Federal Reserve Bank of Chicago

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REVISED January 24, 2007
WP 2006-23

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Abstract

Recent evidence using compulsory schooling laws as instruments for education suggests that education has a causal effect on mortality (Lleras-Muney, 2005). However, little is known about *how* exactly education affects health. This paper uses compulsory schooling laws to try to identify how education impacts health and to indirectly assess the merit of using these laws to infer the causal effect of education on health. I find that previous Census mortality results are not robust to the inclusion of state-specific time trends but that robust effects of education on general health status can be identified using individual level data in the SIPP. However, the pattern of effects for specific health conditions in the SIPP appears to depart markedly from prominent theories of how education should affect health. I also find that vaccination against smallpox for school age children may account for some of the improvement in health and its association with education. These results raise concerns about using early century compulsory schooling laws to identify the causal effects of education on health.

*I thank Douglas Almond for many helpful discussions and for contributing to some of the ideas in this paper. I also thank participants at the 2006 NBER Spring Health meetings for many helpful comments. I also thank Adriana Lleras-Muney for sharing her computer code, data and for helpful discussions. Similarly I thank Claudia Goldin for sharing her data. The views expressed here do not necessarily reflect those of the Federal Reserve system.

1 Introduction

Social scientists have long been aware that there is a strong association between education levels and health (Kitagawa and Hauser 1973) but much less is known about *how* these factors are connected, and whether the relationship is in fact, causal. As Richard Suzman of the National Institute on Aging recently stated, "Education ...is a particularly powerful factor in both life expectancy and health expectancy, though truthfully, we're not quite sure why." (Lyman 2006). A recent study by Lleras-Muney (2005) provides perhaps the strongest evidence that education has a causal effect on health. Utilizing a more compelling research design than most previous work, Lleras-Muney uses state compulsory school and child labor laws as instruments and finds that increased schooling for those born in the first quarter of the twentieth century, led to dramatic reductions in mortality rates during the 1960s and 1970s. Her IV point estimates imply that an additional year of schooling reduces mortality risk by between 30 and 60 percent.[1] These results were prominently featured in a front page story in the New York Times entitled "A Surprising Secret to a Long Life: Stay in School" (Kolata 2007)

The mechanism by which schooling improves health, however, remains illusive. A variety of theories have been proposed to explain how schooling might improve health. These theories emphasize the role of eduction in affecting various proximate determinants of health. Health determinants leveraged by education include: (i) financial resources, (ii) decision making ability, (iii) time preference. Lleras-Muney (2005) found that adjustment for income or occupation did not alter her IV estimates, and therefore discounted the role of resources behind her findings. Instead, her results appeared more consistent with a role of "critical thinking skills." Such skills may allow one to utilize advances in medical technology (e.g., Glied and Lleras-Muney (2003)) or manage chronic conditions better (e.g. Goldman and

[1] Lleras-Muney uses the Census to estimate ten year mortality rates for synthetic cohorts using state of birth, year of birth and sex. The mean death rate in Lleras-Muney's sample is about 10 percent and her IV estimates are as high as -0.06 implying that a 1 year increase in schooling would lower the death rate by as much as 6 percentage points.

Smith (2002)). These hypothesized mechanisms both reference the ability to obtain and process information, which may be improved through education.[2]

In this paper I examine whether compulsory schooling laws can provide insights into exactly how education may improve health. As part of the analysis I also reassess whether compulsory schooling laws can be used to draw inferences about the causal effects of education on health. As is well known, there is no test of instrument exogeneity in the exactly identified case.[3] I therefore conduct some (necessarily indirect) exercises to explore the validity of compulsory schooling law instrument.

I begin by revisiting the mortality results in Lleras-Muney (2005) by adding significantly more data (e.g. doubling the 1970 Census sample and quintupling the 1980 sample) and employing several robustness checks. The key finding is that the Census mortality results are not robust to the inclusion of state-specific time trends. This raises the concern that the instruments might be picking up smooth cohort trends in educational attainment rather than discrete increases induced by more stringent compulsory schooling laws.[4] I also find that the effects of education on mortality appear to be driven primarily by the earliest cohorts (born 1901-1912) during the 1960 to 1970 period.

In a second exercise I use a new microdataset: the Survey of Income and Program Participation (SIPP). The SIPP directly queries the health of each respondent, while the Census must be aggregated by synthetic cohort (by state of birth, sex and year of birth) in order to impute mortality by using estimates of "missing" individuals. The latter estimates arguably could be due in part, to a selection effect if less educated individuals are also less likely to be captured by the Census over time. In contrast to the synthetic cohort approach, with the SIPP we can be sure that those who were “treated" by the compulsory school laws are indeed the same individuals registering the change in health. Moreover, as will be

[2] In addition Lleras-Muney could not rule out possibility that education lowered individual discount rates, and thereby lead to healthier behaviors. See (Grossman 2005).

[3] And the IV estimator is inconsistent when the instrument is endogenous.

[4] I also discovered a coding error that generated some erroneous OLS and IV estimates in Lleras-Muney (2005) that are quantitatively meaningful. I discuss this in more detail in section 4

shown later, the SIPP microdata provides relatively strong statistical power in assessing the relationship between education and health.

Using the SIPP with the same IV strategy, I find large and statistically significant effects of education on health that are robust to the inclusion of state-specific time trends. In particular, I document that the summary health measure from the SIPP – self-reported health – also improves with changes in schooling (as induced by compulsory school laws). So while there may be some uncertainty about the robustness of the Census mortality results based on group level data, the evidence is quite strong with the individual level data using health status as an outcome. This addresses the concern about whether the health effects were due to idiosyncratic changes in the laws.

I then use the SIPP to examine a broad range of health measures in order to isolate which specific health conditions responded to education improvements induced by compulsory schooling laws. This is potentially useful for understanding whether or not the use of compulsory schooling laws as instruments produce sensible results that accord with the leading hypothesized mechanisms for how education affects health. If for example, all of the health effects are concentrated in only one or two health conditions that are unrelated to improvements in medical technology or decision making ability, it might cast some doubt on the validity of the instruments. If on the other hand, we were to assume that the instruments were valid, the results ought to be informative about the critical question of how exactly higher education levels lead to better health.

In fact, I find that among the nineteen health conditions examined, only four show significant declines in incidence due to education. What is striking is the absence of effects among the many health conditions where decision-making ability is believed pivotal. For example, no effect is found for chronic diseases such as arthritis, cancer, heart disease, lung disease, or stroke incidence. The sole exception is diabetes, where the ability to maintain a treatment regime is especially important. Moreover, education is found to increase the likelihood of hypertension and kidney problems: conditions for which self-management and recent technological advance appear to be important. The lack of any effects across most outcomes also suggests that channel underlying the connection between education and health is probably not due to financial resources or unobserved time preferences which would tend to improve health across the board.

What then accounts for the positive relationship between schooling and self-reported health using these instruments? Surprisingly, health conditions where decision-making appears comparatively unimportant underly the relationship. Sensory functions – in particular hearing and vision – exhibit large and significant impacts when using compulsory school laws as instruments. I also find that education reduces stiffness or deformity of the limbs, back problems, senility, and improves the ability to speak in the IV specifications. This pattern of effects suggests that either: (i) the mechanisms by which schooling impacts health depart markedly from those hypothesized, or (ii) the use of compulsory school laws as an instrument may be suspect.

An important caveat is that that with the SIPP I am using a sample of individuals who have survived into their later years (between the ages of 59 and 83) where presumably there has already been considerable positive selection on education and health. Of course among this more selected sample we might expect there to be a bias *against* detecting any health effects, so the finding of a strong effect on overall health status might be considered surprising. Nonetheless the age of the sample raises questions about the extent to which these results generalize to the broader population.

Finally, I hypothesize that schooling law changes may be correlated with other contemporaneous policies either inside or outside of schools that improved long-term health. During the early period of the twentieth century there were fairly dramatic improvements in public health measures and large declines in concurrent mortality.[5] There was also a recognition that compulsory schooling was useless if students were mentally or phsycially unfit to attend school. This led to other reforms in the schools that were designed to

[5] Cutler and Miller (2004), for example argue that the introduction of clean water technologies early in the century can account for half of the reduction in mortality in large cities during that time.

improve children's wellbeing. In a third exercise I examine one potential factor that might account for the observed relationship between schooling laws and improved health, smallpox vaccination. The vaccination of children against smallpox as a requirement for school entry is likely to be correlated with years of education and plausibly exerts effects on adult health outcomes. Data on smallpox incidence and vaccination rates are thin, preventing definitive conclusions. Nevertheless, I find that states that appeared to have more stringent vaccination requirements for school entry experienced most of the gains in long-term health generated by compulsory schooling laws. The fact that survivors of smallpox are known to suffer from compromised vision, hearing and speaking provides some additional suggestive evidence of a possible link between vaccination requirements and the estimated long-term health effects of compulsory schooling laws.

