Treatment Manipulation*

A typical threat to RDDs is manipulation of the assignment variable by the interviewees. This threat is, however, less worrisome in designs where age is the assignment variable (e.g., Lee and Lemieux 2010), and we argue that it is indeed unlikely to be active in our setting. Eligibility strictly followed age; the only exceptions were made for vulnerable individuals or members of a medical staff. So, a bump on the right-hand side of the cutoff can occur only if, for some reason, we find evidence of a relatively larger number of vulnerable and/or medical staff within the 50–54 cohort. To rule out this possibility in Figure 4 we provide a density test as suggested in McCrary (2008). Specifically, Figure 4 plots the frequency of interviewees across age and draws a local polynomial smoothed fit of the individual-level data as well as the area where the fit is statistically significant at the 95 percent level. Reassuringly, the plot shows no jump around the cutoff. We confirm this with the McCray test, which produces a p-value of 0.87 (0.48) globally (locally, ±5 years) around the cutoff. We, therefore, conclude that no manipulation has occurred.

5.2. *Inspecting Covariates*

For the vaccine delivery plan being locally as good as a random instrument, it must be that any other household's characteristics, determined before the realization of the assignment variable, balance locally on both sides of the cutoff. We examine this possibility in this section. We begin by conducting a parallel graphical RD analysis on each of the baseline covariates. This graphical analysis is illustrated in Online Appendix Figures A2, A3, and A4. Specifically, Figure A2 displays the trend for the first group of household's characteristics including: a dummy indicating whether the interviewee is female (panel *a*); a dummy for whether the respondent is non-British (panel *b*); a dummy for whether the respondent has a partner (panel *c*); a dummy for whether they have children aged 5–18 (panel *d*); the household size (panel *e*). A second group, whose trend is displayed in Figure A3, describes household's financial characteristics, that is, the gross current household income in pounds sterling, GBP (panel *a*); a dummy indicating whether the interviewee has experienced a drop in wealth since the start of the pandemic (panel *b*); a dummy indicating whether the interviewee holds any savings (panel *c*); and a dummy for whether holds any

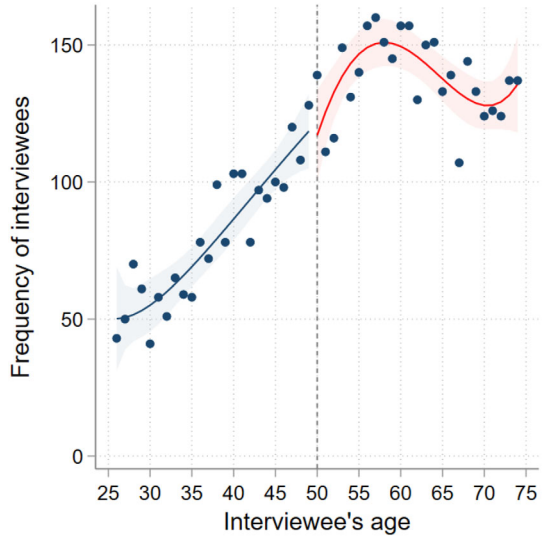

Figure 4. Frequency of Interviewees by Age.
Notes: The figure scatters the frequency of interviewees, in our sample, by age. A third-order polynomial fit of the individual data is also reported with the surrounding area indicating the 95 percent level of confidence.

non-mortgage debts (panel *d*). The last group (Figure A4) includes a dummy indicating whether the respondent experienced a prior COVID-19 infection (panel *a*) and a dummy for whether the individual is categorized as a vulnerable person (panel *b*). Each panel displays the mean value as well as a third-order polynomial fit of the underlying data points. We also add confidence intervals to the fit, at the 95 percent level, to appreciate whether the discontinuity around the cutoff is statistically significant. This graphical analysis suggests that the two samples of interviewees (just above and just below the age of 50) balance along these observable characteristics, with the exception of the drop in wealth after the COVID-19 pandemic. We corroborate this graphical evidence with the estimates of the discontinuity around the cutoff. These estimated coefficients are reported in Figure 5. They confirm that interviewees aged 50, or just more, experienced a relatively larger drop in wealth caused by the pandemic—discontinuity that is precisely estimated when we use non-parametric methods (see panel *b* of Figure 5).[8]

