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Replication of Atwood's (2022) "The Long-Term Effects of Measles Vaccination on Earnings and Employment"*

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Abstract

Atwood (2022) analyzes the effects of the 1963 U.S. measles vaccination on long-run labor market outcomes, using a generalized difference-in-differences approach. We reproduce the results of this paper and perform a battery of robustness checks. Overall, we confirm that the measles vaccination had positive labor market effects. While the negative effect on the likelihood of living in poverty and the positive effect on the probability of being employed are very robust across the different specifications, the headline estimate—the effect on earnings—is more sensitive to the exclusion of certain regions and survey years.

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1 Introduction

Atwood (2022) investigates the impact of the measles vaccine introduction in the United States on long-run labor market outcomes. Prior to the introduction, around 50% children would contract measles by age six and 95% by age 16. The introduction of the vaccine in 1963 was universal, with mass media campaigns and mandatory vaccination for children resulting in high take-up rates. The introduction of the vaccine was followed by an immediate drop in the measles incidence rate, approaching zero in the late 1960s. Arguably, the reductions in mortality and morbidity were larger than measles prevention alone would have suggested. Findings from the medical literature suggest that contracting measles can cause "immune amnesia," resulting in increased susceptibility to other childhood infectious diseases for up to five years after the onset of measles. Against this background, Atwood (2022) hypothesizes that the positive impact of the measles vaccine has been underestimated and that there are potentially long-term effects on human capital and labor market outcomes.

Using a generalized difference-in-differences approach, Atwood (2022) estimates the long-run effects of the measles vaccine introduction on labor market outcomes. The author takes advantage of two sources of variation: first, the prevaccine measles incidence rate differs across states and, second, exposure to the vaccine varies due to individuals' birth cohort. The main driver of differences in prevaccine incidence rates is population density, leading to larger decreases in the measles incidence in states with initially high incidence levels. The author combines two main data sources: Data on disease incidence comes mainly from Morbidity and Mortality Weekly Reports Annual Supplement of the Centers for Disease Control and Prevention (CDC). Labor market outcomes, such as income, employment, and hours worked, come from the American Community Survey for the years 2000–2017.

Atwood (2022) starts by estimating the effects of the measles vaccine introduction on the incidences of measles and other childhood diseases. The vaccine reduces measles incidences rapidly. Simultaneously, it reduces the incidences of other childhood diseases, such as pertussis, chicken pox, mumps, and rubella. Having established the health impact, the author estimates the effects of the vaccine on labor market outcomes: income, employment, hours worked, and poverty. Atwood (2022) describes her main results on page 35 as follows: "My estimates suggest that those individuals born in a state with the average measles incidence rate prevaccine with lifetime exposure to the measles vaccine earn $447 more per year than those without exposure to the measles vaccine, representing a 1.1 percent increase in income as adults. Additionally, I find exposure to the measles vaccine leads to a decrease in the probability of living in poverty, increases the likelihood of being employed, and does not have an impact on the number of hours worked in a week, suggesting income gains are attained through increased productivity."

Our paper investigates whether the analytical results in Atwood (2022) are replicable and tests the robustness of the main results on labor market outcomes with several alternative specifications. We test different sample restrictions, different sets of control variables, and alternative definitions of the treatment variable. Further, we extend the time period in the event-study graph regarding the effect of measles vaccination on the measles incidence rate.

We were able to reproduce the original study results in Atwood (2022) successfully using the provided code and data. Across the different sensitivity analyses that we performed for the effect on six labor-market related outcomes, the effects on poverty and employment status are strikingly robust. In contrast, the small but statistically significant main effect on hours worked is not very robust. The estimated effect loses statistical significance in several specifications and becomes even statistically significant with the opposite sign in other specifications. Even in Atwood (2022), the effect on hours worked is the most jumpy coefficient estimate. She describes the finding of one of her robustness tests as follows: "Hours worked per week changes sign and has statistical significance; however, the economic significance of the coefficients tells the same story–no economically significant change to hours worked per week" (Atwood, 2022, p. 53). The results of our sensitivity analysis align with this conclusion.

In contrast to the outcome "hours worked," the effects on two of the three income measures are rather robust across specifications. The effects on income for individuals with positive income and the effect on the natural logarithm of income are always positive and statistically significant in 13, respectively 14, of the 17 robustness checks. However, in several specifications, the point estimates differ by more than two standard errors from the original estimates. While these two income measures consider only individuals with non-zero income, the third income measure ("Income") takes into account the income information of all individuals. This third income measure is the most prominent outcome in the paper, as only for this outcome is the effect of the measles vaccination quantified in abstract and introduction of the original paper. However, the picture for this general income measure looks less stable in our robustness exercises. While the analyses pool information from the American Community Survey for the years 2000–2017, the income results are basically driven by the 2017 data release, which amounts to about one third of the overall sample size. The exclusion of the information from this data release results in a small and insignificant income effect and the estimated coefficients drop by almost 80 percent. The effect on this income measure is also small and no longer statistically significant in several other robustness checks.

