COVID-19 Vaccines: A Shot in the Arm for the Economy

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Abstract
We quantify the effect of vaccinations on economic activity in the United States using weekly county-level data covering the period end-2020 to mid-2021. Causal effects are identified through instrumenting vaccination rates with county-level pharmacy density interacted with state-level vaccine allocations, and by including county and state-time fixed effects to control for unobserved factors. We find that vaccinations are a significant and substantial shot in the arm of the economy. Specifically, spending rises by 1.3 percentage points (relative to the average spending during January 2020) in response to a 1% point increase in initiated vaccination rates. Initial unemployment decreases by 0.09 percentage points of the 2019 labor force. Vaccinations also increase workplace mobility. Urban counties and counties with initially worse socioeconomic conditions and lower education levels exhibit larger effects of vaccinations.

Keywords COVID-19 · Pandemics · Vaccinations

JEL Classifications E52 · E58 · D43 · L11

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1 Introduction

Around the world, COVID vaccines are being rolled out. The aim of these vaccination programs is to counter the spread of COVID-19 and put an end to the pandemic. This will ultimately save millions of lives. At the same time, vaccination programs can allow economies to restart as consumers and workers feel confident to return to their previous routines. This has led some observers to conjecture the return of the “roaring twenties” as consumers go back to the shops and workers to their offices.1 A first-order question currently facing economists is thus how powerful vaccines are in helping relaunch the economy.

This paper asks the question, “What is the economic effect of vaccines?”. Vaccines are proven to reduce the transmission of COVID-19, which can affect economic activity both directly, as consumers and workers feel confident they can shop and go to work without catching COVID-19, or indirectly, by allowing governments to relax restrictions that hamper economic activity. Vaccines may also raise spending through increased confidence. If vaccines are successful in restarting the economy, it is likely to show up in higher spending, higher workplace mobility, and lower unemployment claims. We estimate the effect of higher vaccination rates on these variables using high-frequency data at the county level in the United States.

We tackle this question using U.S. county variation across time and space. In the United States, the vaccination roll-out started in December 2020 and accelerated over the winter and spring of 2021 before decelerating in the early summer. By August 13, 2021, the average U.S. county had a share of initiated vaccinations (i.e., first doses taken) of 56.0 percent (Panel A, Fig. 1).2 There was, however, substantial variation around this average, with the 10th and 90th percentile standing at 40.4 and 71.7 percent initiated vaccination rates, respectively.3 During the same period, the economy continued rebounding. Panel B of Fig. 1 shows how average credit card spending increased during the same period, while Panel C illustrates how new unemployment insurance claims declined further. Panel D depicts how workplace mobility also rose in the same period.

We rely on an instrumental variable approach to identify the causal effect of vaccinations. Specifically, we instrument county-level vaccination rates with predetermined pharmacy densities at the county level (number of pharmacies per square mile) interacted with weekly allocated vaccines at the state level. That way, the approach exploits variation in the extent to which economic activity picks up in counties with higher pharmacy density when vaccines are allocated to the given state. We believe this is a good instrument for two reasons. First, counties with higher pharmacy densities can deliver vaccines more easily

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1 See, for example, VOXEU “The ‘Roaring Twenties’: Revisiting the evidence for Europe”.

2 Based of Fig. 1.A, the average daily vaccination rate (and the number of weeks required for the number of vaccinated to reach one percent of the population) for the median county were 1.4 (0.75) percent during January 2021, 2.65 (0.42) during February-March, and 0.95 (1.26) during April–May.

3 This point is further highlighted in Fig. 2 showing the full distribution of vaccination rates across counties within the U.S. on August 19, 2021.
**Fig. 1** Vaccinations and economic activity, 2021  
*Notes*: Initiated vaccinations are first doses of vaccines taken. Spending is seasonally adjusted credit/debit card spending expressed relative to Jan 431, 2020. Workplace mobility is compared to its median value for the same weekday in the period of Jan 3 Feb 6, 2020. UI claims is initial unemployment insurance claims in percent of the 2019 labor force. See Section 2 for more details.

**Vaccination Rates on August 19, 2021 (%)**

**Fig. 2** Vaccination rates across US counties
to their population when more vaccines are allocated to the state. Empirically, this is reflected in a strong first stage. Second, our instrument arguably also satisfies the exclusion restriction as pharmacy density is pre-determined and does not directly impact economic activity except through vaccination. Indeed, we show that the relationship between the pre-determined pharmacy density and the level of spending before vaccination is insignificant.

We find that vaccinations are a significant and substantial shot in the arm for the economy. Specifically, the impact after 8 weeks of a 1% point increase in (initiated) vaccination rates is (i) an increase of credit card spending by 1.3 percentage points over the spending level in January 2020, and (ii) a reduction of weekly initial unemployment claims by 0.09 percentage points of the 2019 labor force (around 2 percent of the average ratio of weekly unemployment claims to the labor force). We also find evidence that vaccination increases workplace mobility. Importantly, these effects vary across counties, with larger effects of vaccinations in urban counties and in counties with worse social-economic conditions. This way, vaccinations are also a fair shot in the arm for the economy, which highlights that equitable distribution of vaccines is important to reduce inequality.

Our results are specific to the United States but also hold important lessons for other countries. First, the United States rolled out vaccines early and at a large scale. The U.S. case thus holds important lessons for the economic impact that other countries can expect from rolling out vaccines. Second, our results highlight that ensuring an equitable distribution of vaccines is critical to reducing inequality, either pre-existing or pandemic-induced.