The remainder of the paper is organized as follows. In section 2 I review the relevant literature, in section 3 the Census and SIPP data are described the econometric models are shown, in section 4 the baseline results are presented, in section 5 I consider the possible role of smallpox and vaccination in schools and in section 6 I conclude.

2 Literature Review

It has been over thirty years since Grossman published his seminal economic model of health determination (Grossman 1972). This model includes the assumption that education increases the efficiency of health production. And while Grossman's conceptual framework has served as the "work horse" model for applied work in health economics, little is understood about how or what kinds of education enable the production of health.[6] For example, in 2003, the National Institutes of Health solicited (quite general) research proposals on the "Pathways Linking Education to Health."

This RFA sought "validation of specific measures of abilities crucial to educational attain-

[6] Grossman (2005) noted that "extensive reviews of the literature [concluded that] that years of formal schooling completed is the most important correlate of good health.

ment, such as level of cognitive or language skills" that improved health, and even cautioned that: "The association or pathway between formal education and either important health behaviors or diseases may not be causal. Instead it may reflect the influence of confounding or co-existing determinants or may be bi-directional."(NIH 2003)

Recent years have witnessed an upsurge of interest in education's role in determining health. In one widely-cited paper, Goldman and Smith (2002) noted that more educated patients may manage chronic conditions better. Those with more schooling adhere more closely to treatment regimens for HIV infection and diabetes, which can be fairly complex. For such conditions, the ability to form independent judgements and comprehend treatments is important, and apparently is fostered by schooling. Accordingly, "self-maintenance is an important reason for the very steep SES gradient in health outcomes" (Goldman and Smith (2002):10934).

Glied and Lleras-Muney (2003) looked at health conditions experiencing more rapid technological change, finding that more educated respondents faired better. They argued that "the most educated make the best initial use of new information about different aspects of health" permitting them to respond more adeptly to evolving medical technologies. They noted that no consensus measure existed for assessing the pace of innovation in health. They therefore consider several measures, including the change in mortality rates for specific conditions from 1986 to 1995 and the number of patents issued for particular conditions. They found that education gradients were steeper for diseases that were more innovative by these measures.

A growing literature has also tried to examine whether the education gradient in health is causal by using instrumental variables. Reviews of these studies may be found in (Lleras-Muney 2005) and (Grossman 2005). While these studies typically find an effect of more education leading to better health, in most cases it is questionable whether the instruments are truly exogenous.[7] In contrast, the use of changes in compulsory schooling laws appears

[7] For example Leigh and Dhir (1997) use parent schooling, parent income and state of residence as instruments, all of which could plausibly affect long-term health independently of their effects through

to be a more compelling instrument choice since it is more plausibly exogenous than instruments used in prior work. Previous studies also typically have looked at just one or two health outcomes and have not systematically compared the effects across a range of health outcomes to distinguish between competing theories of *how* education affects health.

3 Data and Methodology

3.1 Mortality Data and Econometric Specification

I begin by describing the procedure used to estimate the effects of education on mortality in Lleras-Muney (2005). This will provide the basic framework for extending the analysis to examining other health outcomes in the SIPP and for expanding the analysis along other dimensions. I briefly describe the approach here, for a more detailed discussion that includes alternative estimation strategies see Lleras-Muney (2005).[8] The key idea is that in the absence of a large sample tracking individuals over their entire lifetime, synthetic cohorts are constructed using Census data. With the Census, we know age, completed education and state of birth which allows us to infer the compulsory schooling laws that affects each cohort in each state of birth. Mortality can be measured by tracking population counts of particular groups across Census years. The mortality rate at time t for cohort $c,$ of gender, g, born in state s, (M_{cgst}) is simply measured as the percentage decline in the population count (N_{cgst}) within these cells over the subsequent ten years:

$$M_{cgst} = \frac{N_{cgst} - N_{cgst+1}}{N_{cgst}} \tag{1}$$

schooling.

[8] Lleras-Muney also uses several other approaches. She estimates the model at the individual level using data from the National Health and Nutrition Examination Survey (NHNES). This is largely for comparative purposes since the sample is too small to estimate statistically significant effects using IV. She also considers Wald estimators and introduces a a "mixed" two stage least squares approach using individual data in the first stage but aggregate data in the second satge. The latter two approaches produce roughly similar estimates to the aggregate IV estimates which are modeled here.

The mortality rate for each cell is then modeled as follows:

$$M_{cgst} = a + E_{cgst}\pi + W_{cs}\delta + \gamma_c + \alpha_s + \theta_{cr} + \tau_t + d + \varepsilon_{cgst} \quad (2)$$

where E_{cgst} is the average education level for that cell at time t, W_{cs}, measures a set of cohort and state specific controls measured at age 14 intended to capture differences in other potential early life determinants of mortality (e.g. manufacturing share of employment, doctors per capita). The model also includes a set of cohort dummies γ_c, state of birth dummies α_s, interactions between cohort and region of birth, θ_{cr},a female dummy d and year dummies, τ_t.

I construct two datasets for the analysis. The first attempts to replicate Lleras-Muney point estimates and uses the same 1 percent ipums samples drawn from the 1960, 1970 and 1980 Censuses that are produced by the Minnesota Population Center.[9] The second estimation sample replaces the 1970 1 percent sample with a 2 percent sample that combines the form1 and form 2 "state" 1 percent samples.[10] For 1980 I use a 5 percent sample. In addition, I also add the 5 percent samples for 1990 and 2000. All of the Censuses are scaled appropriately to produce population estimates that correspond to N_{cgst}[11].

Following Lleras-Muney I restrict the anlaysis to cohorts born between 1901 and 1925. I also follow her sample restrictions to exclude immigrants, blacks, and to topcode years of education at 18 starting in 1980. For the expanded samples I also exclude cases where age, state of birth and education are imputed by the Census Bureau. The descriptive statistics for both samples are shown in Table 1. It is worth noting that the death rate for the 1970 to

[9] Ruggles et al. [2004].

[10] Unfortunately, combining any of the other four 1 percent samples that are available for 1970 would lead to geographically unrepresentative samples.

[11] The 1960, 1970 and 1980 samples are self-weighting samples so the raw population counts can simply be scaled up by multiplying by 100, 50, and 20. The 1990 and 2000 samples require weights to produce representative estimates of the population. We found that using the self-weighting 1 percent samples for 1990 and 2000 instead of the 5 percent samples had little effect on the point estimates but increased the standard errors.

1980 period is quite a bit larger with the expanded sample but that the standard deviation is about 20 percent lower. There are now also 5 additional cells that had missing data when using just the 1 percent samples. The death rates for the 1980-90 and 1990-2000 periods are much higher due to the fact that I follow these same cohorts when they are much older. Figure 1 plots the death rates by age for each Census year. This highlights the importance of controlling for age in the specifications which is done by adding polynomials in age to the models.

One straightforward way to estimate π in (2) would be through weighted least squares (WLS), with the weights corresponding to the population represented by each cell. However, that this would produce a biased estimate due to omitted variables. Any number of factors could plausibly be associated with both higher education and lower mortality even at the group level. Therefore, two stage least squares is used where in the first stage education is instrumented with the set of compulsory schooling laws, CL_{cs}, in place for each cohort and state of birth:

$$E_{cgst} = b + CL_{cs}\rho + X_{cgst}\beta_{iv} + W_{cs}\delta_{iv} + \gamma_{iv,c} + \alpha_{iv,s} + \theta_{iv,cr} + \tau_t + u_{cgst} \tag{3}$$

The instruments for the compulsory schooling laws are constructed in the following way. The variable *childcom* measures the minimum required age for work minus the maximum age required for a child to enter school, by state of birth and by the year the cohort is age 14. This variable takes on one of eight values (that range from 0 to 10). A set of indicator variables (excluding the 0 category) are used as instruments. In addition there is an indicator for whether school continuation laws were in place in that state. These laws required workers of school age to continue school part-time. For comparability, I use the same dataset as Lleras-Muney (2005).[12] In addition to a more detailed description of these variables in Lleras-Muney (2005), there is also an extended discussion of these measures and their appropriateness as instruments in Lleras-Muney (2001). The variables contained in

[12] Downloaded from Lleras-Muney's website.

W_{cs} are the corrected versions of those used by Lleras-Muney (2005).

I also experiment with a second set of data independently collected by Goldin and Katz (Goldin and Katz 2003). Goldin and Katz carefully compared their series with the Lleras-Muney data and the data collected by Acemoglu and Angrist (2000) and rectified differences wherever possible. Since the Goldin and Katz data go back further in time it is possible to match all of the cohorts to the school entry age laws in effect when the cohorts were younger than 14. I use this data to measure the required age for school entry when the cohorts were likely to be at age 8 instead of age 14. In principle, incorporating this data should provide a better measure of the total years of compulsory schooling.

3.2 Health Microdata and Specifications

The second sample is constructed by pooling individuals across various panels of the Survey of Income and Program Participation (SIPP) during the 1980s and 1990s.[13] Because participation in many programs is closely related to an individual's health and disability status, the SIPP routinely collects information on health and medical conditions. The SIPP is also ideally suited for this analysis because it contains the state of birth of all sample members allowing us to implement the IV strategy of using compulsory schooling laws during childhood.