We have no theory for why households who are 50, or just above, were more likely to experience a loss in wealth than those who are 49, or just under. For example, the Job Retention Scheme applied for both cohorts; and so did the Self Employment Income Support Scheme. All the policies or circumstances, we are aware of, did not treat people differently just above and below the age of 50. We therefore ask ourselves whether this significant discontinuity is a *false positive*. In Online Appendix Section D, we report considerable evidence in favor of this hypothesis by combining multiple testing with a Bonferroni correction

[8]The estimated discontinuity in the drop in wealth is 0.068 (standard error = 0.039) when using non-parametric methods, and it is 0.055 (standard error = 0.037) when using parametric methods.

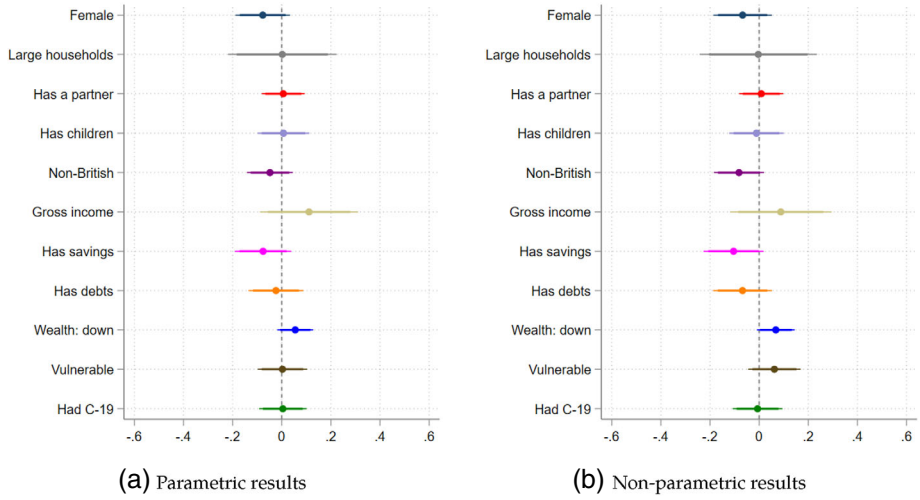

Figure 5. Parametric and Non-parametric Results for Covariates.
Notes: The figure reports the estimated discontinuity in the level of each characteristic between interviewees aged 50, or just above, and those aged 49, or just below. Parametric results are estimated using a local linear regression; non-parametric results are estimated using a LOWESS function as described in Calonico et al. (2014).

or by estimating a Seemingly Unrelated Regression. We conclude that the above discontinuity in the drop in wealth is likely to be due to chance.

5.3. *Pre-Treatment Trends*

A common validation exercise is to test whether there is a jump where there should be no discontinuity (Imbens and Lemieux 2008). For this, we create a placebo eligibility threshold at the median age of those below the true eligibility criteria, which is age 37.5. We estimate this placebo test for our economic insecurity outcome; the results are presented in Table A6. In column 1, we use the full sample of individuals; in column 2, we introduce a 5-year bandwidth to estimate the local placebo effect. In both columns, as expected, we estimate a jump that is very small and not statistically different from zero. As a further test of pretreatment trends, we exploit the full age range below the true cutoff. For every 5 years to the left of the cutoff, we examine whether individuals differ either side of this placebo threshold. For example, we estimate whether individuals aged 25–29 differ in their future financial security to those aged 30–35, and so on. The results are shown in Figure A6. All point estimates are close to zero and not significantly different from 0. Overall, we do not find any evidence of pretreatment trends. These tests lend further credibility to our main result that vaccinations are having an estimable effect on economic insecurity.