Despite this sensitivity of the estimates on income to alternative modeling choices, we conclude that the main conclusion of Atwood (2022) holds: The measles vaccination had positive effects on long-run labor market outcomes. The effects on poverty and employment status are robust across many different specifications and also the effects on income for individuals with positive incomes hold in many specifications.

2 Reproducibility

We could successfully reproduce the results of all tables and figures based on the provided raw data and syntax files. There were only two minor issues. First, the Stata syntax files did not include the version command. This resulted in an error message for one of the syntax files. However, the README file provided the version used, so it was easy to fix. Second, there were some minor rounding/copying errors in the construction of the tables.[1]

3 Replication

After successfully reproducing the tables and figures in the original paper, we now test the robustness of the effect of measles vaccination on labor market outcomes (see Table 2 in Atwood (2022)) to (i) alternative sample restrictions; (ii) alternative sets of control variables; and (iii) alternative definitions of the treatment variable. Further, we extend the event study graph regarding the effect of measles vaccination on the measles incidence rate (see Table 3 in Atwood (2022)) by including additional pre-periods.

[1] Table 1: The share for "Female" in the NHES II panel should be 49.2 % and not 49.1 % and the p-value for "Female" in the NHES III panel should be 0.892 instead of 0.891. Appendix Table 4 reports a positive coefficient for Panel D, but the coefficient is in fact negative.

3.1 Alternative sample restrictions

This section probes the robustness of the main results to alternative sample restrictions. These sample restrictions refer to regions, age, survey years, and cohorts.

The main paper focuses on the full sample with the four census regions Northeast, North Central/Midwest, South, and West. In the first set of alternative sample restrictions, we exclude the Census regions one by one to investigate whether effects are driven by a particular region. In the first row of Table 1, we report the original estimates. In subsequent rows, we report the estimates from samples with excluded regions. In all tables, we follow the outline of Table 2 in Atwood (2022), with results for the different outcome variables in the different columns. For most outcomes, the direction and significance of the effects is the same compared to the original estimates, but effect sizes vary. The first column reports the estimates on income. The original estimate suggests an increase of $2,901, while the estimations excluding region North Central/Midwest or South yield coefficients of 687 (insignificant) and 6,449, respectively. Similarly, when excluding region South the estimates on positive income double in size (column 2). In contrast, the estimates for hours worked switch sign and turn insignificant when excluding region South, but they triple when excluding region North Central/Midwest. The estimates on the natural logarithm of income, poverty and employment are less sensitive to the exclusion of regions.

Now we turn to different age restrictions. While the paper states that the main analysis includes "individuals aged 25 to 60 at the time of survey" (Atwood, 2022, p. 43), the provided code reveals that individuals aged 25 and individuals aged 60 are not included. The last row in Table 1 shows that including these two additional age groups does not change the results in a meaningful way. The point estimates are very similar.

The next set of sample restrictions focuses on the included survey years. Atwood (2022) uses IPUMS microeconomic data from the American Community Survey (ACS) for the years 2000–2017. Data for the years 2005-2017 are multiyear estimates: years 2005-2007 stem from the 3-year-estimate release 2007 and years 2008-2012 and 2013-2017 from the 5-year-estimate releases 2012 and 2017, respectively.[2]

Table 2, Panel A shows the original estimate from Atwood (2022) in comparison to regres-

[2]The use of different multiyear estimates is not discussed in the paper. Here, 5-year-estimates (3-year-estimates) allow for including individuals from areas with a population of less than 65,000 (20,000) and come with the advantage of increased statistical reliability of the data for less populated areas and small population subgroups (see Beaghen et al., 2012, for a discussion of the use of multiyear estimates).

sions that exclude observations from the 2017 ACS multiyear distribution and the year 2017 as single survey year. While most outcomes are robust to the exclusion of different years and change only marginally in size, effects on income are very sensitive. The exclusion of multiyear 2017 results in a small and insignificant effect of the measles vaccination on income. The estimated coefficient drops by almost 80 percent, from 2,901 to 633. The effect on income for individuals with an income greater than zero is also reduced by about 40 percent and statistically different from the original estimate. Strikingly, the estimates for log income are relatively robust to the exclusion of multiyear 2017. This suggests that outliers in the ACS multiyear 2017 might be responsible for Atwood's positive findings on earnings. Excluding the single survey year 2017 also leads to a reduction of the estimate on income of almost 25 percent.

Table 2 also reports results of regressions that use pooled ACS survey years 2000-2004, as well as multiyear estimates 2007, 2012, and 2017 only.[3] Effect estimates on income are extremely sensitive to the use of different ACS multiyears. While the effect is very large and statistically significant for results based on multiyear 2017, the results are statistically indistinguishable from zero and vary in sign for the years 2000-2004, as well as multiyears 2007 and 2012. The results on income of the employed also seem to be largely driven by ACS multiyear 2017. Results for log income are positive and mostly significant across different multiyears, but they are twice as large for regressions based on data from the 2017 multiyear estimate. Results for non-income labor market outcomes, however, are largely robust to the use of different ACS (multi)years.

Ex-ante, it is unclear why the effects on income should depend on the inclusion of multiyear 2017. In light of the robustness of findings for other labor market outcomes, reduced statistical power seems to be an unlikely explanation. The extreme results for income in levels in survey year 2017 (see Appendix Table A.1) suggest that outliers in the 2017 multiyear could drive Atwood's main estimate on income. With this explanation, it is also plausible why results for log income are relatively robust at the same time.