One useful outcome is self-reported health (SRH). SRH is on a 1 to 5 scale where 1 is "excellent", 2 is "very good", 3 is "good", 4 is "fair" and 5 is "poor". SRH has been found to be an excellent predictor of mortality and changes in functional abilities among the elderly.(Case, Lubotsky, and Paxson 2002). I experiment with this measure in a few ways. First it is simply used as a continuous variable. Second, I use indicators for being in poor health or, fair or poor health. Finally I use the health utility scale that scales the differences between the categories based on a health model using the National Health Interview Survey.[14]

[13] This includes the 1984, 1986-1988, 1990-1993 and 1996 panels.

[14] See (Johnson and Schoeni 2005) and the citations there for a discussion of this approach.

A few other general outcomes are also examined. These include whether the individual was hospitalized during the past year, the number of times she was hospitalized, the total number of nights spent in the hospital and the number of days spent in bed in the last four months.

There are also a set of questions dealing with functional activities, activities of daily living and instrumental activities of daily living.[15] I assembled a common set of questions that were consistently asked across surveys. These include whether the individual has "difficulty" with seeing, hearing, speech, lifting, walking, climbing stairs and, whether the person can perform any of these activities "at all". In addition there is information on whether individuals have difficulty getting around inside the house, going outside of the house or getting in or out of bed, and whether they need the assistance of others for these activities.

For a subset of individuals who report limited abilities in certain tasks or who have been classified as having a work disability, detailed information is collected on a number of very specific health conditions including: arthritis or rheumatism; back or spine problems; blindness or vision problems; cancer; deafness or serious trouble hearing; diabetes; heart trouble; hernia; high blood pressure (hypertension); kidney stones or chronic kidney trouble; mental illness; missing limbs; lung problems; paralysis; senility/dementia/Alzheimer's disease; stiffness or deformity of limbs; stomach trouble; stroke; thyroid trouble or goiter; tumors (cyst or growth); or other.[16] Since the specific health ailments are only asked of specific subsamples, they probably only pick up on the most severe cases. Even though, many of

[15] These measures are derived from specific codes of the International Classification of Impairments, Disabilities and Handicaps (ICIDH).

[16] I pool responses from the 1984, 1990-93 and 1996 SIPPs in order to maximize sample size. Unfortunately different criteria were used across the SIPP survey years to select the subsamples for which specific health conditions were asked. For example, in 1996 the health conditions were asked of those who reported being in fair or poor health. I found that it was important to combine all of the subsamples in all of the years in order to have enough power to identify effects. There are also an additional set of 10 outcomes that are not used because they were not available in the 1984 SIPP. Experimentation with a smaller sample suggests that the conclusions are not altered by dropping these other outcomes

our sample individuals are not actually asked about these specific health conditions we still include them in the estimation sample so that our sample is not a selected sample of only those in poor health. The summary statistics for this data are shown in Table 2.

Since most of the outcomes in the SIPP are indicator variables I now use Two Stage Conditional Maximum Likelihood or 2SCML (Rivers and Vuong 1988) rather than IV.[17] Rivers and Vuong show that 2SCML has desirable statistical properties, is easy to implement and produces a simple test for exogeneity. I continue to use IV for the few continuous dependent variables. Also all of the analysis is now done using individual level data. The statistical model is similar to (2) only now I use the latent variable framework:

$$y_{it}^{*} = a + E_i\pi + X_i\beta + W_{cs}\delta + \gamma_c + \alpha_s + trend_s + \tau_t + \varepsilon_{it} \tag{4}$$

$$y_{it} = 1 \text{ if } \ y_{it}^{*} > 0, \quad y_{it} = 0 \text{ if } \ y_{it}^{*} < 0 \tag{5}$$

In the first stage, I run a similar regression as before:

$$E_i\pi = b + CL_{cs}\rho + X_i\beta + W_{cs}\delta + \gamma_c + \alpha_s + trend_s + \tau_t + \varepsilon_{it} \tag{6}$$

To implement 2SCML, I use the predicted residuals from (6), $\widehat{\varepsilon}_{it}$, and include it as an additional right hand side variable (along with the actual value of E_i) when running the second stage probit. For comparability I use the same sample restrictions and covariates as Lleras-Muney with only a few exceptions. Unlike Lleras-Muney I include a quadratic in age. In addition state-specific cohort trends are used to address concerns that region of birth interacted with cohort may not adequately control for state-specific factors that are smoothly changing over time.[18]

[17] I thank Jay Bhattacharya for this suggestion. In a previous version of the paper I found very similar results using two stage least squares for the dichtomous outcomes.

[18] I generally found that the IV results were larger and more significant when using the state trends than when using region of birth interacted with cohort. The OLS results were virtually identical under either

4 Baseline Results

4.1 Mortality

This section begins by describing the replication of Lleras-Muney (2005) and the discovery of a coding error concerning the appropriate base period. Using the correct base period increases the point estimates: the new WLS estimate is approximately doubled and the IV estimate is 50% higher than reported in Lleras-Muney (2005). I then expand the Census data by: (i) expanding beyond the 1% sample to the 2% and 5% IPUMS samples available for 1970 and 1980, respectively, and (ii) incorporate the 1990 and 2000 Census data in the mortality analysis.

4.1.1 Replication and Correction

For the mortality analysis I start with the same sample as Lleras-Muney (2005) I have an identical number of individuals (814,805) drawn from the 1960 and 1970 Census and match nearly all the summary statistics in her Table 1 Nevertheless I find some large differences when implementing WLS or IV at the cell level. This shown in Table 3. The first two columns show the WLS and IV estimates from Lleras-Muney while columns 3 and 4 show my estimates. Compared to her WLS estimate of -.017, I obtain a coefficient of -.036. The difference in the estimates is statistically significant. For IV, once again I obtain a much larger estimate (-.072) than her estimate of (-.051).

After some experimentation I speculated that Lleras-Muney did not use education calculated in the base period, t, but instead calculated education in period $t+1$. If I use education calculated in $t+1$, then my estimates are much closer to hers. These are shown in columns 5 and 6. After graciously providing her computer code I confirmed with Lleras-Muney that this was indeed the case and she has since written an errata (Lleras-Muney 2006) providing corrected estimates.[19] The problem with using education in the later period is that the

specification.

[19] The errata (Lleras-Muney 2006) reports an IV coefficient of -0.063 (0.024) compared to the estimate of

sample has already experienced selective mortality based on education.

The discrepancy between the results are not only statistically meaningful but are quantitatively important. Taken at face value, the "corrected" IV result implies that an extra year of schooling reduces the likelihood of dying over the next ten years by more than 7 percentage points. In the sample, the mean death rate is only about 10.6 percent. This suggests that one more year of schooling lowers mortality risk by nearly 70 percent –a result that is perhaps implausibly large.

4.1.2 Expansion of the Census Sample

In Table 4, I show how the results change as the sample is enlarged and the specifications are modified. I begin by just focusing attention on the first two columns showing the WLS and IV estimates. In panel A I isolate the effects of using larger samples for 1970 and 1980. Row 1 repeats the results from Table 3. In row 2 I find that the WLS estimate rises to -0.045 and that the IV estimates drop considerably to -0.043. The greater precision is evident in the standard error for the IV estimate which declines by about 25 percent. In rows 3 and 4 I control for age and find that this lowers the WLS estimates a little and increases the IV estimates a little. In row 5 I drop the region of birth interactions with cohort and instead use state specific linear (cohort) trends. This raises the WLS estimate but I now find that IV coefficient is significantly lower and is no longer statistically significant at conventional levels.

In panel B I add data from the 5 percent samples of the 1990 and 2000 Censuses. With this larger dataset I construct death rates over four ten year periods and therefore follow cohorts over a longer period of time with a considerably larger sample. Given that the sample also tracks the cohorts later in life when mortality rates are much higher, the age controls are essential. I use a cubic in age although I find that the results are not very sensitive to the choice of the polynomial. Since medical technology and other health-related factors might change over time, I have also interacted the cubic in age with the Census

-0.072 reported here. I was unable to resolve this difference.

year. In my preferred specification (row 6) I now find that both the WLS and IV estimates are about -0.035 which appear to be much more plausible. With this larger sample the inclusion of state specific cohort trends again results in a point estimate that is much smaller in magnitude (-0.02) and not statistically distinguishable from zero (row 7).

The third column shows the effects of using the Goldin and Katz data for constructing the instruments. For most specifications in panels A and B they produce similar estimates as the baseline IV results although the standard errors are a bit higher. It is worth noting that with the Goldin and Katz data the inclusion of state specific cohort trends lowers the size of the point estimates even more dramatically and also yields the same conclusion, that the estimates are not statistically different from zero.