5.4. *Placebo Analysis*

To ensure validity, we conduct a placebo analysis to investigate if the change in economic insecurity was truly caused by COVID-19 immunization or by chance. We

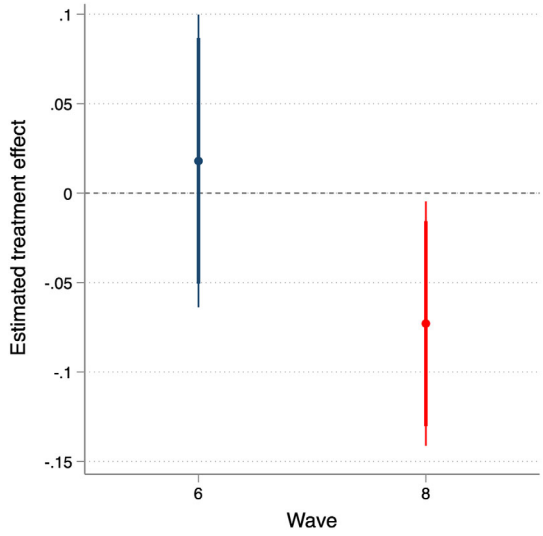

Figure 6. Placebo Analysis.
The figure shows the local average treatment effect on whether an individual thinks their financial situation will be worse in the next 3 months from a reduced form RDD for the below and above 50 years of age in wave 8 (March 2021) and in wave 6 (November 2020) as a placebo test. Thicker spikes denote the 90 percent confidence interval, while thinner ones denote the 95 percent confidence interval.

utilize data from a previous survey conducted in November 2020 (wave 6), where the same question was asked. It's worth noting that wave 6 is the only survey where data on households' financial management beliefs in the future were collected, alongside wave 8.

At that time, the vaccination campaign was yet to start; therefore, a statistically significant discontinuity in the level of economic insecurity between cohorts aged 50 or just over and those aged 49 or just under, estimated using data from wave 6, would indicate that our findings are probably not the effect of vaccination, but that such discontinuity randomly picks up some other effects. We present this placebo analysis graphically in Figure 6, by drawing the point estimations of the reduced form effect. The red dot depicts the point estimation using wave 8; the blue dot is obtained from wave 6. Spikes mark the confidence intervals around the point estimation with thicker ones representing the 90 percent level of confidence and thinner ones the 95 percent level. Reassuringly, we find no discontinuity around the cutoff when using wave 6, resulting in a null effect reported in Figure 6. Online Appendix Figure A5 also reports a graphical RD analysis of the share of respondents who think their financial situation will be worse in 3 months ahead using responses in wave 6. As expected, no jump appears around age 50.

5.5. *Varying Bandwidth*

We further explore the sensitivity of our findings by checking whether the bandwidth choice affects them. In Online Appendix Figure A7, we therefore plot the

local linear discontinuity estimate against a continuum of bandwidths as suggested by Card et al. (2008). The light gray bands represent the 95 percent confidence intervals, while the darker ones represent the 90 percent confidence intervals. The plot also reports two reference point estimates: the baseline estimate of the LATE obtained by setting the bandwidth at five (in red) and the estimated LATE using the optimal bandwidth provided in Calonico et al. (2014). As one can see, our estimate does not vary much in terms of the magnitude of the effect. As expected, statistical precision increases when we enlarge the bandwidth over 5 years at the expense of the estimation's accuracy.

6. Interpretation of Results

6.1. *Heterogeneity Analysis: Who Updated the Most?*

In this section, we explore heterogeneous responses from immunization against COVID-19 using a difference-in-discontinuity setup (see, e.g., Lalive 2008; Leonardi and Pica 2013; Casas-Arce and Saiz 2015; Grembi et al. 2016; Bazzi et al. 2020). We focus on the analysis using the reduced form relationship between the interviewee's age and the perceived level of economic insecurity by adding on top of the discontinuity in age a difference in some predetermined individual characteristics. The difference component of the design, therefore, helps to isolate groups of respondents for which immunization has been the most salient in lessening economic insecurity.