Literature review. Our paper contributes to the emerging literature assessing the economic impact of COVID-19 vaccination programs. Sandmann et al. (2021) study the health and economic impact of mass vaccination in the United Kingdom using an epidemiological model. They measure the impact of vaccines on COVID-19 epidemiological outcomes and then convert those into economic costs. Deb et al. (2021) investigate the effect of COVID-19 vaccinations on economic activity as measured by high-frequency emissions and workplace mobility. They use a comprehensive cross-country sample and identify effects based on unexpected increases in vaccinations. Ganslmeier et al. (2021) study the impact on economic activity, as measured by nighttime lights, emissions, and workplace mobility using 326 regions in 17 countries. They employ an instrumental variable approach using previous vaccine procurement interacted with region-time fixed effects.

We make a conceptual, methodological, and data contribution to the existing literature. Conceptually, we contribute by studying more direct measures of economic activity - credit/debit card spending and unemployment claims - unlike existing papers that use more indirect measures such as emissions, nighttime lights, and workplace mobility. Methodologically, we exploit the pre-determined and arguably exogenous variation in pharmacy density to identify the causal effects of vaccines. We also use more granular county-level data. This strengthens the identification strategy and allows us to go beyond the estimation of average effects. Indeed, we show how the impact of vaccination differs across urban-rural counties and other socioeconomic variables that vary significantly within U.S. states.
This paper is organized as follows: Sect. 2 introduces our data, Sect. 3 details our approach, and Sect. 4 discusses results. Finally, Sect. 5 concludes.

2 Data

Our dataset covers 2808 counties in the 50 U.S. states and the District of Columbia at the weekly frequency spanning end-December 2020 to early July 2021. The dataset includes county-level data for vaccinations and economic activity (Table 1).

Let us describe each variable in more detail. Initiated and completed vaccinations as well as COVID deaths (in percent of county population) are collected from the Centers for Disease Control and Prevention (CDC) through https://covidactnow.org. These data are collapsed to weekly averages. Allocated vaccine doses (in percent of county population) include Pfizer and Moderna vaccines and are also collected from the CDC at a weekly frequency. Data on daily credit/debit card spending seasonally adjusted relative to the daily average during Jan 4–31, 2020, are obtained from the Opportunity Insights Economic Tracker compiled by Chetty et al. (2020), which relies on data from Affinity Solutions. Like the vaccination data, these daily data are also collapsed to weekly averages. The data cover spending on all merchant category codes. Workplace mobility is from Google and includes workplace mobility only, compared to its median value for weekdays in the period of Jan 3–Feb 6, 2020. Our data set also includes initial unemployment claims (in percent of 2019 county labor force) at a weekly frequency. These data are also obtained via the Opportunity Insights Economic Tracker, which relies on data from individual state agencies. Our dataset also includes the number of pharmacies from the NCPDP dataset aggregated to the county level in July 2020 from Guadamuz et al. (2020) kindly updated and provided by Dima Mazen Qato. County surface areas are from the U.S. Census Bureau. Data on county median household income, unemployment, and education levels (for 2019) are from the US Department of Agriculture. The classification of

|                          | Mean    | Standard deviation | Observations |
|--------------------------|---------|--------------------|--------------|
| Initiated vaccinations    | 27.60   | 14.74              | 65,632       |
| Completed vaccinations    | 20.80   | 14.33              | 65,125       |
| Two-week lagged allocated doses, percent of population | 2.10 | 0.56 | 65,632 |
| Credit/debit card spending, relative to Jan2020  | 0.11 | 0.21 | 41,169 |
| Mobility: Work places    | −19.66 | 8.93               | 64,284       |
| Initial claims per 100 persons in 2019 labor force | 0.39 | 0.31 | 14,073 |

Note: These statistics include at most the 2808 counties and 28 weeks from December-2020 to early July 2021 covered in our analysis.
counties into rural and urban stems from CDC. Finally, we use data on the share of population eligible for stimulus payments in December 2020–January 2021 and March 2021 from Chetty et al. (2021).

3 Methodology

This section lays out our empirical strategy and crucially discusses our approach to instrumenting for vaccines.

3.1 Baseline Specification

In the baseline, we run instrumented local projections as follows:

\[
y_{c,t+h} = \beta h \hat{v}_{c,t} + \tau^h s + \tau^h c + \epsilon_{c,t+h}
\]

(1)

\[
v_{c,t} = \gamma z_{c,t-2} + \delta_{s,t} + \delta_c + \xi_{c,t}
\]

(2)

where \(y_{c,t+h}\) denotes an economic outcome of interest in county \(c\), state \(s\), week \(t + h\). Outcomes considered are: (i) average daily credit/debit card spending in week \(t + h\) relative to Jan 4–31, 2020, (ii) weekly initial unemployment claims in percent of county-level 2019 labor force in week \(t + h\), and (iii) average daily workplace mobility in week \(t + h\) relative to Jan 3–Feb 6, 2020. \(v_{c,t}\) are initiated vaccinations in percent of the population at the county level. Depending on the analyzed outcome variable, \(\beta^h\) then denotes the reaction after \(h\) weeks to a 1 percentage points increase in vaccination rates in (i) average daily credit/debit card spending measured as a fraction of the daily spending level in January 2020, (ii) weekly initial unemployment claims after \(h\) weeks in percent of the labor force, and (iii) the index for workplace mobility. The hat on \(v_{c,t}\) denotes the fitted value of vaccinations from Eq. (2). \(\tau^h s\) and \(\delta_{s,t}\) are state-time fixed effects, controlling for common state-level shocks, like the incidence of the pandemic. \(\tau^h c\) and \(\delta_c\) are county fixed effects capturing time-invariant specifics in a given county, such as geography, population density, or industrial structure, that could affect the outcome. Finally, \(z_{c,t-2}\) is an instrument for vaccines explained in the next subsection.

