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The long-term consequences of the global 1918 influenza pandemic: A systematic analysis of census data from 51 countries

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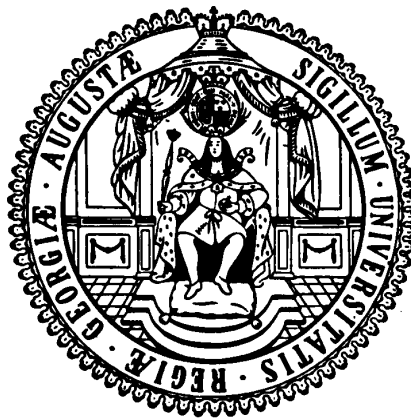
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The long-term consequences of the global 1918 influenza pandemic: A systematic analysis of census data from 51 countries

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Abstract

Several country-level studies have identified long-term adverse effects of in-utero exposure to the 1918 influenza pandemic (also known as the Spanish Flu) on economic outcomes. In-utero conditions are theoretically linked to adult health and socio-economic status through the fetal origins hypothesis. Historical exposure to the Spanish Flu provides a natural experiment to test this hypothesis. Although the Spanish Flu was a global phenomenon, with an estimated 500 million people infected worldwide, no comprehensive global study on its long-term economic effects exists. We address this gap by systematically analyzing harmonized census data from 51 countries. Using the same empirical approach as previous studies, we find no evidence of consistent long-term effects on educational attainment and employment. Overall, our results are difficult to reconcile with the view that in-utero exposure to the 1918 influenza pandemic was associated with important long-term adverse effects on economic outcomes at the population level. A comprehensive set of robustness checks do not alter this conclusion.

Keywords: Spanish Flu, 1918 Influenza Pandemic, Fetal Origins Hypothesis

JEL Classification: I15, N30, O57

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1 Introduction

In recent years, research on the impact of health conditions on economic outcomes has expanded to also investigate how conditions before birth affect individuals' life paths, thereby linking fetal shocks to (economic) outcomes in adulthood. The most prominently cited hypothesis in this context is the fetal origins hypothesis first popularized by British doctor David Barker, who postulated that severe health conditions such as heart disease and diabetes in later adulthood could be linked to the in-utero environment (especially maternal nutritional deprivation) to which the fetus was exposed (Barker 1998). Although Barker's hypothesis was initially met with skepticism in the medical literature (Huxley et al. 2002; Adair and Prentice 2004), it was quickly discovered by economists and used to evaluate the economic consequences of various fetal shocks (Almond and Currie 2011).

In a seminal paper, Almond (2006) first assessed the in-utero impact of the 1918 influenza pandemic on later-life economic outcomes. Based on observational data from three census waves in the United States, he compared long-term health and economic outcomes of cohorts exposed to the pandemic with those of surrounding cohorts. Despite the plausibility of the fetal origins hypothesis on an individual level, it is not guaranteed that such population-level analyses reveal significant impacts. This is because negative health shocks may not only deteriorate average health, but also increase the survival threshold, leading to a so-called 'culling effect' that may offset (or even exceed) any negative impacts on average health (Valente 2015). Nevertheless, Almond (2006) found the studied cohorts to be significantly less likely to graduate from high school and to have lower average income, lower socioeconomic status, and higher risk of being disabled (similar results are also reported by Garthwaite 2008 and Fletcher 2014).

Following Almond (2006), a number of other studies have expanded the investigation to other countries. Neelsen and Stratmann (2012) find that male Swiss birth cohorts exposed to the 1918 influenza pandemic feature lower educational attainment and are less likely to be married compared to the common trend. Lin and Liu (2014) find that Taiwanese cohorts exposed to the pandemic feature lower average educational attainment, are smaller during puberty and more susceptible to severe health conditions such as kidney disease and diabetes in later adulthood compared to surrounding cohorts. Karlsson et al. (2012) find Swedish cohorts exposed to the pandemic to experience elevated poverty rates. Nelson (2010) assesses the effect of the 1918 influenza pandemic for six metropolitan areas in Brazil and finds that, on average, cohorts prenatally exposed to the pandemic are less likely to have graduated from college, have less years of schooling, are less likely to be employed, and earn lower average wages.

As these studies argue, influenza is a particularly suitable case for investigating the long-term effects of in-utero environment as exposure is quasi-random. Specifically, influenza is common in human populations and exposure within the same age group is not determined by socio-economic characteristics (Neelsen and Stratmann 2012). The global spread of the 1918 influenza pandemic was rapid and unanticipated, causing exogenous variation in fetal health between cohorts exposed to influenza in utero and those born shortly before and shortly after. More virulent forms of influenza characterized by a high number of infections and deaths occur every once in a while, leading to pandemics such as in 1889/90, 1918/19, 1957/8, 1968/9, and 1977/8. The influenza pandemic of 1918, often called the 'Spanish Flu', spread around the globe within a few months, killing a multiple of the casualties of World War I and sparing only a few remote regions.¹ New modes of transportation of the era such as steamships and railways as well as the large movements of troops and civilians during the war facilitated the global spread of the pandemic. In most regions, the diffusion happened along major transportation routes. Coastal countries were typically infected first through incoming ships carrying ill passengers or crews, but even remote areas in sub-Saharan Africa got infected. As influenza was not a reportable disease, patients were not detained and, hence, the pandemic spread largely unhindered (Patterson and Pyle 1991; Killingray and Phillips 2003).

Despite the global spread of the 1918 pandemic, with an estimated 500 million people infected worldwide (Taubenberger and Morens 2006), we are not aware of any previous study investigating the long-term effects of the 1918 influenza pandemic globally or across different world regions. This is particularly relevant because it is possible that single-country studies focusing on countries with significant long-term effects were more likely published than studies on countries without statistically significant effects, possibly leading to false conclusions regarding the external validity of Almond's original results and an incomprehensive picture of the long-term impacts of the Spanish Flu.

This paper addresses this gap in the literature by systematically analyzing census data from 51 countries to investigate the long-term effects of the 1918 influenza pandemic following an approach akin to those used in previous studies. Our analysis is based on data provided by IPUMS International (Minnesota Population Center 2014). The main advantage over other sources of census data is that IPUMS International provides harmonized indicators that allow for international comparisons across countries and time. Following Almond (2006), we evaluate

¹ As Spain was a neutral power during the war, newspapers were uncensored and, hence, articles of the disease and its spread were common whereas belligerent countries kept tabs on their reports to avoid mass panics. This is usually considered the reason why this pandemic is referred to as the Spanish Flu (Killingray and Phillips 2003, Almond 2006).

the performance of the 1919 birth cohort against a yearly quadratic trend with respect to different outcomes in adulthood. Given limited data availability for other indicators, we mainly focus on three dependent variables, namely completion of primary education, completion of secondary education, and employment status at the time of enumeration (general and work-related disability are used as alternative outcomes in a smaller sample as part of our robustness checks).

Overall, we find no evidence of systematic adverse effects of the 1918 influenza pandemic on human capital accumulation or employment for cohorts exposed to the pandemic in utero. Rather, our analysis suggests that for the vast majority of the considered countries there were no significant effects at the population level.² Among those countries for which there are statistically significant effects, we more frequently find beneficial than adverse effects, casting further doubt on the existence of a systematic adverse effect on economic outcomes.

Much of our analysis is concerned with verifying the robustness of these findings. In particular, a potential concern with our analysis is that the absence of statistically significant results may be partially due to measurement error stemming from imprecise information in some countries about the time of birth and timing of the pandemic. We carefully address this concern in a series of robustness checks. Most importantly, we show that our key findings also apply when restricting the sample to those censuses for which the expected degree of error is weakly smaller than the one in the analysis of the United States performed in Almond (2006). In addition, we show that our results are robust to different choices in constructing the comparison group, to different assumptions about the exact timing of the pandemic in each country, and to controlling for potentially confounding effects of WWI and of the less lethal first and third waves of the pandemic.

Our findings may at first appear surprising, given that there is now a well-established body of evidence on the fetal origins hypothesis, including evidence on economic outcomes obtained from natural experiments other than the Spanish Flu, such as in-utero effects of adverse weather conditions (Maccini and Yang 2009), natural disasters (Caruso and Miller 2015), maternal malaria (Barreca 2010), and Ramadan observance during pregnancy (Almond and Mazumder 2011). On the other hand, a recent re-evaluation of the 1918 influenza pandemic in the United States by Beach et al. (2022) shows that using a refined approach that controls for selection effects arising from the coincidence of the pandemic and World War I (WWI) conscription causes the effects identified in Almond (2006) to become statistically

² This finding does not rule out the possibility that in-utero exposure to influenza may have had meaningful effects at the individual level.

insignificant.³ While our study aims at contributing in a different direction – reviewing the external validity of past studies rather than their internal validity, – the new insights provided by Beach et al. (2022) may be seen as making our findings appear less puzzling. Moreover, as also noted by these authors, there exists a substantial body of evidence suggesting that early life disadvantages do not automatically and necessarily lead to worse adult economic outcomes (see also Heckman 2006), providing a possible explanation for the absence of systematic long-term adverse effects for individuals who were exposed to health shocks in utero.

The rest of the paper is structured as follows. The next section provides a brief historical background of the pandemic. Section 3 describes our data and identification strategy. Sections 4 present our main findings, including a replication of some of the key results from Almond (2006) using our dataset. Section 5 discusses the robustness and limitations of our results. Section 6 concludes.

2 Historical Background: The 1918 Influenza Pandemic

The 1918 influenza pandemic is commonly thought of as having occurred in three waves, the first wave being a precursor to the deadly second wave and receiving only minor public attention in 1918 (Patterson and Pyle 1991).⁴ It is usually assumed that the virus of the first wave mutated leading to a much more virulent and deadly virus of the second wave, terming it ‘the pandemic’ (Killingray and Phillips 2003). The third wave is usually described as a mild aftermath of the second wave or as “episodic and scattered winter outbreaks” (Patterson and Pyle 1991, p.4) commonly observed after epidemics and without larger impact on mortality trends. A striking characteristic of the 1918 influenza pandemic is the unusually high mortality rate among young adults observed in many countries (Johnson and Mueller 2002).

The literature typically cites Brest in France in August 1918 as the most likely point of origin of the mutated virus, at the time a major port of entry for American troops joining the war in Europe. From there, ships and trains carrying troops and cargo spread the virus around the globe within months. The British ship 'HMS Mantua' arriving in Freetown, Sierra Leone, on August 15, 1918, with 200 sick sailors brought influenza to West Africa. At the end of

³ When examining the effects of in-utero influenza exposure on socioeconomic status (SES), these authors show that the adult SES deficit is reduced when characteristics such as race, birthplace, and parents’ birth countries are controlled for, and becomes statistically insignificant when household fixed effects are included. One of their conclusions is that, “*Replicating Almond’s state-level dose-response analysis, we find no evidence in census data that influenza exposure reduced adult SES.*” (Beach et al. 2022, p. 1964).

⁴ Some authors also describe selected outbreaks occurring in 1920 based on calculations of excess mortality (Johnson and Mueller 2002, Chowell et al. 2010, Chowell et al. 2011, Ansart et al. 2009). Johnson and Mueller (2002) themselves, however, suggest that these outbreaks might have been a single, unrelated epidemic caused by a different strain of the virus.

September 1918, 3 percent of the population of Sierra Leone are estimated to have died from influenza. From Freetown, the virus spread south along the coast and into the continent. Two other ships carrying soldiers back from France brought the disease to Cape Town and influenza quickly spread into southern and central Africa (Killingray and Phillips 2003). Simultaneously, an increased number of deaths from influenza was observed in Boston, USA, where the pandemic spread across the country within two months from east to west (Killingray and Phillips 2003). From Brest in France, influenza spread north, south, and east, infecting all of Europe within weeks, including remoter regions such as Iceland. In mid-October 1918, the pandemic peaked in Europe and reached as far east as Russia and Hungary. Via ships as well as the Trans-Siberian railroad, influenza transmitted into Asia. Latin America and Africa were primarily infected through major sea ports. By January 1919, the pandemic had circled the globe and reached all but a few remote regions that escaped the pandemic through rigorous maritime quarantines such as northern and eastern Iceland, American Samoa, and St. Helena (Patterson and Pyle 1991, Killingray and Phillips 2003).

Table A1 in Online Appendix A provides an overview of the starting dates of the second wave in each country as found in the literature. The table draws extensively from Patterson and Pyle (1991) and a number of additional sources describing the gradual expansion of the pandemic. We do not attempt to report end dates given conceptual problems associated with defining the end of the second wave (historical sources, in general, do not provide information on when exactly the last patient in a country was infected). With that said, reconstructed mortality patterns suggest that nation-wide deaths typically started to increase several weeks after the first reported cases and (nearly) decreased to pre-pandemic levels within three to four months from the initial increase.

While the accurate death toll of the Spanish Flu remains unknown, multiple sources (Killingray and Phillips 2003, Johnson and Mueller 2002, Patterson and Pyle 1991) agree that mortality rates were highest in Africa and Asia,⁵ with India suffering one of the highest influenza-specific mortality rates of up to 6.7 percent.⁶ Fiji, Botswana, and Ghana encountered death rates in the vicinity of 5 percent, Tonga of 10 percent, and Western Samoa even 25 percent.⁷ Markedly higher mortality rates are reported for indigenous populations such as the Maori in New Zealand, the Aborigines in Australia, the Inuit in Canada, and Native Americans

⁵ While for China there is little evidence, Killingray and Phillips (2003) quote a source suggesting that the Chinese mortality rate was about 1 percent.

⁶ The Indian population was likely weakened by food shortages due to rationing and large exports by the British as well as by malaria (Killingray and Phillips 2003).

⁷ In contrast, US-controlled Eastern Samoa escaped influenza through a maritime quarantine (Killingray and Phillips 2003).

in the United States.⁸ On the other hand, Northern America, Europe, and Australia experienced much lower mortality rates of about 0.5 percent. More specifically, Patterson and Pyle (1991) deduce 1.9 – 2.3 million deaths in Africa (14.2 – 17.7 per thousand), 19 – 33 million deaths in Asia (19.7 – 34.2 per thousand), 2.3 million in Europe (4.8 per thousand) and 766,000 – 966,000 deaths in Latin America (8.4 – 10.6 per thousand). The United States is estimated to have had 550,000 - 675,000 deaths (Crosby 2003, Killingray and Phillips 2003). In Canada, influenza spread from one coast to the other within a month, with one in six Canadians contracting the disease and 30,000 - 50,000 dying (Herring and Sattenspiel 2003).

Overall, Patterson and Pyle (1991) estimate global mortality at 30 million or a rate of 16.6 per thousand worldwide. Other figures from previous studies cited by Patterson and Pyle (1991) vary between 15 – 100 million deaths and rates between 8.3 – 55.2 per thousand, though the extent or completeness of these previous studies is unclear. Killingray and Phillips (2003) agree with 30 million deaths but caution that this is only a rough estimate given the lack of data for larger areas and populations. Johnson and Mueller (2002) estimate 50 million deaths but admit that this might be “as much as 100 percent understated” (Johnson and Mueller 2002, p.115). Despite the limited accuracy of these estimates, it is clear that even the lower bounds of these estimates suggest that the Spanish Flu had a relevant death toll across all regions of the world, motivating a global analysis.