Figure 7 illustrates the idea behind the identification strategy for each of the 11 characteristics we study (in order of appearance: (*a*) gender; (*b*) whether white British; (*c*) having a partner; (*d*) having children aged 5–18; (*e*) a household larger than 4; (*f*) a gross income higher than the median; (*g*) having experienced a decline in net wealth during the pandemic; (*h*) having any savings; (*i*) having any non-mortgage debt; (*j*) having had COVID-19; (*k*) being clinically vulnerable). In panel (*a*), for example, we compute the difference in the level of economic insecurity held by female respondents and those held by male respondents, within an age cohort. The pattern of the bins reveals that women's economic insecurity increases by age across non-eligible cohorts, relative to men's. The trajectory has a break at 50 years of age and evolves in a flattening fashion across eligible cohorts. Other panels similarly illustrate a discontinuity in the evolution of the difference, suggesting that eligibility to vaccination was relevant for explaining differences in economic insecurity.

We use a difference-in-discontinuity regression framework to rigorously test whether these differences are statistically significant. We capture the difference component using the dummy Z^k, which is coded with one for respondents that have a certain characteristic k (where k is one of the above-mentioned features). This dummy then interacts with *Eligible* to single out respondents with both that characteristic and vaccination eligibility. This is formally written as follows:

$$y_i = g(Age^*) + \theta_1 \, Z_i^k + \theta_2 \, Eligible_i + \theta_3 \, (Eligible_i \times Z_i^k) + X_i'\gamma + v_i, \tag{3}$$

