On the Long-Term Effects of the 1918 U.S. Influenza Pandemic

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Abstract

Leveraging the 1918 Spanish influenza pandemic, Almond (2006) concludes that in utero influenza exposure has a large, negative impact on adult socio-economic status using two identification strategies. The first strategy compares outcomes of the U.S. birth cohorts in utero during the pandemic with those of surrounding birth cohorts. A key assumption underlying this strategy is that these cohorts are otherwise statistically exchangeable. The validity of that assumption is investigated using data from the 1920 U.S. Census. We document that the exposed cohorts were born to families of lower socio-economic status relative to those who were not exposed. For example, fathers of the 1919 birth cohort were less likely to be literate, worked in lower-earning occupations, had lower socioeconomic status, were older, less likely to be white, and had higher fertility than the fathers of surrounding birth cohorts. To explore the importance of this assumption, proxies for parental background are constructed using the 1920 Census and included in models of adult socio-economic status. Those estimates do not support the conclusion that individuals born in 1919 have systematically worse socio-economic outcomes in adulthood relative to surrounding birth cohorts. Almond's second identification strategy constructs measures of maternal exposure to influenza and estimates dose-response effects. Replication of that approach does not provide evidence in support of Almond's conclusion.

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1. Introduction

Influential work by Almond (2006) leverages the 1918 Spanish Influenza pandemic to estimate the causal effect of fetal health on human capital and economic outcomes in adulthood. He uses U.S. Census data to compare adult outcomes of the exposed birth cohort, those whose mothers had the highest probability of being exposed to influenza during the pregnancy, with comparable cohorts that were not exposed *in utero* using two identification strategies. First, the 1919 birth cohort is compared with those born in surrounding cohorts, 1912-1922. Second, state-specific proxies for influenza exposure (maternal infection rates) are used to isolate a dose-response effect among the 1918-1920 birth cohorts. Almond concludes that the exposed cohorts completed significantly less education and earned less as adults than those who were not exposed and, therefore, fetal health has a causal impact on socio-economic status (SES) in adulthood.

The validity of the first identification strategy depends on the assumption that the exposed and unexposed birth cohorts are statistically exchangeable. We assess the validity of this necessary condition for identification using the 1920 U.S. Census. Relative to those of surrounding cohorts, fathers of the 1919 birth cohort are negatively selected: they are more likely to be illiterate, work in lower-earning occupations, have SES, older, non-white and have more children. These results, which are not explained by age heaping or selective infant mortality, are corroborated by Beach, Ferrie and Saavedra (BFS) (2021) using a sample of World War II male enlistees. The assumption that the exposed and unexposed birth cohorts are statistically exchangeable is rejected which invalidates the identification strategy. Moreover, SES differences by birth quarter fail to line up with the timing of the pandemic providing additional evidence contradicting Almond's conclusion.

Measuring the extent of the bias caused by selection is not straightforward. Using proxies to adjust for paternal background, we find no consistent evidence for males, females or nonwhites that the 1919 birth cohort have worse adult SES outcomes than the comparison cohorts. The same conclusion follows from estimates by BFS that take into account observed and unobserved paternal background in models with sibling fixed effects.

Turning to the dose-response analyses, after correcting data errors in Almond (2006), we reproduce estimates of the effect of in utero influenza exposure on adult SES of males, female and non-white. Only two of 45 estimates are significantly negative and three are significantly positive. We conclude that the estimates from Almond's second identification strategy do not validate the hypothesis that in utero exposure to the 1918 Spanish influenza in the U.S. caused worse SES in adulthood. Following a parallel approach but using city-specific influenza deaths to measure pandemic exposure and comparing the 1918/19 cohorts of male enlistees, BFS find mixed evidence of impacts on human capital.[1]

Overall, our replication of Almond (2006) provides little scientific support for the conclusion that the 1918 flu pandemic in the United States had a causal impact on SES in adulthood. These results are important from the perspective of science and policy, *a fortiori*, as studies investigate the impacts of in utero exposure to economic, health and environmental shocks, including the COVID-19 pandemic.

2. Assessment of the evidence comparing birth cohorts

Adult SES of the 1919 birth cohort

Barker (1994) posited that the long arm of in utero health insults reach into biological markers of health in adulthood tracing out physiological mechanisms underlying the Fetal Origins Hypothesis (FOH). Given the evidence establishing post-natal interventions can change SES trajectories (Heckman, 2006), whether FOH extends to adult socio-economic status (SES) is an empirical question.

Almond (2006) argues the 1918 influenza pandemic in the U.S. is well suited to assess the long-term effects of in utero health on SES in adulthood because when it struck, in October 1918, it was unanticipated, and its impact was immediate, short-lived[2] and widespread with pregnant and child-bearing age females at elevated risk of infection. He contrasts indicators of SES in adulthood of the 1919 birth cohort with surrounding cohorts, 1912 to 1918 and 1920 to 1922 using the 1960 (1% sample), 1970 (combined 3% sample) and 1980 (5% sample) U.S. Censuses from IPUMS. His primary specification measures the effect on adult SES, y_i , of being born in 1919, $I_i(YOB=1919)$, relative to the comparison birth years since he controls for birth year, YOB_i, and its quadratic, YOB_i^2 :

$$y_i = \beta_0 + \beta_1 \cdot YOB_i + \beta_2 \cdot YOB_i^2 + \beta_3 \cdot I_i(YOB=1919) + \varepsilon_i \qquad [1]$$

Table 1 presents estimates of the 1919 birth cohort deviation, $\hat{\beta}_3$ for males in 1960 from Almond (2006), in column 1, and our replication in column 2.[3] Relative to those born in surrounding cohorts, males born in 1919 are significantly less likely to have graduated from high school, completed fewer years of education, have lower wage income, are more likely to be poor and have lower scores on the Duncan's Socioeconomic Index (SEI), an indicator of SES that is based on the occupation of the individual.

The unanticipated onset of the pandemic is key for identification as it rules out

[1]The BFS sample of WWII enlistees precludes examining females or nonwhites.
[2]85% of all influenza deaths in the U.S. occurred between October 1918 and January 1919.
[3]The differences between Almond's estimates and the replication estimates likely reflect differences in the public release versions of the IPUMS samples.

anticipatory behavioral responses related to conception for births in late 1918 and early 1919. It is not clear, however, that conceptions after October 1918 were unrelated to the pandemic. Evidence indicates that experiencing the influenza pandemic affected subsequent fertility, at least outside the U.S. (Boberg-Fazlic et al, 2016). Thus, these later cohorts are unlikely to be valid comparison cohorts and are excluded in column 3 of Table 1. Estimates of $\hat{\beta}_3$ are similar to those using the 1912-1922 cohorts and larger in magnitude for high school graduation, years of education and the Duncan SEI. Restricting to these cohorts has the advantage that it is possible to test whether the 1919 birth cohort is exchangeable with the earlier cohorts by drawing on paternal characteristics measured in the 1920 Census.

Table 1: Differences in adult SES of 1919 birth cohort relative to surrounding cohorts Using males in the 1960 Census data

	Born in 1919			
	Relative to 1912-1922 cohorts		Relative to	Relative to
Socio-economic outcome in adulthood	Almond (2006)	Replication	1912-1918 cohorts	1915-1918 cohorts
	(1)	(2)	(3)	(4)
1. High School Graduate	-0.021 **	-0.021 **	-0.022 *	-0.035 *
	(0.005)	(0.005)	(0.009)	(0.014)
2. Years of Education (completed)	-0.150 **	-0.148 **	-0.188 **	-0.209 *
	(0.038)	(0.039)	(0.064)	(0.101)
3. Total Income ($/month)	-573	-559	-539	-1088
	(295)	(292)	(498)	(795)
4. Wage Income ($/month)	-812 **	-802 **	-550	-1455 *
	(261)	(258)	(451)	(727)
5. Poor (<1.5 times the poverty level)	0.010 *	0.010 *	0.001	-0.003
	(0.005)	(0.005)	(0.008)	(0.013)
6. Duncan's Socioeconomic Index	-0.640 *	-0.631 *	-0.884 *	-0.592
	(0.259)	(0.260)	(0.436)	(0.694)
Observations	114,031	114,032	80,695	51,462

Notes: Estimates of b_3 from [1] and robust standard errors in parentheses reported for each dependent variable in column 1 and for each specification. Statistically significant at 5% (*) and 1% (**) size of test. All income values in 2005 dollars.

As explained in more detail below, restricting the comparison cohorts to the 1915-1918 birth cohorts assures that potential data quality concerns do not contaminate our conclusions. The 1919 birth cohort deficits are even larger in magnitude relative to this narrower set of comparison cohorts (column 4). The high school graduation, years of completed education and wage deficits are statistically significant and 40% to 80% larger than the deficits relative to the 1912-1922 cohorts (column 2).