3.2 Instrumentation

The specification in Eq. (1) is subject to endogeneity concerns. First, a critical worry is reverse causality bias. For example, imagine that a county is hosting an event and thus facilitating vaccination in the run up to the event. If so, the causality would run

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4 Counties in metro areas or with urban populations are coded as urban while completely rural or counties with less than 2500 urban population are coded as rural.

5 For Johnson & Johnson doses, initiated vaccinations are also completed.
in the opposite direction—activity generated vaccines—but Eq. (1) would mistakenly ascribe economic activity to vaccines. Second, the relationship may be spurious if a third underlying factor is driving both vaccination rates and economic activity, e.g., the state of the pandemic could drive both vaccination and changes in economic activity.

We propose an instrumental variables approach to address these concerns. Specifically, we instrument county-level vaccination rates with pharmacy densities at the county level (number of pharmacies per square mile) interacted with weekly allocated vaccines at the state level.\(^6\) Thus, \(z_{c,t-2}\) in Eq. (2) is such that:

\[
z_{c,t-2} = p_c \times va_{s,t-2}
\]

Here, \(p_c\) denotes the number of pharmacies per square mile in county \(c\) within state \(s\), while \(va_{s,t-2}\) denotes the number of statewide allocated vaccines in percent of state population at time \(t - 2\).\(^7\) The former variable varies across counties (Fig. 3a), while the latter varies across time and states (Fig. 3b).

\(^6\) Vaccine doses administered in retail pharmacies represented 35 percent of total doses administered by October 6, 2021.

\(^7\) A two-week lag is chosen to maximize the explanatory power in the first stage regression, denoting some lag between allocations and inoculations.
Our identification strategy exploits whether activity within each state picks up more in counties with higher pharmacy densities when more vaccines are allocated to the state. We show this formally in "Appendix 2". As such, this instrument is reminiscent of a Bartik instrument in the sense that the identification is coming from geographic cross-sectional variation as discussed in Goldsmith-Pinkham et al. (2020). To understand our identification strategy better, consider a single state at a given time. Here, our instrument simply exploits the variation in pharmacy density. Time variation is then introduced into the instrument by interacting county-level pharmacy density with the change in the supply of vaccines at the state level.

For our instrument to be valid it needs to (i) be relevant, that is strongly related to vaccination (the first stage), and (ii) only affect economic activity through vaccines (the exclusion restriction). Relevance is judged by how strongly correlated the instrument is with the endogenous variable. On the other hand, the exclusion restriction is not directly testable.

We assess instrument validity through the relationship with initiated vaccination rates. Table 2 shows that this relationship is positively and highly significant. This is witnessed by the coefficient estimates and F-statistic.

We assess the plausibility of the exclusion restriction by relating the density of pharmacies to changes in pre-vaccine roll-out spending. If the instrument is valid, we should not see a significant relationship to spending before the vaccination campaign began. Table 3 shows that this relationship is indeed insignificant. Note that for the exclusion restriction to be valid, vaccine supply to states does not need to be exogenous to economic activity over time. Instead, our identifying assumption is that the interaction between pharmacy densities and vaccine supplies only affects economic activity through vaccination. Thus, it suffices that the differences in the

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Table 2  First stage regression

|                         | (1) Initiated vaccinations |
|-------------------------|----------------------------|
| Pharmacies per sq. mile x allocations per capita | 1.6894*** [0.2036] |
| Observations            | 40984                      |
| County FE               | Yes                        |
| State-time FE           | Yes                        |
| F-stat                  | 68.9                       |

Notes: The estimates are based on data for 1727 counties over 25 weeks. Robust standard errors are reported. ***, **, and * indicate statistical significance at 1, 5, and 10 percent, respectively.

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8 However, in a strict sense, our instrument is not a Bartik, as it is not the internal product of shares and growth rates across another sub-grouping that is not geographic, like industries.

9 This is akin to the test of pre-trends advised in Goldsmith-Pinkham et al. (2020).
Table 3 Validity of exclusion restriction

| (1) | Spending, 2020M12 over 2020M1 |
|-----|-------------------------------|
| Pharmacies per sq. mile | -0.0048 [0.0060] |
| Constant | 0.0049 [0.0041] |
| Observations | 1730 |

Notes: The estimates are based on data for spending from 2012M1 to 2012M12 for 1730 counties. Spending is seasonally adjusted credit/debit card spending. The regression is purely cross-sectional (one observation per county) who no fixed effects are included. The reported result is robust to inclusion of state fixed effects. Robust standard errors are reported. ***, **, and * indicate statistical significance at 1, 5, and 10 percent, respectively.

(a) Credit card spending, index

(b) Initial UI claims, percent

(c) Workplace mobility, index

Fig. 4 Effect of vaccination on economic activity—local projections

Notes: Spending is an index with the seasonally adjusted credit/debit card spending expressed relative to Jan 431, 2020. UI claims is initial unemployment insurance claims in percent of the 2019 labor force. Workplace mobility is an index compared to its median value for the same weekday in the period of Jan 3 Feb 6, 2020. All specifications include state-time and county fixed effects. Initiated vaccinations are instrumented with the previous 2 week’s allocation of vaccines (Pfizer and Moderna) at the state level (in percent of total population) interacted with pharmacy density at the county-level. The instrumental variables (IV) coefficient point estimate is reported in blue, with associated 95 percent confidence bands, and the OLS coefficient point estimate is reported in red. We follow Stock and Watson, 2018, who note that inference based on standard heteroscedasticity and autocorrelation (HAC) robust standard errors is valid at short to medium horizons in local projection models. Specifically, we use Driscoll and Kraay, 1998 correction to the standard errors, which are robust to very general forms of spatial and temporal dependence. We use a bandwidth of 8 weeks for the auto-correlation structure, but our results are robust to other lag choices. See Appendix Table 4 and Table 5 for the estimates.
effect of a higher vaccine supply between counties with different pharmacy densities only run via vaccinations.

