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

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Abstract

Pioneering work by Douglas Almond (2006) used the 1918 Spanish influenza pandemic to establish that in utero exposure to health insults has a large, negative impact on health and socioeconomic prosperity that reaches well into adulthood. A key assumption underlying this body of research is that in utero exposure to the influenza pandemic can be treated as if it were randomly assigned. The validity of that assumption is investigated using data from the 1920 and 1930 U.S. Censuses. We find that those who were exposed in utero were born to families of lower socioeconomic status relative to the cohorts who were not exposed. Specifically, fathers of the exposed cohort made significantly less income, had lower socioeconomic status, were older, had higher fertility, were less likely to be white, and were less likely to be WWI veterans than the fathers of those who were not exposed in utero. When including controls for childhood environment, the effect of in utero exposure on adult outcomes becomes small in magnitude and not statistically significant. Conclusions about the deleterious impact of in utero exposure to the influenza pandemic on socioeconomic prosperity in adulthood are, at best, premature.

I Introduction

The 1918 Spanish Influenza pandemic has been widely exploited to provide causal estimates of the longer-term impacts of in utero health insults on physical and economic well-being in adulthood since seminal work by Almond (2006). This body of research indicates there are very large, negative impacts of in utero exposure to the influenza pandemic on health and socioeconomic success in adulthood. The work is widely cited and has been very influential.

Essentially, these studies compare adult outcomes of the 1919 birth cohort, whose mothers had the highest probability of being exposed to influenza during the pregnancy, with comparable children who were not exposed to influenza in utero. Comparisons are drawn between the 1919 birth cohort and those born before and after 1919; a second set of analyses focuses on the 1919 birth cohort and compares those born in areas where maternal mortality rates (MMR) were high with those born in areas where MMR are low. Almond (2006) reports that the exposed cohorts completed significantly less education and earned less as adults than those who were not exposed. The results have been interpreted as powerful evidence that fetal health has a long-lasting impact, not only on health, but also on economic prosperity in adulthood.

A key assumption underlying this body of research is that the characteristics of the 1919 birth cohort are following the same linear trend as the surrounding birth cohorts. This paper, using data from the IPUMS samples of the 1920 and 1930 U.S. Censuses, evaluates the validity of this claim. Our results indicate that those who were at highest risk of being exposed in utero were born to families of lower socioeconomic status relative to the cohorts who were not exposed. Specifically, the fathers that produced a child in 1919 were significantly less likely to be WWI veterans, had jobs that produced less income, had lower socioeconomic status (SES), were older, had more total children, and were less likely to be white than fathers of those who were not at high risk of being exposed to influenza in utero.

In an effort to assess the importance of these differences, models of the association between exposure risks and adult outcomes are estimated conditioning on childhood environment. These conditional estimates indicate that the effect of in utero exposure to the pandemic on adult economic prosperity is small in magnitude and not statistically significant. These results suggest that further evidence is required in order to claim that in utero exposure to the influenza pandemic had a persistent impact on a long term outcomes.

This paper is organized as follows. Section II will discuss the fetal-origins hypothesis, how using the 1918 U.S. pandemic as a natural experiment could allow for substantial progress in its verification, and the results of the seminal paper written by Douglas Almond about maternal health's effect on later life outcomes. Section III will highlight a major event that took place during the pertinent period and explain how its occurrence creates potentially damaging selection problems for social scientists that wish to use the 1918 influenza pandemic to study impaired in utero health. Sections IV through VI present the methodology and results of two approaches to identifying the validity of these concerns using data from the 1930 and 1920 IPUMS samples of the U.S. Census. Section VII offers an estimation of the impact this paper's findings have on Almond's results. Lastly, Section VIII provides conclusions.

II Using the 1918 U.S. Influenza Pandemic to Evaluate the Fetal-Origins Hypothesis

For many decades it has been an accepted fact that what happens during several crucial periods of human development have long lasting effects (Rasmussen 2001). What has been in dispute over this time, though, is how early these periods begin and how far their impacts span. At the tail end of the 1980's David J. P. Barker introduced what would later be popularly referred to as the fetal-origins hypothesis (FOH). He suggested that poor health as early as the fetal period had dire consequences for mid to late life chronic diseases (Barker 1994). Based on sound biological mechanisms and results from animal experiments, this theory has gained a great deal of traction in the medical and social science communities. Moreover, due to the fact that many researchers have linked health with economic outcomes, there is reason to speculate that it may also be the case that in utero health has long term effects on adult SES.

This theory, though, is far more difficult to prove than the original FOH. First of all, a clear biological model and/or an established epidemiological literature does not exist. Secondly, the scope for a behavior response such as directed intervention after birth, while likely ineffective, for example, in the case of arteries that are preprogrammed to harden, seems more promising when considering non-health human capital development. This research question is further complicated by the fact that there are numerous common factors that can jointly and independently determine both in utero health and later life economic well-being (e.g. SES of parents, overall health of parents, quality of caregiving, parents preferences for human capital investment in children). As such, evidence of the link between in utero health and adult economic outcomes, must come from studies that are able to disentangle the intrinsic endogeneity between early-life health and later life SES. By innovatively using the 1918 U.S. influenza pandemic as a natural experiment to assess the long-term effects of in utero health on a large, representative population, Douglas Almond's work became the seminal piece of evidence that the FOH extended beyond long term health into other human capital outcomes.