3 Data & Identification Strategy

Following Almond (2006), exposure to influenza should specifically affect those in-utero in 1918 during the second deadly wave, such that the cohort born in 1919 harbors the majority of the prenatally exposed. Our identification strategy therefore focuses on measuring how key post-influenza outcomes (primary education, secondary education, and employment status at time of census) for the 1919 birth cohort differ from the trend of surrounding cohorts that were not affected in-utero by the influenza pandemic.

To this end, we downloaded 107 census datasets from 51 countries collected between 1960 and 1990 provided by IPUMS International (Minnesota Population Center 2014).⁹ 1960 is the first year for which IPUMS International provides census data and coincides with the year of the earliest census sample used in Almond (2006). In order to limit the possible bias arising

⁸ Among the Canadian Inuit, influenza death rates were so high that entire villages ceased to exist (Johnson 2003).

⁹ The datasets were downloaded on August 31st, 2022. With the exception of India (for which IPUMS International provides survey data), downloaded datasets for all countries were census datasets. For simplicity, we therefore use the term census data for our sample in the remainder of this article.

from mortality-related attrition, our main analysis focuses on the first available census in each country and we do not consider censuses collected after 1990.¹⁰

IPUMS International provides harmonized educational attainment as a categorical variable consisting of ‘less than primary education completed’, ‘primary education completed’, ‘secondary education completed’ and ‘higher education completed’ based on the United Nations definition of six years of primary schooling and six years of secondary schooling (Minnesota Population Center 2014).¹¹ This categorical variable is one of the most widely available measures among IPUMS International censuses and was thus chosen to generate binary indicators for completed primary and secondary education, respectively.¹² Similarly, we generate a binary indicator for employment status that equals one if a respondent is employed at the time of census enumeration, and zero for those inactive or unemployed. In addition, two measures of disability status are considered as additional outcomes. First, we create a binary measure for self-reported general disability. Second, we code a binary variable equaling one if a respondent reported being unable to work due to a disability (‘work disability’).¹³ Finally, we use several other outcome variables capturing respondents’ income and education when replicating some of the key findings from Almond (2006). As the disability measures and income-related variables are only available in a small subset of the considered censuses, our main analysis focuses on primary education, secondary education, and employment status.¹⁴

For most of the analysis, we report results for two different samples. The ‘full sample’ comprises the full set of 51 census samples from the 51 countries for which census datasets were downloaded. Our ‘core sample’ comprises a subset of 36 census samples from 36 countries for which the available information on the time of birth is at least as precise as the information for the United States in the dataset used by Almond (2006). Specifically, notice that in order to correctly identify individuals that were exposed in-utero to the 1918 influenza pandemic, information on the precise time of birth is crucial. For the United States, Almond (2006) uses

¹⁰ The definition of what constitutes the first census is outcome-specific, as not every outcome is available in every census. The choice of the 1990 cutoff is motivated by the desire to limit mortality-related attrition. When estimating effects on employment status, we further constrain the analysis to censuses collected until 1976 to rule out retirement-related attrition. In particular, this approach ensures that none of the individuals born in 1912 has reached an age of 65 years, a common retirement age in many countries.

¹¹ For example, a respondent with eleven years of schooling would be reported as having only a primary school degree as opposed to a nearly completed secondary degree. For some datasets, university completion pools those with university and technical degrees.

¹² We code these variables such that they capture whether at least the stated level of education was completed. For instance, all persons with completed secondary education are coded as having completed primary education, too.

¹³ This indicator represents a subcategory of the employment status as it gives a reason for why a respondent is inactive. Analogous to employment status, we limit the analysis to censuses collected until 1976 when discussing work disability in order to rule out retirement effects.

¹⁴ All outcome variables only have a very limited number of missing observations if they are included in a census. Table A2 in Online Appendix A provides an overview on missing data by census.

information on both age (reported in integers as of March 31 of the census year) and quarter of birth. This information allows for deriving the year of birth for each respondent. Almond (2006) then sets his treatment indicator (capturing in-utero exposure to the Spanish Flu) equal to one for all individuals born in 1919. Importantly, this approach is not free of measurement error (in the following called “misclassification error”). As described by Almond (2006, p. 673), “[...] the pandemic struck without warning in October 1918 and had largely dissipated by the beginning of 1919 [...]” Accordingly, the United States faced a high-intensity exposure period of approximately three months during the last quarter of 1918. Despite this fact, Almond’s approach codes individuals born in the last quarter of 1919 as having been exposed in utero, although they were conceived during the first quarter of 1919 when the second wave had already largely receded.¹⁵ Similarly, those born in October to December 1918 were exposed in utero but are not coded as such.¹⁶ This results in a misclassification of six birth months.¹⁷

Our ‘core sample’ is constructed so that it only comprises those censuses for which the implied degree of misclassification is weakly smaller than that of Almond’s approach for the United States. While Online Appendix B provides a more detailed description of our method, we outline the main rationale here. First, it is worth noting that due to the way viruses spread within populations, nation-wide mortality rates typically do not rise immediately after the first case occurs within a country. For instance, Patterson and Pyle (1991) report that the Spanish Flu had already arrived in the United States by early September – approximately one month before national mortality rates started rising, as described by Almond (2006). We therefore add a delay of one month to the start dates shown in Table A1 (see Online Appendix A) before we calculate the degree of misclassification in any of the censuses. Second, we assume a duration of three months before mortality rates fall to (near) pre-pandemic levels, given the previously discussed mortality patterns.¹⁸ Third, while the United States are the only country with a variable for quarter of birth in their census sample, for all other countries, we can either approximate the birth year by subtracting a respondent’s age in years from the census year or

¹⁵ Given that pregnancy duration varies naturally around approximately nine months (counting from time of conception), it is likely that at least some respondents who were born in September 1919 were not conceived during the fall of 1918. Nevertheless, for simplicity reasons (and thus counting in Almond’s favor), we assume that those born in September 1919 were correctly classified as exposed in-utero. We show below that slightly shifting/extending the exposure period has no effect on our conclusions.

¹⁶ More precisely, the cohort born in October 1918 was partially exposed in-utero and partially during the neonatal period. It is beyond the scope of this paper and the capabilities of its authors to provide a medical discussion on how consequential this distinction is. Instead, we assume for the main analysis that those born in the first month of the exposure period were exposed in-utero, and show in a robustness check that conclusions stay the same if we instead assume that they were not exposed in-utero.

¹⁷ It is debatable whether January 1919 should be counted as part of the exposure period given that mortality rates were much lower than those of fall 1918 but not yet fully back to pre-pandemic levels. As we show in a series of robustness checks, the conclusions of this article do not depend on such nuances.

¹⁸ We check the robustness to deviations from these assumptions in sensitivity analyses reported further below.

use the exact birth year if reported. Although it may seem that approximating birth years with information on age and census year is only a second-best solution, it is important to note that the misclassification error does not depend on how many respondents are allocated to the wrong birth year but rather on the misclassification of respondents as exposed in-utero vs. not in-utero. As we demonstrate in Online Appendix B, using age and census year can result in more precise classifications than using exact birth years, depending on the timing of the pandemic and the timing of census enumeration. Our strategy is therefore to use the more precise alternative if a census reports both age and exact birth year.¹⁹ If both alternatives come with the same misclassification error, we use the specification based on exact birth year. Finally, for each census (given the enumeration date and reported local start of the second wave and associated exposure period), we count the number of misclassified birth months as was described above for the United States. For each country, we include the first census that reaches a misclassification error of six months or less.

In addition, it is possible that both the original birth year information and our constructed birth year proxy (census year minus age) are subject to measurement error stemming from poor reporting or data collection methods. To address this issue, we make use of Myers' Blended Index of Digit Preference (Hobbs 2004) to systematically check for evidence of heaping in our birth year variable in all datasets. As described in Hobbs (2004), the index increases with the extent of heaping and ranges from zero (no digit preference) to 90 (complete preference for single digit). Hence, for our analysis, small values of Myers' Index close to zero are preferred.²⁰ Moreover, we limit the analysis to native-born respondents (if nativity status is reported) to increase the probability that respondents were indeed exposed to the pandemic during the lethal second wave in each country.

Table 1 provides summary statistics for the datasets used in the main analysis. Of the 51 censuses in the full sample, 11 stem from Africa, 11 from Asia, 10 from Europe, 17 from Latin America, and 2 from Northern America.²¹ Overall, the average percentage of population covered by the censuses is 6.5% (6.4% across the 36 censuses included in the core sample). Data on secondary education are available for all 51 censuses. Data on primary education are

¹⁹ Age in years is included in all censuses (though sometimes derived from exact birth dates). However, we do not use the variable age for the census France 1962 and all Greek censuses because age was only estimated in these datasets. Table A2 in Online Appendix A shows which census datasets also report the birth year.

²⁰ We calculate Myers' Index using the Stata command "myers". We limit the calculation to the birth year range 1910 to 1919 as the specified range must span a multiple of 10.

²¹ Although we limit attention to the first census per country, in rare cases more than one dataset per country is used across our analyses, because not every outcome is available in each census. For example, the first Mexican census (conducted in 1960) does not report employment data, such that the first Mexican census for the employment outcome was collected in 1970 (please see Table A2 in Online Appendix A for details on census-specific data availability).

unavailable for Austria, St. Lucia and Switzerland due to peculiarities of enumeration methods and local education systems that prevented the creation of harmonized indicators for primary education.²² Employment data are only available for 26 countries, in part because we restrict the employment sample to only those censuses collected before 1977 to minimize bias from retirement as explained above. As reported at the bottom of Table 1, values for Myers' Index are overall rather low, with a mean of 14.2 (12.0 in the core sample).

(Table 1 here)

Our analysis follows a two-step procedure. First, as in Almond (2006), we estimate (separately for each country) the long-term effects of in-utero exposure to the Spanish Flu by calculating deviations of the set of binary dependent variables from the respective squared cohort trend. Each outcome is regressed on a constant, an indicator for being born in 1919 capturing in-utero exposure to the influenza pandemic, and the squared cohort trend. Formally, our regression model for each country can be written as

$$y_i = \beta_0 + \beta_1 * YOB_{i,1919} + \beta_2 * YOB_i + \beta_3 * YOB_i^2 + \varepsilon_i, \quad (\text{Equation 1})$$

where y_i is one of the three binary dependent variables described above, YOB_i is the (estimated) year of birth, and $YOB_{i,1919}$ is the binary exposure indicator which equals 1 for individuals born in 1919, and 0 otherwise. As in Almond (2006), we focus on cohorts born between 1912 and 1922 as our baseline specification and estimate Equation (1) separately for men and women.²³ All specifications are estimated using Ordinary Least Squares with heteroskedasticity-robust standard errors.

The coefficient of interest is β_1 . It should be noted that this type of analysis constitutes an intent-to-treat approach as we lack individual data on actual influenza exposure. Nevertheless, given the findings of previous studies, one would expect the 1919 birth cohort to be worse off, on average, compared to the general trend. Specifically, therefore, β_1 would be expected to be negative when the dependent variable measures educational attainment or employment status, but positive when the dependent variable captures disability. In total, we obtain k coefficients of interest β_1 and corresponding standard errors, with k being the number of countries (censuses) included in that specification.

²² This is also true for the 1971 East Germany census. However, we are able to use the 1981 census instead.

²³ We also show that our main results are robust to estimating a more general trend for cohorts born between 1910 and 1928, which centers the influenza cohort in the middle of the interval and gives equal weight to outcomes of those born before and after 1919 (see Section 5).

In the second step of the analysis, we use these k estimated coefficients and standard errors to perform random-effects meta-analyses allowing us to describe overall patterns across outcomes and world regions.²⁴ We thus mimic a scenario where each of our k coefficients stems from a unique study of in-utero impacts of the Spanish Flu, and we collectively analyze these to obtain a combined average effect assuming that our coefficients represent a random sample of all possible study effects. Notably, this approach allows for between-study heterogeneity and thus accounts for the fact that the impact of the Spanish Flu might differ across countries due to unobservable characteristics. With this approach, each of our estimates is weighted by the inverse of its total variance comprising the sum of the estimation variance within each study (i.e., the sampling error of each census-specific estimate) and the systematic variance between studies (Lipsey and Wilson 2001). The weighted average of the k coefficients yields the so-called average weighted coefficient for a given dependent variable. To facilitate a better understanding of the effect size implied by an average weighted coefficient, we use the same weights to also calculate and report the average of the mean of the respective dependent variable across censuses.

4 Results

We start by replicating some of the key results from Almond (2006). Both our analysis and the results presented by Almond (2006) rely on IPUMS census data, although there are small differences in terms of sample sizes for 1960 and 1980 (see Table 2) due to additional sample refinements imposed by Almond (2006). Larger sample size differences exist for 1970 as IPUMS International only provides a one percent sample, whereas Almond (2006) could rely on a three percent sample. Table 2 reports both Almond’s original estimates (in odd-numbered columns) and our estimates (even-numbered columns) for the three U.S. censuses in 1960, 1970, and 1980 considered in Almond (2006). Except for a few instances, the replication appears to work well across years and considered variables.²⁵

(Table 2 here)

²⁴ Random-effects meta-analyses are conducted with the Stata command ‘metareg’, using the method of moments to estimate between-census variance.

²⁵ As mentioned in Section 3, most of these variables are only available in a small subset of censuses, so that we only use them in the case of the U.S. for the purpose of replication, but not in our main analysis. The outcome “high school graduate” is however equivalent to “completed secondary education” used in our main analysis.

Table 3 reports our main results when expanding the analysis to the global level. For each of the three outcomes, the table reports average weighted coefficients by region both for the full sample and for our core sample (recall that the core sample comprises only those countries for which the degree of measurement error due to imprecise information on the time of birth is at most as large as in Almond 2006). Panel A in Table 3 reports the results for the probability for men to obtain primary education. For the full sample, the all-regions average weighted coefficient (based on a total of 48 census-specific estimates) is small and positive, but statistically insignificant at conventional levels. Importantly, only 7 of the 48 census-specific regressions yield negative and statistically significant coefficients at the 10% level, while 15 estimates are positive and significant. These results appear inconsistent with the existence of a systematic negative effect of in-utero influenza exposure on primary education. This interpretation is further supported by the fact that, within regions, none of the average weighted coefficients is statistically significant. To increase confidence that these findings are not driven by measurement-related attenuation bias, Panel A+ in Table 3 reports the results when we limit the analysis to those censuses fulfilling the requirements for our core sample. Again, we do not find evidence for adverse in utero effects, with the majority of country-level estimates remaining statistically insignificant.²⁶

(Table 3 here)

Next, we turn to the effect of the 1918 influenza pandemic on secondary education among men, as reported in Panels B and B+ in Table 3. Again, we find no evidence of a systematic negative effect. In particular, the all-regions average weighted coefficient is statistically insignificant in both the full sample and the core sample, and only 7 of 51 country-level estimates in the full sample (6 of 36 estimates in the core sample) are negative and statistically significant at the 10% level. Similarly, none of the world regions features a statistically significant average weighted coefficient.

