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How and Why Studies Disagree About the Effects of Education on Health: A Systematic Review and Meta-Analysis of Studies of Compulsory Schooling Laws

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Abstract

Rich literatures across multiple disciplines document the association between increased educational attainment and improved health. While quasi-experimental studies have exploited variation in educational policies to more rigorously estimate the health effects of education, there remains disagreement about whether education and health are causally linked. The aim of this study was to conduct a systematic review and meta-analysis to characterize this literature, with a focus on quasi-experimental studies of compulsory schooling laws (CSLs). Articles from 1990–2015 were obtained through electronic searches and manual searches of reference lists. We searched for English-language studies and included manuscripts if: (1) they involved original data analysis; (2) outcomes were health-related; and (3) the primary predictor utilized variation in CSLs. We identified 89 articles in 25 countries examining over 25 health outcomes, with over 600 individual point estimates. We systematically characterized heterogeneity on key study design features and conducted a meta-analysis of studies with comparable health outcome and exposure variables. Within countries, studies differed in terms of birth cohorts included, the measurement of health outcomes within a given category, and the type of CSL variation examined. Over 90% of manuscripts included multiple analytic techniques, such as econometric and standard regression methods, with as many as 31 "primary" models in a single study. A qualitative synthesis of study findings indicated that educational attainment has an effect on the majority of health outcomes—most beneficial, some negative—while the meta-analysis demonstrated small beneficial effects for mortality, smoking, and obesity. Future work could focus on inconsistent findings identified by this study, or review the health effects of other types of educational policies.

Keywords

compulsory schooling laws; educational attainment; instrumental variables; policy evaluation; regression discontinuity; systematic review; meta-analysis

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Rich literatures across multiple disciplines document the association between increased educational attainment and improved health (1, 2). Proposed mediating pathways include greater employment potential, augmented psychosocial resources, and reduced risky health behaviors (3). Given the recent increased attention to reducing health inequities across international settings, it is important to identify whether population-level policies to address socioeconomic factors like education do in fact lead to improvements in health (4). In this way, societies can determine whether addressing socioeconomic determinants may reduce health inequities, or whether alternative strategies are more appropriate, such as investing in healthcare systems (5, 6).

Yet most studies on the effects of education on health are correlational, making it difficult to establish whether observed relationships are causal, the result of reverse causation, or confounded by unobserved factors such as personality traits or family socioeconomic status (7, 8). Randomization of educational interventions is often logistically difficult or ethically problematic, although a small number of experimental studies have demonstrated potential long-term positive impacts of early childhood education (9, 10). Nevertheless, experimental studies often cannot achieve sufficient follow-up to document long-term outcomes and typically have limited sample sizes.

Recent decades have seen increased efforts to estimate the causal effects of education on various health outcomes at a population level. Often using quasi-experimental and econometric methods, these studies exploit temporal or geographic variations in policies that lead to differences in educational attainment, and then link these to health outcomes among affected populations (11, 12). The most popular policies examined in this way are compulsory schooling laws (CSLs), legislation that has been passed in different countries at different times to establish a minimum number of years of educational attainment among school-aged children. Previous research has confirmed that implementation of CSLs affects educational attainment (13, 14), thereby creating a quasi-randomly assigned exposure whose effect on health can then be examined.

Despite a proliferation of studies on the health effects of CSLs—or perhaps because of it—there remains disagreement about whether educational attainment is in fact causally linked to improved health (15, 16). Because the existing evidence spans multiple disciplines, there is a need to systematically review these studies that examine CSLs and health. While one previous study conducted a systematic review and meta-analysis of CSL studies in Europe, showing small effects of education on mortality, self-reported health, and obesity (17), a significant portion of the CSL literature was not captured by this search. The present study identifies three times as many manuscripts. No systematic review to our knowledge provides such a comprehensive compilation of the literature on CSLs across a broader range of countries and health outcomes.

In this study, we conducted a systematic review of the literature on CSLs, assembling studies that span multiple disciplines and geographic settings. We selected CSL policies in particular because other types of educational policies (e.g., school funding or student-teacher ratios) address fundamentally different aspects of educational attainment, e.g., quality versus quantity, and because of the large number of quasi-experimental CSL studies that have been conducted and the persistent disagreement about study conclusions. We catalogued all health outcomes that have been examined, from fertility and mortality to biomarkers, and conducted a meta-analysis for a subset of studies with comparably constructed health outcome and exposure variables. In doing so, we hope to provide a comprehensive overview of the state of the CSL literature to date. Our goal is to explain the persistent disagreement regarding the causal effects of educational attainment on health, and to guide future research targeting remaining gaps in the evidence.