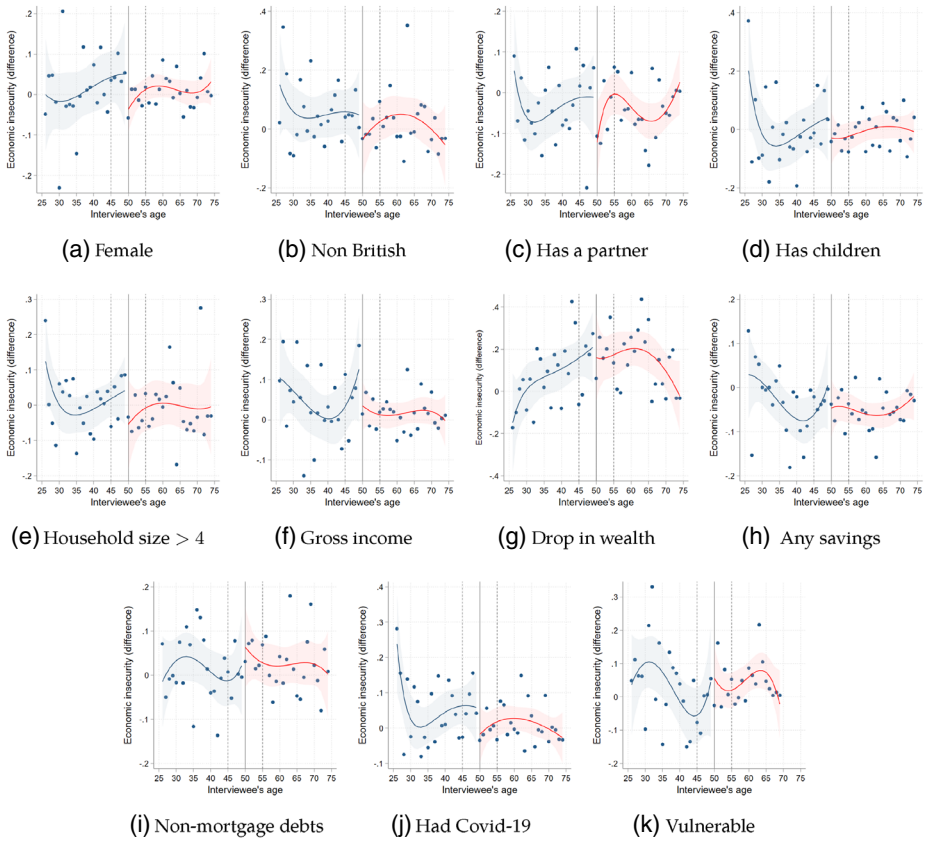

Figure 7. Difference-in-Discontinuity Plots.
Notes: The figure scatters the difference in the shares of individuals who feel economically insecure and have a certain characteristic and individuals who feel economically insecure and do not have that characteristic, within an age cohort. The blue and red curves are third-order polynomial fits of the mean shares on either side of the threshold, 50 years of age. The shaded areas indicate the 95 percent confidence interval. The 5-year bandwidth around the threshold is marked by the two dashed vertical bars.

where i indicates interviewees and y_i the dummy that indicates whether the interviewee feels economically insecure. Our interest is in the difference-in-discontinuity parameter, θ_3, that captures the discontinuity between eligible and non-eligible individuals and the difference in the share of individuals who feel economically insecure between the two groups. We estimate this parameter using a 5-year bandwidth, for each characteristic k.

The identification of the difference-in-discontinuity coefficient relies on the fact that the two groups' levels of economic insecurity follow a parallel trend, which is only broken by the vaccination eligibility rule. This is similar to the standard identifying assumption in difference-in-differences; however, here the parallel trend must be met in the neighborhood of the cutoff and is, therefore, more local (Grembi et al. 2016). In Online Appendix Figure A8, we illustrate graphically the pattern of the point estimates from a set of event studies, where the difference-in-discontinuity

coefficient is allowed to vary between the cohort aged 45 and the cohort aged 54. Reassuringly, no particular pattern emerges across non-eligible cohorts in any of the 11 panels.

We present estimation results in Tables 4, 5, and 6. Households' main characteristics are explored in Table 4. Column 1 examines gender differences in economic insecurity. Its estimates indicate that the share of female respondents reporting pessimist financial feelings about the future is 0.055 larger than the corresponding share of men (which is 0.088). This difference is also statistically significant at a 5 percent level of confidence. Turning to the difference-in-discontinuity coefficient, we obtain a negative and statistically significant estimate, which indicates that eligibility to vaccination reduces this difference by 0.068 percentage points. These findings are in line with recent research both documenting a larger impact of the pandemic recession on women's employment (e.g., Adams-Prassl et al. 2020; Alon et al. 2021; Campa et al. 2021; Fabrizio et al. 2021; Stantcheva 2021)

TABLE 4
Difference-in-Discontinuity Estimates: Households' Main Characteristics

	Economic Insecurity				
	(1)	(2)	(3)	(4)	(5)
Eligible	−0.015	−0.044	−0.027	−0.002	−0.030
	(0.039)	(0.036)	(0.055)	(0.042)	(0.037)
Female	0.055**				
	(0.027)				
Eligible × Female	−0.068*				
	(0.035)				
Non-British		0.049			
		(0.038)			
Eligible × Non-British		−0.062			
		(0.046)			
Has a partner			−0.025		
			(0.037)		
Eligible × Has a partner			−0.038		
			(0.049)		
Has children				0.047*	
				(0.028)	
Eligible × Has children				−0.079**	
				(0.036)	
Large household					0.025
					(0.028)
Eligible × Large household					−0.061*
					(0.035)
Observations	1,160	1,154	1,161	1,161	1,161

Notes: The dependent variable in all columns takes the values 1 if the respondent answered "Worse off than you are now" to the question "Looking ahead, how do you think you will be financially 3 months from now, will you be…", and 0 otherwise. All columns include country fixed effects. Estimates are obtained from a reduced form regression within the 5 year bandwidth around the cut-off. Heteroskedastic robust standard errors are reported in parentheses.