The last set of robustness checks with alternative sample restrictions focuses on cohorts. The original analysis pools cohorts born between 1941 and 1991, while controlling for year-

[3] Appendix Table A.1 shows estimates on labor market outcomes separately for each survey year of the ACS. While all estimates on income are statistically indistinguishable from zero and have different signs, using survey year 2017 only results in an extremely large effect on income of $15,568. The only other significant effect is large but negative, with an estimate of $-6,309. Overall, these results suggest that income data from ACS survey years are subject to influential outliers.

of-birth fixed effects in the regression analysis. To test whether certain cohorts drive the overall effect, we zoom into cohort subsamples. In particular, we start with a narrow band, including individuals born between 1941-1951, and successively expand the cohort band by five year groups until 1971. Different cohort restrictions also imply different composition of exposure duration. In the original sample, 55% of the sample has 16 years of exposure. In our sensitivity test, the narrowest band of cohorts is composed of individuals with zero to three years of exposure, followed by maximum of eight, 13 and 16 years of exposure. Thus, our cohort restrictions can have implications for the results not only through age effects, but also through exposure composition differences. Table 3 shows the results.

While the results always retain the effect direction suggested by the original analysis, the point estimates deviate significantly. We consistently find that the labor market returns are substantially larger in absolute terms for older cohorts, and approach the main estimates as we include younger cohorts. Only the outcome "hours worked" diverges from this general pattern. The results for this outcome vary from large, significant, and positive to negative and insignificant effects, depending on the cohort restrictions.

3.2 Alternative sets of control variables

Table 4 provides results for alternative sets of control variables. While the main specification in Atwood (2022) includes fixed effects for each release of the data (i.e., each multiyear), the first robustness check in this section includes fixed effects for each survey year (which are more granular than the multiyear-fixed effects; see the discussion relating to Table 2). This reduces the standard errors slightly but does not change substantially the estimates of the treatment effect.

Variation in the treatment variable comes from variation across birth cohorts and across states. Hence, cross-sectional data would be sufficient for the identification of vaccination effects. Pooling information from different data releases and survey years is expected to mainly improve statistical power by increasing the number of observations. Therefore, in the following robustness exercise, we include fixed effects for each combination of multiyear and state as well as for each combination of multiyear and birth cohort. While the effects on the outcomes "income (if > 0)," "ln income," "poverty," and "employed" decrease in absolute terms, the point estimates are still statistically significant. In contrast, the effect on "income" decreases to less than one-third of the original point estimate and becomes statistically insignificant. These patterns are very similar to the specification excluding data from the 2017 data release (second specification in Table 2).

3.3 Alternative definitions of the treatment variable

The coefficient of interest in the main model relates to an interaction term of (i) the unweighted average 12-year prevaccination infection rate for children per 100,000 in an individual's state of birth; and (ii) the number of years individuals are exposed to the vaccine. Exposure is zero for older cohorts, increases linearly for children born in the 16 years prior to 1963, and is 16 for children born after 1963. Atwood (2022) uses 16 as the maximum period of exposure because incidence rates of measles after age 16 are negligible.

This model assumes a linear relationship between the vaccine treatment effect and the time of exposure. However, without an available vaccine, 50 percent of all children will have contracted the measles by age 6 and 95 percent by age 16. Hence, the vaccine might be much more relevant and effective for younger children, as the probability to have contracted the measles is also not growing linearly with age.

To test the sensitivity of the findings to the linearity assumption, we alter the model in several ways. First, we use a log-transformation of the exposure variable, setting log(0) to 0. The log transformation assigns larger values to children who are very young when the vaccine was introduced and, at an accelerating rate, smaller values for older individuals. Second, we substitute the linear exposure variable with a dummy indicating whether individuals were below age 16 when the vaccine was introduced. Third, we focus the linear treatment indicator on children with up to six years of exposure, assigning all individuals seven years and older at the time of the measles vaccine to the control group. In a final check, we focus the linear treatment indicator on children up to age ten. All individuals 11 years and older at the time of the measles vaccine enter the control group. As we change the scale of the treatment variable, the scale of the estimated treatment effects also vary. Table 5 shows that the estimated coefficients exhibit very similar patterns to the main results. This reassures that the main findings of the paper are robust to varying the linearity assumption on exposure to the vaccine.

Finally, we also alter the first term of the interaction, the average-12-year measles rate. We split the birth states in two groups and substitute the continuous measure of the pretreatment measles rate with an indicator for whether states had a measles rate above the median. To compare coefficient estimates, we need to take into account that the mean of the new variable is about 49 times larger than the mean of the original variable. Still, Table 5 cannot confirm the significant effect on positive income and the coefficient estimate on hours worked even flips the sign.