A necessary, but not sufficient, condition for these estimates to be interpreted as causal is that the 1919 and comparison cohorts are statistically exchangeable. This condition fails if, for example, parental SES of the 1919 birth cohort is different from that of the comparison cohorts.[4]

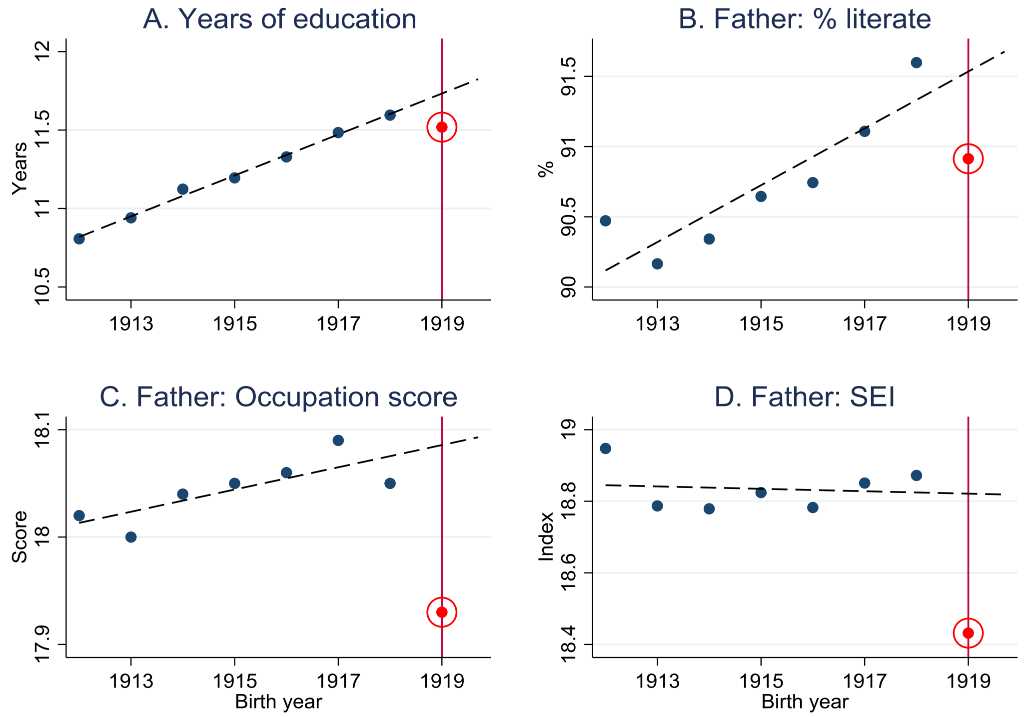
[4] Almond reports one formal test of this assumption by using the 1960 and 1970 Censuses to compare the probability that an individual from the 1919 birth cohort has a foreign-born parent relative to the surrounding

Paternal SES of males in the 1919 birth cohort

As shown in Panel A of Figure 1 (and Almond's Figure 1), the 1919 birth cohort completed less education than predicted by trend.[5] As a graphical description of whether parental SES is different for the 1919 birth cohort, the other three panels of Figure 1 display paternal characteristics from the full count 1920 Census by year of birth of males born between 1912 and 1919, paralleling the figure for own education in panel A.

Panel B displays the percentage of fathers who report themselves as literate: fathers of the 1919 cohort are substantially less likely to be literate relative to fathers of earlier birth cohorts. This pattern is not restricted to literacy. Average Occupation Income Score (panel C) and Duncan's SEI (panel D) establish fathers of the 1919 birth cohort also have lower SES than the earlier cohorts.[6] These figures suggest that the exchangeability assumption should be tested empirically (Thomas, 2010).

Figure 1. Own education and paternal characteristics by own birth year for males in 1960 Census (panel A) and their fathers in 1920 Census (panels B-D)



cohorts and finds no significant differences.

[5]Almond uses the 1912 to 1922 birth cohorts, for reasons described below, the figure is restricted to the 1912 to 1919 birth cohorts. As shown in panel A of Appendix Figure 1 which includes the 1912-1922 cohorts, this restriction does not affect the conclusion.

[6]Parallel figures for the 1912 to 1922 birth cohorts, displayed in panels B-D in Appendix Figure 1, with paternal characteristics measured in the 1930 Census yields the same general conclusion: the fathers of the 1919 birth cohort have lower SES than the fathers of surrounding birth cohorts. Paternal characteristics in both figures are for the same fathers and for each dimension of SES, the level is substantially higher in 1930 than in 1920, indicating SES levels improved over the decade. This is unsurprising and indicates the paternal characteristics in 1920 better reflect resource availability in utero in the 1919 birth cohort than characteristics measured in 1930.

To this end, we compare a broad array of paternal characteristics of each birth cohort using the full count 1920 Census. The advantages of the 1920 Census are that it is proximate to the birth of the 1919 cohort, age is reported at the beginning of the new year and thus can be used to accurately infer birth year and, for those born in the previous 5 years, age in months is also collected. The drawback is that it precludes drawing comparisons with the post-1919 birth cohorts used in Almond (2006). As noted above in Table 1, that restriction does not affect the conclusion that the 1919 birth cohort had significantly worse adult outcomes than the comparison cohorts.

To test the exchangeability assumption, we estimate [1] replacing the dependent variables, adult SES of the child born in each cohort year, with the paternal characteristics of the child born in each cohort year. If the exchangeability assumption is correct, $\hat{\beta}_3$ should not be statistically significant. Results for males are reported in Table 2. Comparisons are drawn with fathers of the 1912-1918 birth cohorts in panel A and the 1915-1918 cohorts in panel B. Means for all birth cohorts for each paternal characteristic are reported in the first column of each panel and the coefficient from [1] on the indicator for the 1919 birth cohort of the child and associated standard error are in column 2 of each panel.

Table 2: Differences in paternal characteristics of 1919 birth cohort of males relative to surrounding cohorts using 1920 Census data

	A. Relative to 1912-1918 cohorts		B. Relative to 1915-1918 cohorts	
Paternal Characteristic	Mean	Born in 1919	Mean	Born in 1919
	(1)	(2)	(3)	(4)
1. Father is Illiterate (%)	9.21%	1.21% **	8.95%	1.20% **
		(0.05)		(0.08)
2. Father's Occupation Income Score	21.68	-0.23 **	21.72	-0.04
		(0.02)		(0.03)
3. Father's Duncan's Socioeconomic Index	22.60	-0.75 **	22.61	-0.60 **
		(0.04)		(0.06)
4. Father is Non-White (%)	9.56%	1.25% **	9.25%	1.28% **
		(0.05)		(0.08)
5. Father's Age at Birth	32.89	0.22 **	33.00	0.30 **
		(0.01)		(0.02)
6. Number of Father's Children in HH	3.67	0.32 **	3.45	0.36 **
		(0.00)		(0.01)
Observations	9,335,388		5,767,400	

Notes: Estimates of b_3 from [1] for each paternal characteristic.

Robust standard errors in parentheses. Statistically significant at 5% (*) and 1% (**) size of test.

As shown in the first and third row of the table, around 9% of fathers of the relevant cohorts reported themselves as being illiterate. When assessing the existence of differences in parental literacy for children exposed in utero to the pandemic, we find that fathers of the 1919 birth cohort are 1.2%age points more likely to be illiterate than predicted by the trend and this deficit in paternal literacy is significant for both sets of comparison cohorts.

Since the 1920 Census does not report education or income, we examine indices of SES based on the occupation of the father. The Occupation Income Score assigns to each occupation the median total income (in hundreds of 1950 dollars) of all people in the 1950 census with that occupation. The Duncan SEI attributes an education and income level to each occupation as of 1950 to create the index. For both SES indicators, relative to the fathers of the 1912-1918 cohorts, the fathers of the 1919 birth cohort are significantly worse off and the Duncan SEI is significantly lower for the 1915-1918 cohort comparison as well. In addition, fathers of the 1919 cohort are significantly more likely to be non-white in both cohort comparisons. All of this evidence points in one direction: the 1919 birth cohort had fathers with lower SES than the comparison cohorts.

Moreover, as shown in the next two rows of the table, fathers of the 1919 cohort tend to be older when the child was born and have more children (as proxied by the number of children in the household). This suggests that the fathers started fertility earlier, continued longer, and produced larger families. Larger family sizes are associated with lower levels of human capital investments in each child and this may also contribute to the lower levels of adult SES of the 1919 birth cohort (Becker and Lewis, 1973). In addition, older fathers are likely to have less education given the secular increase in education across birth cohorts. These paternal characteristics are associated with adult economic wellbeing and reinforce that the fathers of the 1919 birth cohort had lower SES than the comparison cohorts. The results are important given the positive link between parental SES and child outcomes (Corcoran et al. 1992).