4 Results

This section reports and discusses our estimates of the effect of vaccines on credit card spending, workplace mobility, and initial unemployment claims.

Figure 4 shows the results of the instrumented fixed-effects local projections for spending, initial unemployment insurance (UI) claims, and workplace mobility, estimating the full system in Eqs. (1) and (2). Point estimates (line) and 95% confidence intervals (shaded area) are reported in blue computed with Driscoll–Kraay standard errors. Our instrument appears relevant at all horizons, as the F-stats range between 16 and 35 and thus well above conventional thresholds (Table 4).

Over 8 weeks, spending rises by 1.3 percentage points (of average spending during January 2021) for each percentage point increase in initiated vaccination rates (Fig. 4, panel a). Initial unemployment claims decrease by 0.09 percentage points of the 2019 labor force after 8 weeks in response to a 1% point increase in initiated vaccination rates (panel b). Workplace mobility is an index and thus harder to interpret, but our results show that for each percentage point of initiated vaccines, workplace mobility rises by 0.37 percentage points after 8 weeks (panel c). The confidence interval around the estimated effects tends to rise over time. Overall, these results show that vaccination has a causal and relatively quick effect on economic activity.

Figure 4 also compares our instrumented baseline results with those from a simple OLS for spending, initial unemployment insurance (UI) claims, and workplace mobility. For each variable, we show results from (i) the uninstrumented OLS fixed effects regression (point estimates and 95% confidence interval in red), estimating Eq. (1) where \( \hat{v} \) is the initiated vaccination rate, and (ii) the instrumented fixed effects regression (point estimates and 95% confidence interval in blue), estimating the full system in Eqs. (1) and (2). Overall, the instrumentation strengthens our results, although with larger standard errors (Table 5). The average absolute increase in the coefficients across horizons is 4.6x for spending, 2.8x for UI claims and 2.4x for workplace mobility. A possible explanation for the larger effects uncovered by the instrumented regression compared to the simple OLS fixed-effects estimator is the existence of omitted factors that is related to both vaccines and spending, e.g., the local state of the pandemic. The state of the pandemic at the county level is likely positively correlated with vaccine take up at the county level, but negatively correlated with spending at the same level. Our instrument, by proxying for the supply of vaccines, helps clean the negative correlation between COVID-19 outcomes and spending through vaccines (see more on the relationship between COVID-19 outcomes and our instrument in the next paragraph).

These baseline results are robust to a range of robustness checks reported in Appendix 1. First, we replace initiated with completed vaccinations (Fig. 6). Second, we vary the lags of the instrument from 0 to 6 weeks (Fig. 7). Third, we run panel regressions with only time and county fixed effects (Fig. 8). Fourth, we control for county-level eligibility of stimulus payments using the data from Chetty et al. (2021) and per capita new weekly deaths (Fig. 9). Fifth, we modify our
instrument to use the population-implied vaccine allocation to all other counties within the state, i.e., excluding own county allocation from the instrument (Fig. 10). Our results are robust across these checks. We explored “within” nonlinearities by adding quadratic terms of demeaned vaccinations and the instrument, but we found no evidence of significant nonlinear effects in that specification. Finally, we note that using national supply instead of state-level supply does not affect our results meaningfully (Fig. 11).

Our baseline results reflect averages across all counties, which may mask important cross-geographic heterogeneity. In what follows, we study whether these effects are heterogeneous across counties. We do this by augmenting Eqs. (1) and (2) to include county-level characteristics $d_c$ one by one. Specifically, the estimated system of equations becomes:

$$y_{c,t+h} = \beta^h \bar{y}_{c,t} + \theta^h d_c \bar{y}_{c,t} + \tau^h + \epsilon_{c,t+h}$$  \hspace{1cm} (4)

$$v_{c,t} = \gamma^1 z_{c,t-2} + \epsilon^1 z_{c,t-2} d_c + \delta^1_s + \epsilon^1_{c,t}$$ \hspace{1cm} (5)

$$d_c v_{c,t} = \gamma^2 z_{c,t-2} + \epsilon^2 z_{c,t-2} d_c + \delta^2_s + \epsilon^2_{c,t}$$ \hspace{1cm} (6)

where $d_c$ is either (i) a dummy for whether the unemployment level in 2019 was above the median across all counties; (ii) a dummy for whether the median household income in 2019 was above the median; (iii) a dummy for whether the share of population above age 25 with a bachelor degree is above the median in 2019; and (iv) a dummy for whether the county is urban.