Finally, Panels C and C+ in Table 3 report the results for male employment status at the time of census collection. Once more, any evidence of a systematic adverse impact of the 1918 influenza pandemic seems lacking. The only negative result that is statistically significant at the 10% level is the average weighted coefficient for Latin America in the full sample. However, the effect size (-0.2 percentage points) is very small, given that, on average, male employment

²⁶ As shown in Table C1 in Online Appendix C, sample sizes in the census datasets are typically large while standard errors for effect estimates are small. Power limitations are therefore unlikely to drive these findings.

rates were 91% in Latin America. Moreover, the negative coefficient for Latin America seems to be driven by a single country, providing no evidence of a systematic adverse effect.

Figure 1 illustrates our main findings by showing estimated effect sizes and confidence intervals for the countries in our core sample. Most of the depicted estimates are not statistically significant (indicated by the gray shading), and among the significant estimates (shaded in black) positive coefficients are more frequent than negative coefficients. Consequently, the country-level findings clearly do not support the existence of systematic long-term adverse effects of in-utero exposure to the pandemic on economic outcomes across countries.

(Figure 1 here)

It is possible that these results may not apply to women, as the underlying mechanisms and incentives may differ. For instance, average female employment rates in the full sample tend to be substantially lower than those of men (28% vs. 89%) and are likely influenced by family planning considerations. We therefore re-estimate all models using female rather than male respondents. The results are reported in Table C2 in Online Appendix C (country-level estimates for the core sample are reported in Figure C1). Despite the potential differences in underlying mechanisms, the results for men are largely confirmed by the analysis for women. In particular, the all-regions average weighted coefficients are never statistically significant and, among the region-specific estimates, only Africa features significant results for primary education but with absolute effect sizes close to zero.

Finally, we consider the possibility that, while there is no evidence of systematic adverse effects on economic outcomes, the 1918 influenza pandemic may have nevertheless exhibited long-term health impacts. To this end, we re-estimate our main analysis using general disability and work disability as alternative outcomes. The results are reported in Table C3 in Online Appendix C. Note that these results should be interpreted with caution as data on disability are only available for up to 14 countries. With this caveat in mind, we do not observe any statistically significant increases in disability rates. In fact, some of the weighted average coefficients are significantly negative (suggesting a reduction in disability rates).

Overall, our results therefore appear to be difficult to reconcile with the widely-held view that the 1918 influenza pandemic exhibited important long-term adverse effects on cohorts who were exposed to the pandemic in utero. While there are a few census datasets for individual countries for which we find significant differences between the influenza cohort and surrounding cohorts, the vast majority of our results are insignificant and, among the significant

estimates, the beneficial effects outnumber adverse effects. Moreover, average weighted coefficients within and across regions tend to be small in magnitude and provide no indication of economically meaningful impacts.

In what follows, we discuss a series of robustness checks to corroborate this finding and to address potential identification concerns.

5 Robustness and Limitations

Table 4 reports the results of several robustness checks. The table is organized in a similar way as our main results in Table 3, except that Table 4 only reports the all-region average weighted coefficients and the columns correspond to different specifications (i.e., robustness checks) rather than region-specific estimates.

(Table 4 here)

Column (1) in Table 4 reports the results when the considered time window of birth cohorts is extended from our baseline period 1912-1922 (also studied in Almond 2006) to the longer period 1910-1928, which centers the influenza cohort in the middle of the interval and gives equal weight to outcomes of those born before and after 1919. Similar to the results presented in Table 3, the all-region average weighted coefficients for education are always statistically insignificant (across both the full sample and the core sample). The main qualitative difference to our baseline results is that the negative coefficient for employment is now statistically significant (at the 5% level in the full sample and 10% level in the core sample). However, the effect size continues to be very small, corresponding to a reduction of 0.4 percentage points at a mean employment rate of 90%. Moreover, the overwhelming majority of country-specific estimates do not reach statistical significance (i.e., there are only four countries with significantly negative coefficients for employment), making the existence of a systematic adverse effect rather unlikely.

Column (2) in Table 4 reports the results when the considered period of birth cohorts is restricted to 1912-1919. The motivation behind this specification is that our baseline period includes WWI as a major disruptive event. In particular, declines in fertility during the war period (compared to pre-war levels) and a subsequent increase in fertility rates were observable in many European countries (Vandenbroucke 2014). This fertility response implies that post-WWI cohorts may differ systematically from those born during the war. We therefore explore the robustness of our main results to the exclusion of cohorts born after 1919 (i.e., those that

were not in utero during WWI). As shown by the results in column (2) of Table 4, the results remain very similar to our baseline estimates and show no statistically significant effects of the Spanish Flu on education and employment outcomes.

A different view on the role of WWI is that those born before 1919 are war survivor cohorts and are therefore potentially an imperfect control group, either because they too were exposed to various forms of hardship in-utero (e.g. famine), biasing our results towards zero as the control group cannot be considered ‘untreated’, or because those with poor health did not survive, making the pre-war cohorts unusually healthy (positive selection).²⁷ To guard against this type of bias, we next exclude all European countries due to the overall strong impact of WWI and their high number of casualties. The results are reported in column (3) in Table 4 and largely confirm our baseline findings, as we do not find statistically significant adverse effects on any of the outcomes. Moreover, in column (4) in Table 4, we exclude all countries that are classified as belligerent and focus on non-belligerent (i.e. ‘neutral’) countries only.²⁸ While this causes a substantial reduction in sample size, the obtained results lend support to our previous findings. In particular, effect estimates are statistically insignificant in all regressions and effect sizes are either positive or virtually zero. We thus conclude that, despite the theoretical challenges posed by WWI for our identification strategy, it is unlikely that WWI drives our results.

Another possible concern is that our findings may be driven by attenuation bias stemming from measurement error in the reported time of birth. In part, our main analysis already alleviated this concern by showing that our key findings are largely persistent when focusing on datasets for which the implied degree of misclassification due to imprecise information on exposure status is limited and at most as high as in Almond (2006). As described earlier, it is further possible to evaluate the robustness of results with regards to age heaping using Myers’ Blended Index of Digit Preference. We therefore test the robustness of our findings to the exclusion of censuses with relatively high index values. Specifically, columns (5) and (6) in Table 4 report the results when focusing on censuses with index values below 10 and 20, respectively. In both cases, there are no statistically significant average weighted coefficients for any outcome variable, and numbers of individual countries with significantly negative results remain very small. This strongly suggests that our findings are not driven by measurement error in birth timing stemming from poor reporting or data collection methods.

²⁷ Given the null findings of our study, the former effect would have to outweigh the latter in order to explain our results.

²⁸ Belligerent countries are all those that officially declared their participation in WWI. Following Kruijinga (2014), non-belligerent (neutral) countries in our sample are: Argentina, Chile, Mexico, Spain and Switzerland.

While the literature generally identifies the second wave of the Spanish Flu pandemic as the lethal one, most countries experienced an earlier first and/or later third wave as well. These waves are described as much less lethal and comparable to seasonal Flu outbreaks without larger impact on mortality trends. To check whether our main results are biased by comparing our treatment cohort to a trend incorporating (potentially) exposed cohorts, we re-estimate our analysis when including two dummies, one for being born in 1918 and one for being born in 1920. Thus, in this robustness check we explicitly control for the two cohorts surrounding the 1919 influenza cohort. The results are reported in column (7) in Table 4. The results for primary and secondary education continue to show no statistically significant estimates for the all-region average weighted coefficients. The only qualitative difference to our baseline results is that the negative coefficient for employment is now statistically significant at the 10% level in the full sample. However, the magnitude of the effect remains very small (corresponding to a decrease of 0.5 percentage points in employment at a mean rate of 90%) and becomes statistically insignificant when focusing on our core sample (see column (7) of Panel C+ in Table 4). Moreover, very few of the country-specific estimates (5 of 26 in the full sample and 3 of 20 in the core sample) are negative and statistically significant, casting further doubt on the existence of a systematic global adverse effect.

Finally, one may be concerned that the high number of insignificant estimates is driven by sample size limitations. To address this issue, we re-run our main analysis using all available census years rather than only the first census year per country (we still exclude census years after 1990 for primary and secondary education and those after 1976 for employment, for the reasons stated earlier). The results are reported in Table 5, which is organized in the same way as Table 3.²⁹ While several region-specific average weighted coefficients that were statistically insignificant in Table 3 are now significant, many of them are *positive* with small effect sizes.³⁰ Moreover, the all-regions average weighted coefficients remain small and similar to our main estimates from Table 3. The only outcome for which the all-region estimate is negative and statistically significant is employment. However, similar to before, the effect size is very small and only few of the individual census-specific estimates are significantly negative at the 10% level. Specifically, for the full sample there are only 16 out of 100 such estimates for primary education, 17 out of 107 estimates for secondary education, and 6 out of 38 estimates for

²⁹ Individual census-specific results are reported in Table C1 in Online Appendix C.

³⁰ Note that Table 5 reports consistent negative effects on education for Northern America which are driven by the US estimates, in line with the findings in Almond (2006). These results only transpire to the meta-analysis level when all available census samples of Northern America collected between 1960 and 1990 are used (last column of Table 5), but not when only the earliest census is used (last column of Table 3).

employment. Sample size considerations are thus unlikely to be a major concern and key results are robust to the inclusion of additional census years.

(Table 5 here)

The results of a number of additional robustness checks are provided in Online Appendix C. In Table C4, we verify the robustness of our main results to moderate changes in the assumed timing of the 1918 influenza pandemic across countries. Specifically, recall that the selection of our core sample is based on the implied misclassification error assuming that the exposure period started one month after the pandemic reached a country and lasted for 3 months. The insignificant results in Table C4 show that it does not matter for our key findings whether we instead assume that the exposure period started in the same month or two months after the first case. Similarly, results remain insignificant in case the exposure period lasted four months rather than three (regardless of whether we assume that the exposure period started immediately with the first case or one month after).

In addition, the results in Table C5 show that our results are largely robust to changes in the error tolerance threshold underlying the selection of our core sample. Specifically, the lack of significantly negative average weighted coefficients for education in this table indicates that our key findings remain intact when the permitted maximum number of wrongly classified months is reduced from 6 months (corresponding to the implied error in Almond 2006 and in our baseline specification) to any smaller integer, including 0. Effects on employment are only weakly significant in two out of six specifications and remain very small in size. Together, the results in Tables C4 and C5 show that our main findings are overall not sensitive to the specific assumptions underlying the construction of the core sample, which greatly increases our confidence that the findings are not driven by these assumptions.

Finally, we test how the results for our core sample change if we assume that respondents born in the first month of the exposure period were not exposed in-utero. For this purpose, we repeat the exercise from Table C5 using this alternative assumption (see Table C6). While in this scenario there are no censuses with zero misclassification error, we find no evidence for statistically significant negative average weighted coefficients across the various misclassification tolerance thresholds.

In summary, the robustness checks confirm our main finding of a lack of systematic adverse effects of in-utero exposure to the 1918 influenza pandemic on a global level. We therefore conclude that it is unlikely that the Spanish Flu exhibited important long-term adverse

effects on human capital accumulation and employment propensities for cohorts exposed to the influenza pandemic in utero.

6 Conclusion

We set out to study the long-term effects of the 1918 influenza pandemic on a global level, thereby complementing the insights from previous single-country studies. Overall, we were unable to detect a clear pattern regarding the effects of in-utero exposure to the deadly second wave of the pandemic on a population level. A series of robustness checks show that this finding remains intact when varying the construction of the comparison group, adjusting the made assumptions about the exact timing of the pandemic in each country, and excluding countries likely to be highly affected by WWI. The latter point is important, given that the concurrence of the influenza pandemic and WWI has been a key point of criticism in past studies (Brown and Thomas 2011, Beach et al. 2020). Moreover, the fact that increasing the number of countries, years, and censuses beyond our preferred specification (core sample) does not alter the key findings strongly suggests that our results are not driven by sample size limitations.

Overall, therefore, we conclude that there is little empirical evidence to support the widely-held view that the 1918 influenza pandemic exhibited important long-term adverse effects on economic outcomes for cohorts exposed to the pandemic in utero. This also suggests that existing single-country studies reporting statistically significant adverse effects may have limited external validity and are unable to provide a comprehensive picture of the long-term impacts of the Spanish Flu.

Nevertheless, some limitations of this study may arise from unobserved heterogeneity. While the harmonization effort of IPUMS International made this research possible, the very nature of harmonization also introduces a certain measurement error. Despite largely comparable educational categories, some underlying country-specific educational standards remain. Differences in the employment and disability indicators are possible as well but less likely. Apart from the question of measurement, it may also be argued that overall education levels across countries are too heterogeneous, such that pooling advanced and less advanced countries may bias results towards zero. Such concerns are, however, unwarranted as our conclusions do not simply rely on the reported small and mostly insignificant average weighted coefficients. Rather, we find that the vast majority of census-specific estimates are statistically insignificant, which raises our confidence in the overall results.

A second possible limitation of this study arises from measurement error in the definition of exposed versus unexposed cohorts. As discussed above, the influenza literature is not always

unanimous in the definition of waves, their duration, and their timing. Hence, measurement error is possible and augmented by the fact that limited data availability forces us to rely on yearly rather than monthly birth data. While our results should therefore be interpreted with caution, it is worth noting that our methodology allows us to quantify the expected degree of misclassification for each census, with our core sample analysis limiting the attention to datasets with errors as low as (or better than) the censuses used by Almond (2006). Moreover, further restricting the error tolerance does not lead to differential results.

A possible reason for insignificant results could also be selective mortality. In Bombay, India, stillbirths are reported to have risen by 50 percent during the height of the influenza pandemic (Ramanna 2003, p.89). If fetuses in bad health were more likely to be stillborn, the resulting population of surviving fetuses would display a positive selection in terms of health and, therefore, negative effects would only surface if the effects of health shocks overcompensate those of the positive selection (Almond and Currie 2011). Similarly, disentangling the effects of in-utero shocks from those occurring during infancy might prove difficult. For example, Echeverri (2003) states that influenza also increased the death rate among post-partum women in Spain due to puerperal septicaemia which could mean that infants that survived fetal exposure to influenza could still be affected by influenza albeit in an indirect manner. In this case, influenza would not operate through the channel proposed by the fetal origins hypothesis but through economic deprivation during childhood. That said, as discussed in detail in Section 2, mortality rates, while subject to considerable uncertainty, were likely to be low in most European and Northern American countries, and the large number of casualties was driven by high infection rates rather than low survival probability conditional on being infected. The absence of economically relevant adverse effects on our main outcomes is however highly robust across world regions. Although it is not possible to fully rule out selection effects using the data at hand, the overall homogeneity of findings raises confidence that selective mortality is not a major concern.