Appendix Table 1 provides estimates using the 1930 Census and the 1912-1922 birth cohorts. Relative to the surrounding cohorts, the fathers of the 1919 birth cohort have significantly lower SES, paralleling the results in Table 2. These results provide evidence that the pattern of negative paternal selection found in Table 2 does not depend on the choice of comparison cohorts.[7]

The consistent conclusion from Table 2 (and Appendix Table 1) is that, based on observed paternal characteristics, the exchangeability assumption is rejected by the data. This is important because it is necessary to draw causal inferences from comparisons of the 1919

[7]Whereas the 1930 Census has the advantage of adding the 1920-1922 cohorts to the comparison group, it has several limitations. Specifically, it is not possible to identify year of birth exactly, there is age heaping in the 1920 birth cohort, there is a long hiatus between the births of interest and measurement of paternal characteristics and there is potential selective migration out of the parental home by older cohorts. Therefore, evidence from the 1930 Census is not included in the main analyses reported here. However, none of our conclusions depends on this choice.

birth cohort of males with comparison cohorts. Thus, the estimates reported by Almond cannot be interpreted as evidence of the causal effect of in utero exposure to flu on adult SES. If there is selection on observed paternal characteristics, it is likely that there is also selection on unobserved characteristics which makes assessing the importance of the failure of the exchangeability assumption far from straightforward.

Adult SES of males in the 1919 birth cohort conditional on family background

In an effort to assess the sensitivity of the results in Table 1 to the inclusion of paternal background characteristics, we adopt an approach that allows us to analyze the same public use 1960, 1970 and 1980 Census data used in Almond (2006). Since parental background of adult respondents, other than parental place of birth, is not recorded in those censuses, we construct proxies for parental background from the 1920 full count Census for each birth cohort, b, by state of birth, s, and race, r, and extend models of adult SES, [1], by adding these proxies, P_{bsr}:

$$y_i = \gamma_0 + \gamma_1 \cdot YOB_i + \gamma_2 \cdot YOB_i^2 + \gamma_3 \cdot I_i(YOB = 1919) + \gamma_4 P_{bsr} + \upsilon_i \qquad [2]$$

The parental background proxies are calculated using 1920 Census data on paternal literacy, paternal occupation income score, paternal age at the birth of the index child, whether the father is white, and the number of the father's children living in the household. For each of these characteristics, the paternal proxy is the average over all children born in each state and year of birth cohort calculated separately for whites and non-whites.

Using the 1960 Census, Panel A of Table 3 reports estimates of β_3 from model [1] without paternal controls in column 1 and estimates of γ_3 from model [2] which adjusts for paternal characteristic proxies in column 2 with the 1912-1918 births as the comparison cohorts. Panel B of Table 3 reports the analogous estimates with the 1915-1918 births as the comparison cohorts. Each element in the table represents a separate regression.[8] Appendix Tables 2 and 3 report parallel estimates using the 1970 and 1980 Censuses, respectively.

As shown in the first row of each panel of Table 3, males born in 1919 were between 2.2 and 3.5 percentage points less likely to graduate from high school and this gap is statistically significant. After controlling for paternal characteristic proxies, however, the 1919 birth cohort are more likely to have graduated from high school and this positive difference is statistically significant. Similarly, without background controls, the 1919 birth cohort completed between 0.19 and 0.21 fewer years of schooling but, after taking into account paternal characteristics, they completed between 0.27 and 0.34 more years of

[8] Indicator variables are included in the model when the value of a paternal characteristic proxy is missing.

schooling. Again, all of these differences are statistically significant. When these analyses are performed using either the 1970 or 1980 Census, as seen in Appendix Tables 2 and 3, they produce the same pattern of results.

Table 3: Differences in adult SES for males of the 1919 birth cohort in the 1960 Census relative to comparison cohorts with and without proxies for paternal characteristics calculated using 1920 Census data

	A. Relative to 1912-1918 cohorts		B. Relative to 1915-1918 cohorts	
Socio-economic outcome in adulthood	No paternal controls [1]	w/ proxies for paternal characteristics [2]	No paternal controls [1]	w/ proxies for paternal characteristics [2]
1. High School Graduate	-0.022 *	0.036 **	-0.035 *	0.036 *
	(0.009)	(0.009)	(0.014)	(0.015)
2. Years of Education (completed)	-0.188 **	0.271 **	-0.209 *	0.339 **
	(0.064)	(0.062)	(0.101)	(0.099)
3. Total Income ($/month)	-539	2,796 **	-1,088	2,849 **
	(498)	(504)	(795)	(808)
4. Wage Income ($/month)	-550	2,186 **	-1,455 *	1,949 **
	(451)	(452)	(727)	(731)
5. Poor (<1.5 times the poverty level)	0.001	-0.037 **	-0.003	-0.052 **
	(0.008)	(0.008)	(0.013)	(0.013)
6. Duncan's Socioeconomic Index	-0.884 *	1.143 **	-0.592	1.974 **
	(0.436)	(0.441)	(0.694)	(0.701)
Observations	80,695	80,695	51,462	51,462

Notes: Robust standard errors are in parentheses. Statistically significant at 5% (*) and 1% (**) size of test. All income values in 2005 dollars.

In addition, across the 1960, 1970, and 1980 Censuses and both sets of comparison cohorts, when paternal characteristic proxies are not controlled, the 1919 birth cohort earned less income, were more likely to be poor and scored lower on the Duncan SEI; 6 of the 24 coefficients are significantly negative. When paternal characteristic proxies are included in the model, 18 of the 24 coefficients are statisically significant and all of them indicate that, conditional on background, the 1919 birth cohort achieve better economic outcomes than those in the comparison cohorts.[9] Uncontrolled, the 1919 birth cohort is more likely to have disability that affects work in 1980 but this effect is no longer significant if paternal characteristic proxies are controlled. In 1970, when the 1919 cohort was 51, they received lower Social Security income than the comparison cohorts who are likely to be older when they started taking Social Security. This interpretation is bolstered by the fact that in 1980 the pattern is reversed: the 1919 birth cohort receives about $700 more per month in Social Security, with and without paternal controls. There are no differences in welfare income received by the 1919 birth cohort.[10]

[9] Two of the estimates do not reverse in sign. In both cases, the 1919 birth cohort disadvantage is reduced and not statistically significant after controlling paternal background.

[10] As noted above, there are legitimate concerns with using the 1930 Census to measure paternal characteristics,

Since paternal selection is likely related to fertility choices, in the adjusted models, it is appropriate to include a proxy for fertility which is calculated using the number of children in the household. Excluding that variable does not affect our primary conclusion: there is no evidence that the 1919 birth cohort has significantly worse SES in adulthood relative to the comparison cohorts. Specifically, as shown in Appendix Table 5, 10% of the 1919 birth cohort estimates indicate significantly higher SES, 8% indicate significantly lower SES and 82% indicate no significant difference.

The results in Table 3 and Appendix Tables 2-5 challenge the conclusions drawn by Almond (2006). In the unadjusted models, as documented by Almond, the 1919 birth cohort have less human capital, lower earnings, and more work limitations than the surrounding cohorts. Using proxies to adjust for paternal characteristics erases these deficits for almost every outcome and provides no evidence to support the claim of persistent damage to adult economic outcomes of the 1919 birth cohort from in utero exposure to influenza.

Additional evidence in support of this conclusion is provided by BFS using WWII enlistee data matched with the 1920 and 1930 Censuses which links male enlistees to their fathers. They confirm our first result: the 1919 birth cohort are significantly negatively selected on paternal characteristics. Second, in models that adjust for observed paternal characteristics, the 1919 birth cohort deficits for schooling are substantially reduced but remain significant in this predominantly white sample. Importantly, BFS also account for unobserved differences in background characteristics by comparing brothers. None of the education deficits in those models is different from zero at the 5% level. Taking all of the evidence together, the data do not support the conclusion that in utero exposure to influenza in the U.S. caused adverse SES outcomes in adulthood.

Evaluating competing explanations: Treating the 1918 pandemic as a natural experiment

Exchangeability of the exposed and unexposed cohorts is a necessary condition for identification. It is also important to rule out other potential confounding sources of variation. For example, the 1918 flu was first documented in the U.S. in January 1918 and in Europe in the spring. Thus, it is not clear the pandemic was completely unanticipated and that fertility in 1919 was not selective on parental characteristics as a result of anticipatory behavior. Second, Floris et al, (2021) document fetal death rates of the 1919 cohort were elevated among higher SES females so that live births in 1919 may be negatively selected on parental SES. On the other hand, Grantz et al (2016) document the burden of disease fell most heavily

but as shown in Appendix Table 4, none of the conclusions differs when we use those data to draw comparisons with the 1912-1922 birth cohorts. Without paternal controls, the 1919 birth cohort has significantly worse adult outcomes, but those deficits are not present when the models include paternal control proxies.