Effects of vaccines on economic activity appear larger in counties with higher unemployment and lower income levels (Fig. 5, rows 1 and 2). We find that these interactions are positive for spending and workplace mobility and negative for unemployment claims. The coefficient on the high unemployment interaction seems weaker than the one on low income particularly in the case of spending where the former is insignificant. Taken together, these results suggest that the economic effect of

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10 The coefficients for both spending and UI claims are larger when controlling for time rather than state-time fixed effects. This could reflect either omitted variable bias in the former specification, for example, concurrent state-level restrictions on economic activity, or that vaccines prompt such statewide policy responses, which themselves have an economic effect. If the former, then it is preferable to control for state-time effects as done in the baseline. If the latter, our baseline specification would underestimate the full extent of the economic effect of vaccines. Controlling for stimulus payment eligibility and COVID-19 per capita deaths as a proxy for the state of the pandemic is done by including either or both variables in Eqs. (2) and (1). None of these controls, either by themselves or jointly, lead to an estimate of the effect of vaccines that is statistically distinct from the ones presented in Fig. 4. In addition, we also exclude the months when stimulus payments were distributed, which similarly does not affect our estimates significantly.

11 Section 2 has more details on these variables.

12 The first stage regression for the high unemployment interaction is also weaker, with the Kleibergen and Paap (2006) F-statistic below 10 for spending and mobility throughout all horizons. For the low-income interaction, the F-stats also fall below 10 for some horizons for spending and mobility.
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Vaccines was largest in counties with worse pre-existing socioeconomic conditions, or put in other words that vaccines represented a “fair shot” in the arm of the economy by disproportionately supporting economic activity in less well-off counties.

Counties with lower education levels also seem to have experienced larger effects of vaccines (Fig. 5, row 3). This coefficient on the low-education and vaccination interaction is positive for spending and workplace mobility and negative for unemployment claims, and significant for all three variables. This suggests that the

Fig. 5 Estimates of the interaction between initiated vaccination and county characteristics
Notes: The figures show the estimates of the interaction of credit card spending (column a), new weekly unemployment insurance claims (column b), and workplace mobility interacted (column c) with dummies capturing county characteristics in 2019, including: if the unemployment rate in the county was above the national median (row 1), if the household income level in the county was below the national median (row 2), if the share of population with at least college education was below the national median (row 3), and if the county is urban (row 4). The coefficients are reported at various horizons (on the x-axis) with their associated 95 percent confidence bands. Standard errors are clustered at the state-time level. See Appendix Table A3 for the estimates.

13 The Kleibergen and Paap (2006) F-stats fall somewhat below 10 for spending and mobility, while they remain above 10 for UI claims (except in week 8).
vaccination roll-out provides larger benefit to counties with comparatively less educated inhabitants.

Finally, the effect of vaccines is strongest in urban counties (Fig. 5, row 4). In this table, we include an interaction for whether a county is urban. We see that this is positive and significant for spending up to 8 weeks and negative and significant for unemployment claims from week 3. Workplace mobility has the opposite sign as spending but it becomes insignificant after week 8.

5 Conclusion

We conclude by answering the question we posed in the beginning: Yes, vaccinations are an important shot in the arm for the economy. Specifically, we find that after 8 weeks, a 1 percentage points increase in initiated vaccination rates has increased spending by 1.3 percentage points (of average spending during January 2020) and reduces the weekly inflow to unemployment by 0.09 percentage points of the 2019 labor force (around 2 percent of the average ratio of weekly unemployment claims to the labor force). Consistent with these, we also find that vaccinations increase workplace mobility over the same time horizon. Overall, these results show that vaccination has a causal and relatively quick effect on economic activity. The effects tend to strengthen over the 1-8 week horizon. Effects are also larger than effects from uninstrumented regressions. This downwards bias in the uninstrumented regressions may be due to an underlying factor that drives outcomes and vaccination, such as the local state of the pandemic. Our instrument, because it focuses on the supply of vaccines, is able to “clean” that potential spurious relationship and uncover the causal effect of vaccines on economic outcomes.

Importantly, these effects vary across counties, with larger effects in urban counties and in counties with more vulnerable populations as measured by lower levels of education, income, and higher pre-COVID-19 unemployment rates. What explains this heterogeneity? The severity of the pandemic along the social dimension is one potential reason. In our data, counties in urban areas, with lower education rates, and with lower income levels saw significantly higher deaths per capita during 2020. Unemployment is also linked to higher death rates, although the effect is not significant after controlling for the other factors. Vaccination could thus have released more activity and spending in such countries. This way, vaccinations are also a fair shot in the arm for the economy, which highlights that equitable distribution of vaccines is important to reduce inequality.

Extra Tables and Figures

See Tables 4, 5, and 6.
Table 4  Effect of vaccination on economic activity at the county level across horizons, instrumented regressions