4.1.3 Effects by Subgroups

In the remaining panels of Table 4 I examine how the effects vary by year, age and cohort. In panel C I separately estimate the education coefficient for each Census year. Since the specification includes a full set of cohort dummies these are equivalent to age controls when using a single Census year. Although the WLS estimates are significant in all years they peak in 1970 at -0.061 and drop to only -0.015 by 1990. The IV estimates have large standard errors so they are likely to be imprecisely estimated. Nonetheless the estimates are large only for 1960 and are essentially zero for 1980 and 1990. This is true whether I use the Lleras-Muney data or the Goldin Katz data. In panel D I stratify the sample by three age ranges: 35-55, 55-65 and 65-89. Here I observe different patterns across the three columns making it difficult to interpret the estimates. The WLS and IV estimates from column 2 suggest that the largest effect may be for those aged 55 to 64, while the IV estimates with the Goldin Katz data suggest the opposite. Given the imprecision of the estimates I cannot draw any meaningful inferences regarding the age pattern

Panel E however, provides a striking result that appears to be consistent across the two IV specifications. It appears that the entire effect of education on mortality arising from compulsory schooling laws is due to cohorts born from 1901 to 1912 who constitute just over 40 percent of the sample. In fact for those born from 1913 to 1925, the point estimate is actually positive in both column 2 and column 3. Using the Lleras-Muney data, the estimate is 0.035 and is significant at the 7 percent level. These results taken as a whole suggest that upon closer inspection, the results from Lleras-Muney are driven by cohorts born very early in the century and their mortality experience during the 1960-1970 period. One possible explanation could be that the effect of education stayed roughly constant but that compulsory schooling laws had its biggest bite for those born earlier in the century. However, I have run the first stage regressions by these cohort groupings and found that the partial F-statistics on the instruments are similar for both cohorts when using the Lleras-Muney data and are actually higher for the 1913 to 1925 cohorts when using the Goldin Katz data. This suggests that the schooling laws may actually have been more binding for the later cohorts casting doubt on this alternate explanation. In other estimates that are not shown in the table I found no statistically significant difference between men and women although the point estimates were larger (in absolute value) for men using the Lleras-Muney data and very imprecisely estimated using the Goldin Katz data.

4.2 Health Outcomes from the SIPP

In Table 5 the results using the microdata on health outcomes using the SIPP are presented. The first column shows the effects of education using a simple probit (or OLS) which does not account for endogeneity. The second column presents the 2SCML (or IV) estimates using the compulsory schooling laws as instruments. Given the possible effects of education on mortality and the fact that outcomes in the SIPP are not observed until at least 1984, one might not expect any remaining health effects to be apparent. As it turns out I do find significant effects using the instruments for several broad health outcomes. The first row shows that self reported health measured as a continuous variable is affected by education. The IV estimate of -0.23 is more than twice the OLS estimate (-0.09). In column 4 using a Hausman test one can reject that the OLS and IV coefficients are the same at the 7 percent level. Translating SRH into a health index on a 1 to 100 scale following Johnson and Schoeni's (2005) approach, the IV estimate implies that an increase in schooling by one year improves the health index by 4.5 points or about 7 percent evaluated at the mean (column 3). I also estimate that the probability of being in fair or poor health is reduced by 8.2 percentage points with an additional year of schooling –a fairly large effect that is statistically different from the naive probit at the 18 percent signficance level. I do not find, however, that any of the measures of hospitalization or days spent in bed are significant when accounting for endogeneity.

Looking across a variety of measures of physical functional abilities, I find that while all of the naive probit estimates are signficiant and of the expected sign, the two stage estimates are typically not significant. Given the large health effects discussed above it is striking that those who have an additional year of schooling due to compulsory schooling laws are no more likely to have trouble lifting, walking, climbing stairs, getting around the house, getting around inside the house or getting into or out of bed. In fact for many of these outcomes the coefficients are actually positive! On the other hand, those with greater schooling associated with compulsory schooling laws are dramatically less likely to experience problems with vision, hearing or speaking. In almost all of these cases the differences between the simple probit and the 2SCML estimates are very large and statistically different at about the 10 percent level. For example, the 2SCML estimates imply that an additional year of schooling reduces the probability of having trouble "seeing" by 5.6 percentage points. In this sample the mean rate of this health outcome is 13.6 percent. These results might suggest that the channel by which general health is compromised for those with less schooling, may be related to sensory functions.

The next set of results estimate the incidence of specific health conditions. Recall that these conditions are only identified for subsets of individuals and that the screening criteria has changed across SIPP survey years. Also recall that all individuals are included regardless of whether they were screened for this question so as to avoid using a selected sample of only those in poor health. Generally, the underlying health conditions were only asked of individuals who reported particular kinds of activity limitations, reported having a work disability or reported being in fair or poor health. This is captured by the variable "difficulty" which, not surprisingly, is signficiant under both probit and 2SCML. When I turn to the estimated likelihood of having one of the underlying health conditions, the probit estimates once again are significant in every case. The 2SCML estimates, however, are only negative and signficant for four outcomes: back or spine problems; stiffness or deformity of a limb; diabetes and senility/dementia/Alzheimer's disease. It is important to point out that "trouble seeing", "trouble hearing" and "trouble speaking" were never used as a screening criteria for asking about an underlying health condition. This likely explains why blindness and deafness are not significant with the subsamples.

Another interesting result is that both kidney problems and hypertension appear to *positively* associated with more schooling. This is especially notable because these are two outcomes for which self-management and recent technological advance appear to be especially important. According to Appendix Table B of Glied and Lleras-Muney (2003), treatment of kidney infections experienced substantial innovation. Among the 56 causes of death, it experienced the fastest decline in age-adjusted mortality from 1986 to 1995 - falling at more than nine percent per year (Glied and Lleras-Muney (2003)):8, Appendix Table B). Accordingly, a steep (negative) gradient between education and kidney disease would presumably be expected. It is therefore of note that the 2SCML specification finds an *increase* in the incidence of kidney problems among those with high education. Treatment of diabetes is “often considered the prototype for chronic disease management." Goldman and Smith (2002). Our findings, which analyze a broad range of health conditions and chronic diseases, would suggest that insofar as the formal schooling is concerned, diabetes appears to be an exception. In the SIPP data, only diabetes enters enters in the expected direction – i.e. increases in schooling appear to reduce diabetes incidence. An alternate explanation for the diabetes result could be that states that had higher compulsory schooling levels also promoted nutritional policies that might have reduced adult onset of diabetes. Overall, however, one conclusion that may be drawn from this table is that there is little support for the "decision-making" hypothesis.

It is also worth noting that explanations for the link between education and health that focus on resources (e.g. income, occupation) or unobserved time preferences do not appear to be consistent with these results. These explanations would likely imply that *all* outcomes ought to be affected not just a few.

The major caveat to this analysis is that we observe individuals only if they have survived into the 1980s and 1990s when they are anywhere between the ages of 59 and 83 This sample is almost certainly positively selected on education and health, making it unclear how generalizable these results are. I suspect that due to this selection the results are biased against finding any effects of education on improving health, making it still surprising why there are very large negative coefficients on the incidence of several negative health outcomes.

5 Smallpox Vaccination

5.1 Alternative Explanations

The results thus far suggest present something of a puzzle as to exactly how compulsory schooling laws early in the twentieth century led to improved long-term health status. While the results cast doubt on the traditional explanations offered in the literature of how education improves health the results do not appear to point to any obvious alternative explanation. One general hypothesis worth considering is that schools served as an important place for implementing a variety of policies that may have impacted both education and health directly. It could be that states and cities during this period were introducing many reforms contemporaneously and schools were one obvious target for these reforms.

In fact it was noted at the time that it was pointless to force kids to attend schools if they were unable to learn. In 1904, Robert Hunter wrote in the book *Poverty*: "There must be thousands -very likely sixty or seventy thousand children-in New York City alone who often arrive at school hungry and unfitted to do well the work required. It is utter folly, from the point of view of learning, to have a compulsory school law which compels children, in that weak physical and mental state which results from poverty, to drag themselves to school and to sit at their desks, day in and day out, for several years, learning little or nothing." In fact in response to this situation Philadelphia, Boston, Milwaukee, New York, Cleveland, Cincinnati and St. Louis all began large scale programs to provide food in public schools during the 1900s and 1910's (Gunderson 1971).

It seems plausible, then that coincident with the enactment of compulsory schooling laws there were likely many efforts (legislative or otherwise) to improve the general condition of children. In this section I pursue one specific alternative hypothesis that might explain some of the findings. Specifically, I examine whether the association between compulsory schooling laws and health may have been due in part, to early century school requirements concerning vaccination against smallpox.

5.2 Background on Smallpox

Before Edward Jenner invented the first vaccine in 1797, smallpox was a widespread and brutal disease killing about 400,000 Europeans a year with survivors accounting for about one-third of all cases of blindness (Henderson and Moss 1999). Smallpox was especially concentrated among children, in the early 19th century smallpox accounted for one-third of the deaths of all children(George Palmer and Ingen 1930). More than a century after the development of the vaccine, smallpox remained a deadly threat in the United States. A report in the *New England Journal of Medicine* in 1930 showed that between 1919 and 1928 there were more than half a million cases of smallpox in the US and argued that "...the United States remains now ...the most smallpox ridden country in the world bar possibly

China, India and (doubtfully) Russia."