Despite these potential limitations, our study provides a fresh perspective on the fetal origins hypothesis. At the very least, the lack of a systematic adverse effect across countries documented in this study raises concerns about the widely-held view that health shocks such as the Spanish Flu are associated with important long-term adverse effects on economic outcomes of those exposed in utero.

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Table 1: Summary statistics

	Full sample	Core sample
Countries	51	36
Africa	11	7
Asia	11	8
Europe	10	6
Latin America	17	13
Northern America	2	2
Censuses reporting outcome^a		
Primary education	48	34
Secondary education	51	36
Employment	26	20
Census characteristics		
Mean coverage ^b	6.5%	6.4%
First year	1960	1960
Last year	1990	1990
Mean Myers' Index ^c	14.2	12.0

All reported statistics are calculated using only the first census per country (men only). ^a Number of censuses providing information. ^b Coverage is the percentage of a country's population included in the census sample of a given year (mean calculated across censuses). ^c Myers' Blended Index of Digit Preference (using original birth year information or census year minus age as birth year proxy depending on which of these alternatives has a smaller expected error; mean calculated across censuses).

Table 2: Replication of key results from Almond (2006)

Outcome	1960	1960	1970	1970	1980	1980
	(original)	(replicated)	(original)	(replicated)	(original)	(replicated)
	(1)	(2)	(3)	(4)	(5)	(6)
High school graduate	-0.021*** (0.005)	-0.021*** (0.005)	-0.020*** (0.003)	-0.015** (0.005)	-0.014*** (0.003)	-0.014*** (0.002)
Years of education	-0.150*** (0.038)	-0.155*** (0.037)	-0.176*** (0.023)	-0.133*** (0.038)	-0.117*** (0.019)	-0.119*** (0.018)
Total income	-573* (295)	-585* (285)	-1236*** (253)	-1124** (415)	-1065*** (191)	-475** (174)
Wage income	-812*** (261)	-700** (253)	-875*** (233)	-1207** (378)	-688*** (179)	-630*** (188)
Poor	0.010** (0.005)	0.010* (0.005)	0.009*** (0.002)	0.006 (0.004)	0.006*** (0.002)	0.005** (0.002)
N ¹	114,031	119,539	308,785	109,775	471,803	479,885

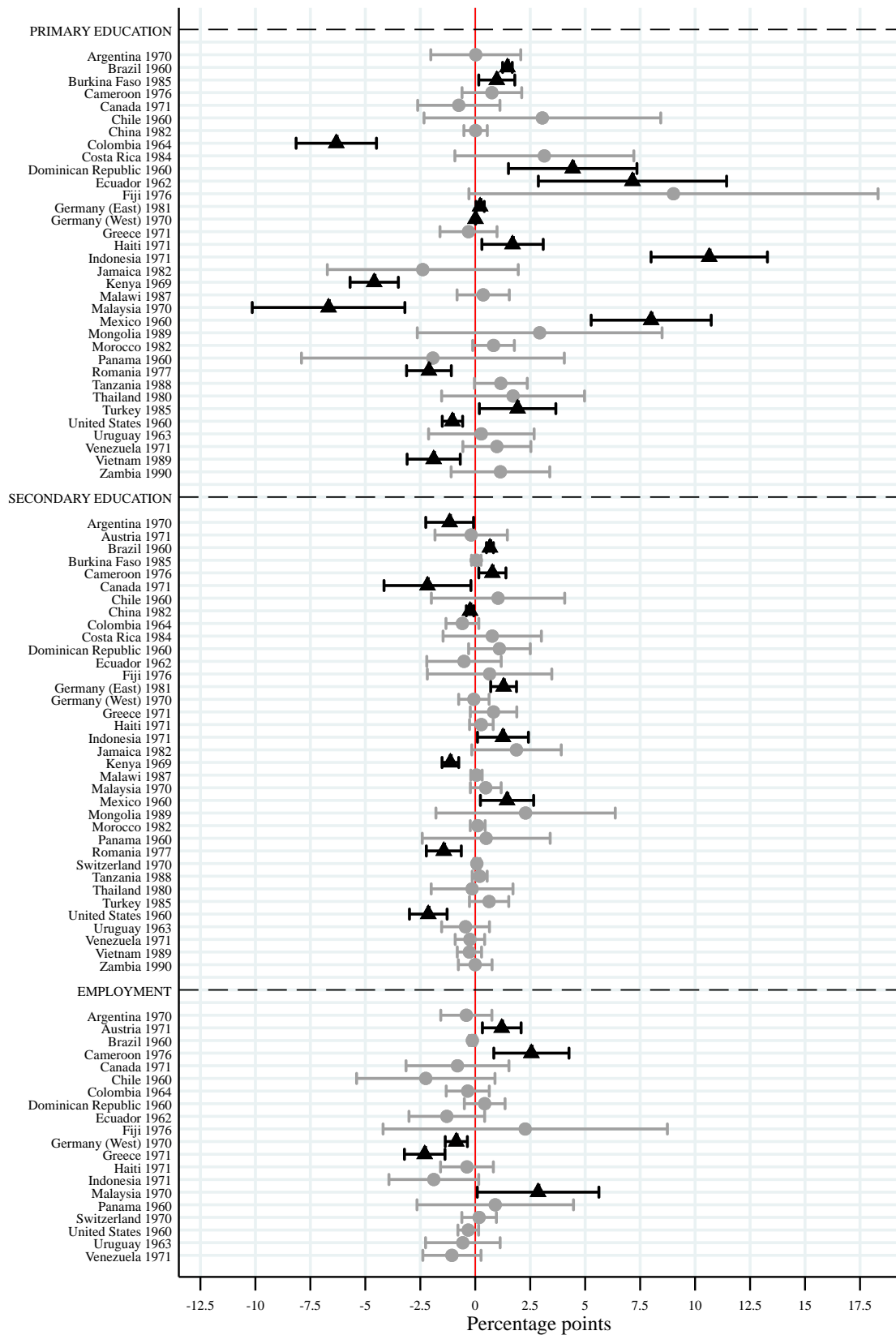
¹Number of observations refers to outcomes “high school graduate” and “years of education”. All results are for men only. We define “high school graduate” as anyone finishing at least grade 12. Years of education is top-coded at 18 years. Total income and wage income measure (wage) income for those receiving any (wage) income at all in 2005 USD. “Poor” is a binary equal to 1 if a respondent is below 150% of the poverty line.

Table 3: Weighted effects on education and employment (men)

	All regions	Africa	Asia	Europe	Latin America	Northern America
Primary education						
<i>Panel A: Full sample</i>						
Average weighted coefficient	0.006 (0.005)	-0.000 (0.006)	0.044 (0.024)	0.000 (0.002)	0.015 (0.009)	-0.010 (0.003)
Weighted average of mean of DV	0.31	0.05	0.29	0.62	0.27	0.91
Countries	48	11	11	8	16	2
Positive significant coefficients	15	1	5	2	7	0
Negative significant coefficients	7	2	2	1	1	1
<i>Panel A+: Core sample</i>						
Average weighted coefficient	0.002 (0.005)	0.001 (0.008)	0.015 (0.019)	-0.001 (0.003)	0.013 (0.011)	-0.010 (0.003)
Weighted average of mean of DV	0.33	0.06	0.29	0.77	0.27	0.91
Countries	34	7	8	4	13	2
Positive significant coefficients	10	1	2	2	5	0
Negative significant coefficients	6	1	2	1	1	1
Secondary education						
<i>Panel B: Full sample</i>						
Average weighted coefficient	0.001 (0.002)	-0.001 (0.002)	0.016 (0.011)	0.001 (0.002)	0.002 (0.002)	-0.021 (0.005)
Weighted average of mean of DV	0.11	0.01	0.05	0.33	0.04	0.30
Countries	51	11	11	10	17	2
Positive significant coefficients	9	2	4	1	2	0
Negative significant coefficients	7	2	1	1	1	2
<i>Panel B+: Core sample</i>						
Average weighted coefficient	0.000 (0.001)	-0.000 (0.002)	0.001 (0.002)	0.001 (0.004)	0.002 (0.002)	-0.021 (0.005)
Weighted average of mean of DV	0.12	0.01	0.03	0.49	0.04	0.30
Countries	36	7	8	6	13	2
Positive significant coefficients	5	1	1	1	2	0
Negative significant coefficients	6	1	1	1	1	2
Employment						
<i>Panel C: Full sample</i>						
Average weighted coefficient	-0.003 (0.002)	0.026** (0.010)	-0.001 (0.013)	-0.002 (0.006)	-0.002* (0.001)	-0.003 (0.003)
Weighted average of mean of DV	0.89	0.85	0.82	0.91	0.91	0.87
Countries	26	1	4	5	14	2
Positive significant coefficients	4	1	1	2	0	0
Negative significant coefficients	3	0	0	2	1	0
<i>Panel C+: Core sample</i>						
Average weighted coefficient	-0.003 (0.002)	0.026** (0.010)	0.006 (0.019)	-0.004 (0.007)	-0.002 (0.001)	-0.003 (0.003)
Weighted average of mean of DV	0.89	0.85	0.82	0.90	0.92	0.87
Countries	20	1	3	4	10	2
Positive significant coefficients	3	1	1	1	0	0
Negative significant coefficients	2	0	0	2	0	0

Average weighted coefficients are derived from a random-effects meta-analysis as described in Section 3. Stars indicate statistical significance (* = 10%, ** = 5%, *** = 1%). The weighted average of the mean of the dependent variable (DV) is calculated using the same weights as for the average weighted coefficients to ensure comparability. n/a = not available. Single country-level coefficients are counted as significant if corresponding p-values are smaller than 0.1.

Figure 1: Effects by country (men)



Note: Grey circles show insignificant estimates. Black triangles show estimates that are significant on a 10% level. Bars are 90% confidence intervals.

Table 4: Key robustness tests (men)

	1910 – 1928 (centered)	1912 – 1919 (no post- war)	Excluding Europe	Neutral only	Myers < 10	Myers < 20	Controlling for 1918 and 1920
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Primary education							
<i>Panel A: Full sample</i>							
Average weighted coefficient	0.005 (0.005)	0.008 (0.012)	0.013* (0.007)	0.027 (0.019)	-0.002 (0.002)	-0.001 (0.004)	0.003 (0.004)
Weighted avg. of mean of DV	0.33	0.31	0.25	0.38	0.47	0.36	0.31
Countries	48	48	40	4	22	37	48
Positive significant coefficients	13	8	13	1	3	8	12
Negative significant coefficients	7	6	6	0	3	7	8
<i>Panel A+: Core sample</i>							
Average weighted coefficient	0.001 (0.004)	-0.007 (0.006)	0.007 (0.006)	0.037 (0.029)	-0.003 (0.002)	-0.002 (0.004)	0.001 (0.005)
Weighted avg. of mean of DV	0.34	0.33	0.27	0.35	0.50	0.37	0.33
Countries	34	34	30	3	18	28	34
Positive significant coefficients	9	4	8	1	2	6	8
Negative significant coefficients	6	6	5	0	3	6	7
Secondary education							
<i>Panel B: Full sample</i>							
Average weighted coefficient	0.001 (0.002)	0.002 (0.004)	0.002 (0.002)	0.004 (0.004)	-0.001 (0.002)	-0.000 (0.001)	0.000 (0.001)
Weighted avg. of mean of DV	0.11	0.10	0.05	0.25	0.16	0.13	0.11
Countries	51	51	41	5	23	40	51
Positive significant coefficients	7	9	8	2	2	5	6
Negative significant coefficients	7	7	6	1	5	7	5
<i>Panel B+: Core sample</i>							
Average weighted coefficient	-0.000 (0.001)	-0.002 (0.002)	0.000 (0.001)	0.001 (0.005)	-0.002 (0.002)	-0.000 (0.002)	0.000 (0.001)
Weighted avg. of mean of DV	0.13	0.12	0.05	0.29	0.21	0.14	0.12
Countries	36	36	30	4	20	30	36
Positive significant coefficients	3	3	4	1	1	4	4
Negative significant coefficients	6	7	5	1	5	6	4
Employment							
<i>Panel C: Full sample</i>							
Average weighted coefficient	-0.004** (0.002)	0.003 (0.003)	-0.003 (0.002)	-0.001 (0.004)	-0.005 (0.003)	-0.002 (0.002)	-0.005* (0.003)
Weighted avg. of mean of DV	0.90	0.88	0.89	0.91	0.88	0.89	0.90
Countries	26	26	21	4	13	21	26
Positive significant coefficients	1	2	2	0	1	4	3
Negative significant coefficients	4	2	1	0	2	3	5
<i>Panel C+: Core sample</i>							
Average weighted coefficient	-0.004* (0.002)	-0.001 (0.003)	-0.002 (0.002)	-0.001 (0.004)	-0.005 (0.003)	-0.003 (0.003)	-0.004 (0.003)
Weighted avg. of mean of DV	0.90	0.88	0.89	0.92	0.88	0.89	0.89
Countries	20	20	16	3	12	17	20
Positive significant coefficients	1	1	2	0	1	3	2
Negative significant coefficients	4	2	0	0	2	2	3

Average weighted coefficients are derived from a random-effects meta-analysis as described in Section 3. Stars indicate statistical significance (* = 10%, ** = 5%, *** = 1%). The weighted average of the mean of the dependent variable (DV) is calculated using the same weights as for the average weighted coefficients to ensure comparability. n/a = not available. Single country-level coefficients are counted as significant if corresponding p-values are smaller than 0.1.