3.4 Event-Study for Measles Incidence Rate

The original paper reports the effects of the measles vaccine introduction on measles incidence rates with an event study approach. The author shows five pre-treatment periods that allow the reader to assess the plausibility of the common trend assumption. The subsequent analysis of long-term labor market outcomes also incorporates cohorts born much earlier. For this reason, it is also important that the common trend assumption between higher and lower incidence regions holds for a longer pre-treatment period. The data includes seven additional pre-period years that we use to extend the time-frame of the original event study. Figure 1, Panel A, replicates the original results. Panel B reports the results with the extended pre-period. It shows that, even for older cohorts, there are no differences in time trends between regions with higher and lower measles incidence rates, providing additional support for the underlying common trend assumption in the measles incidence rate.

4 Conclusion

The present paper tests the reproducibility and replicability of the main results in Atwood (2022). The original paper estimates the effect of the measles vaccine introduction on six labor market outcomes. The author uses an empirical design akin to a generalized difference-in-differences approach. The necessary variation comes from cross-state differences in pre-vaccine infection prevalence rates and the length of exposure to the vaccine, determined by individuals' year of birth.

Using the author's Stata code, we first document that the results are reproducible. We then conduct several sensitivity checks. We impose alternative sample restrictions, include alternative sets of control variables, alter the definitions of the treatment variable, and extend the periods in the event study graph. Our checks suggest that two outcomes in particular–employment and poverty rate–are very robust to the choice of specifications. The outcome "hours worked," on the other hand, shows significant variability depending on the specification, including changes in magnitude, significance levels, and even signs.

The results for the central outcome, income, are mixed. While the majority of the specifications suggest a positive effect of the vaccine on income, the sign, statistical significance, and the magnitude vary depending on the specification. The estimates vary most when income is measured in levels. We find that the income results are largely driven by the 2017 data release, which amounts to about one third of the overall sample.

These fluctuations in the estimates notwithstanding, we conclude that the general findings of Atwood (2022) remain unaffected: The measles vaccination had positive effects on long-run labor market outcomes.

References

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Tables

Table 1: Robustness: Alternative sample restrictions I (Census regions)

	Income	Income if >0	ln Income	Poverty	Employed	Hours worked
Original						
Vaccination effect	2,901***	4,681***	0.11005***	-0.03259***	0.01855***	-1.2511***
	(863.78)	(1201.8)	(0.02332)	(0.00395)	(0.00324)	(0.37693)
	[15,710,435]	[12,126,516]	[12,126,516]	[15,429,840]	[12,593,724]	[15,710,435]
Excluded region: Northeast						
Vaccination effect	2,120**	4,145***	0.08665***	-0.01953***	0.01584***	-0.67509*
	(901.36)	(1262.5)	(0.02875)	(0.00446)	(0.00384)	(0.39758)
	[12,145,778]	[9,310,127]	[9,310,127]	[11,910,575]	[9,665,231]	[12,145,778]
Excluded region: North Central/Midwest						
Vaccination effect	691.46	3,606**	0.08146***	-0.04003***	0.02073***	-4.6986***
	(1208.7)	(1513)	(0.02485)	(0.0059)	(0.00392)	(0.42024)
	[11,163,172]	[8,523,636]	[8,523,636]	[10,942,892]	[8,851,693]	[11,163,172]
Excluded region: South						
Vaccination effect	6,422***	8,055***	0.14189***	-0.03867***	0.01769***	0.30357
	(1072.2)	(1480.7)	(0.02738)	(0.00472)	(0.00373)	(0.41238)
	[10,552,218]	[8,270,636]	[8,270,636]	[10,399,814]	[8,608,147]	[10,552,218]
Excluded region: West						
Vaccination effect	3,205***	4,352***	0.13049***	-0.03277***	0.01867***	-0.39655
	(873.05)	(1235.1)	(0.0224)	(0.00405)	(0.00334)	(0.3464)
	[13,270,137]	[10,275,149]	[10,275,149]	[13,036,239]	[10,656,101]	[13,270,137]
Additional ages						
Vaccination effect	2,576***	4,208***	0.10819***	-0.03166***	0.01852***	-1.3543***
	(821.41)	(1176.3)	(0.02302)	(0.00381)	(0.00314)	(0.36874)
	[16,610,082]	[12,762,121]	[12,762,121]	[16,310,481]	[13,236,849]	[16,610,082]

Source: Authors' calculations using ACS data for the years 2000–2017 (individual outcomes) and Current Population Reports and MMWR Annual Reports (1952-1963 measles prevaccine incidence rate).
Notes: The table shows regression estimates for the impact of the measles vaccine on adult labor market outcomes based on Eq. 2 of Atwood (2022). Each column represents a separate outcome as listed in the column heading, each row represents results from a separate regression that excludes a different Census region. The first rows replicate the original results from Atwood (2022). Number of observations in brackets. All dollar values are in 2018 dollars. Standard errors are clustered at the state-of-birth by year-of-birth level and reported in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 2: Robustness: Alternative sample restrictions II (Different multiyears)