|               | $h = 1$ | $h = 2$ | $h = 3$ | $h = 4$ | $h = 5$ | $h = 6$ | $h = 7$ | $h = 8$ |
|---------------|---------|---------|---------|---------|---------|---------|---------|---------|
| Spending      | 0.006*** | 0.007*** | 0.007*** | 0.008*** | 0.009*** | 0.011*** | 0.012*** | 0.013*** |
|               | [0.001]  | [0.001]  | [0.001]  | [0.002]  | [0.002]  | [0.003]  | [0.003]  | [0.004]  |
| UI claims     | 0.003    | 0.001    | 0.000    | −0.001   | −0.002   | −0.004** | −0.006*** | −0.009*** |
|               | [0.002]  | [0.002]  | [0.002]  | [0.002]  | [0.002]  | [0.001]  | [0.001]  | [0.001]  |
| Mobility      | 0.286*** | 0.310*** | 0.301*** | 0.302*** | 0.318*** | 0.353*** | 0.352*** | 0.368*** |
|               | [0.057]  | [0.049]  | [0.042]  | [0.047]  | [0.062]  | [0.060]  | [0.060]  | [0.058]  |
| Spending: F   | 24.6     | 24.6     | 24.6     | 23.2     | 21.7     | 20.0     | 18.3     | 16.5     |
| UI claims: F  | 32.6     | 32.9     | 33.5     | 34.6     | 33.4     | 32.2     | 31.2     | 30.0     |
| Mobility: F   | 22.4     | 22.4     | 22.4     | 22.4     | 22.5     | 22.5     | 21.3     | 20.0     |
| Spending: Obs | 40984    | 40980    | 40973    | 39246    | 37519    | 35792    | 34065    | 32338    |
| UI claims: Obs| 13714    | 13528    | 13338    | 13145    | 12555    | 11963    | 11372    | 10771    |
| Mobility: Obs | 64251    | 64255    | 64260    | 64266    | 64267    | 64249    | 61540    | 58830    |
| Spending: Cty | 1727     | 1727     | 1727     | 1727     | 1727     | 1727     | 1727     | 1727     |
| UI claims: Cty| 609      | 609      | 609      | 609      | 609      | 609      | 609      | 607      |
| Mobility: Cty | 2741     | 2741     | 2740     | 2739     | 2736     | 2736     | 2735     | 2734     |
| Spending: Wks | 25       | 25       | 25       | 25       | 24       | 23       | 22       | 21       |
| UI claims: Wks| 25       | 25       | 25       | 25       | 25       | 24       | 23       | 22       |
| Mobility: Wks | 25       | 25       | 25       | 25       | 25       | 24       | 23       | 23       |

Note: Spending is an index with the seasonally adjusted credit/debit card spending expressed relative to Jan 4-31, 2020. UI claims are initial unemployment insurance claims in percent of the 2019 labor force. Workplace mobility is an index compared to its median value for the same weekday in the period of Jan 3 - Feb 6, 2020. All specifications include state-time and county fixed effects. Initiated vaccinations are instrumented with the previous 2 week’s allocation of vaccines (Pfizer and Moderna) at the state level (in percent of total population) interacted with pharmacy density at the county level. We follow Stock and Watson (2018), who note that inference based on standard heteroscedasticity and autocorrelation (HAC) robust standard errors is valid at short to medium horizons in local projection models. Specifically, we use Driscoll and Kraay (1998) correction to the standard errors, which are robust to very general forms of spatial and temporal dependence. In the middle of the table, the Kleibergen and Paap (2006) F-statistic from the first stage regression is reported. At the bottom, the number of observations, counties and weeks used in each of the regressions. ***, **, and * indicate statistical significance at 1, 5, and 10 percent, respectively.
|                  | $h = 1$ | $h = 2$ | $h = 3$ | $h = 4$ | $h = 5$ | $h = 6$ | $h = 7$ | $h = 8$ |
|------------------|---------|---------|---------|---------|---------|---------|---------|---------|
| Spending         | 0.002***| 0.002***| 0.002***| 0.002***| 0.002***| 0.002***| 0.002***| 0.002***|
|                  | [0.001] | [0.001] | [0.001] | [0.001] | [0.001] | [0.001] | [0.001] | [0.001] |
| UI claims        | 0.000   | −0.000  | −0.001  | −0.001  | −0.001**| −0.002***| −0.002***| −0.001**|
|                  | [0.001] | [0.001] | [0.001] | [0.001] | [0.001] | [0.000]  | [0.000]  | [0.001] |
| Mobility         | 0.100** | 0.108***| 0.118***| 0.130***| 0.151***| 0.170***| 0.171***| 0.170***|
|                  | [0.040] | [0.036] | [0.032] | [0.027] | [0.019] | [0.020] | [0.018] | [0.016] |
| Spending: Obs    | 40984   | 40980   | 40973   | 39246   | 37519   | 35792   | 34065   | 32338   |
| UI claims: Obs   | 13714   | 13528   | 13338   | 13145   | 12555   | 11963   | 11372   | 10771   |
| Mobility: Obs    | 64251   | 64255   | 64260   | 64266   | 64267   | 64249   | 61540   | 58830   |
| Spending: Cty    | 1727    | 1727    | 1727    | 1727    | 1727    | 1727    | 1727    | 1727    |
| UI claims: Cty   | 609     | 609     | 609     | 609     | 609     | 609     | 609     | 609     |
| Mobility: Cty    | 2741    | 2741    | 2740    | 2739    | 2736    | 2736    | 2735    | 2734    |
| Spending: Wks    | 25      | 25      | 25      | 24      | 23      | 22      | 21      | 20      |
| UI claims: Wks   | 25      | 25      | 25      | 25      | 24      | 23      | 22      | 21      |
| Mobility: Wks    | 25      | 25      | 25      | 25      | 25      | 25      | 24      | 23      |