Table 5: Weighted effects using all available censuses (men)

	All regions	Africa	Asia	Europe	Latin America	Northern America
Primary education						
<i>Panel A: Full sample</i>						
Average weighted coefficient	0.007** (0.003)	-0.001 (0.005)	0.035** (0.014)	-0.002 (0.002)	0.014** (0.005)	-0.006** (0.002)
Weighted av. of mean of DV	0.34	0.05	0.27	0.65	0.29	0.93
Countries	48	11	11	8	16	2
Censuses	100	14	23	15	42	6
Pos. significant coefficients	31	2	10	2	17	0
Neg. significant coefficients	16	4	3	3	3	3
<i>Panel A+: Core sample</i>						
Average weighted coefficient	0.005 (0.004)	-0.003 (0.006)	0.020 (0.013)	-0.002 (0.003)	0.012** (0.006)	-0.007*** (0.001)
Weighted av. of mean of DV	0.33	0.06	0.27	0.76	0.26	0.92
Countries	34	7	8	4	13	2
Censuses	60	9	17	5	24	5
Pos. significant coefficients	18	1	5	2	10	0
Neg. significant coefficients	12	3	3	2	1	3
Secondary education						
<i>Panel B: Full sample</i>						
Average weighted coefficient	0.001 (0.001)	-0.001 (0.001)	0.011** (0.005)	0.001 (0.002)	0.002 (0.001)	-0.014*** (0.003)
Weighted av. of mean of DV	0.13	0.01	0.04	0.38	0.05	0.43
Countries	51	11	11	10	17	2
Censuses	107	14	23	21	43	6
Pos. significant coefficients	20	2	9	3	6	0
Neg. significant coefficients	17	2	2	3	4	6
<i>Panel B+: Core sample</i>						
Average weighted coefficient	-0.000 (0.001)	-0.000 (0.002)	0.004 (0.002)	-0.000 (0.002)	0.001 (0.001)	-0.016*** (0.002)
Weighted av. of mean of DV	0.14	0.00	0.03	0.56	0.04	0.40
Countries	36	7	8	6	13	2
Censuses	65	9	17	10	24	5
Pos. significant coefficients	10	1	4	1	4	0
Neg. significant coefficients	13	1	2	2	3	5
Employment						
<i>Panel C: Full sample</i>						
Average weighted coefficient	-0.005** (0.002)	0.026** (0.010)	-0.012 (0.017)	-0.008 (0.007)	-0.004*** (0.001)	-0.003 (0.002)
Weighted av. of mean of DV	0.87	0.85	0.82	0.89	0.88	0.88
Countries	26	1	4	5	14	2
Censuses	38	1	5	7	22	3
Pos. significant coefficients	4	1	1	2	0	0
Neg. significant coefficients	6	0	1	3	2	0
<i>Panel C+: Core sample</i>						
Average weighted coefficient	-0.004* (0.002)	0.026** (0.010)	-0.011 (0.023)	-0.004 (0.007)	-0.004** (0.002)	-0.003 (0.002)
Weighted av. of mean of DV	0.88	0.85	0.82	0.90	0.90	0.88
Countries	20	1	3	4	10	2
Censuses	24	1	4	4	12	3
Pos. significant coefficients	3	1	1	1	0	0
Neg. significant coefficients	4	0	1	2	1	0

Average weighted coefficients are derived from a random-effects meta-analysis as described in Section 3. Stars indicate statistical significance (* = 10%, ** = 5%, *** = 1%). The weighted average of the mean of the dependent variable (DV) is calculated using the same weights as for the average weighted coefficients to ensure comparability. n/a = not available. Single country-level coefficients are counted as significant if corresponding p-values are smaller than 0.1.

The long-term consequences of the global 1918 influenza pandemic: A systematic analysis of census data from 51 countries

Online Appendix

Appendix A: Descriptive Statistics

Table A1: Timing of second wave and implied misclassification error

Country	Census: month/year	Influenza start: month/year	Misclassification error (number of birth months)	Sources for Influenza dates
Argentina	09/1970	09/1918, 10/1918	0	Carbonetti (2010), Patterson & Pyle (1991)
Argentina	10/1980	09/1918, 10/1918	2	see above
Austria	05/1971	09/1918	6	Patterson & Pyle (1991)
Austria	05/1981	09/1918	6	see above
Bolivia	07/1976	11/1918	8	Patterson & Pyle (1991)
Brazil	09/1960	09/1918	0	Nelson (2010), Patterson & Pyle (1991)
Brazil	09/1970	09/1918	0	see above
Brazil	09/1980	09/1918	0	see above
Burkina F.	12/1985	10/1918	4	Patterson & Pyle (1983), Patterson & Pyle (1991)
Cameroon	04/1976	10/1918	4	Patterson & Pyle (1983)
Cameroon	04/1987	10/1918	12	see above
Canada	06/1971	09/1918	6	McGinnis (1977), Patterson & Pyle (1991)
Canada	06/1981	09/1918	6	see above
Chile	11/1960	10/1918	2	Chowell et al. (2014), Patterson & Pyle (1991)
Chile	04/1970	10/1918	12	see above
Chile	04/1982	10/1918	12	see above
China	07/1982	09/1918	4	Patterson & Pyle (1991)
China	07/1990	09/1918	4	see above
Colombia	07/1964	09/1918	4	Patterson & Pyle (1991)
Colombia	10/1973	09/1918	2	see above
Colombia	10/1985	09/1918	2	see above
Costa Rica	03-04/1963	09/1918	12	Patterson & Pyle (1991)
Costa Rica	05-06/1973	09/1918	8	see above
Costa Rica	06/1984	09/1918	6	see above
Dom. Rep.	08/1960	11/1918	6	Killingray (1994)
Dom. Rep.	01/1970	11/1918	20	see above
Dom. Rep.	12/1981	11/1918	2	see above
Ecuador	11/1962	11/1918, 12/1918	0	Patterson & Pyle (1991), Rodas Chavez (2015)
Ecuador	06/1974	11/1918, 12/1918	10	see above
Ecuador	11/1982	11/1918, 12/1918	0	see above
Ecuador	11/1990	11/1918, 12/1918	0	see above
Fiji	09/1976	11/1918	2	McLane (2013)
Fiji	08/1986	11/1918	2	see above
France	03/1962	08/1918	8	Patterson & Pyle (1991)
France	03/1968	08/1918	8	see above
France	02-03/1975	08/1918	8	see above
France	03-04/1982	08/1918	8	see above
France	03-04/1990	08/1918	8	see above
E. Germany ^a	01/1971	09/1918	16	Patterson & Pyle (1991)
E. Germany ^a	12/1981	09/1918	6	see above
W. Germany ^a	05-06/1970	09/1918	6	see above
Greece	03/1971	09/1918	6	Patterson & Pyle (1991)
Greece	04/1981	09/1918	6	see above
Guinea	02/1983	08/1918	12	Patterson & Pyle (1991)
Haiti ^b	08/1971	11/1918	6	Killingray (1994)
Haiti ^b	??/1982	11/1918	n/a	see above
Hungary	01/1970	10/1918	18	Patterson & Pyle (1991)
Hungary	01/1980	10/1918	18	see above
Hungary	01-02/1990	10/1918	18	see above
India ^c	01-12/1983	09/1918	16	Patterson & Pyle (1991)
India ^c	??/1987	09/1918	n/a	see above
Indonesia	09/1971	10/1918	2	Chandra (2013)
Indonesia	02/1976	10/1918	4	see above
Indonesia	10/1980	10/1918	0	see above
Indonesia	10/1985	10/1918	0	see above
Indonesia	10/1990	10/1918	0	see above

Table continued on next page. Influenza start dates are the earliest reported infections/deaths in a country. ^a We do not distinguish between East and West Germany with respect to pandemic dates. ^b The start date for Haiti is based on the start date reported for the Dominican Republic. ^c National Sample Survey Organization (NSSO) employment survey.

Table A1 (continued): Timing of second wave and implied misclassification error

Country	Census: month/year	Influenza start: month/year	Misclassification error	Sources for Influenza dates
Jamaica	06/1982	09/1918	6	Killingray (1994), Patterson & Pyle (1991)
Kenya	08/1969	09/1918	2	Patterson & Pyle (1983)
Kenya	08/1979	09/1918	2	see above
Kenya	10/1989	09/1918	2	see above
Liberia	02/1974	11/1918	18	Patterson & Pyle (1983)
Malawi	09/1987	10/1918, 11/1918	2	Ellison (2003), Patterson (1983), Patterson & Pyle (1983)
Malaysia	08-09/1970	11/1918	6	Patterson & Pyle (1991)
Malaysia	06/1980	11/1918	2	see above
Mali	04/1987	09/1918	10	Patterson & Pyle (1983)
Mexico	06/1960	09/1918	6	Patterson & Pyle (1991)
Mexico	01/1970	09/1918	16	see above
Mexico	03/1990	09/1918	12	see above
Mongolia	01/1989	11/1918	2	Patterson & Pyle (1991)
Morocco	09/1982	09/1918	0	Patterson & Pyle (1991)
Nicaragua	04/1971	09/1918	10	Patterson & Pyle (1991)
Pakistan	01/1973	11/1918	20	Patterson & Pyle (1991)
Pakistan	03/1981	11/1918	16	see above
Panama	12/1960	09/1918	6	Patterson & Pyle (1991)
Panama	05/1970	09/1918	8	see above
Panama	05/1980	09/1918	8	see above
Panama	05/1990	09/1918	8	see above
Philippines	05/1990	10/1918	10	Patterson & Pyle (1991)
Portugal	02-06/1981	08/1918	12	Patterson & Pyle (1991)
Puerto Rico	04/1970	09/1918	10	Killingray (1994)
Puerto Rico	04/1980	09/1918	10	see above
Puerto Rico	04/1990	09/1918	10	see above
Romania	01/1977	10/1918	4	Patterson & Pyle (1991)
Saint Lucia	05/1980	11/1918	12	Killingray (1994)
Senegal	05-06/1988	09/1918	8	Patterson (1983)
Spain	03/1981	08/1918, 09/1918	8	Echeverri (2003), Patterson & Pyle (1991), Trilla et al. (2008)
Switzerland	11/1970	09/1918	4	Neelsen & Stratmann (2012), Patterson & Pyle (1991)
Switzerland	11-12/1980	09/1918	4	see above
Switzerland	11-12/1990	09/1918	4	see above
Tanzania	08/1988	10/1918	4	Patterson & Pyle (1983)
Thailand	04/1970	10/1918	12	Patterson & Pyle (1991)
Thailand	04/1980	10/1918	4	see above
Thailand	04/1990	10/1918	4	see above
Turkey	10/1985	10/1918	0	Patterson & Pyle (1991)
Turkey	10/1990	10/1918	0	see above
United States	04/1960	09/1918	6	Patterson & Pyle (1991)
United States	04/1970	09/1918	6	see above
United States	04/1980	09/1918	6	see above
United States	04/1990	09/1918	10	see above
Uruguay	10/1963	10/1918	0	Patterson & Pyle (1991)
Uruguay	05/1975	10/1918	10	see above
Uruguay	10/1985	10/1918	0	see above
Venezuela	11/1971	09/1918, 10/1918	4	Patterson & Pyle (1991), Soyano & Esparza (2020)
Venezuela	10-11/1981	09/1918, 10/1918	2	see above
Venezuela	10/1990	09/1918, 10/1918	2	see above
Vietnam	04/1989	10/1918	4	Patterson & Pyle (1991)
Zambia	08-09/1990	10/1918	4	Patterson & Pyle (1983), Patterson & Pyle (1991)

Influenza start dates are the earliest reported infections/deaths in a country. ^a We do not distinguish between East and West Germany with respect to pandemic dates. ^b The start date for Haiti is based on the start date reported for the Dominican Republic. ^c National Sample Survey Organization (NSSO) employment survey.

Table A2: Data set characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Birth year	Primary education	Secondary education	Employment	General disability	Work disability	IPUMS sample coverage	Region	Non-natives excluded	Myers' Blended Index
Argentina 1970	n/a	99.2	99.2	99.4	n/a	n/a	2.0	L. America	yes	1.0
Argentina 1980	n/a	100.0	100.0	n/a	n/a	n/a	10.0	L. America	yes	2.4
Austria 1971	100.0	n/a	100.0	100.0	n/a	n/a	10.0	Europe	no	8.8
Austria 1981	100.0	n/a	100.0	n/a	n/a	n/a	10.0	Europe	no	8.6
Bolivia 1976	n/a	98.4	98.4	99.3	n/a	n/a	10.0	L. America	yes	18.2
Brazil 1960	n/a	99.3	99.3	99.9	n/a	n/a	20.0	L. America	yes	11.8
Brazil 1970	n/a	100.0	100.0	99.1	n/a	100.0	25.0	L. America	yes	7.3
Brazil 1980	n/a	99.9	99.9	n/a	n/a	n/a	25.0	L. America	yes	4.0
Burkina Faso 1985	n/a	93.5	93.5	n/a	99.4	n/a	10.0	Africa	no	31.1
Cameroon 1976	70.2	95.4	95.4	98.9	n/a	98.9	10.0	Africa	yes	19.6
Cameroon 1987	n/a	93.0	93.0	n/a	n/a	n/a	10.0	Africa	yes	25.1
Canada 1971	n/a	100.0	100.0	100.0	n/a	n/a	1.0	N. America	yes	0.6
Canada 1981	100.0	98.2	98.2	n/a	n/a	n/a	2.0	N. America	yes	1.2
Chile 1960	n/a	96.5	96.5	100.0	n/a	n/a	1.2	L. America	yes	9.0
Chile 1970	n/a	100.0	100.0	99.4	n/a	n/a	10.0	L. America	yes	8.0
Chile 1982	n/a	100.0	100.0	n/a	n/a	n/a	10.0	L. America	yes	4.8
China 1982	n/a	100.0	100.0	n/a	n/a	n/a	1.0	Asia	no	1.1
China 1990	100.0	100.0	100.0	n/a	n/a	n/a	1.0	Asia	no	1.4
Colombia 1964	n/a	100.0	100.0	100.0	n/a	n/a	2.0	L. America	yes	13.4
Colombia 1973	n/a	97.8	97.8	99.9	n/a	n/a	10.0	L. America	yes	13.3
Colombia 1985	n/a	96.4	96.4	n/a	n/a	n/a	10.0	L. America	yes	16.8
Costa Rica 1963	n/a	99.8	99.8	100.0	n/a	n/a	6.0	L. America	yes	9.0
Costa Rica 1973	n/a	100.0	100.0	100.0	n/a	n/a	10.0	L. America	yes	7.3
Costa Rica 1984	n/a	100.0	100.0	n/a	n/a	n/a	10.0	L. America	yes	5.0
Dominican Rep. 1960	n/a	100.0	100.0	100.0	n/a	n/a	6.6	L. America	yes	20.5
Dominican Rep. 1970	n/a	89.6	89.6	99.8	n/a	99.8	6.8	L. America	yes	19.8
Dominican Rep. 1981	n/a	100.0	100.0	n/a	100.0	n/a	8.5	L. America	yes	8.9
Ecuador 1962	n/a	100.0	100.0	100.0	n/a	n/a	3.0	L. America	yes	22.3
Ecuador 1974	n/a	98.4	98.4	98.6	n/a	n/a	10.0	L. America	yes	14.7
Ecuador 1982	n/a	94.8	94.8	n/a	n/a	n/a	10.0	L. America	yes	17.4
Ecuador 1990	n/a	97.2	97.2	n/a	n/a	n/a	10.0	L. America	yes	18.4
Fiji 1976	99.9	93.5	93.5	100.0	n/a	100.0	10.0	Asia	yes	8.6
Fiji 1986	99.6	94.8	94.8	n/a	n/a	n/a	10.0	Asia	yes	10.9
France 1962	100.0	100.0	100.0	100.0	100.0	n/a	5.0	Europe	yes	10.9
France 1968	100.0	100.0	100.0	100.0	n/a	n/a	5.0	Europe	yes	10.7
France 1975	100.0	100.0	100.0	100.0	n/a	n/a	5.0	Europe	yes	10.6
France 1982	100.0	100.0	100.0	n/a	n/a	n/a	5.0	Europe	yes	10.6
France 1990	100.0	100.0	100.0	n/a	n/a	n/a	4.2	Europe	yes	10.3
Germany (East) 1971	n/a	n/a	100.0	n/a	n/a	n/a	25.0	Europe	no	10.9
Germany (East) 1981	n/a	100.0	100.0	n/a	n/a	n/a	25.0	Europe	no	9.4
Germany (West) 1970	100.0	100.0	100.0	100.0	n/a	n/a	5.0	Europe	no	8.3
Greece 1971	100.0	98.2	98.2	100.0	n/a	n/a	10.0	Europe	no	4.8
Greece 1981	100.0	100.0	100.0	n/a	n/a	n/a	10.0	Europe	no	5.5
Guinea 1983	n/a	99.6	99.6	n/a	n/a	n/a	10.0	Africa	yes	30.6
Haiti 1971	n/a	100.0	100.0	100.0	n/a	n/a	10.0	L. America	yes	17.3
Haiti 1982	n/a	88.5	88.5	n/a	n/a	n/a	2.5	L. America	no	24.3
Hungary 1970	n/a	100.0	100.0	n/a	n/a	n/a	5.0	Europe	no	10.8
Hungary 1980	n/a	99.9	99.9	n/a	n/a	n/a	5.0	Europe	no	10.2
Hungary 1990	n/a	100.0	100.0	n/a	n/a	n/a	5.0	Europe	no	10.1
India 1983	n/a	99.8	99.8	n/a	n/a	n/a	0.1	Asia	no	47.8
India 1987	n/a	100.0	100.0	n/a	n/a	n/a	0.1	Asia	no	46.3
Indonesia 1971	n/a	100.0	100.0	99.6	n/a	n/a	0.5	Asia	yes	28.4
Indonesia 1976	68.8	99.7	99.7	99.9	n/a	n/a	0.2	Asia	yes	16.6
Indonesia 1980	48.9	100.0	100.0	n/a	100.0	n/a	5.0	Asia	yes	34.5
Indonesia 1985	n/a	100.0	100.0	n/a	n/a	n/a	0.4	Asia	yes	26.8
Indonesia 1990	94.5	100.0	100.0	n/a	n/a	n/a	0.5	Asia	yes	22.0
Jamaica 1982	n/a	87.3	87.3	n/a	n/a	n/a	10.0	L. America	yes	8.7
Kenya 1969	n/a	100.0	100.0	n/a	n/a	n/a	6.0	Africa	yes	19.1
Kenya 1979	n/a	99.6	99.6	n/a	n/a	n/a	6.7	Africa	yes	17.4
Kenya 1989	n/a	98.9	98.9	n/a	n/a	n/a	5.0	Africa	yes	16.2
Liberia 1974	n/a	100.0	100.0	n/a	n/a	n/a	10.0	Africa	yes	18.2
Malawi 1987	n/a	99.9	99.9	n/a	n/a	n/a	10.0	Africa	yes	14.3
Malaysia 1970	n/a	100.0	100.0	99.5	n/a	n/a	2.0	Asia	yes	12.1
Malaysia 1980	100.0	100.0	100.0	n/a	99.1	n/a	2.0	Asia	yes	12.5
Mali 1987	n/a	95.7	95.7	n/a	100.0	n/a	10.0	Africa	yes	22.1
Mexico 1960	n/a	99.6	99.6	n/a	n/a	n/a	1.5	L. America	yes	19.4
Mexico 1970	n/a	100.0	100.0	95.6	n/a	n/a	1.0	L. America	yes	15.1
Mexico 1990	n/a	100.0	100.0	n/a	n/a	n/a	10.0	L. America	yes	15.2
Mongolia 1989	100.0	100.0	100.0	n/a	n/a	n/a	10.0	Asia	yes	4.7
Morocco 1982	n/a	86.7	86.7	n/a	n/a	n/a	5.0	Africa	yes	31.3
Nicaragua 1971	n/a	95.7	95.7	98.0	n/a	n/a	10.0	L. America	yes	25.3
Pakistan 1973	n/a	99.9	99.9	100.0	100.0	n/a	2.0	Asia	yes	60.1
Pakistan 1981	n/a	99.9	99.9	n/a	n/a	n/a	10.0	Asia	no	51.4
Panama 1960	n/a	99.7	99.7	98.1	n/a	n/a	5.0	L. America	yes	5.0
Panama 1970	n/a	99.8	99.8	99.1	n/a	n/a	10.0	L. America	yes	5.2
Panama 1980	n/a	99.3	99.3	n/a	n/a	n/a	10.0	L. America	yes	5.0
Panama 1990	n/a	97.9	97.9	n/a	100.0	n/a	10.0	L. America	yes	3.8