The justification for using the 1918 U.S. influenza pandemic as a natural experiment revolves around a few keys aspects of its history. The first, and possibly most crucial element is the onset of the disease; the pandemic began unexpectedly in October 1918.[1] This creates the necessary criteria that subjects are unable to change behavior prior to the exposure period in a way that would affect the researcher's sample or group assignment. Further,

[1]Most historians now note that the first wave of influenza appeared in March 1918 in an army base in Kansas. This wave though received minimal media coverage at the time and was not reported as influenza until years later, and thus has little potential to impact behavior (Almond 2006).

the disease struck violently, yet quickly, and was almost completely inert by the end of January 1919.[2] In fact, the disease's impact was so condensed that approximately 85% of all the U.S. influenza deaths occurred between October 1918 and January 1918 (Almond 2006).

The swift onset and departure of the disease also is a useful element as it allows the researcher to assume that there is very little room for meaningful behavior adjustment during the exposure period. Additionally, the pandemic struck an incredibly large portion of the population, 28%, and unlike previous influenza pandemics, this one had particularly high incidence amongst pregnant women and women of childbearing age. This factor allows Almond to treat the the entire 1919 birth cohort as an "intent-to-treat" exposure group (Jordan 1927 as cited in Almond 2006). Moreover, mortality, though severe in terms of typical influenza exposure, was very low, and thus the concern that selective mortality will hinder the accuracy of the estimates is limited.

Finally, the disease is portrayed as having no prejudices. Avoiding the disease was nearly impossible as it was transmitted and obtained through the common air everyone shares. As the old children's rhyme popular at the time explained, "I opened up the window and in-flu-Enza" (Crawford 2005). Thus, there were extremely variant exposure intensities throughout the country, but most importantly, the heterogeneity in exposure seems to have had no discernable pattern with regard to an area's wealth, climate, or topographical characteristics (Brainerd and Siegler 2003). In summary, the seemingly ideal methodological construct of the 1918 influenza flu pandemic, created the platform for the most influential analysis to date of the impact of a pregnant mother's health on the later-life outcomes of the child in utero.

Almond (2006) used the 1% sample of the 1960, a combined 3% sample of the 1970, and a 5% sample of the 1980 U.S. Censuses from IPUMS. With this data, he was able to analyze outcomes such as educational attainment, wage and total income, and SES. The primary methodology in

[2] There was a final mild flare up of the disease in the spring of 1919, but it was quite benign and went relatively unnoticed and is thus not considered a threat to the validity of the natural experiment (1918.pandemic.gov).

this paper treats those born in 1919 as the intent-to-treat group and the surrounding birth cohorts, in this case those individuals born between 1912 to 1918 and 1920 to 1922, as the controls. As shown below, his specification measures the effect of being born in 1919, $I_i(YOB = 1919)$, on a later life outcome, y_i, while controlling for the yearly trend, YOB_i, and a quadratic of the yearly trend, YOB_i^2:

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

Table 1 presents a replication of Almond's estimates of the coefficient on the 1919 year of birth indicator for regressions run on males in the IPUMS sample of the 1960 U.S. Census.[3] Almost every one of the economic outcomes of interest are statistically significantly adversely affected by being born in 1919. These results are further amplified by the fact that they are based on a group in which only approximately a third of the mothers were infected (Jordan 1927 cited in Almond 2006).

These incredibly stark results have made this work the seminal proof of the connection between maternal health and the long-term future of one's child. In fact, graphs such as Figure 1, from Almond's 2006 paper, have become common starting points for policy makers and scientists who would like to stress the importance of fetal programming.

The results from this natural experiment, though, rest on the assumption of random exposure to the pandemic. Thus, it is critical to investigate the theoretical foundation on which this natural experiment is built, because, while there is no denying the clarity of Figure 1, the interpretation of the diagram becomes quite different if exposure status is non-random in a manner correlated with poor later life outcomes.

Figure 2, a replication of a similar graph found in Thomas (2010), plots the average socioeconomic status in 1930, as measured by Otis Duncan's socioeconomic index (SEI), of the fathers of people born between 1912 and

[3]In his paper, Almond provides similar results when using the IPUMS samples of the 1970 and 1980 U.S. Census data as well, which we have replicated in the Online Appendix, Table A1. Additionally, in Almond's 2006 paper, estimates for women and non-white respondents are also included and are qualitatively similar.

1922 by year of birth from the 1930 U.S. Census. This figure, strongly suggests that the 1919 birth cohort, the cohort of interest in Almond's work, had fathers of substantially lower socioeconomic quality. This fact greatly hinders the assumption of randomness necessary for the natural experiment used in Almond 2006. The next section of this paper will highlight a major event in U.S. history that was taking place during the "exposure" period, describe how the impact of this event may help to clarify the cause of the non-random selection implied by Thomas's figure, and suggest additional characteristics on which the parents of interest may have been selected.

III The Great War and its Implications

The major threat to Almond's natural experiment framework is the fact that overlapping the 1918 U.S. influenza pandemic was an event that significantly impacted fertility during the entire "treatment" period; World War I. Not only is a war of its magnitude always of great demographic significance when evaluating a particular time period, but, in addition, the timing of the United States involvement in WWI is directly correlated with the creation and spread of the 1918 influenza bug.

The United States declared war on Germany in April 1917, was regularly sending troops in the summer of 1918, and had accepted Germany's surrender by November 1918. Thus, during a non-trivial part of the conception period of the exposed cohort in Almond's study a large and select group of child bearing age men were either stationed in army barracks or overseas and unable to contribute to the production of the 1919 birth cohort. In other words, the 1919 birth cohort is made up of children whose fathers are predominately less likely to have served in WWI. For this selection issue to be a problem, though, it would have to be the case that WWI veterans were, on average and significantly, men of higher parental quality. While in many wars this may be unlikely, there are some legitimate reasons for concern in this case.