*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

TABLE 5
Difference-in-Discontinuity Estimates: Households' Financial Characteristics

	Economic Insecurity			
	(1)	(2)	(3)	(4)
Eligible	−0.049	−0.051	−0.070*	−0.082**
	(0.034)	(0.039)	(0.036)	(0.035)
Low income	0.074**			
	(0.035)			
Eligible × Low income	−0.052			
	(0.043)			
Has savings		−0.029		
		(0.027)		
Eligible × Has savings		−0.021		
		(0.035)		
Has debt			0.010	
			(0.028)	
Eligible × Had debt			0.031	
			(0.037)	
Wealth: down				0.112*
				(0.061)
Eligible × Wealth: down				0.087
				(0.080)
Observations	1,161	1,155	1,152	1,095

Notes: The dependent variable in all columns takes the values 1 if the respondent answered "Worse off than you are now" to the question "Looking ahead, how do you think you will be financially 3 months from now, will you be … ", and 0 otherwise. All columns include country fixed effects. Estimates are obtained from a reduced form regression within the 5 years bandwidth around the cut-off. Heteroskedastic robust standard errors are reported in parentheses.

*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

and a gender-based differential reaction to financial treatments (e.g., Gray et al. 2021).

In column 2, we further examine differences based on ethnic group belonging.[9] We find weak evidence that non-British households hold a lower level of economic insecurity, relative to white British ($\hat{\theta}_1$ is not statistically different from zero). Furthermore, while the difference-in-discontinuity coefficient is negative, the estimate is imprecise and not statistically significant. Likewise, we find no effect of eligibility for vaccination on households having a partner, relative to single households (column 3). Large households with children aged 5–18 are particularly affected by the rule. Households with children are in general more anxious about their future financial situation, relative to families with no child (column 4). The estimated coefficient is 0.047, and it is statistically significant at the 10 percent level. Importantly, we find that this difference reduces by 0.079 points for eligible households with children. This is in line with Agrawal et al. (2021) findings, which indicate

[9]There is surprisingly little attention on minority groups' economic insecurity in the literature. In a sample of six post-Soviet countries, Ivlevs (2014), for example, finds limited evidence that forward-looking measures of insecurity about hunger, healthcare protection, and housing differ between ethnic minorities and the majority of the population. We are not aware, however, of any studies on a differential reaction of these two populations following a shock.

TABLE 6
Difference-in-Discontinuity Estimates: Households' Health Characteristics

	Economic Insecurity	
	(1)	(2)
Eligible	−0.040	−0.074**
	(0.035)	(0.035)
Had COVID-19	0.065*	
	(0.035)	
Eligible × Had COVID-19	−0.062	
	(0.044)	
Vulnerable		−0.014
		(0.031)
Eligible × Vulnerable		0.054
		(0.041)
Observations	1,153	1,159

Notes: The dependent variable in all columns takes the values 1 if the respondent answered "Worse off than you are now" to the question "Looking ahead, how do you think you will be financially 3 months from now, will you be…", and 0 otherwise. All columns include country fixed effects. Estimates are obtained from a reduced form regression within the 5 years bandwidth around the cut-off. Heteroskedastic robust standard errors are reported in parentheses.

*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

that vaccines reduced anxiety and depression, particularly within households with children.

Similarly, we find that eligible large households have a lower level of economic insecurity (the estimated difference-in-discontinuity coefficient is −0.061, and it is statistically significant), though we obtain weak evidence on a different level of anxiety between large and small households (column 5). These last findings are consistent with the recent and growing literature concerning the harsh consequences the pandemic particularly laid on families with children: school closures, loss in children's educational skills, and work-from-home facility have triggered anxiety in parents about their ability to cope with the burden brought forward by the pandemic (e.g., Agostinelli et al. 2020; Fuchs-Schündeln et al. 2020; Engzell et al. 2021; Grewenig et al. 2021; Werner and Woessmann 2023).