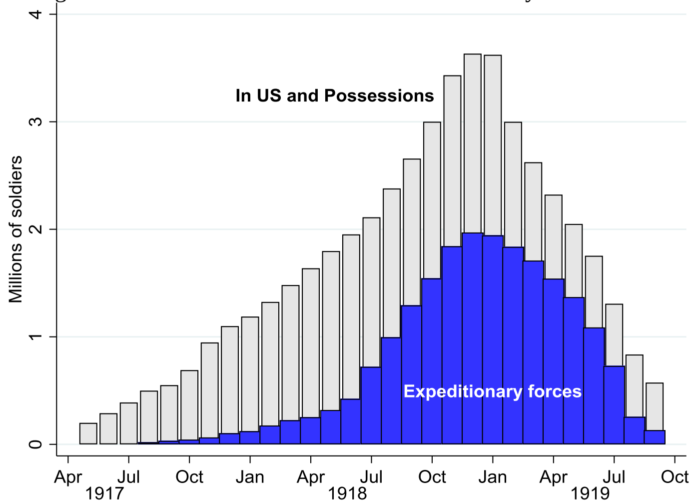
on the poorest in one city, Chicago.

Third, the fact that the timing of the pandemic coincided with the end of World War I (WWI) suggests an alternative, plausible explanation for the evidence on parental selection. The U.S. declared war on Germany in April 1917 and started deploying troops to Europe in the summer of 1918. Thus, during the conception period of the 1919 birth cohort, the U.S. was involved in a major global conflict.

There are multiple pathways through which WWI may have affected fertility decisions of couples and, therefore, the distribution of parental characteristics of the 1919 birth cohort. Over and above troop deployments and the threat of future deployments, there was also greater uncertainty along with elevated levels of stress, reductions in income and food consumption as well as the potential for rationing.[11]

Whereas it is difficult to measure expectations and uncertainty at that time, there is good data on troop deployments. Figure 2, from Ayers (1919), documents that the number of men deployed in the American Army rose very dramatically in the last half of 1918, peaked at the end of the year and declined slowly during the first three months of 1919. This exactly coincides with the timing of the influenza pandemic. According to the 1930 Census, the 1919 birth cohort was significantly less likely to have a father that was a WW1 veteran (Appendix Table 1).

Figure 2. Millions of soldiers in the American Army on 1st of month



Source: Ayers (1919), "The War with Germany: A Statistical Analysis"

Key for explaining the negative selection of parents in the 1919 cohort, those men who were actually deployed and those who were at risk of being deployed were unlikely to be drawn from the lower rungs of the SES ladder for several reasons. First, WWI was the first war in which a U.S. citizen was not allowed to hire a proxy to serve in his place. This ruled out the possibility of the upper class buying their way out of service. Second, due to the draft categories in use in 1917, men with greater levels of resources were more likely to be conscripted. While almost all draft eligible men were put in Class I, one of the main reasons for a deferment was the income dependency of the family of a potential draftee. A man was placed in a lower priority group if his family had little financial support apart from himself,

[11]For example, war-related famine in Allied countries triggered the U.S. to launch a major government food conservation campaign entitled "Food Will Win the War" and urged citizens to restrict their consumption of meat, wheat, fats, and sugars.

because the family would have "insufficient" income to sustain itself if he were drafted (Nudd 2004). Third, draft eligibility was partly based on age with older men being less likely to be conscripted. Since educational attainment was rising substantially between these cohorts of men, an older father is likely to have less education. Fourth, deferments were awarded to men for health reasons and so the less healthy were less likely to be drafted.[12,13]

A different approach to distinguishing the fetal origins hypothesis from selective fertility is to exploit the quarter of birth of the 1919 cohort. In the 1920 Census, this can only be determined for the 1915-1919 birth cohorts, which provides another advantage to restricting attention to these cohorts. As Almond points out, the timing of the Spanish flu in the last four months of 1918 and ending in early 1919 yields tight predictions about the timing of its impacts: the deleterious effects on adult outcomes should be greatest on those born in the first two quarters of 1919 and there should be no effects on those born in the fourth quarter of 1919. In contrast, there are no clear predictions about the impacts of WWI, broadly construed, or on the specific timing of fertility outcomes since behaviors of couples and their expectations are involved.

Panel I of Table 4 displays the deviation from trend in the paternal characteristics of the 1919 birth cohort, quarter by quarter relative to the same birth quarters in the 1915 to 1918 cohorts. While selection is large in magnitude, negative and significant for those born in the first two quarters, the selection is also negative and significant for 5 of the 6 markers among those born in the third and fourth quarters.

Panel II of the table displays estimates of [1] for completed years of education of males by quarter of birth in 1919, controlling for birth quarter fixed effects to capture seasonality of births, and using the 1915 to 1918 cohorts for comparison. In 1960, 1970 and 1980, males born in the fourth quarter have the largest deficits in education and those are the only deficits that are significantly different from zero in all three censuses. This evidence is not consistent with predictions of the fetal origins hypothesis.[14]

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[12]In addition to the change in parental composition caused by WW1, the war may have impacted several other aspects of life that could bias the estimates of the effect of prenatal exposure to the flu found in Almond (2006). The loss of enlisted fathers as breadwinners and changes in food prices (Rotwein, 1945), along with a national food conservation campaign, may have caused a restriction in nutrients consumed by pregnant mothers. Moreover, the mobilization effort may have caused elevated stress as some pregnant women were in a position where they needed to enter the workforce or make non-trivial lifestyle changes.

[13]Some evidence of this is provided by the 1930 Census which records veteran status. We use age at date of census (31 March 1930) to approximate birth cohort and examine the 1912-1922 birth cohorts. For example, overall, 7.6% of the fathers were illiterate and veterans were 1.2 percentage points less likely to be literate (standard error=0.02). The Duncan SEI is 24.7 overall and 6.3 points higher (standard error=0.03) for veterans. Brown and Thomas (2019) show that in adulthood the 1919 birth cohort's adverse outcomes are no longer present after the sole inclusion of the WWI veteran for male in the 1960, 1970 and 1980 Censuses.

[14]Males born in the first quarter of 1920 have completed 0.12 fewer years of education than the comparison

Table 4: Departure of 1919 birth cohort by birth quarter for males relative to the 1915-1918 Cohorts

	A. Year of birth differenc	B. Quarter of birth differences				
	Born in 1919	1919Q1	1919Q2	1919Q3	1919Q4	Missing Birth Month in 1919
	(1)	(2)	(3)	(4)	(5)	(6)
I. Paternal characteristics in 1920 Census						
1. Father is Illiterate	1.20% **	2.10% **	1.28% **	1.89% **	0.53% **	-1.07% *
	(0.08)	(0.10)	(0.10)	(0.10)	(0.10)	(0.43)
2. Father's Occupation Income Score	-0.04	-0.25 **	-0.47 **	0.05	0.26 **	0.57 **
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.16)
3. Father's Duncan's Socioeconomic Index	-0.60 **	-0.98 **	-1.07 **	-0.62 **	-0.14 *	1.00 **
	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)	(0.31)
4. Father is Non-White (%)	1.28% **	2.14% **	2.04% **	1.85% **	0.44%	1.69% **
	(0.08)	(0.10)	(0.10)	(0.10)	(0.10)	(0.45)
5. Father's Age at Birth	0.30 **	0.32 **	0.60 **	0.38 **	0.08 **	-0.37 *
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.15)
6. Number of Father's Children in HH	0.36 **	0.30 **	0.42 **	0.42 **	0.36 **	0.18 **
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)
II. Completed years of education						
1. Measured in 1960 Census	-0.209 *	-0.159	-0.223	-0.138	-0.310 *	
	(0.101)	(0.122)	(0.122)	(0.123)	(0.121)	
2. Measured in 1970 Census	-0.182 **	-0.095	-0.152 *	-0.174 *	-0.320 **	
	(0.062)	(0.074)	(0.074)	(0.074)	(0.073)	
3. Measured in 1980 Census	-0.112 *	-0.108	-0.083	-0.055	-0.203 **	
	(0.053)	(0.063)	(0.064)	(0.063)	(0.062)	

Notes: ** indicates statistical significance at the 1% level, * indicates statistical significance at the 5% level. Robust standard errors in parentheses. There are 5,767,400 males in panel I and 51,462, 139,757 and 213, 481 in panels II rows 1 through 3, respectively. Birth quarter regressions use model [1] replacing the 1919 birth cohort dummy variable with four 1919 birth quarter cohort indicator variables and birth quarter fixed effects. Models in panel I include an indicator variable for missing birth month in 1919 and a missing birth month indicator for all cohorts.