First of all, this 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 simply buying their way out of service. In fact, due to the draft categories in use in 1917, men with means were more likely to be conscripted. While almost all draft eligible men were put in Class I, one of the main deferments was based on the income dependency of one's family. A man who's family had little financial support apart from himself, such that they would have "insufficient" income if he were drafted, were placed in a lower priority group (Jean Nudd 2004). Further, as with all drafts, men of particularly low health were either less likely to be drafted or completely removed from the conscription process. These draft classifications suggest a major issue for the assumption of random selection, as the more financially stable and healthy men were more likely to be at war. Thus, it is possible that the 1919 birth cohort is made up of a significantly larger portion of poorer and less healthy families.

Additionally, since the military selection criteria is related to age, men not at war, were likely to be significantly older then the surrounding cohorts. This presents a problem for Almond's strategy as educational cohort trends suggest that younger men were significantly more likely to be literate and educated in this time period. Thus, having an older father meant, on average, having a father with less human capital.

Another avenue through which the war may impact the parental distribution is through systematic reactions to the experience of living in wartime. Gary Becker has posited a well-known theory of income-driven fertility patterns based on child quantity versus child quality (1960). In essence, he suggests that, like many other durable goods, high-income individuals choose fewer, higher quality children, while low-income individuals choose more, lower quality progeny. This theory offers some intriguing hypotheses when applied to fertility during wartime.

Since, during wartime, families experience more stress, less certainty, and the threat of rationing, parents interested in producing high quality children may wait until the adverse conditions subside. A reasonable hypothesis that follows from this theory is that, during wartime, families with higher income, or at least, families concerned with having higher quality children, may postpone family enlargement until the war is over.

These aspects of life in the U.S. preceding the influenza outbreak suggest that the income, health, and education of the parents of the 1919 birth cohort may have been significantly lower than surrounding birth cohorts and that the exposure cohort families may have had a lower preference for child quality than the comparison cohorts. This type of sorting would present a major problem for identifying the impact of maternal health on the child's later life wealth and education conditions, as numerous studies have connected parental wealth, health, and schooling with these very same outcomes (Davis-Kean 2005, Duflo 2000, Thomas and Strauss 1998, Brooks-Gunn and Duncan 1997, Corcoran et al. 1992, Hill and Duncan 1987). In summary, the non-random selection of the draft and the hypothesized non-arbitrary family planning of those experiencing a war, create legitimate concerns over the assumption of random experimental assignment.

While Figure 2 implies that the concerns presented previously are real, the goal of the next section of this paper is to rigorously compare the family characteristics of those born in 1919 with the surrounding birth cohorts. Namely, this paper will test the hypotheses that assert that the parents of children born in 1919 were significantly worse in the areas of income and socioeconomic status, that they were older, and that they desired a larger quantity of, rather than higher quality, children, than the parents of children from surrounding cohorts.[4] The next section will present two approaches to analyzing the validity of these suppositions.

IV Methodology

To examine the hypotheses presented in the previous section, it was imperative to find data that contained the parental characteristics of the early 1900's birth cohorts. As Almond, this research takes advantage of the comprehensive and demographically rich U.S. Census data. The IPUMS 1%

[4]Unfortunately, this paper is unable to directly test the hypothesis that the 1919 birth cohort had significantly less healthy parents as no variable that measured or could proxy for parental health existed in the data. Further, there is no measure of a parent's completed education, thus the Duncan's socioeconomic index, which contains an element of education in its calculation can be scene as the closest proxy.

sample of the 1930 U.S. Census data is particularly useful as it contains information on the parents of U.S. born children over the entire time period of Almond's 2006 analysis, 1912 - 1922. Although the range of parental characteristics is not exhaustive in relation to this study's hypotheses, the 1930 U.S. Census contains ample demographic statistics to provide informative analysis.[5]

One area in which the 1930 census is particularly thorough is in information about the economic status of the parents. The data includes both the father's Duncan's SEI score and the father's occupational income score.[6] Furthermore, family size can be used to address the quantity versus quality hypothesis. In this case, the number of the father's children in the household will be used as a signal of a family's preference. Another nice element of the 1930 U.S. Census data is that it can be used to directly test the inference that children born in 1919 were less likely to be the child of a WWI veteran. Finally, the age of the father at the time of the child's birth will be used to test if the 1919 birth cohort had significantly older parents than those in surrounding cohorts.

One complication to this study was that the 1930 U.S. Census was collected on April 1, 1930 and age information was obtained as of March 31, 1930. As such, this study is limited to placing people into birth cohort bins between April 1st and March 31st rather than January 1st and December 31st. This hinders the analysis, in that, the birth cohort of interest, 1919, loses an important quarter of exposure, those conceived in the 2nd quarter of 1918, and replaces them with an unexposed group, those conceived in the 2nd quarter of 1919. Following the intuition proposed in the previous section, this would cause the results to be a lower bound, but this issue cannot be tested or solved directly. When discussing the results from the 1930 U.S. Census data, reference to any birth year indicates that the person was born between April 1st of that year and March 31st of the subsequent year.

[5] All data is as of March 31, 1930.

[6] Otis Duncan's SEI is a measure of occupational status based upon the income level and educational attainment associated with each occupation in 1950. Occupational income score assigns each occupation a value representing the median total income (in hundreds of 1950 dollars) of all persons with that particular occupation in 1950.

Additionally, there are two main areas of sampling concern with respect to using the 1930 U.S. Census data for this study. First, we posit that fathers of the 1919 birth cohort were less likely to be in WWI. To be included in the regressions related to a father's characteristics, one's father must be alive in 1930. If it were the case that smarter and more economically viable soldiers were less likely to be killed at war, then the sample of pre-war birth cohort fathers may be biased because the weakest fathers are missing. If this issue is a valid concern, it should be the case that the children born before the war are significantly more likely to be missing data on their fathers. There is no evidence in the data to support this claim.