Note: Spending is an index with the seasonally adjusted credit/debit card spending expressed relative to Jan 4-31, 2020. UI claims is initial unemployment insurance claims in percent of the 2019 labor force. Workplace mobility is an index compared to its median value for the same weekday in the period of Jan 3 - Feb 6, 2020. All specifications include state-time and county fixed effects. Initiated vaccinations are not instrumented in this table. We follow Stock and Watson (2018), who note that inference based on standard heteroscedasticity and autocorrelation (HAC) robust standard errors is valid at short to medium horizons in local projection models. Specifically, we use Driscoll and Kraay (1998) correction to the standard errors, which are robust to very general forms of spatial and temporal dependence. ***, **, and * indicate statistical significance at 1, 5, and 10 percent, respectively.
|                  | h = 1 | h = 2 | h = 3 | h = 4 | h = 5 | h = 6 | h = 7 | h = 8 |
|------------------|-------|-------|-------|-------|-------|-------|-------|-------|
| **Spending High Unemp** | 0.000 | 0.000 | 0.000 | 0.001 | 0.001 | 0.001 | 0.001 | 0.001 |
| **UI claims High Unemp** | −0.001** | −0.001** | −0.002*** | −0.002*** | −0.003*** | −0.003*** | −0.004*** | −0.004*** |
| **Mobility High Unemp** | 0.070*** | 0.072*** | 0.061*** | 0.059*** | 0.064*** | 0.067*** | 0.068*** | 0.071*** |
| **Spending Urban** | 0.001*** | 0.001*** | 0.001*** | 0.001*** | 0.001*** | 0.001*** | 0.001*** | 0.001*** |
| **UI claims Urban** | 0.000 | −0.000 | −0.000* | −0.001** | −0.001** | −0.001** | −0.001** | −0.001** |
| **Mobility Urban** | −0.073*** | −0.065*** | −0.052*** | −0.034*** | −0.025*** | −0.016*** | −0.010*** | −0.003 |
| **Spending Low Inc** | 0.001*** | 0.001*** | 0.001*** | 0.001*** | 0.001*** | 0.001*** | 0.001*** | 0.001*** |
| **UI claims Low Inc** | −0.001 | −0.002 | −0.003** | −0.004** | −0.005** | −0.005** | −0.007*** | −0.008*** |
| **Mobility Low Inc** | 0.071*** | 0.073*** | 0.074*** | 0.079*** | 0.082*** | 0.082*** | 0.083*** | 0.085*** |
| **Spending No BA** | 0.000*** | 0.001*** | 0.001*** | 0.002*** | 0.002*** | 0.003*** | 0.003*** | 0.003*** |
| **UI claims No BA** | −0.002** | −0.002** | −0.003** | −0.003** | −0.004*** | −0.005** | −0.006*** | −0.006*** |
| **Mobility No BA** | 0.048*** | 0.060*** | 0.062*** | 0.063*** | 0.068*** | 0.074*** | 0.079** | 0.084** |
| **Spending High Unemp: F** | 7.9 | 7.9 | 7.9 | 7.5 | 7.0 | 6.4 | 5.8 | 5.2 |
| **UI claims High Unemp: F** | 16.7 | 17.0 | 17.4 | 18.1 | 17.6 | 17.1 | 16.7 | 16.2 |
| **Mobility High Unemp: F** | 7.4 | 7.4 | 7.5 | 7.5 | 7.5 | 7.5 | 7.0 | 6.6 |
| **Spending Urban: F** | 12.7 | 12.7 | 12.7 | 12.0 | 11.2 | 10.3 | 9.4 | 8.4 |
| **UI claims Urban: F** | 17.8 | 18.0 | 18.4 | 19.1 | 18.4 | 17.7 | 17.1 | 16.4 |
Table 6 (continued)

|                  | $h = 1$ | $h = 2$ | $h = 3$ | $h = 4$ | $h = 5$ | $h = 6$ | $h = 7$ | $h = 8$ |
|------------------|---------|---------|---------|---------|---------|---------|---------|---------|
| Mobility Urban: F | 11.6    | 11.6    | 11.6    | 11.6    | 11.7    | 11.7    | 11.0    | 10.3    |
| Spending Low Inc: F | 11.1    | 11.1    | 11.1    | 10.5    | 9.9     | 9.1     | 8.3     | 7.4     |
| UI claims Low Inc: F | 15.8    | 16.7    | 17.7    | 18.8    | 19.9    | 21.8    | 24.6    | 30.2    |
| Mobility Low Inc: F | 9.4     | 9.4     | 9.4     | 9.4     | 9.4     | 9.4     | 8.9     | 8.3     |
| Spending No BA: F | 9.4     | 9.4     | 9.4     | 8.7     | 8.0     | 7.3     | 6.5     | 5.7     |
| UI claims No BA: F | 12.2    | 12.2    | 12.3    | 12.6    | 12.0    | 11.3    | 10.6    | 9.9     |
| Mobility No BA: F | 7.0     | 7.1     | 7.1     | 7.1     | 7.1     | 7.1     | 6.4     | 5.8     |
| Spending All Interactions: Obs | 40984   | 40990   | 40973   | 39246   | 37519   | 35792   | 34065   | 32338   |
| UI claims All Interactions: Obs | 13714   | 13528   | 13338   | 13145   | 12555   | 11963   | 11372   | 10771   |
| Mobility All Interactions: Obs | 64251   | 64255   | 64260   | 64266   | 64267   | 64249   | 61540   | 58830   |
| Spending All Interactions: Cty | 1727    | 1727    | 1727    | 1727    | 1727    | 1727    | 1727    | 1727    |
| UI claims All Interactions: Cty | 609     | 609     | 609     | 609     | 609     | 609     | 609     | 607     |
| Mobility All Interactions: Cty | 2741    | 2741    | 2740    | 2739    | 2736    | 2736    | 2735    | 2734    |
| Spending All Interactions: Wks | 25      | 25      | 25      | 24      | 23      | 22      | 21      | 20      |
| UI claims All Interactions: Wks | 25      | 25      | 25      | 25      | 24      | 23      | 22      | 21      |
| Mobility All Interactions: Wks | 25      | 25      | 25      | 25      | 25      | 25      | 24      | 23      |

Note: The table shows the estimates of the interaction of credit card spending (“Spending”), new weekly unemployment insurance claims (“UI claims”), and workplace mobility interacted (“Mobility”) with dummies capturing county characteristics in 2019, including: if the unemployment rate in the county was above the national median (“High Uempl”), if the household income level in the county was below the national median (“Low Inc”), if the share of population with at least college education was below the national median (“No BA”), and if the county is urban (“Urban”). All specifications include state-time and county fixed effects. Initiated vaccinations are instrumented with the previous 2 week’s allocation of vaccines (Pfizer and Moderna) at the state level (in percent of total population) interacted with pharmacy density at the county-level. Coefficients are at the top of the table. We follow Stock and Watson (2018), who note that inference based on standard heteroscedasticity and autocorrelation (HAC) robust standard errors is valid at short to medium horizons in local projection models. Specifically, we use Driscoll and Kraay (1998) correction to the standard errors, which are robust to very general forms of spatial and temporal dependence. 