Evaluating competing explanations: Age heaping

Age heaping is a legitimate concern in survey data (Myers, 1954; Coale 1955; A'Hearn et al 2009). In this context, if less educated parents are more likely to heap on preferred digits, it is possible that heaping could explain our results. This concern underlies our second motivation for reporting results for the 1915-1919 birth cohorts in the 1920 Census. First, for these cohorts, age is less likely to be heaped on years since it is reported in both years and months while, for older cohorts, age at last birthday is reported in years. Second, birth registration data were first collected in the U.S. in 1915 and so for the 1915-1919 cohorts, it is possible to compare the number of births reported in the Census with vital statistics. We compare the number of births reported in the 1920 Census in each year with the reported number of births in the natality data minus age and state-specific mortality through 1919, taking into account changes in the states that are covered in the vital

cohorts in 1960 and 1970 which is larger than the deficit of the 1919 first quarter births. The deficit, which is statistically significant in the 1970 Census, cannot be explained by in utero influenza exposure. Almond (2006) reports quarter of birth results for only the 1980 Census explaining that its larger sample supports those analyses. His Figure 5b displays high school graduation deficits which are as large for the 1919Q4 cohort as for the 1919 Q1 and Q2 cohorts which is also not consistent with the fetal origins hypothesis. He does not report standard errors.

statistics.[15,16] As shown in Appendix Table 6, there is a very high degree of concordance in the number of children alive at the date of the 1920 Census according to the Census and vital statistics data for each of the 1915-1919 birth cohorts. The ratio of the two numbers is very close to unity in every birth year (column 4). We conclude that it is implausible to attribute the systematic negative and significant selection of fathers of the 1919 birth cohort to age heaping in the 1920 Census.[17] This conclusion is also supported by the presence of the same pattern and severity of negative parental selection amongst the 1919 birth cohort in the matched WWII enlistee data, which is protected from the concern of age heaping as it infers age from two distinct sources (BFS 2021).[18]

Evidence on 1919 birth cohort differences among females and non-whites

Almond (2006) describes results for females and nonwhites. The analyses described in Table 3 for males are repeated for these two demographic groups and summarized in Table 5. Paternal characteristics of the 1919 birth cohort, relative to the 1912-1918 cohorts are displayed in the first column of panel A of the table for females in the upper half and for nonwhites in the lower half. Results for the 1915-18 comparison cohorts are reported in the first column of panel B. Three of the paternal characteristics, measured in the 1920 Census, are reported: literacy, occupation income score and Duncan's SEI.

[15]The 1915 registration area covered 10 states (Connecticut, Maine, Massachusetts, Michigan, Minnesota, New Hampshire, New York, Pennsylvania, Rhode Island, Vermont) and the District of Colombia. Maryland was added in 1916. Indiana, Kansas, Kentucky, and North Carolina were added in 1917. Ohio, Utah, Virginia, Washington, and Wisconsin were added in 1918. California, Oregon, and South Carolina were added, and Rhode Island was dropped in 1919. Natality data are drawn from "Birth, Stillbirth, and Infant Mortality Statistics for the Birth Registration Area of the United States" (Roper and Austin, 1931). Mortality data are drawn from the annual "Mortality Statistics" reports published by the Census Bureau. The mortality data is subject to potential measurement error from migration and misreporting in age of death, but this error is unlikely to be systematic in a way that would affect our age heaping analysis.

[16] For example, the vital statistics natality data records that 776,304 children were born in 1915 within the original registration area. To calculate the expected number of children from these births still alive by the 1920 Census, age-specific mortality counts from the original registration states are subtracted from the count of births. Specifically, under age 1 mortality counts in 1915, age 1 mortality counts in 1916, age 2 mortality counts in 1917, age 3 mortality counts in 1918, and age 4 mortality counts in 1919 are totaled and subtracted from the 776,304 recorded 1915 births.

[17] It is possible that age-heaping is less severe in registration states. We do not find that to be the case. Comparing the 1915 registration states with later states there is no difference in age-heaping. Moreover, the paternal selection results are confirmed when analysis is restricted to 1915 registration states.

[18]An additional potential concern with using the 1920 Census for this analysis is that the focal cohort, the 1920 birth cohort, is less than 1 year old at the time of enumeration, and thus has not experienced the effects of infant mortality to the same extent as the surrounding cohorts. It is reasonable to assume that children from low SES families are more likely to die within the first year of life and this selective mortality could possibly lead to positive selection on paternal characteristics in the older cohorts and bias our conclusions. One of the advantages of the 1920 Census is that this concern can be explored directly in the data using the information provided on birth month. Specifically, if the paternal selection found in Table 2 is driven by selective mortality in the first year, then this relationship should be largest for the 1919Q4 births who have had the least amount of time to experience selective mortality and smallest for the 1919Q1 births who have had the most amount of time to experience selective mortality. The empirical facts presented in Panel I of Table 4, is not consistent with this hypothesis. This is not a surprising result as infant mortality is characterized by substantial duration dependence and the vast majority of infant mortality happens in the first month.

Table 5: Differences of 1919 birth cohort relative to surrounding cohorts: Females and nonwhite males, paternal characteristics measured in 1920 Census and adult outcomes measured in 1960 Census

	A. Relative to 1912-1918 cohorts			B. Relative to 1915-1918 cohorts		
	Paternal characteristics	Outcomes in adulthood (1960 Census)		Paternal characteristics	Outcomes in adulthood (1960 Census)	
	1919 cohort dev from trend [1]	No paternal controls [2]	w/ proxies for paternal characteristics [3]	1919 cohort dev from trend [1]	No paternal controls [2]	w/ proxies for paternal characteristics [3]
I. Females						
A. Paternal characteristics						
A.1. Father is Illiterate (%)	1.32% **			1.61% **		
	(0.05)			(0.08)		
A.2. Father's Occupation Income Score	-0.27 **			-0.22 **		
	(0.02)			(0.03)		
A.3. Father's Duncan's SES Index	-0.76 **			-0.81 **		
	(0.04)			(0.06)		
B. Outcomes in adulthood						
B.1. High School Graduate		-0.008	0.030 **		-0.027	0.019
		(0.009)	(0.009)		(0.014)	(0.014)
B.2. Years of Education (completed)		-0.077	0.198 **		-0.212 *	0.113
		(0.054)	(0.053)		(0.085)	(0.085)
B.3. Total Income (2005$/month)		211	837 **		224	893 *
		(221)	(230)		(348)	(361)
Observations	9,117,591	83,730	83,730	5,272,543	53,402	53,402
II. Nonwhites						
A. Paternal characteristics						
A.1. Father is Illiterate (%)	0.95% **			0.99% **		
	(0.19)			(0.30)		
A.2. Father's Occupation Income Score	-0.05			-0.08 *		
	(0.03)			(0.04)		
A.3. Father's Duncan's SES Index	-0.17 **			-0.15 *		
	(0.04)			(0.06)		
B. Outcomes in adulthood						
B.1. High School Graduate		-0.015	0.008		-0.030	0.008
		(0.017)	(0.016)		(0.027)	(0.027)
B.2. Years of Education (completed)		-0.039	0.147		-0.269	0.077
		(0.155)	(0.150)		(0.252)	(0.243)
B.3. Total Income (2005$/month)		698	934		661	1,521
		(566)	(575)		(926)	(928)
Observations	2,016,559	15,995	15,995	1,004,351	10,258	10,258

Notes: Robust standard errors in parentheses. Statistically significant at 5% (*) and 1% (**) size of test. Paternal characteristics measured in 1920 Census.

The fathers of the 1919 birth cohort are negatively selected on each of these characteristics. The gaps for fathers of females are very similar to those for males; the gaps for non-white fathers are smaller in magnitude. For example, relative to the 1912-1918 cohorts, fathers of females born in 1919 are 1.32% points less likely to be literate and fathers of nonwhites are 0.95% points less likely to be literate. The 1919 birth cohort is more disadvantaged relative to the 1915-1918 cohorts for both females and non-whites. For females, the deviations from the trend for the 1919 cohort are negative and significant relative to both comparisons cohorts for all three paternal characteristics and the gaps are very similar in magnitude to those for males. For nonwhites, the deviations indicate negative selectivity in all cases, the gaps are significant for 5 of the 6 comparisons and generally smaller in magnitude than the gaps for whites.

Outcomes in adulthood of the 1919 birth cohort relative to the comparison cohorts are displayed in column 2 of each panel. The outcomes, high school graduation, years of completed education and total monthly income are measured in the 1960 Census. The results in panel A, column 2, provide estimates that do not control for parental characteristics. For females and nonwhites, there is no evidence that the 1919 birth cohort has worse outcomes than the 1912-1918 cohorts. The results in column 3 of the panel include measures of paternal background from the 1920 Census. All of the gaps turn positive and, for women, the estimates are statistically significant.