A second area of concern is that the 1930 U.S. Census does not contain data for one's parents if the person was living independently from their parents. This is particularly problematic if those children that move out and live by themselves earlier are the children from lower quality households. To determine the severity of this problem this study examined if early birth cohorts, the older children in 1930, had significantly less parental information. In the end, only the earliest birth cohort in the trend, 1912, exhibited this problem. Estimates using a smaller birth cohort group (1913-1922) are qualitatively and quantitatively equivalent to those found in the Results section and are available in the Online Appendix, Table A2.

As this study purposefully follows Almond's own model, the 1919 birth cohort will be isolated to test if it is significantly different than the surrounding cohorts, 1912 to 1922, while controlling for the time trend.[7] The only difference in the two models is that where his outcomes, y_i, were individual i's outcomes in later years, the dependent variables in these specifications are the individual's parent's characteristics in 1930:

$$y_i = \beta_0 + \beta_1 \cdot YOB_i + \beta_2 \cdot YOB_i^2 + \beta_3 \cdot I_i(YOB = 1919) + \epsilon_i \qquad (2)$$

A second approach to testing the hypotheses of the previous section is to turn to the 1920 U.S. Census. Using the 1920 U.S. Census data provides

[7] The actual period used in the analysis was April 1st, 1911 to March 31st, 1923, in order to capture all the respondents born between 1912 and 1922.

some straightforward gains. First and most importantly, the 1920 census was taken on January 1st, 1920, thus age perfectly predicts the respondent's year of birth and each birth cohort can be accurately identified. Additionally, due to the fact that the cohorts of interest are 10 years younger in 1920, there is no concern that lower quality older children will have moved out, and as such, left the sample.[8] Along with these beneficial elements of the 1920 census data, though, are some obvious shortcomings.

The major problem with using data obtained on January 1st, 1920 is that the comparison group loses almost the entire post pandemic cohort.[9] Although all indications from the 1930 U.S. Census analysis suggest that this is not the case, losing the post pandemic cohort leaves the significant differences found in the 1919 birth group open to the interpretation that they are simply the result of the start of a new trend.[10]

The primary specification will be the same as equation (2) except the indicator for being born in 1919 will actually refer to being born between January 1st 1919 and December 31st 1919 and the trend will be from 1912 to 1919.

V Results

Table 2 presents the estimates of β_3 from analysis of the IPUMS 1% samples of the 1920 and 1930 U.S. Censuses. Starting with the 1920 U.S. Census results, we see that for both the occupational income score as well as the Duncan SEI outcome, fathers of children born in 1919 are doing significantly worse in 1920, after controlling for the time trend, than the fathers of the previous cohorts. Further, we find that the 1919 birth cohort is a member of significantly larger families, suggesting that Becker's theory of quality versus quantity may be biasing Almond's findings. Additionally, analysis of another marker of parental composition, age of the father at

[8] As before, the father's data is not missing significantly more for the pre-war cohorts.

[9] Only the 4th quarter 1919 birth cohort can be considered relatively unexposed.

[10] In the 1920 U.S. Census they do not ask about military status, so this outcome is not analyzed.

birth, suggests that the fathers of the 1919 birth cohort were significantly older at the time of the child's birth.

Aside from the negative distributional change this suggests with respect to the education of the parents of the 1919 birth cohort, having older parent's may also effect a child's long-term socioeconomic outcomes in an additional way. Having older parents translates into needing to provide care at a younger age. Caregiving which is associated with significantly higher levels of stress (Deimling and Bass 1986; Noelker and Townsend 1987; Stoller and Pugliesi 1989) also may stunt educational and income trajectories, as the time, effort, and money spent on caring for the aging parent can limit the child's ability to take advantage of all opportunities and fully realize their potential.

Finally, the 1920 U.S. Census results reveal that a child born in the U.S. in 1919 was significantly less likely to be Caucasian. This composition change is a clear signal of being born into a less ideal environment as, during this time period, being white provided not just circumstantially better educated and more economically viable parents but, due to rampant racism, also better long term opportunities for one's own achievement.

When evaluating the results for the 1930 U.S. Census we find qualitatively similar results. While the magnitudes of the estimates are smaller due to the loss in precision of the 1919 birth cohort indicator, the direction of the coefficients are always in the hypothesized direction. Finally, as expected, the 1919 birth cohort is significantly less likely to be the child of a World War I veteran. As mentioned, the draft classifications would suggest that children of non-WWI veterans are more likely to be born into financially unstable households. To more firmly establish this claim, we have examined the correlation between being a WWI veteran father and other demographic characteristics while controlling for the father's age, father's age squared, and state of birth fixed effects. For each variable, being a WWI veteran was significantly positively related to having more desirable traits.[11]

Taken as a whole, analysis of the 1920 and 1930 U.S. Censuses indicate

[11] The results of this analysis is available in the Online Appendix, Table A3.

that the parents of the 1919 birth cohort were not randomly assigned. Further, the attributes on which they were selected into the "treatment" group are all negatively related to the child's future educational and economic outcomes.

VI Re-Evaluation of the Impact of In Utero Exposure to the 1918 Influenza Pandemic on Adult Economic Outcomes

The previous section makes the case that the parents of the 1919 birth cohort were significantly different than the parents of surrounding cohorts in attributes that hinder the identification strategy used in Almond (2006). The next appropriate step to take, after identifying this bias, is to estimate to what extent controlling for parental characteristics reduces the magnitude and significance of Almond's findings. Unfortunately, testing this directly is not possible as the data sources used in Almond (2006) do not contain information on parental or family background characteristics. With this first-best option unavailable we proceed by taking two alternative approaches to estimating the persistent effect of in utero exposure to the 1918 U.S. influenza pandemic when controlling for selection into the 1919 birth cohort.