	Income	Income if >0	ln Income	Poverty	Employed	Hours worked
Panel A:						
Original						
Vaccination effect	2,901***	4,681***	0.11005***	-0.03259***	0.01855***	-1.2511***
	(863.78)	(1201.8)	(0.02332)	(0.00395)	(0.00324)	(0.37693)
	[15,710,435]	[12,126,516]	[12,126,516]	[15,429,840]	[12,593,724]	[15,710,435]
w/o multiyear 2017						
Vaccination effect	633.22	2,766**	0.08599***	-0.02537***	0.01743***	-1.9235***
	(821.81)	(1158)	(0.02236)	(0.00402)	(0.00337)	(0.39549)
	[10,516,927]	[8,160,217]	[8,160,217]	[10,365,626]	[8,470,515]	[10,516,927]
w/o survey year 2017						
Vaccination effect	2,208**	4,039***	0.10269***	-0.03194***	0.01938***	-1.356***
	(856.64)	(1200.3)	(0.0232)	(0.004)	(0.00326)	(0.37882)
	[14,679,446]	[11,328,818]	[11,328,818]	[14,424,879]	[11,770,117]	[14,679,446]
Panel B:						
Survey years 2000-2004						
Vaccination effect	-342.39	1,354	0.04473	0.00182	0.01229**	-1.8538***
	(1270.6)	(1531.7)	(0.02884)	(0.00588)	(0.00523)	(0.47398)
	[1,931,886]	[1,542,325]	[1,542,325]	[1,931,886]	[1,567,710]	[1,931,886]
Multiyear 2007						
Vaccination effect	763.82	3,608**	0.10512***	-0.01306**	0.00767*	-3.4113***
	(1164.1)	(1466.4)	(0.03275)	(0.00555)	(0.00449)	(0.56597)
	[3,237,244]	[2,540,001]	[2,540,001]	[3,201,785]	[2,600,711]	[3,237,244]
Multiyear 2012						
Vaccination effect	-1,425	1,287	0.07536*	-0.03277***	0.01785***	-2.8479***
	(1432.7)	(2165.6)	(0.04007)	(0.00655)	(0.00578)	(0.59732)
	[5,347,797]	[4,077,891]	[4,077,891]	[5,231,955]	[4,302,094]	[5,347,797]
Multiyear 2017						
Vaccination effect	6,127**	9,077**	0.20192**	-0.03624***	0.03619***	-3.2596***
	(2650.3)	(4160)	(0.08055)	(0.01244)	(0.00937)	(0.9952)
	[5,193,508]	[3,966,299]	[3,966,299]	[5,064,214]	[4,123,209]	[5,193,508]

Source: Authors' calculations using ACS data for the years 2000–2017 (individual outcomes) and Current Population Reports and MMWR Annual Reports (1952-1963 measles prevaccine incidence rate).
Notes: The table shows regression estimates for the impact of the measles vaccine on adult labor market outcomes based on Eq. 2 of Atwood (2022). Each column represents a separate outcome as listed in the column heading, each row represents a separate regression with varying underlying ACS (multi)years. The first rows replicate the original results from Atwood (2022). Number of observations in brackets. All dollar values are in 2018 dollars. Standard errors are clustered at the state-of-birth by year-of-birth level and reported in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 3: Robustness: Alternative sample restrictions III (Different cohorts)

	Income	Income if >0	ln Income	Poverty	Employed	Hours worked
Original: 1941-1991						
Vaccination effect	2,901***	4,681***	0.11005***	-0.03259***	0.01855***	-1.2511***
	(863.78)	(1201.8)	(0.02332)	(0.00395)	(0.00324)	(0.37693)
	[15,710,435]	[12,126,516]	[12,126,516]	[15,429,840]	[12,593,724]	[15,710,435]
Cohorts 1941-1951						
Vaccination effect	21,652***	14,545*	0.4519***	-0.03889	0.08196**	9.5956***
	(6666.7)	(8487.4)	(0.15325)	(0.03632)	(0.03315)	(2.434)
	[1,192,478]	[829,641]	[829,641]	[1,188,632]	[858,736]	[1,192,478]
Cohorts 1941-1956						
Vaccination effect	8,767***	7,400**	0.12523**	-0.04327***	0.03091***	3.8099***
	(2576.9)	(3291.5)	(0.04857)	(0.01185)	(0.00978)	(0.91945)
	[3,183,950]	[2,282,293]	[2,282,293]	[3,164,101]	[2,390,551]	[3,183,950]
Cohorts 1941-1961						
Vaccination effect	8,200***	8,278***	0.14667***	-0.04616***	0.02482***	1.9229***
	(1422.2)	(1638.6)	(0.02612)	(0.0059)	(0.00509)	(0.53865)
	[5,961,961]	[4,364,347]	[4,364,347]	[5,910,210]	[4,584,643]	[5,961,961]
Cohorts 1941-1966						
Vaccination effect	5,633***	7,107***	0.11069***	-0.03046***	0.0155***	0.17052
	(952.16)	(1211.1)	(0.0242)	(0.00413)	(0.00339)	(0.43126)
	[[8,507,463]	[6,337,925]	[6,337,925]	[8,417,235]	[6,654,731]	[8,507,463]
Cohorts 1941-1971						
Vaccination effect	5,205***	7,071***	0.11174***	-0.02781***	0.01338***	-0.45221
	(854.51)	(1071.3)	(0.02139)	(0.00381)	(0.00328)	(0.40187)
	[10,709,934]	[8,087,014]	[8,087,014]	[10,580,109]	[8,471,427]	[10,709,934]