These results indicate that after controlling for paternal characteristic proxies, the limited evidence suggesting a negative relationship between in utero exposure to the 1918 influenza pandemic and adult outcomes for women and non-whites no longer exists. Comparisons in panel B are drawn with the 1915-1918 cohorts and corroborate this conclusion: without paternal background controls, for women and non-whites only one gap is significantly negative and with background controls all of the estimates are positive with one gap significantly different from zero.

3. Assessment of the evidence exploiting variation in influenza exposure

Almond (2006) also investigated whether adult SES is explained by variation in virulence of the influenza measured with the year- and state-specific maternal mortality rate, *MMR*. Restricting attention to the 1918 through 1920 birth cohorts in order to isolate the effect of fetal exposure, he investigated how each adult outcome, y_i, varies with the MMR measured in the year before the birth, *t-1*, in a model that included state fixed effects, μ_s and birth year fixed effects μ_t:

$$y_i = \alpha_0 + \alpha_1 MMR_{s_{t-1}} + \mu_s + \mu_t + \xi_{is_t} \qquad [3]$$

where y_i is adult SES, μ_s and μ_t are state and birth year fixed effects, respectively. MMR is indicative of excess mortality presumably due to influenza. Estimates of α_1 from Almond (2006) using SES of adult males in the 1960 Census are displayed in Appendix Table 7 (column 1). Our replication (Appendix Table 7, column 2), yields estimates that are very close. When conducting the replication, we discovered two errors in the MMR data used by Almond[19], which, when corrected in column 1 of Table 6, leaves only two of the five adult SES markers as significantly related to MMR. Results for males in the 1970 and 1980 Census are reported in columns 2 and 3 of Table 6. Whereas in 1960, males who were born in states

[19] Almond assigns an MMR of 6.3 for Virginia in 1919; the rate recorded in US PHS (1947) is 8.3. Almond excluded Washington D.C. which is recorded in the same source.

with higher levels of excess maternal mortality are significantly less likely to have graduated from high school and completed significantly fewer years of education, by 1970 these same males are no less likely to have graduated from high school and report having completed significantly more years of education. The reason for this reversal is unclear. It is unlikely that these men completed more years of education in their forties. Moreover, in 1970, these males are also significantly less likely to be poor. By 1980, none of the estimated difference-in-differences is statistically significant.

Table 6: Estimate of maternal infection rates on adult SES outcomes for males, females and nonwhites born between 1918 and 1920 in the 1960, 1970 and 1980 U.S. Censuses

	A. Males			B. Females			C. Non-whites		
Census:	1960	1970	1980	1960	1970	1980	1960	1970	1980
Socio-economic outcome in adulthood	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1. High School Graduate	-0.083 *	0.014	0.003	0.024	0.030	-0.024	-0.321	-0.044	0.110
	(0.039)	(0.021)	(0.020)	(0.043)	(0.024)	(0.015)	(0.188)	(0.101)	(0.077)
2. Years of Education (completed)	-0.659 *	0.386 **	0.046	0.050	0.185	-0.118	-0.789	0.634	0.423
	(0.272)	(0.157)	(0.098)	(0.237)	(0.109)	(0.092)	(1.130)	(0.363)	(0.454)
3. Log of Total Income	-0.158	0.061	-0.070	-0.109	0.055	0.072	0.377	0.011	0.387
	(0.086)	(0.050)	(0.039)	(0.115)	(0.040)	(0.050)	(0.312)	(0.220)	(0.223)
4. Poor (< 1.5 times the poverty level)	0.030	-0.064 **	0.018	0.003	-0.004	-0.018	-0.235	-0.142	0.011
	(0.031)	(0.017)	(0.012)	(0.033)	(0.011)	(0.168)	(0.195)	(0.076)	(0.089)
5. Duncan's Socioeconomic Index	-2.580	2.258	0.139	2.831	-0.204	-0.370	-2.839	8.230 *	-5.026
	(1.545)	(1.511)	(0.958)	(1.682)	(1.002)	(0.999)	(7.629)	(3.467)	(4.002)
Observations	16,659	46,241	68,872	17,164	49,387	77,806	1,866	5,311	7,616

Notes: Standard errors clustered at the state and year of birth level are in parentheses. Statistically significant at 5% (*) and 1% (**) size of test.

Estimates for females and non-whites are displayed in panels B and C of Table 6, respectively. The only statistically significant estimate indicates that nonwhites who were born in states with higher levels of excess maternal mortality have higher SES in 1970.

In sum, of 45 estimated coefficients only two indicate a statistically significant negative link between adult SES and excess maternal mortality and those estimates are not consistent over time. In contrast, three of the estimates indicate that the link is significantly positive. Evidence in support of the conclusion that there is a significant negative dose response effect is very weak. Indeed, adopting a testing procedure that takes into account the multiple comparisons in these analyses (Hommel, 1988), results in the conclusion that variation in the intensity of exposure to the 1918 influenza pandemic *in utero* has no statistically significant impacts on SES in adulthood.

Using arguably better measures of exposure, and controlling parental background, BFS report no statistically significant dose-responses (at 5%) for the 1918-1919 cohorts of male enlistees. When they add the 1912-1917 cohorts, dose-response estimates for two of three education outcomes are significantly negative, but the effect on height is significantly positive. Interpretation of the 1912-1919 estimates is complicated by the potential for other, unobserved differences across the cohorts that are correlated with flu mortality.[20]

[20]Recognizing this, BFS report results for one outcome including birth state by cohort fixed effects. In addition,

4. Evidence from other studies

Studies have followed Almond's approaches to investigate the influenza pandemic's impact on SES in adulthood in other countries. That evidence is also mixed.

For example, Neelsen and Stratmann (2012) report that high school completion and occupation status of the 1919 birth cohort in Switzerland are not statistically different from comparison cohorts but the 1919 cohort is 0.3 percentage points more likely to have never married and 0.5 percentage points less likely to have a vocational degree. The effect sizes for the significant estimates are extremely small and the welfare implications are not clear.

Bengtsson and Helgertz (2015) use both a cohort comparison approach and variation in mortality at the county and birth month level to isolate the impact of exposure to influenza in Sweden. Their conclusions are the reverse of those from the U.S.: the exposed cohorts have higher SES in adulthood.

On the other hand, Lin and Liu (2014) investigate the relationship between human capital outcomes and in utero exposure to influenza during the 1918 and 1920 outbreaks in Taiwan using the 1980 census. Relative to 1916-26 cohorts, educational attainment of the 1919 cohort is significantly lower as is education attainment of females in the 1921 cohort. It is unclear that the 1920 pandemic was unexpected and with life expectancy at birth of only 37 years, these estimates are potentially also contaminated by selective mortality.

Selective mortality is also a concern in a study using Brazilian annual labor force surveys from 1986 through 1998. Nelson (2010) reports that, relative to the 1912-1922 trend, the 1919 birth cohort completed fewer years of education and were less likely to complete college. The ages of these cohorts ranged between 64 and 85, yet, for them, life expectancy at birth was less than 50 years: according to census data, of those who survived to 1970, more than one third of the cohorts had died by 1991. Moreover, there is no evidence in the 1980 Census, 1982 PNAD or 1990 Census that the 1919 birth cohort attained less education than the surrounding cohorts.

Vollmer and Wójcik (2017) conduct a comprehensive evaluation of the literature including a systematic analysis of the cohort comparison model for education, employment and disability outcomes using 117 Censuses from 53 countries. Not only do they find that the vast majority of estimates are not significantly different from zero but those that are different from zero are equally likely to be positive as negative. They conclude that publication bias is a legitimate concern in this literature.

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the BFS sample of linked enlistees is whiter and more affluent than the general population (from the 1920 full count Census). Moreover, they have insufficient data to explore these relationships for females and non-whites.

5. Conclusion

Almond (2006) establishes that, relative to surrounding birth cohorts, the 1919 birth cohort in the U.S. attained lower levels of adult SES. Since this birth cohort was in utero during the 1918 influenza pandemic, this result has been interpreted as evidence of the long-term economic effects of in utero exposure to health insults. A key assumption underlying this inference is that the 1919 birth cohort is exchangeable with surrounding birth cohorts. This paper has carefully tested that assumption and found it is rejected.

Using data from the 1920 Census, we have shown that the fathers of the 1919 birth cohort have lower levels of SES than the fathers of surrounding cohorts. Specifically, fathers of the 1919 birth cohort are less likely to be literate, have lower occupation income scores and Duncan SEI, and are less likely to be white. Using vital statistics, we establish these results cannot be explained by age heaping or selective infant mortality.

When assessing the sensitivity of Almond's results to this selection there is no evidence of an adult SES disadvantage among the 1919 birth cohort in models that adjust for paternal characteristics using proxies constructed from the 1920 Census. Comparisons of brothers that take into account both observed and unobserved parental characteristics using matched enlistee data yields the same conclusion (BFS, 2021).