The first strategy we employed was to replicate Douglas Almond's 2006 work, which uses the 1960, 1970, and 1980 IPUMS samples of the U.S. Censuses, and compare his findings to the same models when they additionally include as close a control for parental characteristics as is available in the data. The most useful data to proxy parental characteristics from the U.S. Census is the information contained in the 1920 and 1930 U.S. Censuses. While we can cannot directly connect an individual record in the later censuses with their parents in the 1920 or 1930 U.S. Censuses, we can apply to each individual the calculated average parental/family information of an individual born in their state of birth, in their year of birth, and of their race from the earlier Censuses.[12]

[12]Race categories were limited to white or non-white.

This analysis was conducted by first replicating Almond's 2006 findings. These estimates are shown in the second column of Table 3. Then we next compare the magnitude and significance of the point estimates on the 1919 birth cohort dummy variable to estimates from the same model that additionally includes state of birth-year of birth-race level parental characteristics from the 1930 U.S. Census. These results are shown in the third column of Table 3.[13]

The inclusion of proxies for parental characteristics has a substantial impact on the estimates and implications of Almond's original analysis. Evaluating this exercise one can see that the sign on the coefficients, in all but one case, has reversed. Further, the lone result that has not flipped directions, high school graduation, has been reduced in magnitude by over 75%. Lastly, none of the estimates remain statistically significant at the 5% level.[14] This first approach strongly suggests that accounting for parental characteristics is of first order importance when evaluating the impact of in utero health using the 1918 influenza pandemic as a natural experiment, but as the attributes being used are only proxies, an attempt was made to find data which could both replicate Almond's findings and contained individual level parental characteristics for the cohorts of interest.

The 1973 Occupational Changes in a Generation (OCG) dataset was collected in order to study the importance of a man's background on their adult economic success. As such, it contains information on the subject's employment outcomes as well as on the respondent's family and parental characteristics.[15] The OCG data is made up of a sample of the male 20-65 year old non-institutionalized population in 1973, thus it provides adult outcomes and family characteristics for all of the birth cohorts used in Almond's original study. While the timing of this data does not perfectly match any

[13] The 1920 U.S. Census can not be used in this analysis as information is only available for cohorts before 1920.

[14] Conducting the same analysis on the IPUMS samples of the 1970 or 1980 U.S. Censuses provide qualitatively similar results; magnitudes of point estimates are reduced by at least 91% and all statistical significance is lost. These results can be found in the Online Appendix, Table A1

[15] The OCG includes a respondent's parents' education and family income when the respondent was 16 years old and the respondent's number of siblings.

of the U.S Censuses used in the Almond paper, it is temporally closest to the 1970 census and so this will be used as the comparison to determine if the OCG data can closely replicate Almond's original findings. In Table 4, the 2nd and 4th column contain the coefficients from replication of (1) when using the highest grade completed by the respondent as the dependent variable and utilizing the 1970 U.S. Census and the OCG data, respectively.

Comparing the two results, the magnitudes of the coefficients are quite similar, with the Census analysis suggesting being born in 1919 leads to a .18 reduction in completed grade level and the OCG estimate implying that being born in 1919 leads to a .16 decrease in completed grade level. The noticeable difference between the two analyses is that the standard error for the Census result is significantly small than the standard error for the OCG analysis. This difference is expected, though, as the Census sample is more than 45 times larger than the OCG sample. To more properly compare the two estimates, we calculate proxy standard errors for the OCG analysis that represent the standard errors of the analysis if the OCG sample size was scaled up to the size of the IPUMS sample of 1970 U.S. Census. This result is seen in row 2 of Table 4. Once the OCG data is of comparable size to the 1970 U.S. Census sample, the standard error and thus significance level of the OCG data analysis mirrors very closely what is found in Almond (2006).

Given the similarity of the results, we move forward by introducing the individual level parental and family characteristics to test the impact of controlling for the underlying non-random selection into the 1919 birth cohort. These results can be found in the 5th column of Table 4, and, as we found using the first approach, the magnitude of the impact of being born in 1919 is diminished by close to 100%. Even after scaling the OCG sample size to over 300,000, the result is still not statistically significantly different from 0.

Along with the methodology already mentioned, in which only temporal variation is used to identify the impact of fetal health, Almond (2006) contains an alternative strategy in which adult outcome differences using both temporal *and* geographic variation in influenza exposure are examined. In this analysis Almond uses maternal mortality rates (MMR) by state and the year prior to birth to proxy for infection intensity.

This methodology though, does not control for the identification biases discussed in this paper, as high MMRs in one year are likely to be correlated with poor parental characteristics and a weaker health environment for the next birth cohort. High MMRs, particularly when the rate is trending up, can serve as a signal of poor quality health conditions. Moreover, in states where MMRs where relatively high or steadily increasing in the previous year, the families that still choose to conceive a child are likely to have weaker preferences for health. This implies that the fetal health variation Almond is using in this analysis may be significantly correlated with parental and environmental characteristics and, similar to the primary methodology, a failure to control for these factors may lead to biased results. In an effort to test the impact of controlling for these factors, a strategy similar to the first analysis described in this section was conducted in which state of birth-year of birth-race level proxies are generated from U.S. Census data and added to Almond's estimation.

Table 5, column 2 contains the results reported in Almond's original 2006 paper.[16] While attempting to replicate this analysis we found a slight error in the MMRs used in the original analysis. It appears that a transcription error lead to assigning Virginia, one of only 19 states with MMR data, a 1919 maternal mortality rate of 6.3 rather than 8.3. Additionally, MMR information for an effected region, Washington D.C., was available in the historic data, and thus added to the analysis. Replication of Almond's work with these corrections is found in Table 5, column 3.