In addition to blindness, survivors of smallpox are also known to have a higher rate of encephalitis[20] (inflammation of the brain). Although encephalitis is relatively rare, milder forms of the condition are likely to go unreported.[21] Symptoms of encephalitis include problems with speech, hearing and double vision.[21] This suggests that vaccination against smallpox in schools may have reduced the incidence of compromised sensory functions as we find in our SIPP sample.

5.3 Vaccination in schools

States began to require vaccination against smallpox in schools beginning in the late 19th century (Hanlon 1969). I have been able to compile information concerning state laws regarding school vaccination for the years 1915, 1921, 1926 and 1941. In the first snapshot in 1915, fourteen states had requirements for vaccination. In the other three years I found no cases of any additional states requiring vaccination for schools. Similarly I found no cases of any states repealing these laws. Therefore, I am unable to construct an analogous panel design as employed by Lleras-Muney for compulsory schooling laws since there is no variation over time.

I also assembled data on states who had laws authorizing the use of vaccination in the case of outbreaks but found that these laws relied critically on enforcement. There were also a few cases where states changed laws regarding the prohibition of vaccination in schools but it is doubtful that these law changes have enough power to identify effects in the samples used in thsi study.

Fortunately, a 1930 White House sponsored report on the state of young children's health does contain detailed data on young children's vaccination rates by age for 156 of the largest cities (George Palmer and Ingen 1930). For the time period this was an impressive data collection effort where information was collected from around 3000 doctors and other

[20] See AMA (1999)

[21] See http://www.ninds.nih.gov/disorders/encephalitis_meningitis/detail_encephalitis_meningitis.htm

health providers on the frequency of health exams and dental exams in addition to rates of vaccination against smallpox and immunization against diptheria. The data on smallpox vaccination rates was aggregated to the state level. This is displayed in Table 6. What is striking is how the vaccination rate jumps sharply from age four to age five in many states as children prepare for school entry. Although some of the richer states in the Northeast like New York have sizable jumps, there is a great deal of variation even within regions. For example, Colorado, Georgia and Kentucky are among the states with largest increases in vaccination between age four and age five. These data also illustrate the potential pitfall of using actual state laws since some of the states that ostensibly required vaccination did not exhibit big increases (e.g. Arkansas, South Carolina) while other states that did not legally require vaccination, in practice, exhibited large increases in vaccination by school age.

Although there is only vaccination rate data for one year, 1930, I use this data to test the extent to which the health effects of education may be operating through differences in vaccination policy. Specifically, I consider how different the health effects of education are for states that have stringent vaccination laws versus those that don't I assume that the stringency of school vaccination requirements can be proxied by the change in the vaccination rate from age four to age five. Obviously, this approach is only ideal for the youngest cohort (born in 1925), who would have turned five in 1930. I analyze this first for the baseline mortality sample (row 6 in Table 4). The sample is split by states with a change in vaccination rate of more than 10 percentage points (the median change) and those with a change that is 10 points or fewer.

The results are shown in Table 7. I find that the IV estimates of the effects of schooling on mortality are statistically significant only in the states with large increases in vaccination by school age and that the IV coefficients are of the wrong sign in the states with less stringent vaccination requirements. I experimented with randomly splitting the sample 100 times and found that the odds of finding an equivalent result by chance are only about 15 percent.

I then performed the same exercise with the SIPP sample looking at the outcomes that

I found to be significant for the full sample. In this sample the results are more mixed. I show a representative set of results from the SIPP in Table 7 For self reported health, the estimates are actually larger and more statistically significant in the states with relatively less stringent vaccination requirements. However, the effect on being in poor health is only apparent in the high vaccination states. Since poor health is a strong predictor of mortality this appears to be consistent with ther mortality finding. Most of the other estimates by subsample are too noisy to say much of anything but it does appear that hearing is strongly affected in the states with more stringent school vaccination requirements. Since I do not have a time series on vaccination rates and given the relative bluntness of the approach I only claim that these results are suggestive of a possible mechanism relating compulsory schooling laws and long-term health that operates through school vaccination.

6 Conclusion

This paper expands upon the growing literature that attempts to identify whether there is a causal effect of education on health by also considering how education might affect health. I closely examine the effects of education induced by compulsory schooling laws early in the twentieth century on long-term health using several approaches. First I revisit the results in Lleras-Muney (2005) by expanding the Census sample and employing a variety of robustness checks. The main finding is that the effects of education on mortality induced by changes in compulsory school laws are not robust to including state specific time trends. I also find that all of the effects are for cohorts born between 1901 and 1912 and their mortality experience during the 1960s.

Second, I use the SIPP to identify not only general health effects but also specific health outcomes that were induced by changes in state compulsory schooling laws to see if these outcomes correspond to our existing theories of *how* education affects schooling. The results suggest that there is a large effect of education on general health status arising from compulsory schooling laws that is robust to state time trends. However, I find that with the sole exception of diabetes none of the other specific health conditions that are associated with education (e.g. vision, hearing, speaking ability, back problems, deformities, senility) correspond to the leading theories of how education improves health (e.g. technological improvements, better decision-making, higher income). This suggests that either our theories are incorrect or that the compulsory schooling laws are suspect instruments. An important caveat, however, is that the SIPP analysis uses a sample of older individuals who are almost surely positively selected on education and health. While this likely makes it more difficult to detect effects of education on improved health it also raises questions as to how generalizable these results are.

Third, I look at one specific alternate hypothesis of how state-level compulsory schooling laws might have influenced long-term health, namely through requirements for smallpox vaccination as a condition for school entry. I stratify the samples by states with stringent versus nonstringent vaccination requirements and find that all of the effects of education on mortality and poor health status were registered in states with stringent vaccination requirements. This provides some suggestive evidence that smallpox vaccination may account for some of the link between education and health induced by compulsory schooling laws. It is also worth noting that survivors of smallpox are known to suffer from compromised vision, speaking and hearing which are among the few effects that we detected in our IV results with the SIPP. I conclude from these exercises there is reason to be concerned about whether compulsory schooling laws can be used as instruments to draw meaningful inferences about the causal effects of education on long-term health. Instead it could well be that either other school-based reforms directly impacted long-term health or that other reforms with long term impacts took place at the same time that compulsory schooling requirements became more stringent. In any event, the results suggest that even if there is a causal effect of education on health there is still a great deal of uncertainty about how education improves health that should remain an important topic for further research.

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Table 1: Summary Statistics for IPUMs samples

Variables	1960-1980 1% only			1960-2000 1%, 2% or 5%		
	Mean	Std. dev.	N	Mean	Std. dev.	N
Ten Year death rates						
overall	0.108	0.136	4792	0.213	0.173	8636
1960-70	0.110	0.119	2395	0.113	0.105	2397
1970-80	0.105	0.152	2397	0.154	0.125	2400
1980-90	--	--	--	0.287	0.170	2399
1990-00	--	--	--	0.433	0.122	1440
Individual Characteristics						
Education	10.548	0.990	4795	10.729	1.002	8636
1960 Dummy	0.471	0.499	4795	0.325	0.469	8636
1970 Dummy	--	--	--	0.289	0.453	8636
1990 Dummy	--	--	--	0.142	0.349	8636
Female	0.517	0.500	4795	0.532	0.499	8636
Age	50.366	8.482	4795	56.811	11.287	8636
Born in 1905	0.031	0.174	4795	0.025	0.157	8636
Born in 1910	0.038	0.191	4795	0.031	0.174	8636
Born in 1915	0.044	0.205	4795	0.047	0.211	8636
Born in 1920	0.048	0.213	4795	0.052	0.222	8636
Born in 1925	0.050	0.217	4795	0.057	0.232	8636
State of Birth Characteristics						
% Urban	53.523	21.279	4795	53.778	21.153	8636
% Foreign	11.737	8.523	4795	11.562	8.430	8636
% Black	8.983	11.901	4795	8.945	11.787	8636
% Emp.in Mfg.	0.067	0.038	4795	0.066	0.037	8636
Ann. Mfg. Wage	7171.387	1343.089	4795	7206.147	1353.573	8636
Val. of Farm per Acre	540.048	276.353	4795	535.182	272.569	8636
P.C. # of Doctors	0.001	0.000	4795	0.001	0.000	8636
P.C Educ. Expenditures	97.006	42.054	4795	99.779	41.706	8636
# Schl Bldgs/ Sq. Mile	0.174	0.090	4795	0.172	0.090	8636

Notes: Summary statistics are for state of birth, cohort and gender cells. All means and standard deviations use sample weights where the weights are the population estimates for the cell in the base period.