Table continued on next page. Column 1 shows percentage of observations in census with non-missing information on birth year (or n/a if birth year information was not collected). Columns 2 to 6 show percentage of observations among cohorts of interest with non-missing information on the specified indicator (or n/a if indicator was not collected or excluded because of 1976-cutoff for employment and work disability). Column 7 shows percentage of a country's population included in the census sample of a given year. Myers' Blended Index of Digit Preference is calculated using original birth year information or census year minus age as birth year proxy if birth year information is unavailable.

Table A2 (continued): Data set characteristics

	(1) Birth year	(2) Primary education	(3) Secondary education	(4) Employment	(5) General disability	(6) Work disability	(7) IPUMS sample coverage	(8) Region	(9) Non-natives excluded	(10) Myers' Blended Index
Philippines 1990	n/a	98.3	98.3	n/a	100.0	n/a	10.0	Asia	yes	6.5
Portugal 1981	n/a	100.0	100.0	n/a	n/a	n/a	5.0	Europe	yes	3.0
Puerto Rico 1970	n/a	100.0	100.0	n/a	n/a	n/a	1.0	L. America	yes	5.1
Puerto Rico 1980	n/a	100.0	100.0	n/a	n/a	n/a	5.0	L. America	yes	1.6
Puerto Rico 1990	n/a	100.0	100.0	n/a	n/a	n/a	5.0	L. America	yes	3.0
Romania 1977	99.5	100.0	100.0	n/a	n/a	n/a	10.0	Europe	yes	8.5
Saint Lucia 1980	n/a	n/a	97.8	n/a	n/a	n/a	10.0	L. America	yes	10.2
Senegal 1988	n/a	96.6	96.6	n/a	99.4	n/a	10.0	Africa	yes	15.5
Spain 1981	100.0	99.9	99.9	n/a	n/a	n/a	5.0	Europe	yes	2.2
Switzerland 1970	n/a	n/a	97.2	100.0	n/a	n/a	5.0	Europe	yes	2.3
Switzerland 1980	n/a	n/a	95.5	n/a	n/a	n/a	5.0	Europe	yes	2.3
Switzerland 1990	n/a	n/a	96.3	n/a	n/a	n/a	5.0	Europe	yes	2.9
Tanzania 1988	n/a	99.9	99.9	n/a	n/a	n/a	10.0	Africa	yes	34.6
Thailand 1970	n/a	94.0	94.0	n/a	n/a	99.3	2.0	Asia	yes	2.3
Thailand 1980	68.9	98.3	98.3	n/a	n/a	n/a	1.0	Asia	yes	2.7
Thailand 1990	85.8	99.1	99.1	n/a	99.5	n/a	1.2	Asia	yes	3.3
Turkey 1985	n/a	99.9	99.9	n/a	100.0	n/a	5.0	Asia	yes	19.5
Turkey 1990	n/a	100.0	100.0	n/a	n/a	n/a	5.0	Asia	yes	15.7
United States 1960	n/a	100.0	100.0	100.0	n/a	n/a	1.0	N. America	yes	1.0
United States 1970	n/a	100.0	100.0	100.0	n/a	n/a	1.0	N. America	yes	0.9
United States 1980	n/a	100.0	100.0	n/a	n/a	n/a	5.0	N. America	yes	0.8
United States 1990	n/a	100.0	100.0	n/a	n/a	n/a	5.0	N. America	yes	1.0
Uruguay 1963	n/a	99.0	99.0	99.5	n/a	n/a	10.0	L. America	yes	3.3
Uruguay 1975	n/a	99.6	99.6	99.5	n/a	n/a	10.0	L. America	yes	2.8
Uruguay 1985	n/a	99.3	99.3	n/a	n/a	n/a	10.0	L. America	yes	3.3
Venezuela 1971	n/a	93.0	93.0	100.0	n/a	100.0	2.0	L. America	yes	4.9
Venezuela 1981	n/a	100.0	100.0	n/a	n/a	n/a	10.0	L. America	yes	2.4
Venezuela 1990	n/a	89.0	89.0	n/a	97.7	n/a	10.0	L. America	yes	4.2
Vietnam 1989	100.0	99.4	99.4	n/a	n/a	n/a	5.0	Asia	no	3.2
Zambia 1990	n/a	100.0	100.0	n/a	100.0	n/a	10.0	Africa	yes	15.5

Column 1 shows percentage of observations in census with non-missing information on birth year (or n/a if birth year information was not collected). Columns 2 to 6 show percentage of observations among cohorts of interest with non-missing information on the specified indicator (or n/a if indicator was not collected or excluded because of 1976-cutoff for employment and work disability). Column 7 shows percentage of a country's population included in the census sample of a given year. Myers' Blended Index of Digit Preference is calculated using original birth year information or census year minus age as birth year proxy if birth year information is unavailable.

Appendix B: Construction of core sample

Our core sample is constructed so that it only comprises those countries and censuses for which the expected misclassification error is weakly smaller than the one for the United States in the approach of Almond (2006). Almond's approach considers all individuals born in 1919 as exposed to the pandemic in utero. As explained in the main text of the paper, assuming that the exposure period started one month after the wave onset date reported in Table A1 and lasted for three months, this leads to a misclassification of six birth months (comprising three months wrongly classified as 'exposed' and three months wrongly classified as 'not exposed' to the Spanish Flu in utero). Consequently, we only include censuses with a misclassification error of up to six months in the core sample. The misclassification error associated with each census is calculated as follows. First, note that information on the time of birth is available in two possible forms: the respondent's birth year or the age (in years) at the time of the census. Age in years is included in all censuses (though sometimes derived from exact birth dates).¹ Table A2 shows which census datasets also report the birth year. For each census, we calculate the implied misclassification error for both options and then select the one with the smaller error if both birth year and age are available.

To illustrate the procedure, consider the 1980 census in Argentina as an example. As described in Table A1, the census took place in October and the earliest reported month in which the pandemic reached Argentina is September 1918. Assuming that national mortality rates increased one month after the first reported case and remained elevated for three months (i.e., the 'exposure period'), wide-spread in-utero exposure can be expected for October, November, and December of 1918. We now calculate, for each possible birth month, the true exposure status based on the assumptions stated above as well as the exposure status implied when using either information about respondents' age (and timing of the census) or information about respondents' birth year. The results are shown in Table B1. Accordingly, those born in or before September 1918 were not exposed in utero, given that the exposure period was October to December 1918 as described earlier. Similarly, those born in or after October 2019 were not exposed in utero because they were conceived in the first quarter of 1919 (assuming a pregnancy duration of 9 months). Thus, the only cohorts with in-utero exposure are those born between October 1918 and September 1919 (as indicated in Table B1 in the 'True exposure status' column).

¹ With that said, we do not use the variable age for the French census of 1962 and all Greek censuses because age was only estimated in these datasets.

If respondents are classified as exposed in-utero vs. not-exposed in-utero based on reported year of birth, the resulting misclassification error amounts to six birth months, as those born in fall 1918 are wrongly classified as not-exposed in-utero and those born in fall 1919 are wrongly classified as exposed in-utero. Please note that this result is independent of census timing and only varies with exposure period onset/duration.

By contrast, if we approximate the birth year using the census year minus respondents' age at the time of the census, the resulting misclassification error additionally depends on census timing and may be larger or smaller than the misclassification error generated by exact birth years. In the case of the 1980 Argentina census (which took place in October), all individuals born between November 1918 and October 1919 will be coded as exposed in-utero (i.e., all those with age 61 in the first column of Table B1, given that 1980 minus 61 equals 1919). As shown in the last column of Table B1, this leads to a misclassification error of two birth months; that is, those born in October 1918 are incorrectly coded as not exposed and those born in October 1919 are incorrectly coded as exposed.

Since the implied misclassification error is weakly smaller than the cutoff of six birth months, the 1980 census in Argentina would in principle qualify for inclusion in the core sample. However, as explained in the main text of the paper, our main analysis focuses on the first census in each country that fulfils our criteria. Since there was also a census in 1970 in Argentina which features a misclassification error of less than six birth months (see Table A1), the core sample comprises the 1970 Argentina census rather than the 1980 census.

For a few countries, census enumeration was reported to have lasted multiple months (see Table A1). In these instances, we use the first month of enumeration. Furthermore, there are multiple datasets which only report respondents' age but not the birth year (see Table A2). In these cases, the selection of the core sample is exclusively based on the misclassification error implied when approximating birth year as census year minus age, rather than by the minimum across the two specifications (this also applies to Argentina). Moreover, Haiti and India each feature one dataset with unknown enumeration timing and unknown birth year. In these two cases, we are unable to calculate the implied misclassification error and therefore consider them too imprecise for inclusion in the core sample.

Finally, note that the robustness checks (discussed in Section 5 in the paper) which vary the assumed duration of the pandemic and delay from the first reported case in each country, can be conducted in the same way as illustrated in Table B1 after adjusting the implied exposure period.

Table B1: Illustration of implied misclassification error

Age	Birth year	Birth month	Census: month/year	Exposure period: months/year	Exposure: True status	Exposure: Census year minus age	Exposure: Birth year
62	1918	1	10/1980	10-12/1918	0	0	0
62	1918	2	10/1980	10-12/1918	0	0	0
62	1918	3	10/1980	10-12/1918	0	0	0
62	1918	4	10/1980	10-12/1918	0	0	0
62	1918	5	10/1980	10-12/1918	0	0	0
62	1918	6	10/1980	10-12/1918	0	0	0
62	1918	7	10/1980	10-12/1918	0	0	0
62	1918	8	10/1980	10-12/1918	0	0	0
62	1918	9	10/1980	10-12/1918	0	0	0
62	1918	10	10/1980	10-12/1918	1	0	0
61	1918	11	10/1980	10-12/1918	1	1	0
61	1918	12	10/1980	10-12/1918	1	1	0
61	1919	1	10/1980	10-12/1918	1	1	1
61	1919	2	10/1980	10-12/1918	1	1	1
61	1919	3	10/1980	10-12/1918	1	1	1
61	1919	4	10/1980	10-12/1918	1	1	1
61	1919	5	10/1980	10-12/1918	1	1	1
61	1919	6	10/1980	10-12/1918	1	1	1
61	1919	7	10/1980	10-12/1918	1	1	1
61	1919	8	10/1980	10-12/1918	1	1	1
61	1919	9	10/1980	10-12/1918	1	1	1
61	1919	10	10/1980	10-12/1918	0	1	1
60	1919	11	10/1980	10-12/1918	0	0	1
60	1919	12	10/1980	10-12/1918	0	0	1

The last two columns report implied exposure status (1=exposed; 0=not exposed). Values marked in red (bold) indicate misclassified birth months. Column “Exposure: Census year minus age” classifies everyone whose age was 61 years at enumeration time as exposed in-utero because $1918 - 61 = 1919$. Column “Exposure: Birth year” instead directly classifies everyone born in 1919 as exposed in-utero.