Source: Authors' calculations using ACS data for the years 2000–2017 (individual outcomes) and Current Population Reports and MMWR Annual Reports (1952-1963 measles prevaccine incidence rate).
Notes: The table shows regression estimates for the impact of the measles vaccine on adult labor market outcomes based on eq. 2 of Atwood (2022). Each column represents a separate outcome as listed in the column heading, each row represents a separate regression model with varying restrictions on cohorts. The first rows replicate the original results from Atwood (2022). All dollar values are in 2018 dollars. Standard errors are clustered at the state-of-birth by year-of-birth level and reported in parentheses. $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 4: Robustness: Alternative sets of control variables

	Income	Income if >0	ln Income	Poverty	Employed	Hours worked
Original						
Vaccination effect	2,901***	4,681***	0.11005***	-0.03259***	0.01855***	-1.2511***
	(863.78)	(1201.8)	(0.02332)	(0.00395)	(0.00324)	(0.37693)
	[15,710,435]	[12,126,516]	[12,126,516]	[15,429,840]	[12,593,724]	[15,710,435]
With survey-year FE						
Vaccination effect	2,917***	4,688***	0.11023***	-0.03269***	0.01856***	-1.2442***
	(861.63)	(1199.9)	(0.02328)	(0.00395)	(0.00322)	(0.37661)
	[15,710,435]	[12,126,516]	[12,126,516]	[15,429,840]	[12,593,724]	[15,710,435]
Multiyear-cohort and multiyear-state FE						
Vaccination effect	842.26	3,350***	0.09633***	-0.02219***	0.01521***	-2.8712***
	(851.43)	(1288.8)	(0.02536)	(0.00394)	(0.00312)	(0.3917)
	[15,710,435]	[12,126,516]	[12,126,516]	[15,429,840]	[12,593,724]	[15,710,435]

Source: Authors' calculations using ACS data for the years 2000–2017 (individual outcomes) and Current Population Reports and MMWR Annual Reports (1952-1963 measles prevaccine incidence rate).
Notes: The table shows regression estimates for the impact of the measles vaccine on adult labor market outcomes based on Eq. 2 of Atwood (2022). Each column represents a separate outcome as listed in the column heading, each row represents results from a separate regression that includes additional variables. The first rows replicate the original results from Atwood (2022). Number of observations in brackets. All dollar values are in 2018 dollars. Standard errors are clustered at the state-of-birth by year-of-birth level and reported in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 5: Robustness: Alternative definition of treatment variable

	Income	Income if >0	ln Income	Poverty	Employed	Hours worked
Original						
$M^{pre}_{1952--1963}$ * *Exposure 0-16 years*	2,901***	4,681***	0.11005***	-0.03259***	0.01855***	-1.2511***
	(863.78)	(1201.8)	(0.02332)	(0.00395)	(0.00324)	(0.37693)
	[15,710,435]	[12,126,516]	[12,126,516]	[15,429,840]	[12,593,724]	[15,710,435]
Log exposure						
— * *log(Exposure)*	24887***	35228***	0.78011***	-0.22103***	0.12915***	-4.5539*
	(5556.8)	(7433.1)	(0.14794)	(0.02761)	(0.02318)	(2.7387)
	[15,710,435]	[12,126,516]	[12,126,516]	[15,429,840]	[12,593,724]	[15,710,435]
0/1 exposure						
— * $1\{0 < \textit{Exposure} \leq 16 \textit{ years}\}$	75963***	81599***	1.8044***	-0.40946***	0.38715***	11.187
	(19116)	(24987)	(0.5203)	(0.09463)	(0.09756)	(9.7898)
	[15,710,435]	[12,126,516]	[12,126,516]	[15,429,840]	[12,593,724]	[15,710,435]
Exposure until age 6						
— * *Exposure 0-6 years*	2,600	6,116**	0.16687***	-0.05062***	0.02824***	-3.3677***
	(1681.9)	(2428.8)	(0.04356)	(0.00736)	(0.00557)	(0.59289)
	[15,710,435]	[12,126,516]	[12,126,516]	[15,429,840]	[12,593,724]	[15,710,435]
Exposure until age 10						
— * *Exposure 0-10 years*	2,848**	5,180***	0.12787***	-0.03855***	0.02151***	-1.947***
	(1124.4)	(1592)	(0.03025)	(0.00499)	(0.00386)	(0.43652)
	[15,710,435]	[12,126,516]	[12,126,516]	[15,429,840]	[12,593,724]	[15,710,435]
With indicator for measles rate above median						
$1\{M^{pre}_{1952--1963} > \text{Median}\}$ * —	34.619***	13.93	0.00044*	-0.00019***	0.00014***	0.00791*
	(12.344)	(17.017)	(0.00025)	(0.00005)	(0.00004)	(0.00436)
	[15,710,435]	[12,126,516]	[12,126,516]	[15,429,840]	[12,593,724]	[15,710,435]