Table 5 explores differences in reaction for several financial characteristics. We find that households with an income level below the median are more economically insecure than high-income households (column 1). The difference-in-discontinuity coefficient, while negatively estimated, is however imprecise. We do not find evidence that savings and indebtedness are factors that neither prompted a differential level of anxiety nor affected the reaction to vaccination (columns 2 and 3). Finally, we find that households that experienced a drop in wealth during the pandemic exhibit higher levels of economic insecurity about the future. However, we document that vaccination was not salient for this group of respondents (column 4).

Finally, in Table 6, we inspect differences with respect to health characteristics. We find that respondents with a prior infection show higher levels of economic insecurity (column 1). We find, however, limited evidence that eligibility for vaccination caused a reduction in this domain (the estimated difference-in-discontinuity

coefficient is negative, yet statistically insignificant). In column 2, we document that individuals vulnerable to the COVID-19 are neither more insecure nor more affected by the eligibility rule relative to non-vulnerable ones. This likely stems from the fact that vulnerable people are *always-takers* as they were prioritized in the roll-out of the vaccine, irrespective of their age.

6.2. *Potential Mechanisms*

In this section, we examine some potential mechanisms that may further help interpret our findings. In particular, we consider three possible channels that facilitate the relationship between vaccination and economic insecurity: perceived protection against future COVID-19 infection; trust and other glue-like attitudes; and psychological effects.[10]

First, we explore whether vaccination reduced one's subjective risk of future infection, which, in turn, increased economic security. This is plausible since knowingly being protected from the virus with antibodies would allow one to return to normality. Unfortunately, the COVID-19 Study only asks respondents what is the perceived likelihood of catching COVID-19 in a month ahead—a time window that differs considerably from the 3-month span utilized in the economic insecurity section. With this caveat in mind, we code the pattern of responses on an ordinal scale ranging between 1 ("Very unlikely") and 4 ("Very likely"), which is then standardized to have a mean of 0 and standard deviation of 1. Therefore, values above 0 describe individuals that are more concerned about the possibility of getting COVID-19 within 30 days than the average interviewee. We compute the average value of this measure, at the age cohort-level, and plot it on either side of vaccination eligibility criteria in Figure A9. While negative, the estimated discontinuity around the cutoff is not statistically significant (column 1 of Table A7). Vaccination seems to be associated with a reduction in the anxiety of getting the virus (column 2); however, once we turn to the fuzzy RD design we find the effect is statistically imprecise (column 3). One explanation is that there is a temporal delay in an individual's sense of security against COVID-19, which is consistent with the rational expectations idea introduced above.

Second, we examine whether vaccination had an effect on social cohesion with one's neighbors. It is possible that once an individual is vaccinated they would then change their behavior to become more pro-social, which could influence their economic perceptions. We again plot out the share of responses to three survey items in Figure A10: trust in your neighbors (panel *a*); feeling similar to your neighbors (panel *b*); and whether people are willing to help their neighbors (panel *c*). Across all three outcomes we observe no jump at the eligibility threshold. This is confirmed when we repeat our fuzzy RDD specification (columns 6, 9, and 12 of Table A7). The effects we estimate are all small in magnitude and statistically not different from zero.

[10]Indeed, due to data availability, we are only able to cover a subset of all possible mechanism. It is possible that other channels also operate: trust in the institution of government, for example. This would be in line with the findings of Bol et al. (2021) who find that lockdowns in Western Europe caused an increase in the trust in government, which implies that vaccinations may also work in the same manner.