Appendix C: Additional results

Table C1: Results by country (men) for all census samples collected between 1960 and 1990 available on IPUMS

	PRIMARY EDUCATION				SECONDARY EDUCATION				EMPLOYMENT			
	N	Mean of dep. variable	Effect estimate	SE	N	Mean of dep. variable	Effect estimate	SE	N	Mean of dep. variable	Effect estimate	SE
AFRICA												
Burkina Faso (1985)	11168	0.010	0.010**	(0.005)	11168	0.001	0.000	(0.001)				
Cameroon (1976)	11910	0.061	0.008	(0.008)	11910	0.007	0.008**	(0.004)	12341	0.854	0.026**	(0.010)
Cameroon (1987)	9403	0.062	0.023**	(0.010)	9403	0.003	-0.001	(0.002)				
Guinea (1983)	8713	0.013	0.011	(0.007)	8713	0.003	0.009*	(0.005)				
Kenya (1969)	17323	0.130	-0.046***	(0.007)	17323	0.015	-0.011***	(0.002)				
Kenya (1979)	15128	0.054	-0.014***	(0.005)	15128	0.004	0.000	(0.002)				
Kenya (1989)	9281	0.063	-0.015**	(0.007)	9281	0.005	-0.002	(0.002)				
Liberia (1974)	4432	0.070	0.006	(0.015)	4432	0.021	-0.005	(0.007)				
Malawi (1987)	9252	0.039	0.004	(0.007)	9252	0.002	0.001	(0.002)				
Mali (1987)	9924	0.038	0.009	(0.008)	9924	0.006	0.004	(0.004)				
Morocco (1982)	19128	0.020	0.008	(0.006)	19128	0.003	0.001	(0.002)				
Senegal (1988)	7232	0.060	-0.029***	(0.007)	7232	0.008	-0.010***	(0.002)				
Tanzania (1988)	26926	0.061	0.012	(0.007)	26926	0.003	0.002	(0.002)				
Zambia (1990)	6104	0.080	0.011	(0.014)	6104	0.009	-0.000	(0.005)				
ASIA												
China (1982)	254025	0.420	0.000	(0.003)	254025	0.023	-0.002**	(0.001)				
China (1990)	176990	0.288	0.001	(0.004)	176990	0.026	0.001	(0.001)				
Fiji (1976)	1185	0.346	0.090	(0.057)	1185	0.024	0.007	(0.017)	1268	0.819	0.023	(0.039)
Fiji (1986)	819	0.332	-0.003	(0.062)	819	0.023	0.007	(0.022)				
India (1983)	9961	0.268	0.174***	(0.026)	9961	0.074	0.095***	(0.020)				
India (1987)	10698	0.285	0.083***	(0.019)	10698	0.091	0.044***	(0.013)				
Indonesia (1971)	15372	0.352	0.106***	(0.016)	15372	0.043	0.013*	(0.007)	15310	0.817	-0.019	(0.012)
Indonesia (1976)	3633	0.341	0.165***	(0.033)	3633	0.051	0.044**	(0.018)	3639	0.830	-0.074***	(0.027)
Indonesia (1980)	132545	0.143	0.066***	(0.005)	132545	0.016	0.013***	(0.002)				
Indonesia (1985)	8058	0.217	0.028	(0.021)	8058	0.023	-0.003	(0.007)				
Indonesia (1990)	8532	0.261	0.029	(0.019)	8532	0.025	0.004	(0.006)				
Malaysia (1970)	3842	0.270	-0.067***	(0.021)	3842	0.005	0.005	(0.004)	3824	0.831	0.029*	(0.017)
Malaysia (1980)	2586	0.220	-0.077***	(0.023)	2586	0.010	-0.009**	(0.005)				
Mongolia (1989)	2392	0.230	0.029	(0.034)	2392	0.097	0.023	(0.025)				
Pakistan (1973)	29397	0.185	0.194***	(0.023)	29397	0.036	0.136***	(0.018)	29428	0.828	-0.015	(0.016)
Pakistan (1981)	232277	0.307	0.033***	(0.005)	232277	0.033	0.019***	(0.002)				
Philippines (1990)	54471	0.372	0.029***	(0.008)	54471	0.133	0.013**	(0.006)				
Thailand (1970)	17413	0.211	-0.003	(0.011)	17413	0.071	0.002	(0.007)				
Thailand (1980)	4204	0.116	0.017	(0.020)	4204	0.041	-0.001	(0.011)				
Thailand (1990)	4109	0.139	-0.016	(0.017)	4109	0.050	-0.013	(0.010)				
Turkey (1985)	34561	0.373	0.019*	(0.011)	34561	0.059	0.006	(0.005)				
Turkey (1990)	25234	0.383	0.033**	(0.013)	25234	0.061	0.024***	(0.007)				
Vietnam (1989)	31026	0.179	-0.019**	(0.007)	31026	0.032	-0.003	(0.003)				
EUROPE												
Austria (1971)					36287	0.529	-0.002	(0.010)	36287	0.887	0.012**	(0.005)
Austria (1981)					30232	0.549	-0.010	(0.011)				
France (1962)	120546	0.529	0.004	(0.006)	120546	0.155	0.001	(0.004)	120546	0.955	0.005**	(0.002)
France (1968)	116993	0.605	-0.009	(0.006)	116993	0.196	-0.003	(0.005)	116993	0.909	-0.004	(0.003)
France (1975)	107912	0.612	0.002	(0.006)	107912	0.210	-0.001	(0.005)	107912	0.730	-0.039***	(0.005)
France (1982)	91017	0.565	-0.020***	(0.006)	91017	0.192	-0.012**	(0.005)				
France (1990)	61098	0.635	-0.005	(0.007)	61098	0.207	0.006	(0.007)				
E. Germany (1971)					165117	0.766	0.003	(0.005)				
E. Germany (1981)	137728	0.979	0.002**	(0.001)	137728	0.817	0.013***	(0.004)				
W. Germany (1970)	151527	1.000	0.000***	(0.000)	151527	0.238	-0.001	(0.004)	151527	0.882	-0.009***	(0.003)
Greece (1971)	48306	0.698	-0.003	(0.008)	48306	0.154	0.008	(0.006)	49182	0.872	-0.023***	(0.006)
Greece (1981)	43155	0.721	-0.014*	(0.008)	43155	0.165	-0.014**	(0.007)				
Hungary (1970)	29377	0.803	0.010	(0.009)	29377	0.147	0.010	(0.010)				
Hungary (1980)	24252	0.809	0.005	(0.010)	24252	0.156	0.021*	(0.011)				
Hungary (1990)	14455	0.835	0.009	(0.012)	14455	0.174	-0.011	(0.014)				
Portugal (1981)	21142	0.101	-0.007	(0.008)	21142	0.041	-0.004	(0.005)				
Romania (1977)	78927	0.389	-0.021***	(0.006)	78927	0.187	-0.014***	(0.005)				
Spain (1981)	83541	0.471	0.007	(0.006)	83541	0.085	0.010***	(0.004)				
Switzerland (1970)					14984	0.999	0.001	(0.001)	15412	0.965	0.002	(0.005)
Switzerland (1980)					13011	0.997	0.000	(0.002)				
Switzerland (1990)					9647	0.994	0.001	(0.003)				
LATIN AMERICA												
Argentina (1970)	20923	0.448	0.000	(0.012)	20923	0.081	-0.012*	(0.007)	20954	0.877	-0.004	(0.007)
Argentina (1980)	76747	0.358	0.020***	(0.007)	76747	0.064	-0.003	(0.003)				
Bolivia (1976)	10626	0.178	0.043**	(0.017)	10626	0.051	0.004	(0.009)	10731	0.907	-0.021*	(0.012)
Brazil (1960)	737002	0.060	0.015***	(0.001)	737002	0.029	0.007***	(0.001)	741995	0.958	-0.001	(0.001)
Brazil (1970)	808750	0.081	0.012***	(0.001)	808750	0.039	0.005***	(0.001)	801520	0.873	-0.007***	(0.001)
Brazil (1980)	637188	0.092	0.001	(0.002)	637188	0.047	0.001	(0.001)				
Chile (1960)	4330	0.437	0.031	(0.033)	4330	0.073	0.010	(0.018)	4485	0.911	-0.023	(0.019)
Chile (1970)	31771	0.454	0.009	(0.012)	31771	0.103	0.010	(0.008)	31568	0.835	0.007	(0.008)
Chile (1982)	26433	0.417	-0.026**	(0.011)	26433	0.111	-0.016**	(0.007)				
Colombia (1964)	13164	0.232	-0.063***	(0.011)	13164	0.031	-0.006	(0.005)	13164	0.948	-0.003	(0.006)
Colombia (1973)	46798	0.285	0.014*	(0.008)	46798	0.044	-0.004	(0.003)	47827	0.796	-0.007	(0.007)
Colombia (1985)	39325	0.336	0.003	(0.009)	39325	0.068	-0.012**	(0.005)				
Costa Rica (1963)	2888	0.190	-0.017	(0.028)	2888	0.040	-0.002	(0.013)	2895	0.933	-0.006	(0.018)
Costa Rica (1973)	4464	0.247	-0.026	(0.022)	4464	0.055	-0.007	(0.012)	4464	0.881	-0.001	(0.016)
Costa Rica (1984)	3757	0.245	0.031	(0.025)	3757	0.060	0.008	(0.014)				

Table continued on next page. Stars indicate statistical significance (* = 10%, ** = 5%, *** = 1%).

Table C1 (continued): Results by country (men)

	PRIMARY EDUCATION				SECONDARY EDUCATION				EMPLOYMENT			
	N	Mean of dep. variable	Effect estimate	SE	N	Mean of dep. variable	Effect estimate	SE	N	Mean of dep. variable	Effect estimate	SE
Dominican R. (1960)	8775	0.085	0.044**	(0.018)	8775	0.016	0.011	(0.009)	8775	0.985	0.004	(0.006)
Dominican R. (1970)	6335	0.206	0.033	(0.027)	6335	0.041	0.006	(0.013)	7058	0.719	-0.035	(0.028)
Dominican R. (1981)	8670	0.115	0.005	(0.014)	8670	0.017	0.007	(0.006)				
Ecuador (1962)	6121	0.220	0.072***	(0.026)	6121	0.032	-0.005	(0.010)	6124	0.972	-0.013	(0.010)
Ecuador (1974)	15677	0.259	-0.060***	(0.011)	15677	0.044	-0.004	(0.005)	15722	0.891	-0.005	(0.008)
Ecuador (1982)	14198	0.277	0.037**	(0.016)	14198	0.050	0.009	(0.008)				
Ecuador (1990)	10243	0.350	0.106***	(0.022)	10243	0.095	0.027*	(0.015)				
Haiti (1971)	13295	0.073	0.017**	(0.008)	13295	0.012	0.003	(0.003)	13294	0.937	-0.004	(0.007)
Haiti (1982)	1961	0.263	0.067	(0.056)	1961	0.048	-0.020	(0.022)				
Jamaica (1982)	5024	0.608	-0.024	(0.026)	5024	0.037	0.019	(0.012)				
Mexico (1960)	21595	0.179	0.080***	(0.017)	21595	0.025	0.014**	(0.007)				
Mexico (1970)	13225	0.205	0.065***	(0.017)	13225	0.033	0.021**	(0.009)	12637	0.898	-0.005	(0.012)
Mexico (1990)	80995	0.239	0.061***	(0.008)	80995	0.053	0.024***	(0.004)				
Nicaragua (1971)	3981	0.158	0.036*	(0.022)	3981	0.028	-0.001	(0.010)	4078	0.860	-0.025	(0.020)
Panama (1960)	2634	0.358	-0.019	(0.036)	2634	0.048	0.005	(0.018)	2593	0.899	0.009	(0.022)
Panama (1970)	5039	0.323	0.035	(0.026)	5039	0.050	0.010	(0.013)	5003	0.909	0.007	(0.014)
Panama (1980)	4474	0.341	0.068**	(0.028)	4474	0.065	0.015	(0.015)				
Panama (1990)	3305	0.366	0.092***	(0.032)	3305	0.071	0.009	(0.018)				
Puerto Rico (1970)	997	0.491	0.023	(0.057)	997	0.182	0.055	(0.048)				
Puerto Rico (1980)	5383	0.487	-0.002	(0.025)	5383	0.188	-0.028	(0.019)				
Puerto Rico (1990)	4272	0.555	0.046*	(0.028)	4272	0.209	0.012	(0.024)				
Saint Lucia (1980)					269	0.019	0.122	(0.089)				
Uruguay (1963)	14719	0.375	0.003	(0.015)	14719	0.056	-0.004	(0.007)	14792	0.856	-0.006	(0.010)
Uruguay (1975)	13307	0.440	-0.005	(0.016)	13307	0.064	0.001	(0.008)	13300	0.738	-0.021	(0.013)
Uruguay (1985)	9620	0.483	-0.027	(0.019)	9620	0.077	0.008	(0.010)				
Venezuela (1971)	27158	0.235	0.010	(0.009)	27158	0.035	-0.002	(0.004)	29213	0.814	-0.011	(0.008)
Venezuela (1981)	21632	0.235	0.013	(0.010)	21632	0.032	-0.007*	(0.004)				
Venezuela (1990)	16141	0.190	0.016	(0.011)	16141	0.035	-0.001	(0.005)				
N. AMERICA												
Canada (1971)	8986	0.900	-0.007	(0.011)	8986	0.124	-0.022*	(0.012)	8986	0.823	-0.008	(0.014)
Canada (1981)	15269	0.904	-0.011	(0.008)	15269	0.355	-0.029**	(0.014)				
United States (1960)	119539	0.922	-0.010***	(0.003)	119539	0.469	-0.021***	(0.005)	119539	0.917	-0.003	(0.003)
United States (1970)	109775	0.931	-0.007**	(0.003)	109775	0.497	-0.015***	(0.005)	109775	0.885	-0.002	(0.003)
United States (1980)	479885	0.928	-0.006***	(0.001)	479885	0.528	-0.014***	(0.002)				
United States (1990)	352947	0.952	0.000	(0.001)	352947	0.591	-0.005*	(0.003)				