Source: Authors' calculations using ACS data for the years 2000–2017 (individual outcomes) and Current Population Reports and MMWR Annual Reports (1952-1963 measles prevaccine incidence rate).
Notes: The table shows regression estimates for the impact of the measles vaccine on adult labor market outcomes based on Eq. 2 of Atwood (2022). Each column represents a separate outcome as listed in the column heading, each row represents a separate regression model with varying definitions of the main treatment variable. The first rows replicate the original results from Atwood (2022). All dollar values are in 2018 dollars. Number of observations in brackets. Standard errors are clustered at the state-of-birth by year-of-birth level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figures

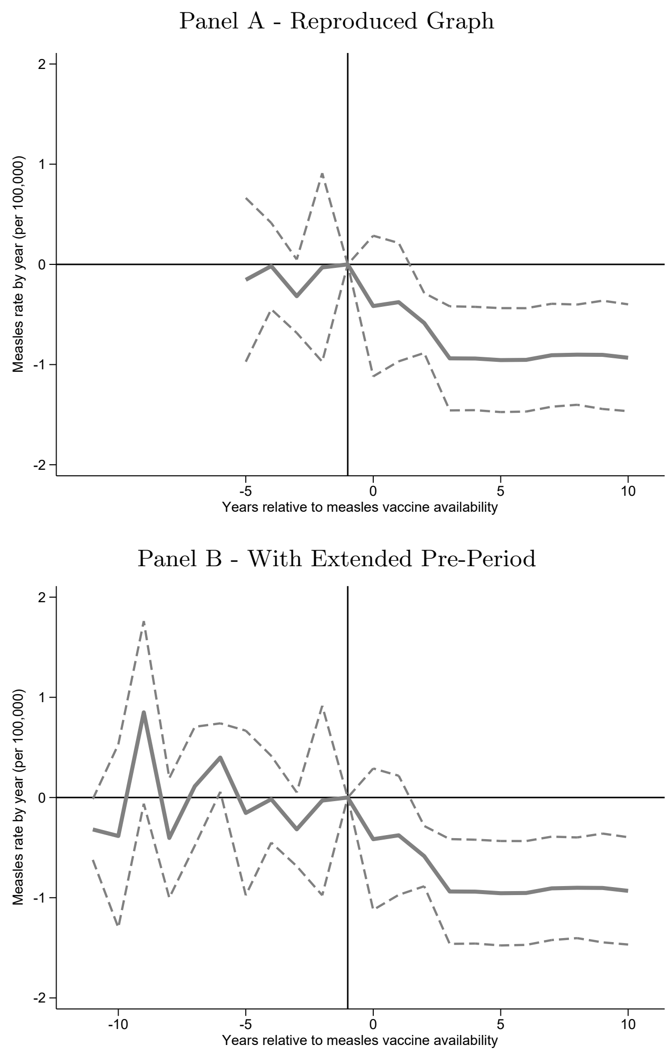

Figure 1: Event Study — Effect of Measles Vaccine on Measles Incidence

Notes: The figure shows regression-adjusted estimates of the measles vaccine's intention-to-treat effect on measles incidence with an extended pre-period if compared to Table 3 in Atwood (2022). Dependent variable: Number of measles cases by year for a state per 100,000 of the population. The solid line plots the estimated coefficients on interactions between the time to measles vaccine dummies and the average 12-year pre–measles vaccine measles incidence. The model includes state fixed effects and controls for the susceptible population. The dashed lines are pointwise 95 percent confidence intervals based on standard errors clustered at the state level.

Appendices

Table A.1: Robustness: Alternative sample restrictions IV (different survey years)