In order to more precisely estimate the impact of influenza exposure on long term economic outcomes using lagged MMRs, we needed to control for two sets of potentially endogenous factors; regional demographic characteristics correlated with MMR but unrelated to the level of influenza exposure

[16]Two of the reported point estimates, standard errors, and significance levels do not have statistical coherence. While the standard error for the high school graduation regression was reported to be 7.0, from replication we believe the standard error is actually closer to 3.6, making the reported significance level (1% level) in the 2006 paper accurate. The log of total income regression also has an inconsistency in its reported estimates and significance level. In this case, from replication, it appears that the standard error and point estimates are correct but the significance level should be lowered to the 5% level rather than the 1% level.

and parental attributes of the children born in 1919. Since the MMR analysis is only conducted over the cohorts born between 1918 and 1920, the state of birth-year prior to birth-race level information from the 1920 U.S. Census can be used to control for the demographic factors correlated with a state's MMR in the year prior to birth. Additionally, as in the first analysis mentioned in this section, state of birth-year of birth-race level proxies for family characteristics from the 1930 U.S. Census are added to control for the selection bias inherent in the 1919 birth cohort.

Results from this analysis can be found in Table 5, column 4. As before, controlling for parental and environmental factors significantly reduces the magnitude of the point estimates and removes statistical significance in each regression.

While the analysis described in this section is not able to perfectly correct for the identification issues presented in the previous section, each makes a consistent point; the sample selection issue expressed in this paper has a significant attenuating effect on the magnitude and power of results that use the 1918 U.S. influenza pandemic as a natural experiment for in utero health and do not control for parental characteristics. Furthermore, these results suggest that influenza exposure in utero was either non-detrimental to long-term economic success, or that some form of intervention on the part of the caregivers of the exposure cohort was effective in remediating the damage to the mechanisms that drive future SES.

VII Conclusion

Testing the fetal-origins hypothesis using methods other than a natural experiment is rife with empirical and logistical issues. Controlling for all the typically unobserved parental characteristics correlated with both a parent's health and a child's later life outcomes, as well as, obtaining data which includes the health of pregnant mothers, family characteristics, and follows the child to adulthood is currently not possible for researchers. Given this reality, Douglas Almond's clever use of the 1918 U.S. influenza pandemic and its landmark findings was an incredible breakthrough in the study of fetal health's persistent impact on adult economic outcomes.

This paper set out to explore the underlying assumptions necessary to support Almond's influential findings. What we discover is that due to the historical circumstances surrounding the 1918 influenza epidemic, namely WW1's impact on family planning behavior and the systematic selection process of conscription, the 1919 birth cohort was not only exposed to a poorer disease environment in utero, but was also born into families that were significantly less wealthy, larger, and had lower SES.

Most damaging to Almond's inference is that each of these characteristics is a direct or theoretical sign of low quality parentage that can impact a child's later life wealth and educational outcomes. Analysis attempting to replicate Almond's work while adding controls for aspects of the child's family environment consistently return results that suggest that the 1919 birth cohort were not statistically significantly different than surrounding cohorts in their later life education, wages, or SES.

While this paper in no way comments on the overall legitimacy of the fetal-origins hypothesis, it does assert that its most seminal work linking early-life health to adult economic outcomes has large enough identification ambiguity to make its estimates untenable. Furthermore, the findings in this paper, which indicate that those exposed in utero to the 1918 influenza pandemic have equivalent later life socioeconomic status as surrounding cohorts, may provide supportive evidence that there is scope for post-birth intervention to mitigate the adverse impacts of early life health insults on long-term economic well-being.

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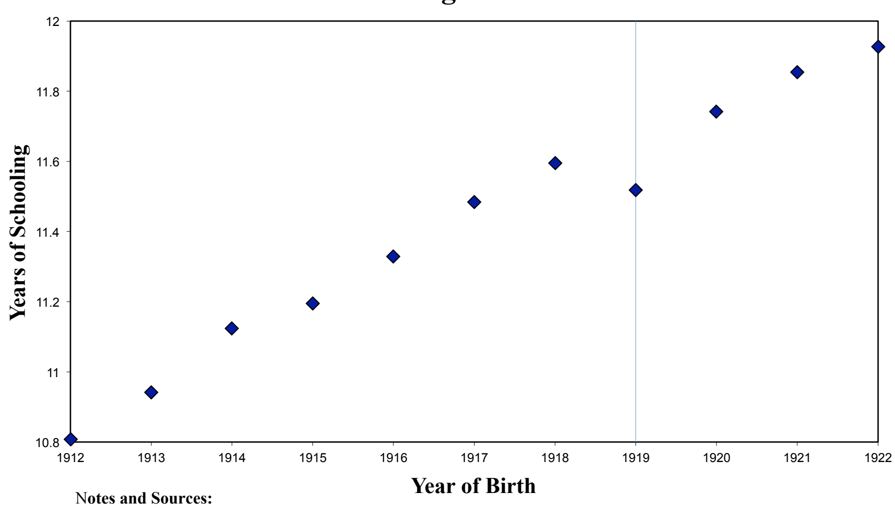
IX Tables and Figures

Table 1
Replication of the Almond 2006 Estimates of the Impact of Being Born in 1919 for Men

Long-Term Outcome	Mean	Born in 1919
High School Graduate (%)	47.20%	-2.12% **
		(0.54)
Years of Education (completed)	13.48	-0.15 **
		(0.04)
Total Income ($/month)	5864	-85
		(44)
Wage Income ($/month)	5696	-122 **
		(39)
Poor (% below 1.5 times the poverty level)	27.42%	1.00% *
		(0.49)
Duncan's Socioeconomic Index	35.13	-0.63 *
		(0.26)