Table 2: Summary Statistics for SIPP sample

Variables	Mean	Std. dev.	N
Outcomes			
Self Reported Health	3.084	1.138	26030
Poor Health	0.119	0.324	26030
Fair or Poor Health	0.357	0.479	26030
Health Index	67.992	24.842	26030
Hospitalized in Last Year	0.180	0.384	26484
Days in Bed, last 4 months	3.937	17.030	25223
Number of Times Hospitalized	0.282	1.029	22229
Number of Nights in Hospital	1.908	7.898	26274
Trouble Seeing	0.136	0.342	20853
Trouble Hearing	0.152	0.359	20845
Trouble Speaking	0.021	0.144	20834
Trouble Lifting	0.237	0.425	20837
Trouble Walking	0.289	0.453	20799
Trouble with Stairs	0.276	0.447	20820
Trouble Getting Around Outside the Home	0.129	0.335	17401
Trouble Getting Around Inside the Home	0.059	0.235	17643
Trouble Getting In/Out of Bed	0.079	0.270	17636
Trouble Seeing at all	0.023	0.149	20811
Trouble Hearing at all	0.013	0.114	20819
Trouble Speaking at all	0.003	0.052	15138
Trouble Lifting at all	0.115	0.319	20789
Trouble Walking at all	0.154	0.361	20723
Trouble with Stairs at all	0.116	0.321	20775
Needs Help Getting Around Outside	0.088	0.283	13610
Needs Help Getting Around Inside	0.024	0.154	13893
Needs Help Getting In/Out of Bed	0.025	0.156	13868
Work limitation due to health conditions	0.423	0.494	19073
Arthritis	0.129	0.335	19073
Back	0.062	0.242	19073
Blind	0.026	0.159	19073
Cancer	0.016	0.125	19073
Deaf	0.023	0.149	19073
Deformity	0.027	0.162	19073
Diabetes	0.030	0.170	19073
Heart	0.090	0.287	19073
Hernia	0.006	0.080	19073
Hypertension	0.036	0.185	19073
Kidney	0.005	0.067	19073
Lung	0.043	0.203	19073
Mental Illness	0.005	0.067	19073
Missing Limb	0.003	0.056	19073
Paralysis	0.006	0.075	19073
Senility	0.007	0.084	19073
Stomach	0.010	0.099	19073
Stroke	0.021	0.144	19073
Thyroid	0.003	0.056	19073
Other	0.066	0.247	19073

Table 2: Summary Statistics for SIPP sample

Variables	Mean	Std. dev.	N
Individual Characteristics			
Education	11.432	3.208	26030
Female	0.580	0.494	4795
Age	72.079	5.606	4795

Notes:

Table 3: Replicating Lleras-Muney's Estimates of Effects of Education on Mortality

Dependent variable is ten year mortality rate

	Lleras-Muney (2005)		Replication		Replication with Wrong Base Year	
	WLS	IV	WLS	IV	WLS	IV
Individual Characteristics						
Education	-0.017	-0.051	-0.036	-0.072	-0.016	-0.059
	(0.004)	(0.026)	(0.004)	(0.025)	(0.004)	(0.026)
Female	-0.074	-0.071	-0.072	-0.067	-0.074	-0.070
	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)
State of Birth Characteristics						
% Urban	-1.0E-04	0.001	9.3E-04	0.002	4.4E-04	0.002
	-(9.4E-04)	(0.001)	(9.9E-04)	(0.001)	(1.0E-03)	(0.001)
% Foreign	-5.6E-04	-0.0001	-0.001	-0.0005	-0.002	-0.0011
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
% Black	-0.002	-0.0009	-8.1E-04	-5.9E-05	-0.001	-5.2E-04
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
% employed in mfg.	-0.071	-0.11	-9.3E-04	-0.039	0.010	-0.066
	(0.101)	(0.108)	(0.236)	(0.246)	(0.234)	(0.246)
Annual mfg. wage	7.4E-07	0.000	3.4E-07	4.7E-07	3.0E-07	5.6E-07
	(3.1E-06)	(0.000)	(4.1E-06)	(4.3E-06)	(4.0E-06)	(4.3E-06)
Val. of farm per acre	2.7E-06	0.000	-1.2E-06	-8.9E-06	3.6E-06	-4.9E-06
	(1.7E-05)	(0.000)	(1.9E-05)	(2.0E-05)	(1.8E-05)	(2.0E-05)
Per capita # of doctors	0.242	7.926	16.394	42.372	-0.511	26.405
	(13.891)	(15.059)	(20.993)	(26.445)	(20.897)	(25.656)
Per capita education exp.	1.9E-05	0.000	5.3E-05	6.2E-05	4.4E-05	4.5E-05
	(7.9E-05)	(0.000)	(8.3E-05)	(9.4E-05)	(7.6E-05)	(8.5E-05)
# school bldgs/sq. mile	-0.008	-0.005	-0.0135001	-0.012	-0.015	-0.013
	(0.062)	(0.065)	(0.063)	(0.067)	(0.062)	(0.065)
N	4792	4792	4792	4792	4792	4792
R squared	0.3575	--	0.3606	0.3536	0.3549	0.3425

Notes: All specifications include a dummy for 1970, 24 cohort dummies, 47 state of birth dummies, region of birth interacted with cohort and an intercept. Estimates are weighted using the number of observations in the cell in the base year. Standard errors, shown in parentheses, are clustered at the state of birth and cohort level.

Table 4: New Estimates of Effects of Education on Mortality

Dependent variable is ten year mortality rate, Table entries are the Coefficient on Education

Sample and specification	WLS	IV	N	Goldin/Katz Instruments
Panel A (1960-80)				
(1) 1% 1960-1980	-0.036	-0.072	4792	--
	(0.004)	(0.025)		
(2) 1% 1960, 3% 1970, 5% 1980	-0.045	-0.043	4797	-0.045
drop allocated age, education, birthplace	(0.004)	(0.020)		(0.024)
(3) Sample (2) with age cubic	-0.039	-0.046	4797	-0.047
	(0.004)	(0.020)		(0.024)
(4) Sample (2) with age cubic*yr	-0.040	-0.046	4797	-0.047
	(0.004)	(0.020)		(0.024)
(5) Sample (2) with state*cohort trend	-0.048	-0.032	4797	-0.016
	(0.004)	(0.021)		(0.024)
Panel B (1960-2000)				
(6) 1% 1960, 2% 1970, 5% 1980-00	-0.034	-0.035	8636	-0.026
age cubic*year	(0.003)	(0.014)		(0.015)
(7) Sample (6) with state*cohort trend	-0.036	-0.020	8636	-0.012
	(0.003)	(0.015)		(0.016)
Panel C (1960-2000 by year)				
(8) Sample (6) 1960 only	-0.025	-0.085	2397	-0.081
	(0.006)	(0.045)		(0.052)
(9) Sample (6) 1970 only	-0.061	-0.022	2400	-0.023
	(0.005)	(0.032)		(0.033)
(10) Sample (6) 1980 only	-0.043	-0.006	2399	0.023
	(0.004)	(0.025)		(0.029)
(11) Sample (6) 1990 only	-0.012	0.021	1440	0.027
	(0.005)	(0.040)		(0.039)
Panel D (1960-2000 by age)				
(12) Sample (6) 35-54 year olds	-0.017	-0.059	2879	-0.067
	(0.005)	(0.040)		(0.036)
(13) Sample (6) 55-64 year olds	-0.039	-0.066	2398	0.063
	(0.005)	(0.041)		(0.053)
(14) Sample (6) 65-89 year olds	-0.030	-0.005	3071	-0.047
	(0.003)	(0.019)		(0.023)
Panel E (1960-2000 by cohort)				
(15) Sample (6) cohorts 1901-1912	-0.019	-0.098	3644	-0.203
	(0.004)	(0.037)		(0.125)
(16) Sample (6) cohorts 1913-1925	-0.017	0.039	4992	0.025
	(0.004)	(0.022)		(0.023)

Notes: All specifications include year dummies, cohort dummies, state of birth dummies, region of birth interacted with cohort and an intercept (except for rows 5 and 7). Estimates are weighted using the number of observations in the cell in the base year. Standard errors, shown in parentheses, are clustered at the state of birth and cohort level.