Almond also reports dose-response estimates comparing the 1918/1919 birth cohorts to identify the effect of influenza exposure on adult SES. Our replication of that approach does not support the conclusion of a statistically negative impact of in utero exposure. This is consistent with results in BFS (2021) for the same birth cohorts using matched enlistee data.

From these findings we conclude that drawing inferences about the deleterious impact of *in utero* exposure to the 1918 influenza on SES in adulthood in the U.S. is, at best, premature. The evidence we present is consistent with a long line of inquiry that has shown parental background is a key predictor of success. It is also consistent with evidence that post-natal interventions can mitigate early life disadvantage. It is important to underscore that our results speak only to impacts of fetal health on markers of socioeconomic success in adulthood; they do not speak to whether in utero health insults affect biological health risks.

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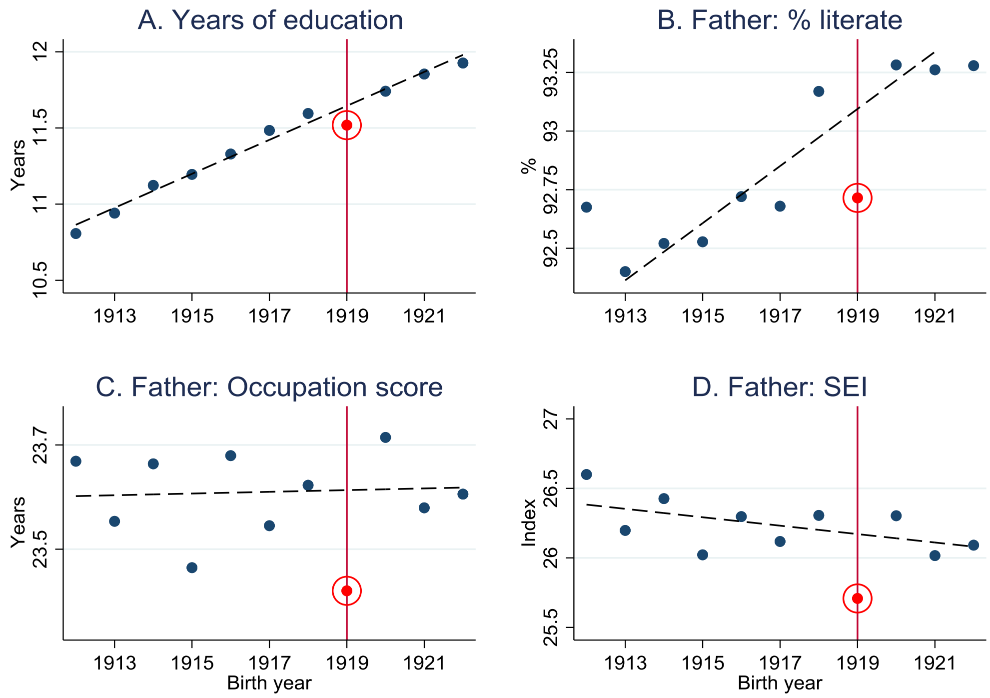
APPENDIX

Appendix Figure 1. Own education and paternal characteristics by own birth year
Males in 1960 Census (panel A) and their fathers in 1930 Census (panels B-D)



Appendix Table 1
Differences in paternal characteristics of 1919 birth cohort of males relative to surrounding cohorts using 1930 Census data

Paternal Characteristic	Relative to 1912-1922 cohorts: Mean	Born in 1919	
1. Father is Illiterate (%)	7.60%	0.29%	**
		(0.03)	
2. Father's Occupation Income Score	22.74	-0.17	**
		(0.01)	
3. Father's Duncan's Socioeconomic Index	24.73	-0.35	**
		(0.02)	
4. Father is Non-White (%)	10.20%	0.81%	**
		(0.03)	
5. Father's Age at Birth	32.10	0.26	**
		(0.01)	
6. Number of Father's Children in HH	4.12	0.07	**
		(0.01)	
7. Father is a WWI Veteran (%)	6.65%	-1.20%	**
		(0.03)	
Observations	12,175,857		

Notes: Estimates of β_3 from [1] for each paternal characteristic.

Robust standard errors in parentheses. Statistically significant at 5% (*) and 1% (**) size of test.

Trend estimates are from from April 1, 1911 to March 31, 1923.

Birth cohorts are from from April 1 through March 31 of the following year.

Appendix Table 2

Differences in adult SES of the 1919 birth cohort in the 1970 Census relative to comparison cohorts with and without proxies for paternal characteristics calculated using 1920 Census data

Socio-economic outcome in adulthood	A. Relative to 1912-1918 cohorts: No paternal controls [1]	A. Relative to 1912-1918 cohorts: w/ proxies for paternal characteristics [2]	B. Relative to 1915-1918 cohorts: No paternal controls [1]	B. Relative to 1915-1918 cohorts: w/ proxies for paternal characteristics [2]
1. High School Graduate	-0.018 **	0.042 **	-0.022 *	0.046 **
	(0.005)	(0.006)	(0.009)	(0.009)
2. Years of Education (completed)	-0.169 **	0.247 **	-0.182 **	0.264 **
	(0.039)	(0.038)	(0.062)	(0.060)
3. Total Income ($/month)	-644	3,080 **	-1,044	3,308 **
	(427)	(433)	(685)	(692)
4. Wage Income ($/month)	-927 *	2,337 **	-1,395 *	2,460 **
	(393)	(397)	(630)	(635)
5. Poor (<1.5 times the poverty level)	0.004	-0.006	0.003	-0.005
	(0.004)	(0.004)	(0.006)	(0.006)
6. Duncan's Socioeconomic Index	-0.471	1.266 **	-0.406	1.602 **
	(0.267)	(0.271)	(0.427)	(0.432)
7. Disability Limits Work	0.005	0.002	0.005	0.001
	(0.004)	(0.004)	(0.006)	(0.006)
8. Disability Prevents Work	0.001	-0.005	-0.002	-0.006
	(0.003)	(0.003)	(0.004)	(0.004)
9. Social Security Income ($/month)	-22.503	-49.265 **	-19.552	-47.277
	(16.532)	(17.320)	(26.389)	(27.622)
10. Welfare Income ($/month)	18.154	17.388	18.132	25.961
	(9.338)	(9.883)	(14.608)	(15.574)
Observations	216,633	216,633	139,757	139,757

Notes: Robust standard errors are in parentheses. Statistically significant at 5% (*) and 1% (**) size of test. All income values in 2005 dollars.

Appendix Table 3
Differences in adult SES of the 1919 birth cohort in the 1980 Census relative to comparison cohorts with and without proxies for paternal characteristics calculated using 1920 Census data

Socio-economic outcome in adulthood	A. Relative to 1912-1918 cohorts: No paternal controls [1]		A. Relative to 1912-1918 cohorts: w/ proxies for paternal characteristics [2]		B. Relative to 1915-1918 cohorts: No paternal controls [1]		B. Relative to 1915-1918 cohorts: w/ proxies for paternal characteristics [2]	
1. High School Graduate	-0.012	**	0.046	**	-0.016	*	0.048	**
	(0.004)		(0.004)		(0.007)		(0.007)	
2. Years of Education (completed)	-0.161	**	0.264	**	-0.112	*	0.320	**
	(0.033)		(0.033)		(0.053)		(0.052)	
3. Total Income ($/month)	-775		1,788	**	1,370		3,856	**
	(486)		(489)		(1,334)		(1,315)	
4. Wage Income ($/month)	-1,085	*	-111		1,109		1,944	
	(440)		(447)		(1,219)		(1,216)	
5. Poor (<1.5 times the poverty level)	0.016	**	0.009		-0.034	*	-0.042	**
	(0.005)		(0.005)		(0.015)		(0.014)	
6. Duncan's Socioeconomic Index	-0.218		1.369	**	-0.145		1.693	**
	(0.234)		(0.240)		(0.373)		(0.380)	
7. Disability Limits Work	0.006		-0.009	*	0.017	**	0.000	
	(0.004)		(0.004)		(0.006)		(0.007)	
8. Disability Prevents Work	0.008	*	-0.005		0.020	**	0.006	
	(0.003)		(0.004)		(0.005)		(0.006)	
9. Social Security Income ($/month)	770.069	**	699.993	**	789.782	**	713.056	**
	(59.434)		(61.671)		(173.367)		(173.856)	
10. Welfare Income ($/month)	10.846		15.197		73.492		65.770	
	(19.704)		(20.357)		(54.164)		(54.239)	
Observations	323,089		323,089		213,481		213,481	

Notes: Robust standard errors are in parentheses. Statistically significant at 5% (*) and 1% (**) size of test. All income values in 2005 dollars.