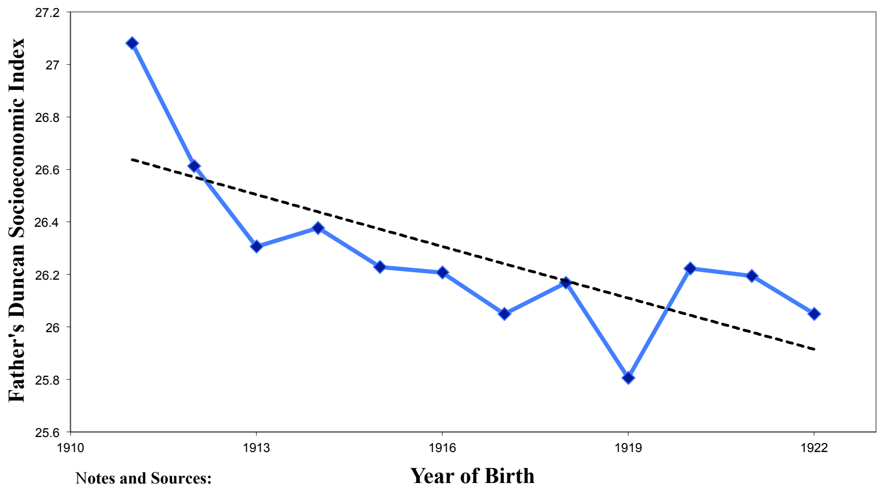
Notes:
** indicates statistical significance at the 1% level, * indicates statistical significance at the 5% level.
Regressions use the same models as in Almond (2006) and data from the IPUMS 1% sample of the 1960 U.S. Census.
Robust standard errors are in parenthesis. Regressions based on 114,032 observations.

Figure 1



Notes and Sources:
Data obtained from 1 percent sample of 1960 U.S. Census

Figure 2



Notes and Sources:
Data obtained from 1 percent sample of 1930 U.S. Census
Year of birth covers 4/1/YOB - 3/31/YOB+1

Table 2
Departure of 1919 Birth Cohort Parental Characteristics from Trend[1] for Men

	1920 U.S. Census		1930 U.S. Census
Parental Characteristic	Mean[2]	Born in 1919	Born in 1919[3]
Father's Duncan's Socioeconomic Index	24.39	-1.07 **	-0.23
		(0.36)	(0.22)
Father's Occupation Income Score	22.73	-0.45 *	-0.19
		(0.18)	(0.11)
Number of Father's Children in HH	3.67	0.34 **	0.09 **
		(0.04)	(0.02)
Father's Age at Birth	32.86	0.46 **	0.23 **
		(0.14)	(0.08)
Child is Non-White (%)	11.24%	1.70% **	1.08% **
		(0.53)	(0.33)
Father is a WWI Veteran (%)	6.8%		-1.11% **
			(0.28)

Notes:
** indicates statistical significance at the 1% level, * indicates statistical significance at the 5% level, and robust standard errors are in parenthesis.
Data comes from 1% IPUMS samples of the 1920 and 1930 U.S. Census.
1930 U.S. Census regressions are based on 141,658 observations and 1920 U.S. Census are based on 93,291 observations.
[1]Due to the timing of the 1930 U.S. Census, the trend is from April 1, 1911 to March 31, 1923. For the 1920 analysis, the trend is from January 1, 1912 to December 31, 1919.
[2]Mean for "Father is a WW1 Veteran" comes from 1930 U.S. Census data.
[3]Due to the timing of the 1930 U.S. Census, the 1919 birth cohort consists of people born between April 1, 1919 and March 31, 1920.

Table 3
Replication of the Almond 2006 Estimates of the Impact of Being Born in 1919 for Men Compared to Estimates that Control for Parental Characteristics[1]

	Born in 1919		
Long-Term Outcome	Replication		w/ Parent Controls[1]
High School Graduate (%)	-2.1%	**	-0.5%
	(0.54)		(0.53)
Years of Education (completed)	-0.15	**	0.01
	(0.04)		(0.04)
Total Income ($/month)	-85		69
	(44)		(43)
Wage Income ($/month)	-122	**	8
	(39)		(38)
Poor (% below 1.5 times the poverty level)	1.00%	*	-0.66%
	(0.49)		(0.48)
Duncan's Socioeconomic Index	-0.63	*	0.12
	(0.26)		(0.26)

Notes:
** indicates statistical significance at the 1% level, * indicates statistical significance at the 5% level.
Regressions use the same models as in Almond (2006) and data from the IPUMS 1% sample of the 1960 U.S. Census.
Robust standard errors are in parenthesis. Regressions based on 114,032 observations.
[1]Specification includes birth cohort-state-race level parental characteristics from the IPUMS 1% sample of the 1930 U.S. Census.

Table 4
Departure of 1919 Male Birth Cohort From 1912-1922 Trend Using 1973 Occupational Changes in a Generation (OCG) Data

	1970 U.S. Census			1973 Occupational Changes in a Generation			
		Born in 1919			Born in 1919		
Long-Term Outcome	Mean	Baseline		Mean	Baseline		w/ Parent Controls[1]
Years of Education	13.74	-0.18		13.68	-0.16		-0.010
Standard Error Using 1970 Sample Size		(0.02)	**		(0.02)	**	(0.02)
Standard Error Using OCG Sample Size		-			(0.16)		(0.13)

Notes:
** indicates statistical significance at the 1% level, * indicates statistical significance at the 5% level.
Regressions use the same models as in Almond (2006) and data from an IPUMS combined 3% sample of the 1970 U.S. Census and the 1973 Occupational Changes in a Generation Data. Robust standard errors are in parenthesis.
1970 U.S. Census regressions are based on 308,785 observations and OCG regressions are based on 6,852 observations.
[1]Regressions included individual level parental characteristics.