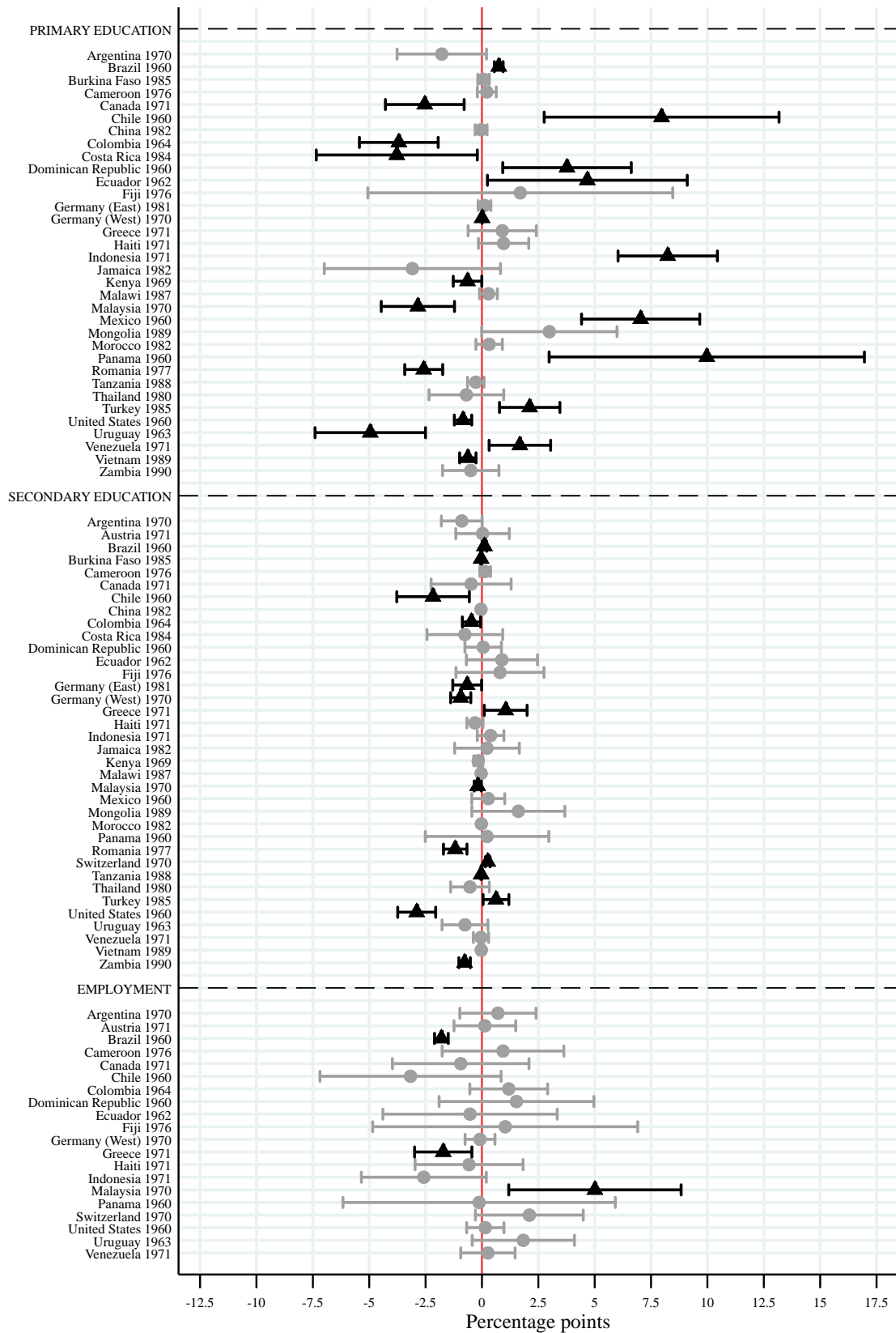
Stars indicate statistical significance (* = 10%, ** = 5%, *** = 1%).

Table C2: Weighted effects on education and employment (women)

	All regions	Africa	Asia	Europe	Latin America	Northern America
Primary education						
<i>Panel A: Full sample</i>						
Average weighted coefficient	0.000 (0.002)	-0.001* (0.001)	0.016 (0.009)	-0.003 (0.004)	0.013 (0.011)	-0.014 (0.008)
Weighted average of mean of DV	0.24	0.01	0.10	0.55	0.24	0.94
Countries	48	11	11	8	16	2
Positive significant coefficients	16	0	5	1	10	0
Negative significant coefficients	11	4	2	1	2	2
<i>Panel A+: Core sample</i>						
Average weighted coefficient	-0.000 (0.003)	0.000 (0.001)	0.008 (0.011)	-0.004 (0.006)	0.007 (0.012)	-0.014 (0.008)
Weighted average of mean of DV	0.25	0.01	0.07	0.68	0.26	0.94
Countries	34	7	8	4	13	2
Positive significant coefficients	10	0	2	1	7	0
Negative significant coefficients	9	1	2	1	3	2
Secondary education						
<i>Panel B: Full sample</i>						
Average weighted coefficient	-0.000 (0.000)	-0.000 (0.000)	0.001 (0.001)	-0.001 (0.002)	-0.001 (0.001)	-0.019 (0.012)
Weighted average of mean of DV	0.07	0.00	0.02	0.20	0.02	0.30
Countries	50	10	11	10	17	2
Positive significant coefficients	7	0	3	3	1	0
Negative significant coefficients	11	3	1	4	2	1
<i>Panel B+: Core sample</i>						
Average weighted coefficient	-0.001 (0.001)	-0.000 (0.000)	-0.000 (0.001)	-0.003 (0.004)	-0.002 (0.001)	-0.019 (0.012)
Weighted average of mean of DV	0.08	0.00	0.01	0.32	0.02	0.30
Countries	36	7	8	6	13	2
Positive significant coefficients	4	0	1	2	1	0
Negative significant coefficients	10	3	1	3	2	1
Employment						
<i>Panel C: Full sample</i>						
Average weighted coefficient	0.002 (0.003)	0.009 (0.016)	0.010 (0.016)	-0.003 (0.004)	0.004 (0.006)	0.001 (0.005)
Weighted average of mean of DV	0.28	0.60	0.22	0.38	0.22	0.41
Countries	26	1	4	5	14	2
Positive significant coefficients	1	0	1	0	0	0
Negative significant coefficients	2	0	0	1	1	0
<i>Panel C+: Core sample</i>						
Average weighted coefficient	-0.000 (0.004)	0.009 (0.016)	0.010 (0.026)	-0.002 (0.006)	-0.000 (0.006)	0.001 (0.005)
Weighted average of mean of DV	0.31	0.60	0.28	0.36	0.24	0.41
Countries	20	1	3	4	10	2
Positive significant coefficients	1	0	1	0	0	0
Negative significant coefficients	2	0	0	1	1	0

Average weighted coefficients are derived from a random-effects meta-analysis as described in Section 3. Stars indicate statistical significance (* = 10%, ** = 5%, *** = 1%). The weighted average of the mean of the dependent variable (DV) is calculated using the same weights as for the average weighted coefficients to ensure comparability. n/a = not available. Single country-level coefficients are counted as significant if corresponding p-values are smaller than 0.1. The number countries with estimates on secondary education is reduced by one in the full sample (compared to the analysis for men), because none of the women in the age cohorts of interest completed secondary education in the Guinea 1983 census.

Figure C1: Effects on education and employment by country (women, core sample)



Note: Grey circles show insignificant estimates. Black triangles show estimates that are significant on a 10% level. Bars are 90% confidence intervals.

Table C3: Weighted effects on disability (men)

	All regions	Africa	Asia	Europe	Latin America	Northern America
Work disability						
<i>Panel A: Full sample</i>						
Average weighted coefficient	-0.003* (0.001)	-0.012** (0.005)	-0.005 (0.002)	n/a n/a	-0.002 (0.001)	n/a n/a
Weighted av. of mean of DV	0.03	0.04	0.02	n/a	0.04	n/a
Countries	6	1	2	0	3	0
Pos. significant coefficients	0	0	0	n/a	0	n/a
Neg. significant coefficients	3	1	1	n/a	1	n/a
<i>Panel A+: Core sample</i>						
Average weighted coefficient	-0.004 (0.002)	-0.012** (0.005)	0.005 (0.023)	n/a n/a	-0.002 (0.001)	n/a n/a
Weighted av. of mean of DV	0.04	0.04	0.04	n/a	0.04	n/a
Countries	4	1	1	0	2	0
Pos. significant coefficients	0	0	0	n/a	0	n/a
Neg. significant coefficients	2	1	0	n/a	1	n/a
General disability						
<i>Panel B: Full sample</i>						
Average weighted coefficient	0.004 (0.011)	-0.003 (0.005)	0.016 (0.029)	-0.003 (0.003)	0.001 (0.005)	n/a n/a
Weighted av. of mean of DV	0.06	0.08	0.04	0.08	0.06	n/a
Countries	14	4	6	1	3	0
Pos. significant coefficients	1	0	1	0	0	n/a
Neg. significant coefficients	0	0	0	0	0	n/a
<i>Panel B+: Core sample</i>						
Average weighted coefficient	-0.004* (0.002)	-0.007 (0.006)	-0.004 (0.002)	n/a n/a	0.002 (0.006)	n/a n/a
Weighted av. of mean of DV	0.05	0.06	0.03	n/a	0.07	n/a
Countries	8	2	4	0	2	0
Pos. significant coefficients	0	0	0	n/a	0	n/a
Neg. significant coefficients	0	0	0	n/a	0	n/a

Average weighted coefficients are derived from a random-effects meta-analysis as described in Section 3. Stars indicate statistical significance (* = 10%, ** = 5%, *** = 1%). The weighted average of the mean of the dependent variable (DV) is calculated using the same weights as for the average weighted coefficients to ensure comparability. n/a = not available. Single country-level coefficients are counted as significant if corresponding p-values are smaller than 0.1.

Table C4: Weighted effects on education and employment (men, core sample): Robustness to assumed timing of pandemic

	Begin: two months after 1 st case Duration: 3 months	Begin: same month as 1 st case Duration: 3 months	Begin: one month after 1 st case Duration: 4 months	Begin: same month as 1 st case Duration: 4 months
Panel A: Primary education				
Average weighted coefficient	0.000 (0.004)	0.004 (0.006)	-0.000 (0.005)	0.007 (0.007)
Weighted av. of mean of DV	0.34	0.26	0.33	0.25
Countries	32	33	30	29
Pos. significant coefficients	7	9	7	8
Neg. significant coefficients	6	6	6	5
Panel B: Secondary education				
Average weighted coefficient	-0.000 (0.001)	-0.001 (0.001)	-0.001 (0.001)	0.000 (0.001)
Weighted av. of mean of DV	0.14	0.09	0.14	0.07
Countries	34	35	32	30
Pos. significant coefficients	5	4	4	4
Neg. significant coefficients	7	7	7	5
Panel C: Employment				
Average weighted coefficient	-0.003 (0.003)	-0.003 (0.002)	-0.004 (0.003)	-0.001 (0.002)
Weighted av. of mean of DV	0.90	0.89	0.89	0.89
Countries	17	20	16	15
Pos. significant coefficients	3	2	2	2
Neg. significant coefficients	2	2	2	0

Average weighted coefficients are derived from a random-effects meta-analysis as described in Section 3. Stars indicate statistical significance (* = 10%, ** = 5%, *** = 1%). The weighted average of the mean of the dependent variable (DV) is calculated using the same weights as for the average weighted coefficients to ensure comparability. n/a = not available. Single country-level coefficients are counted as significant if corresponding p-values are smaller than 0.1.

Table C5: Weighted effects on education and employment (men, core sample): Robustness to error tolerance threshold

Expected error:	5 months	4 months	3 months	2 months	1 month	0 months
Panel A: Primary education						
Average weighted coefficient	0.003 (0.007)	0.003 (0.007)	0.011 (0.010)	0.011 (0.010)	0.024* (0.011)	0.024* (0.011)
Weighted av. of mean of DV	0.21	0.21	0.24	0.24	0.23	0.23
Countries	24	24	16	16	7	7
Pos. significant coefficients	5	5	5	5	4	4
Neg. significant coefficients	5	5	2	2	0	0
Panel B: Secondary education						
Average weighted coefficient	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.002)	-0.001 (0.002)	0.004 (0.003)	0.004 (0.003)
Weighted av. of mean of DV	0.08	0.08	0.03	0.03	0.04	0.04
Countries	25	25	16	16	7	7
Pos. significant coefficients	3	3	2	2	2	2
Neg. significant coefficients	5	5	4	4	1	1
Panel C: Employment						
Average weighted coefficient	-0.002 (0.003)	-0.002 (0.003)	-0.002* (0.001)	-0.002* (0.001)	-0.001 (0.001)	-0.001 (0.001)
Weighted av. of mean of DV	0.89	0.89	0.88	0.88	0.92	0.92
Countries	11	11	8	8	4	4
Pos. significant coefficients	1	1	0	0	0	0
Neg. significant coefficients	0	0	0	0	0	0

Average weighted coefficients are derived from a random-effects meta-analysis as described in Section 3. Stars indicate statistical significance (* = 10%, ** = 5%, *** = 1%). The weighted average of the mean of the dependent variable (DV) is calculated using the same weights as for the average weighted coefficients to ensure comparability. n/a = not available. Single country-level coefficients are counted as significant if corresponding p-values are smaller than 0.1.

Table C6: Weighted effects on education and employment (men, core sample): Robustness to error tolerance threshold (assuming those born in first month of exposure period were not exposed in-utero)

Expected error:	6 months	5 months	4 months	3 months	2 months	1 month	0 months
Panel A: Primary education							
Average weighted coefficient	-0.000 (0.005)	-0.000 (0.005)	0.007 (0.007)	0.007 (0.007)	0.014 (0.008)	0.014 (0.008)	n/a
Weighted av. of mean of DV	0.33	0.33	0.22	0.22	0.24	0.24	n/a
Countries	30	30	21	21	15	15	0
Pos. significant coefficients	7	7	6	6	5	5	n/a
Neg. significant coefficients	6	6	4	4	2	2	n/a
Panel B: Secondary education							
Average weighted coefficient	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	0.001 (0.002)	0.001 (0.002)	n/a
Weighted av. of mean of DV	0.14	0.14	0.09	0.09	0.03	0.03	n/a
Countries	32	32	22	22	15	15	0
Pos. significant coefficients	4	4	3	3	2	2	n/a
Neg. significant coefficients	7	7	4	4	3	3	n/a
Panel C: Employment							
Average weighted coefficient	-0.004 (0.003)	-0.004 (0.003)	-0.003 (0.003)	-0.003 (0.003)	-0.002 (0.001)	-0.002 (0.001)	n/a
Weighted av. of mean of DV	0.89	0.89	0.88	0.88	0.88	0.88	n/a
Countries	16	16	11	11	7	7	0
Pos. significant coefficients	2	2	1	1	0	0	n/a
Neg. significant coefficients	2	2	0	0	0	0	n/a

Average weighted coefficients are derived from a random-effects meta-analysis as described in Section 3. Stars indicate statistical significance (* = 10%, ** = 5%, *** = 1%). The weighted average of the mean of the dependent variable (DV) is calculated using the same weights as for the average weighted coefficients to ensure comparability. n/a = not available. Single country-level coefficients are counted as significant if corresponding p-values are smaller than 0.1.

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