***, **, and * indicate statistical significance at 1, 5, and 10 percent, respectively. In the middle of the table, the Kleibergen and Paap (2006) $F$-statistic from the first stage regression is reported. At the bottom, the number of observations, counties and weeks used in each of the regressions.
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Fig. 6 Effect of completed vaccinations on economic activity at the county level

Fig. 7 Varying the lag between vaccinations and the instrument

Fig. 8 Varying the extent of included fixed effects
Fig. 9 Estimates of the effect of instrumented initiated vaccination controlling for stimulus eligibility and the local state of the pandemic

Fig. 10 Effect of vaccination on economic activity—Baseline versus leave-one-out
Details on the Instrumentation

We create an instrument, $z_{c,t−2}$, for vaccines that proxies for the exogenous supply of vaccines from an individual county’s perspective and is defined as:

$$z_{c,t−2} = \frac{\#\text{pharmacies}_c}{\text{area}_c} \times \frac{\text{vaccine allocations}_{s,t−2}}{\text{population}_s},$$

where $p_c = \frac{\#\text{pharmacies}_c}{\text{area}_c}$ is the density of pharmacies per county in 2020 and $\omega_{s,t−2} = \frac{\text{vaccine allocations}_{s,t−2}}{\text{population}_s}$ is the per capita number of state-level allocated vaccines (Pfizer and Moderna) for state $s$ in week $t−2$. In terms of identification, this instrument has the appeal that: (i) the number of allocated vaccines is largely exogenous to county $s$, and (ii) the number of pharmacies in 2020 is largely pre-determined.

Take Eq. (2), the first stage in our framework:

$$v_{c,t} = \gamma z_{c,t−2} + \delta_{c,t} + \xi_{c,t}$$

This county and state-time fixed effects regression above is equivalent to running:

$$\tilde{v}_{c,t} = \gamma \tilde{z}_{c,t−2} + \tilde{\xi}_{c,t}$$
where

\[
\begin{align*}
\tilde{v}_{c,t} &= v_{c,t} - \sum_t \frac{v_{c,t}}{T} - v_{s,t} \\
\tilde{z}_{c,t-2} &= \frac{z_{c,t-2}}{T} - \frac{z_{s,t-2}}{T} \\
\tilde{g}_{c,t} &= g_{c,t} - \sum_t \frac{g_{c,t}}{T} - g_{s,t}
\end{align*}
\]

Note that for any variable \( w \), \( w_{s,t} = \sum_{c \text{ in state } s} w_{c,t} / \text{(#counties in states)} \) denotes the average state-level \( w \) at time \( t \). Denote \( N_s = \text{#counties in state } s \)

Using our instrument, and recalling that \( p_c \) denotes pharmacy density and \( \omega_{s,t} = \sum_{c \text{ in state } s} \omega_{c,t} \) denotes vaccine allocations:

\[
\begin{align*}
\tilde{z}_{c,t-2} &= \frac{z_{c,t-2}}{T} - \frac{z_{s,t-2}}{T} \\
\tilde{z}_{c,t-2} &= \sum_{c \text{ in state } s} p_c \omega_{c,t-2} - \sum_{c \text{ in state } s} p_c \sum_{c \text{ in state } s} \omega_{c,t-2} - \sum_{c \text{ in state } s} \omega_{c,t-2} \sum_{c \text{ in state } s} p_c / N_s \\
\tilde{z}_{c,t-2} &= p_c \omega_{s,t-2} - p_c \omega_s - \omega_{s,t-2} \bar{p}_s
\end{align*}
\]

where \( \omega_{s,t-2} = \sum_{c \text{ in state } s} \omega_{c,t-2} \), \( \omega_s = \sum_t \frac{\omega_{c,t-2}}{T} \) and \( \bar{p}_s = \sum_{c \text{ in state } s} p_c / N_s \). And thus

\[
\begin{align*}
\tilde{z}_{c,t-2} &= (p_c - \bar{p}_s) \omega_{s,t-2} - p_s \omega_s \\
\tilde{z}_{c,t-2} &= (p_c - \bar{p}_s) \omega_{s,t-2} - p_s \omega_s + \bar{p}_s \omega_s - \bar{p}_s \omega_s \\
\tilde{z}_{c,t-2} &= (p_c - \bar{p}_s) (\omega_{s,t-2} - \omega_s) - \bar{p}_s \omega_s
\end{align*}
\]

The above means that identification focuses on whether activity picked up most in counties with where pharmacies are densest as state level allocations of vaccines picked up relative to counties with lower pharmacy density within each state.\(^{14}\)

**Declarations**

**Conflict of interest** On behalf of all authors, the corresponding author states that there is no conflict of interest.

\(^{14}\) Note in our formulation of the instrument, we could have demeaned pharmacy density at the state level, which would make the last term in the equation above drop out.
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