Table 5
The Impact of the Previous Year's Estimated Maternal Infection Rate on Men Born from 1918 to 1920

Long-Term Outcome	Almond (2006)	Corrected[1]	w/Parental Controls[2]
High School Graduate (%)[3]	-10.10% **	-8.64% *	-5.38%
	(7.00)	(4.12)	(4.56)
Years of Education (completed)	-0.756 **	-0.692 *	-0.450
	(0.259)	(0.322)	(0.332)
Log of Total Income[4]	-0.165 **	-0.166	-0.070
	(0.072)	(0.091)	(0.107)
Poor (% below 1.5 times the poverty level)	4.24%	3.17%	-1.27%
	(2.59)	(3.26)	(4.48)
Duncan's Socioeconomic Index	-2.71	-2.39	0.24
	(1.74)	(2.04)	(2.38)
Observations[5]	16,566	16,659	16,659

Notes:
** indicates statistical significance at the 1% level, * indicates statistical significance at the 5% level.
Regressions use the same models as in Almond (2006) and data is from the IPUMS 1% sample of the 1960 U.S. Census.
Standard errors clustered at the state and year of birth level are in parenthesis.
[1]In Almond (2006) there is an error in the Virginia 1919 maternal mortality rate. The error is fixed in this analysis (6.3 changed to 8.3).
Additionally, in Almond (2006), District of Colombia births are excluded, but maternal mortality rate information is available for this region.
[2]Specification includes lagged birth cohort-state-race level parental characteristics from the IPUMS 1% sample of the 1920 U.S. Census and contemporaneous birth cohort-state-race level parental characteristics from the IPUMS 1% sample of the 1930 U.S. Census.
[3]While the standard error for the high school graduation regression was reported to be 7.0, from replication we believe the standard error is actually closer to 3.6, making the reported significance level (1% level) in Almond (2006) accurate.
[4]When replicating the total income analysis, it appears that the standard error and point estimates are correct but the significance level should be lowered to the 5% level rather than the 1% level reported in Almond (2006).
[5]This is the total number of observations available, but due to the varying number of missing values for each dependent variable, the total is not the same for each regression.

Table A1
Replication of the Almond 2006 Estimates of the Impact of Being Born in 1919 for Men
Compared to Estimates that Control for Parental Characteristics[1]

	1970 U.S. Census		1980 U.S. Census	
	Born in 1919		Born in 1919	
Long-Term Outcome	Replication	w/ Parent Controls[1]	Replication	w/ Parent Controls[1]
High School Graduate	-2.0% **	-0.1%	-1.4% **	0.5% *
	(0.32)	(0.31)	(0.26)	(0.25)
Years of Education	-0.18 **	-0.02	-0.12 **	0.05 *
	(0.02)	(0.02)	(0.02)	(0.02)
Total Income	-242 **	33	-444 **	-16
	(50)	(49)	(80)	(79)
Wage Income	-172 **	58	-287 **	3
	(46)	(45)	(75)	(75)
Poor (below 150% of the poverty level)	0.9% **	-0.2%	0.6% **	-0.2%
	(0.22)	(0.21)	(0.20)	(0.20)
Duncan's Socioeconomic Index	-0.81 **	-0.01	-0.81 **	-0.07
	(0.16)	(0.16)	(0.14)	(0.14)

Notes:
** indicates statistical significance at the 1% level, * indicates statistical significance at the 5% level.
Regressions use the same models as in Almond (2006) and data from the IPUMS combined 3% sample of the 1970 and the 5% sample of the 1980 U.S. Censuses.
Robust standard errors are in parenthesis. Regressions based on 308,785 observations for 1970 and 471,803 observations for 1980.
[1]Specification includes birth cohort-state-race level parental characteristics from the IPUMS 1% sample of the 1930 U.S. Census.

Table A2
Departure of 1919 Birth Cohort[1] Parental Characteristics from 1913-1922 Trend[2]
Using 1930 U.S. Census Data

Parental Characteristics	Mean	Born in 1919[1]
Father's Duncan's Socioeconomic Index	26.19	-0.31
		(0.16)
Father's Occupation Income Score	23.60	-0.16 *
		(0.08)
Number of Father's Children in HH	4.14	0.07 **
		(0.02)
Father's Age at Birth	32.14	0.25 **
		(0.06)
Child is Non-White (%)	12.28%	0.73% *
		(0.23)
Father is a WWI Veteran (%)	7.22%	-1.03% **
		(0.20)

Notes:
** indicates statistical significance at the 1% level, * indicates statistical significance at the 5% level.
Robust standard errors are in parenthesis. Regressions based on 259,062 observations.
[1]Due to the timing of the 1930 U.S. Census, the 1919 birth cohort consists of people born between April 1, 1919 and March 31, 1920.
[2]Due to the timing of the 1930 U.S. Census, the trend is from April 1, 1911 to March 31, 1923.

Table A3
Departure of WWI Veteran Fathers from All Fathers in 1912 to 1922[1]

Parental Characteristics	Mean	WWI Veteran	
Father's Duncan's Socioeconomic Index	26.24	6.50	**
		(0.19)	
Father's Occupation Income Score	23.62	2.96	**
		(0.10)	
Father's Occupation Earnings Score	44.65	7.90	**
		(0.24)	
Parents Own a Radio	34.61%	9.28%	**
		(0.39)	
Number of Father's Children in HH	4.13	-0.85	**
		(0.01)	

Notes:
** indicates statistical significance at the 1% level, * indicates statistical significance at the 5% level.
Robust standard errors are in parenthesis. Regressions based on 281,463 observations.
[1]Due to the timing of the 1930 U.S. Census, the trend is from April 1, 1911 to March 31, 1923.
Specification uses age of father, age of father squared, state of birth fixed effects, an indicator for being white, and an indicator for WWI veteran as the independent variables.