Appendix Table 4
Differences in adult SES of 1919 birth cohort relative to 1912-1922 cohorts with and without proxies for paternal characteristics calculated using 1930 Census data

Socio-economic outcome in adulthood	A. 1960 Census		B. 1970 Census		C. 1980 Census	
	No paternal controls	w/ proxies for paternal characteristics	No paternal controls	w/ proxies for paternal characteristics	No paternal controls	w/ proxies for paternal characteristics
	[1]	[2]	[1]	[2]	[1]	[2]
1. High School Graduate	-0.021 **	0.002	-0.020 **	0.008 *	-0.014 **	0.018 **
	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)
2. Years of Education (completed)	-0.148 **	0.092 *	-0.178 **	0.053 *	-0.117 **	0.149 **
	(0.039)	(0.037)	(0.023)	(0.023)	(0.020)	(0.019)
3. Total Income ($/month)	-559	1,140 **	-1,218 **	989 **	-1,051 **	721 **
	(292)	(290)	(250)	(250)	(189)	(190)
4. Wage Income ($/month)	-802 **	572 *	-864 **	987 **	-679 **	720 **
	(258)	(256)	(230)	(229)	(177)	(178)
5. Poor (<1.5 times the poverty level)	0.010 *	-0.020 **	0.009 **	-0.008 **	0.006 **	-0.007 **
	(0.005)	(0.005)	(0.002)	(0.002)	(0.002)	(0.002)
6. Duncan's Socioeconomic Index	-0.631 *	0.627 *	-0.806 **	0.432 **	-0.813 **	0.470 **
	(0.260)	(0.259)	(0.157)	(0.158)	(0.137)	(0.138)
7. Disability Limits Work			0.005 **	0.001	0.005 *	-0.008 **
			(0.002)	(0.002)	(0.002)	(0.002)
8. Disability Prevents Work			0.004 **	0.000	0.001	-0.009 **
			(0.001)	(0.001)	(0.002)	(0.002)
9. Welfare Income ($/month)			12.281 *	11.902 *	16.936 *	8.733
			(5.844)	(6.053)	(7.039)	(7.267)
10. Social Security Income ($/month)			5.364	-9.393	81.687 **	75.180 **
			(9.228)	(9.383)	(18.658)	(19.350)
Observations	114,032	114,032	308,785	308,785	471,803	471,803

Notes: Robust standard errors are in parentheses. Statistically significant at 5% (*) and 1% (**) size of test. All income values in 2005 dollars.
A birth cohort is defined as from April 1 through March 31 of the following year in the 1930 Census.

Appendix Table 5

Differences in adult SES of the 1919 birth cohort in the 1960, 1970 and 1980 Census relative to comparison cohorts with and without proxies for paternal characteristics calculated using 1920 Census data excluding a control for the number of children in the household

Census year	1960				1970				1980			
Comparison cohorts	A. 1921-1918		B. 1915-1918		A. 1921-1918		B. 1915-1918		A. 1921-1918		B. 1915-1918	
Paternal controls	None	Proxies	None	Proxies	None	Proxies	None	Proxies	None	Proxies	None	Proxies
	[1]	[2]	[3]	[4]	[1]	[2]	[3]	[4]	[1]	[2]	[3]	[4]
SES in adulthood												
1. High School Graduate	-0.022 *	-0.010	-0.035 *	-0.025	-0.018 *	-0.006	-0.022 *	-0.013	-0.012 *	-0.003	-0.016 *	-0.014 *
	(0.009)	(0.009)	(0.014)	(0.014)	(0.005)	(0.005)	(0.009)	(0.009)	(0.004)	(0.004)	(0.007)	(0.007)
2. Years of Education (completed)	-0.188 *	-0.036	-0.209 *	-0.082	-0.169 *	-0.036	-0.182 *	-0.084	-0.161 *	-0.051	-0.112 *	-0.080
	(0.064)	(0.060)	(0.101)	(0.096)	(0.039)	(0.037)	(0.062)	(0.059)	(0.033)	(0.032)	(0.053)	(0.050)
3. Total Income ($/month)	-539	331	-1,088	-313	-644	390	-1,044	-176	-775	-684	1,370	1,755
	(498)	(483)	(795)	(773)	(427)	(418)	(685)	(672)	(486)	(480)	(1,334)	(1,312)
4. Wage Income ($/month)	-550	204	-1,455 *	-662	-927 *	22	-1,395 *	-608	-1,085 *	-972 *	1,109	1,245
	(451)	(436)	(727)	(703)	(393)	(384)	(630)	(617)	(440)	(439)	(1,219)	(1,212)
5. Poor (<1.5 times the poverty level)	0.001	-0.016 *	-0.003	-0.022	0.004	-0.008 *	0.003	-0.008	0.016 *	0.009	-0.034 *	-0.040 *
	(0.008)	(0.008)	(0.013)	(0.012)	(0.004)	(0.004)	(0.006)	(0.006)	(0.005)	(0.005)	(0.015)	(0.014)
6. Duncan's Socioeconomic Index	-0.884 *	-0.074	-0.592	0.180	-0.471	0.229	-0.406	0.172	-0.218	0.271	-0.145	0.106
	(0.436)	(0.424)	(0.694)	(0.676)	(0.267)	(0.261)	(0.427)	(0.418)	(0.234)	(0.230)	(0.373)	(0.366)
7. Disability Limits Work					0.005	0.005	0.005	0.005	0.006	0.004	0.017 *	0.018 *
					(0.004)	(0.004)	(0.006)	(0.006)	(0.004)	(0.004)	(0.006)	(0.006)
8. Disability Prevents Work					0.001	-0.002	-0.002	-0.004	0.008 *	0.005	0.020 *	0.020 *
					(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	(0.005)	(0.005)
9. Social Security Income ($/month)					-22.503	-27.755	-19.552	-23.380	770.069 *	852.978 *	789.782 *	812.072 *
					(16.532)	(16.651)	(26.389)	(26.526)	(59.434)	(59.948)	(173.367)	(173.310)
10. Welfare Income ($/month)					18.154	8.121	18.132	8.143	10.846	0.653	73.492	62.592
					(9.338)	(9.336)	(14.608)	(14.688)	(19.704)	(19.854)	(54.164)	(54.113)
Observations	80,695	80,695	51,462	51,462	216,633	216,633	139,757	139,757	323,089	323,089	213,481	213,481

Notes: Robust standard errors are in parentheses. Statistically significant at 5% (*) size of test. All income values in 2005 dollars.

Appendix Table 6. Comparison of 1920 Census and Vital Statistics Data

Number of children by birth year alive at date of 1920 Census

Year of birth	Age on 1/1/20	# children alive		Census / Vital Statistics
		1920 Census	Vital Statistics	
[1]	[2]	[3]	[4]	[5]
1919	0	1,213,871	1,254,438	0.97
1918	1	1,211,084	1,226,002	0.99
1917	2	1,228,156	1,200,609	1.02
1916	3	736,800	710,894	1.04
1915	4	678,708	674,484	1.01

Notes: To calculate the number of children alive based on vital statitistics, we used natality data adjusting for age-specific mortality, taking into account changes in the states that are covered in the natality data. The 1915 registration area covered 10 states (Connecticut, Maine, Massachusetts, Michigan, Minnesota, New Hampshire, New York, Pennsylvania, Rhode Island, Vermont) and the District of Colombia. Maryland was added in 1916. Indiana, Kansas, Kentucky, and North Carolina were added in 1917. Ohio, Utah, Virginia, Washington, and Wisconsin were added in 1918. California, Oregon, and South Carolina were added and Rhode Island was dropped in 1919. Natality data are drawn from "Birth, Stillbirth, and Infant Mortality Statistics for the Birth Registration Area of the United States" (Roper and Austin, 1931). Mortality data are drawn from the annual "Mortality Statistics" reports published by the Census Bureau.

Appendix Table 7
Maternal infection rate in year before birth on adult SES in the 1960 Census for males born between 1918 and 1920

	Almond (2006)	Replication	Corrected
Socio-economic outcome in adulthood	(1)	(2)	(3)
1. High School Graduate	-0.101 **	-0.099 **	-0.083 *
	(0.070)	(0.034)	(0.039)
2. Years of Education (completed)	-0.756 **	-0.697 **	-0.659 *
	(0.259)	(0.212)	(0.272)
3. Log of Total Income	-0.165 **	-0.162 *	-0.158
	(0.072)	(0.067)	(0.086)
4. Poor (< 1.5 times the poverty level)	0.042	0.040	0.030
	(0.026)	(0.024)	(0.031)
5. Duncan's Socioeconomic Index	-2.711	-2.913 *	-2.580
	(1.735)	(1.283)	(1.545)
Observations	16,566	16,566	16,659

Notes: Standard errors clustered at the state and year of birth level are in parentheses.
Statistically significant at 5% (*) and 1% (**) size of test.