Table 5: Estimates of Effects of Education on Health Outcomes in the SIPP

Dependent Variable	*OLS*/Probit	*IV*/2SCML	*IV*/2SCML % effect	exogeneity test p-value	N
		Panel A: General Health Outcomes			
Self Reported Health *(1 is excellent, 5 is poor)*	**-0.0941** **(0.0023)**	**-0.2289** **(0.0745)**	-0.074	0.074	26030
Health Index (1 to 100 scale)	**1.9674** **(0.0511)**	**4.5345** **(1.6738)**	0.067	0.131	26030
Fair or Poor Health	**-0.0359** **(0.0010)**	**-0.0824** **(0.0343)**	-0.230	0.176	26030
Poor Health	**-0.0141** **(0.0006)**	-0.0269 (0.0206)	-0.226	0.533	26030
Hospitalized in Last Year	**-0.0049** **(0.0008)**	-0.0268 (0.0241)	-0.149	0.364	26484
Days in Bed, last 4 months	**-0.3310** **(0.0364)**	2.1526 (1.4848)	0.547	0.074	25223
Number of Times Hospitalized	**-0.0101** **(0.0024)**	-0.0944 (0.0884)	-0.335	0.329	22229
Number of Nights in Hospital	**-0.0730** **(0.0186)**	-1.0828 (0.7668)	-0.567	0.185	26289
		Panel B: Functional Limitations/ADL/IADL			
Trouble Seeing	**-0.0122** **(0.0007)**	**-0.0559** **(0.0254)**	-0.412	0.085	20853
Trouble Hearing	**-0.0103** **(0.0007)**	**-0.0499** **(0.0247)**	-0.329	0.109	20845
Trouble Speaking	**-0.0019** **(0.0002)**	**-0.0192** **(0.0079)**	-0.909	0.039	20573
Trouble Lifting	**-0.0198** **(0.0009)**	-0.0055 (0.0330)	-0.023	0.667	20837
Trouble Walking	**-0.0251** **(0.0011)**	0.0130 (0.0325)	0.045	0.242	20797
Trouble with Stairs	**-0.0250** **(0.0010)**	-0.0066 (0.0324)	-0.024	0.993	20820
Trouble Getting Around Outside the Home	**-0.0120** **(0.0008)**	-0.0146 (0.0257)	-0.114	0.918	17401
Trouble Getting Around Inside the Home	**-0.0048** **(0.0005)**	0.0051 (0.0208)	0.087	0.635	17463
Trouble Getting In/ Out of Bed	**-0.0056** **(0.0006)**	0.0013 (0.0230)	0.016	0.764	17621
Trouble Seeing at all	**-0.0020**	-0.0078	-0.343	0.490	20589

Table 5: Estimates of Effects of Education on Health Outcomes in the SIPP

Dependent Variable	*OLS*/Probit	*IV*/2SCML	*IV*/2SCML % effect	exogeneity test p-value	N
	(0.0002)	(0.0084)			
Trouble Hearing at all	-0.0008	-0.0100	-0.758	0.060	20256
	(0.0001)	(0.0045)			
Trouble Speaking at all	0.0000	-0.0008		0.000	7516
	(0.0001)	**			
Trouble Lifting at all	-0.0100	-0.0029	-0.025	0.775	20789
	(0.0007)	(0.0250)			
Trouble Walking at all	-0.0148	0.0107	0.069	0.328	20723
	(0.0008)	(0.0260)			
Trouble with Stairs at all	-0.0114	0.0071	0.061	0.359	20775
	(0.0006)	(0.0202)			
Needs Help	-0.0066	0.0044	0.050	0.470	13598
Getting Around Outside	(0.0007)	(0.0153)			
Needs Help	-0.0010	0.0108	0.446	0.125	13757
Getting Around Inside	(0.0002)	(0.0078)			
Needs Help	-0.0011	0.0092	0.372	0.191	13794
Getting In/Out of Bed	(0.0003)	(0.0080)			
		Panel C: Specific Health Conditions			
Difficulty	-0.0250	-0.0743	-0.175	0.157	19073
	(0.0013)	(0.0348)			
Arthritis	-0.0088	-0.0043	-0.034	0.836	19012
	(0.0008)	(0.0217)			
Back	-0.0028	-0.0349	-0.561	0.061	18924
	(0.0005)	(0.0167)			
Blind	-0.0014	0.0145	0.557	0.060	18454
	(0.0003)	(0.0084)			
Cancer	-0.0007	0.0025	0.161	0.677	18569
	(0.0002)	(0.0078)			
Deaf	-0.0003	-0.0041	-0.179	0.568	18422
	(0.0002)	(0.0064)			
Deformity	-0.0006	-0.0159	-0.591	0.018	18821
	(0.0002)	(0.0066)			
Diabetes	-0.0023	-0.0258	-0.868	0.007	18688
	(0.0003)	(0.0082)			
Heart	-0.0062	-0.0014	-0.016	0.804	19025
	(0.0006)	(0.0194)			
Hernia	-0.0003	0.0023	0.362	0.454	17179
	(0.0001)	(0.0037)			

Table 5: Estimates of Effects of Education on Health Outcomes in the SIPP

Dependent Variable	*OLS*/Probit	*IV*/2SCML	*IV*/2SCML % effect	exogeneity test p-value	N
Hypertension	-0.0031	0.0376	1.053	0.000	18683
	(0.0004)	(0.0124)			
Kidney	-0.0001	0.0042	0.938	0.072	16593
	(0.0001)	(0.0027)			
Lung	-0.0037	0.0203	0.472	0.106	19060
	(0.0005)	(0.0152)			
Mental Illness	-0.00009	-0.0002	-0.045	0.932	15794
	(0.00008)	(0.0424)			
Missing Limb	-0.00007	-0.0019	-0.580	0.155	14565
	(0.00005)	(0.0016)			
Paralysis	-0.00011	0.0016	0.287	0.348	17301
	(0.00006)	(0.0020)			
Senility	-0.00005	-0.0015	-0.214	0.070	17993
	(0.00002)	(0.0006)			
Stomach	-0.0006	0.0069	0.695	0.195	17701
	(0.0002)	(0.0060)			
Stroke	-0.0008	0.0084	0.397	0.295	18918
	(0.0003)	(0.0090)			
Thyroid	-0.0000001	0.000001	0.000	0.000	14559
	(0.000000)	**			
Other	-0.0023	-0.0013	-0.019	0.947	19060
	(0.0005)	(0.0152)			

Table 6: Small Pox Vaccination Rates of Young Children in 1930

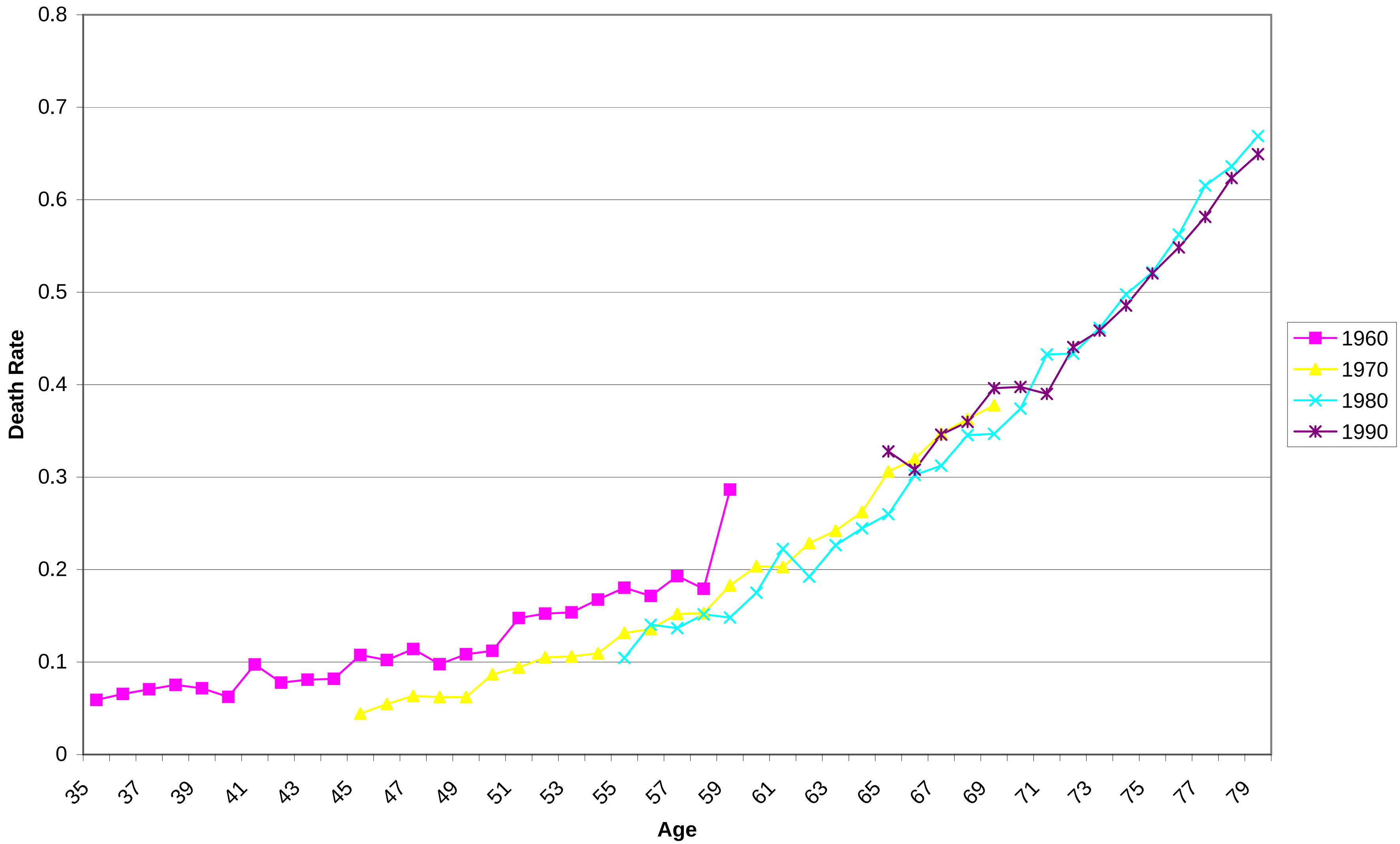
state	*at age 4*	*at age 5*	*change, age 4 to 5*	*% change, age 4 to 5*
Alabama	9	15	6	66.7
Alaska	NA	NA	NA	NA
Arizona.	22	25	3	13.6
Arkansas.	5	23	18	360.0
California.	23	33	10	43.5
Colorado.	13	53	40	307.7
Connecticut	35	65	30	85.7
Delaware.	8	4	-4	-50.0
DC	14	35	21	150.0
Florida	NA	NA	NA	NA
Georgia	30.5	56.5	26	85.2
Hawaii	33	38	5	15.2
Idaho	11	18	7	63.6
Illinois.	16	26	10	62.5
Indiana	13	14	1	7.7
Iowa.	18	26	8	44.4
Kansas	14	26	12	85.7
Kentucky.	20.5	50	29.5	143.9
Louisiana	23	46	23	100.0
Maine.	28	50	22	78.6
Maryland.	34	60	26	76.5
Massachusetts	25	62	37	148.0
Michigan.	17	25	8	47.1
Minnesota	10	15	5	50.0
Mississippi.	21.5	31.5	10	46.5
Missouri	21	37	16	76.2
Montana	9	8	-1	-11.1
Nebraska	16	15	-1	-6.3
Nevada.	28	28	0	0.0
NH	28	76	48	171.4
NJ	25	53	28	112.0
NM	NA	NA	NA	NA
NY	23	63	40	173.9
NC	3.5	10	6.5	185.7
ND	33	37	4	12.1
Ohio.	15	34	19	126.7
Oklahoma	19	27	8	42.1
Oregon	13	15	2	15.4
Pennsylvania	9	29	20	222.2
RI	51	86	35	68.6
SC	11	17	6	54.5
SD	25	40	15	60.0
Tennessee.	10	23	13	130.0
Texas	13	27	14	107.7
Utah.	13	13	0	0.0
Vermont	NA	NA	NA	NA
Virginia	10	16	6	60.0
Washington	14.5	24.5	10	69.0
WV	NA	NA	NA	NA
Wisconsin	18	27	9	50.0
Wyoming.	NA	NA	NA	NA
Median	17.0	27.0	10.0	66.7