	Income	Income if >0	ln Income	Poverty	Employed	Hours worked
Panel A: Survey years 2000–2004						
Survey year 2000						
Vaccination effect	2,742	3,849	0.05317	0.03893**	0.01184	0.70004
	(3647.1)	(4406.7)	(0.08305)	(0.01731)	(0.01487)	(1.2247)
	[145,383]	[118,456]	[118,456]	[145,383]	[118,208	[145,383]
Survey year 2001						
Vaccination effect	-189.69	2,561	0.04103	0.00499	0.00605	-3.0213***
	(2459.2)	(2836.7)	(0.04853)	(0.01018)	(0.00918)	(0.80601)
	[463,665]	[372,206]	[372,206]	[463,665]	[376,595	[463,665]
Survey year 2002						
Vaccination effect	-555.51	-864.16	0.00059	0.01478	0.02035*	-0.44789
	(2539.5)	(2970.9)	(0.04983)	(0.01097)	(0.0108)	(0.87723)
	[415,008]	[331,868]	[331,868]	[415,008]	[337,597	[415,008]
Survey year 2003						
Vaccination effect	-1,363	-6.2592	0.07277	-0.01752	0.00246	-1.5423*
	(2241.2)	(2696.7)	(0.05431)	(0.01139)	(0.01015)	(0.9142)
	[455,415]	[361,424]	[361,424]	[455,415]	[369,588	[455,415]
Survey year 2004						
Vaccination effect	-49.363	2,873	0.05884	-0.00071	0.0182*	-3.2254***
	(2115.1)	(2442)	(0.05152)	(0.01158)	(0.01032)	(0.91983)
	[452,415]	[358,371]	[358,371]	[452,415]	[365,722	[452,415]
Panel B: Multiyear 2007						
Survey year 2005						
Vaccination effect	1,105	3,672*	0.1079**	-0.01451*	0.0033	-2.6317***
	(1743)	(2017.5)	(0.04395)	(0.00871)	(0.00722)	(0.74505)
	[1,069,486]	[844,302]	[844,302]	[1,069,486]	[860,334	[1,069,486]
Survey year 2006						
Vaccination effect	-185.2	2,437	0.10945***	-0.02316***	0.00853	-3.4762***
	(1782)	(2185)	(0.04124)	(0.0086)	(0.00736)	(0.71297)
	[1,087,691]	[850,054]	[850,054]	[1,070,441]	[873,106	[1,087,691]
Survey year 2007						
Vaccination effect	1,053	4,764*	0.09885**	-5.8e-05	0.01151	-4.5011***
	(2106.4)	(2507.2)	(0.04659)	(0.00804)	(0.00735)	(0.83759)
	[1,080,067]	[845,645]	[845,645]	[1,061,858]	[867,271	[1,080,067]
Panel C: Multiyear 2012						
Survey year 2008						
Vaccination effect	-2,337	-1,706	0.01611	-0.02225**	0.01257	-1.7189**
	(2315)	(2899.6)	(0.05416)	(0.00979)	(0.00843)	(0.84286)
	[1,080,522]	[849,017]	[849,017]	[1,061,442]	[883,084	[1,080,522]
Survey year 2009						
Vaccination effect	-384.66	2,094	0.07444	-0.02229**	0.00713	-3.5729***
	(2231.7)	(3211)	(0.05076)	(0.01071)	(0.0099)	(0.86276)
	[1,074,520]	[833,259]	[833,259]	[1,055,064]	[874,738	[1,074,520]
Survey year 2010						
Vaccination effect	-6,309***	-686.22	0.07324	-0.00244	0.03441***	-4.1541***
	(2241.1)	(3067.7)	(0.06258)	(0.01216)	(0.01081)	(0.88795)
	[1,072,686]	[815,119]	[815,119]	[1,054,215]	[869,254	[1,072,686]
Survey year 2011						
Vaccination effect	2,144	7,095*	0.09386	-0.03264**	0.01297	-3.7601***
	(3016.5)	(4180.1)	(0.08351)	(0.01554)	(0.01339)	(1.2174)
	[1,064,511]	[790,746]	[790,746]	[1,033,975]	[840,219	[1,064,511]
Survey year 2012						
Vaccination effect	-2,661	-426.14	0.07631	-0.03838**	0.0075	-4.5101***
	(2863.3)	(3902.7)	(0.08781)	(0.01749)	(0.01363)	(1.0054)
	[1,055,558]	[789,750]	[789,750]	[1,027,259]	[834,799	[1,055,558]

Notes: Continued on next page

	Income	Income if >0	ln Income	Poverty	Employed	Hours worked
Panel D: Multiyear 2017						
Survey year 2013						
Vaccination effect	4,590	6,098	0.11816	-0.04064**	0.02882	-2.0352
	(4116.5)	(5555.1)	(0.10285)	(0.01957)	(0.01855)	(1.6561)
	[1,058,287]	[800,310]	[800,310]	[1,032,321]	[838,878]	[1,058,287]
Survey year 2014						
Vaccination effect	3,858	4,140	0.17234*	-0.02672	0.07135***	-0.69277
	(4339)	(5762.5)	(0.1006)	(0.0198)	(0.01519)	(1.6199)
	[1,042,157]	[790,486]	[790,486]	[1,016,314]	[824,494]	[1,042,157]
Survey year 2015						
Vaccination effect	5,019	12651**	0.28705**	-0.05073**	0.04365**	-5.4681***
	(4260.3)	(5853.8)	(0.13948)	(0.02431)	(0.01716)	(1.7386)
	[1,036,059]	[789,419]	[789,419]	[1,010,626]	[820,194]	[1,036,059]
Survey year 2016						
Vaccination effect	1,029	2,493	0.2029	-0.06044**	0.02363	-5.8711***
	(5272.8)	(8181)	(0.16083)	(0.02965)	(0.02072)	(1.8828)
	[1,026,016]	[788,386]	[788,386]	[999,992]	[816,036]	[1,026,016]
Survey year 2017						
Vaccination effect	15,568*	26,374**	0.32108*	-0.04016	0.02252	-7.3944***
	(8086.9)	(11847)	(0.17944)	(0.04703)	(0.02422)	(2.1318)
	[1,030,989]	[797,698]	[797,698]	[1,004,961]	[823,607]	[1,030,989]

Source: Authors' calculations using ACS data for the years 2000–2017 (individual outcomes) and Current Population Reports and MMWR Annual Reports (1952-1963 measles prevaccine incidence rate).
Notes: The table shows regression estimates for the impact of the measles vaccine on adult labor market outcomes based on Eq. 2 of Atwood (2022). Each column represents a separate outcome as listed in the column heading, each row represents a regression for separate ACS survey years. The first row replicates the original results from Atwood (2022). All dollar values are in 2018 dollars. Horizontal lines indicate different ACS multiyears. Number of observations in brackets. Standard errors are clustered at the state-of-birth by year-of-birth level and reported in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.