Finally, we consider the psychological effects that vaccination may have had. One could argue that an increase in one's general sense of well-being could spill over into sub-domains, that is, financial well-being. First, insights into mental health during the pandemic are provided by Brodeur et al. (2021) who find a significant deterioration due to the impact of the lockdown policies. Further, Bagues and Dimitrova (2021) explore, using the same data, to what extent did vaccination affect one's psychological well-being, as measured by the Generalised Health Questionnaire (GHQ). They present evidence that vaccination increased well-being by 0.12 of a standard deviation by exploiting the full panel of individuals in the COVID-19 Study. They show that the vaccine had a restorative effect in that it increased well-being back to about one half of the pre-pandemic levels. We repeat the analysis of vaccination on mental health using our (fuzzy) RDD. Online Appendix Figure A11 reports the graphical RD analysis for the reduced form of eligibility and mental distress. The results are reported in Table A8. The coefficient is correctly signed and statistically significant at the 10 percent level in both the reduced form and second stage when estimating the effect locally around the cutoff. This suggests that the act of vaccination does indeed result in a reduction in one's mental distress. Given this body of evidence and the fact that the heterogeneity analysis in Agrawal et al. (2021) and Bagues and Dimitrova (2021) is broadly in line with our own findings, we are confident that psychological well-being is indeed a channel in which vaccination can affect one's economic security.

7. Concluding Remarks

This study provides the first causal evidence of the effect of immunization from COVID-19 on the recipients' perceived level of economic insecurity. Our results indicate that individuals who got the COVID-19 vaccine because they were aged 50, or just over, rather than 49, or just under, while the Understanding Society COVID-19 Study interviews took place, are less likely to feel economically insecure. The effect we document is economically relevant, explaining a reduction in the share of individuals who declared being economic insecure of 41 percentage points. We interpret this finding as evidence of a rise in the perceived recipients' level of economic security induced by immunization from COVID-19.

Our main finding is supported by a battery of robustness checks and further analyses that can help understand the mechanisms behind the rise in economic security—mechanisms that can be replicated in other contexts similar to ours. Using variation in the households' characteristics, we find the effect to be more salient for women, large households, and those with children—three of the most impacted groups of individuals during the pandemic. Furthermore, while we note that we are not able to rule out all possible channels, owing to the lack of data, we document that the more plausible channels of transmission pass through a reduction in the perceived risk of catching the disease in the future and an increase in psychological well-being in those who received the vaccine.

Our findings have important policy implications and shine a light on the efficacy of the vaccination strategy proposed by the JCVI (and of other interventions put forward by the UK government) in restoring economic security and allowing

a return to normality. For one thing, our results indicate that mass vaccination campaigns can have economic-enhancing spill-over effects well beyond the inoculated individuals' antibody presence and can contribute to a boost in the level of optimism in the economy—optimism that can in turn help the economy to recover faster. The expected return of these health policies is, therefore, likely higher than presumed. This is important because policy makers and tax payers still lack quantitative studies on the economic evaluation of one of the most expensive policy measures since World War II.

For another, our ancillary results show that governments can effectively act to contain the rise of economic insecurity we observe these years, through timely and specific policies. For example, we show that during the COVID-19 pandemic job insecurity and perceived poverty have not increased thanks to the social welfare schemes that the UK government offered after the outbreak of the pandemic.

It is worth highlighting two limitations of our study that further research should help to bridge. First, owing to the lack of data, we are not able to test the effects of receiving more doses of the vaccine on economic insecurity—a test that can provide further insights into the short-term impacts of vaccines. For the same reason, we do not estimate the medium-longer term effects, that is, whether vaccination returned individuals back to baseline *behavior*. More research is needed on the pass-through from economic security to consumption behavior, for instance. Second, our study's main conclusions rely on the estimation of LATEs. To provide internally valid results, we indeed compare cohorts with ages around 50. Our results are therefore not likely to be informative about the entire UK population, but may serve to assess the economic impact of the immunization of around 6 million individuals who are also the richest in terms of median weekly pay (ONS 2021)—an assessment that is, in our view, of paramount importance for policy makers and tax payers.

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Appendix S1: Supporting Information.