Table 7: IV Estimates of Mortality and Health by Stringency of Compulsory Vaccination Laws

	Mortality	Health Index	Poor Health	Trouble Seeing	Trouble Hearing	Trouble Speaking	Back	Stiffness or deformity	Senility
Baseline	-0.028	4.535	-0.035	-0.057	-0.053	-0.022	-0.035	-0.021	-0.013
Sample	(0.014)	(1.674)	(0.024)	(0.028)	(0.027)	(0.012)	(0.019)	(0.012)	(0.007)
	8636	26030	26030	20853	20845	20834	19073	19073	19073
Sample	-0.025	4.854	-0.046	-0.050	-0.052	-0.020	-0.032	-0.020	-0.012
with Vacc data	(0.015)	(1.590)	(0.023)	(0.027)	(0.026)	(0.011)	(0.019)	(0.012)	(0.007)
	7736	24958	24958	20045	20036	20027	18371	18371	18371
Stringent States	-0.023	3.348	-0.059	-0.029	-0.038	-0.002	-0.018	-0.014	-0.001
	(0.012)	(1.804)	(0.027)	(0.030)	(0.024)	(0.007)	(0.020)	(0.012)	(0.006)
	3600	13841	13841	11225	11219	11207	10417	10417	10417
Non-Stringent	0.020	3.774	0.001	-0.023	0.026	-0.009	-0.030	-0.016	-0.022
States	(0.027)	(1.760)	(0.029)	(0.033)	(0.032)	(0.012)	(0.029)	(0.018)	(0.014)
	4136	11117	11117	8820	8817	8820	7954	7954	7954

Notes: All IV regressions include female dummy, cohort dummies, state of birth dummies, and 7 time varying state of birth characteristics (at age 14) from Lleras-Muney (2005). These are % urban, % foreign born, %black, %mfg, mfg wage, doctors per-capita, education expenditures per-capita, and schools per sq. mile. The mortality results also use region of birth interacted with cohort while the SIPP results use state-specific cohort trends. Instruments are categories of required years of schooling in state of residence at age 14. Standard errors are clustered on state of birth and cohort

Figure 1: Ten Year Mortality Rates by Age Across Census Years



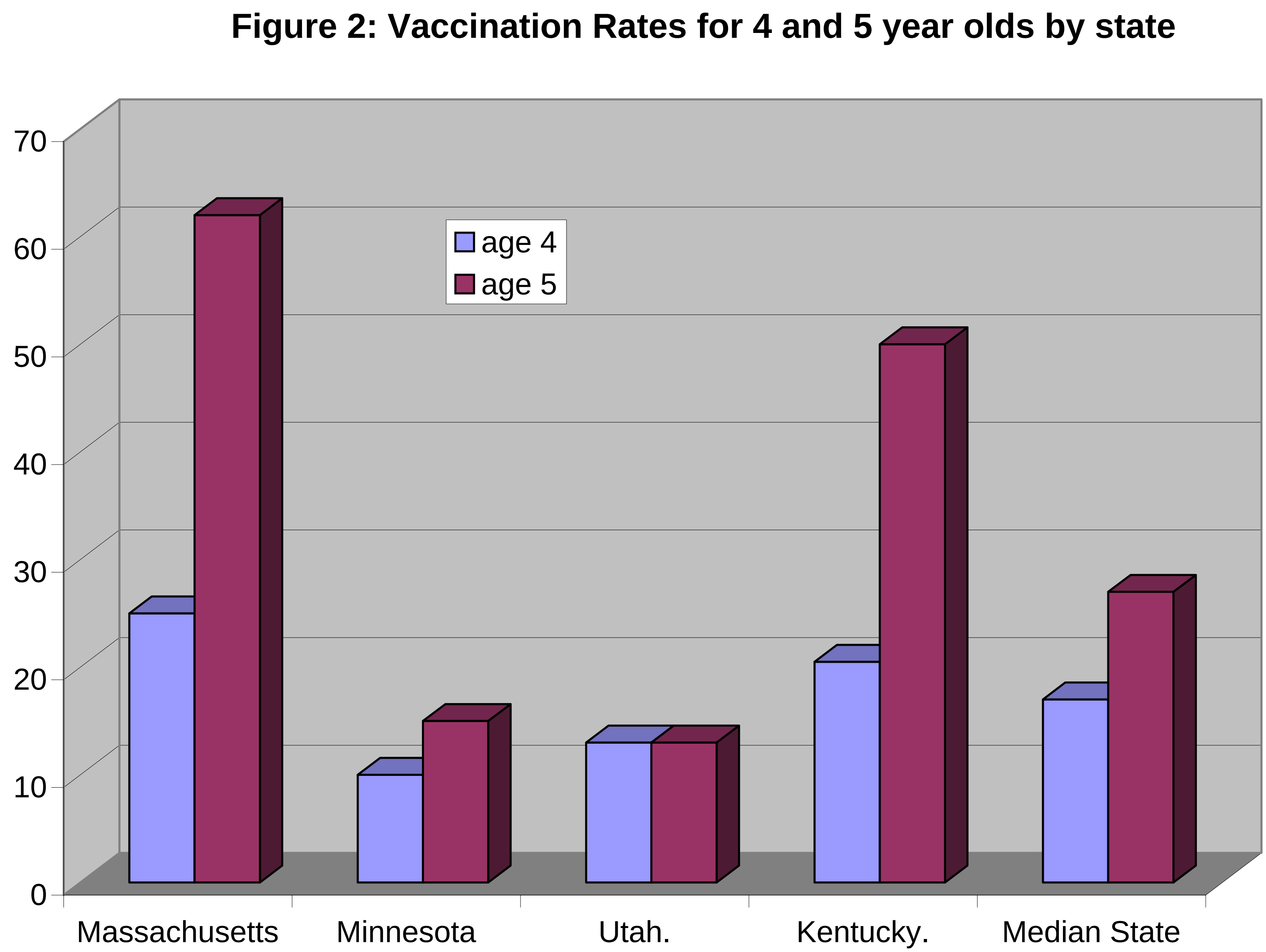


Figure 2: Vaccination Rates for 4 and 5 year olds by state
70
60
50
40
30
20
10
0
age 4
age 5
Massachusetts
Minnesota
Utah.
Kentucky.
Median State

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