METHODS

Search Strategy

We conducted a search on Google Scholar, a comprehensive resource that includes published and unpublished works. Guidelines for the conduct of systematic reviews and meta-analyses highlight the importance of including both published and unpublished studies, given the possibility for publication bias that would otherwise lead to underrepresentation of null and unpopular results (18). The search included English-language articles from January 1, 1990 to August 1, 2015. Studies were included if they used the terms "health" AND "compulsory schooling." A similar search on PubMed found no additional studies. We also examined the reference lists of relevant review articles.

Manuscripts were screened by three investigators (XX, XX, and XX) preliminarily for relevance based on study titles, abstracts, and main text. If more than one version of a manuscript was identified, only the most recent version was included. Potentially relevant manuscripts were then read in full, and those that met the following inclusion criteria were included in the analysis:

1. The study must involve original data analysis.
2. Study outcomes must be health-related.
3. The primary predictor must be related to compulsory schooling laws. This includes policy variations such as school entry age, exit age, total number of years of compulsory schooling, and quarter-of-birth.

The coding instrument—using the online database REDCap (19)—was initially piloted with double entry of a small subset of articles to ensure intercoder reliability. Data were then extracted from the final sample.

Several of the manuscripts in the sample have been published in peer-reviewed journals since the search completion, and these have been updated in our data set (e.g., Brunello et al., 2016). Our review does not, however, include manuscripts produced after August 2015 when our initial search was completed.

Data Elements

For each manuscript, we documented the first author's name, the year of the most recent version, and whether it was published in a peer-reviewed journal.

We next tabulated study characteristics, focusing on features that might explain conflicting findings in the literature. First, we abstracted the health outcomes under examination. Those that only appeared in a single manuscript were categorized as "Other." We next documented the countries in which health outcomes were examined. Another key feature that we abstracted was the birth cohorts included, since the effects of education might differ based on period effects and historical context (20–22). In some studies that examined intergenerational effects of CSLs, we abstracted both the cohort that was affected by the legislation as well as the cohort of their children (e.g., Birgisdóttir, 2013). We also characterized the type of CSL variation that was used in each study's identification strategy. For example, those that exploit variation in school entry age might result in different findings than those that exploit school exit age, since an additional year of schooling in early childhood may have dissimilar effects compared with a year of schooling in adolescence.

We also recorded the analytic methods employed, i.e., standard techniques (e.g., ordinary least squares and Cox regression) versus econometric techniques based on quasi-experimental variation in the exposure (e.g., instrumental variables (IV) or regression discontinuity) (23, 24). Different types of analyses might be expected to produce different results, which may explain inconsistent findings across studies. For example, IV estimates are a local average treatment effect representing the effect on the compliers rather than an overall average effect. The presence of multiple models within a given study might also complicate the selection of a primary finding that would be included in a meta-analysis. We then explored whether each study restricted analyses to a given sociodemographic subset (e.g., only men, only those without a college education) or whether subgroup analyses were conducted (e.g., by gender or race). Again, heterogeneity might inform differences in study findings across manuscripts and the ability to conduct meta-analysis. For example, early childhood education has been shown to differentially affect health outcomes in men versus women (25). For each study, we documented the largest and smallest sample sizes that were analyzed in each manuscript (e.g., an overall model versus the smallest subgroup analysis). Larger studies might be better powered to produce statistically significant results. We also abstracted the "first stage" coefficient from each study, i.e., the primary coefficient when regressing educational attainment on the policy variable(s) of interest.

Finally, we abstracted the effect sizes and uncertainty measures (i.e., confidence intervals, standard errors, or *P*-values) for the primary models in each manuscript (N = 621 models). We prioritized abstracting confidence intervals or standard errors, although in some cases only *P*-values were reported. In most cases, no single model was highlighted as the primary model by study authors. Consequently, we followed several steps in selecting which estimates to abstract and which model(s) to consider as the primary. First, we prioritized models that included the overall sample, rather than subgroup analyses. Second, we selected the more rigorous models, e.g., those which included adjustment for additional covariates or which employed econometric rather than standard analytic methods. Third, for studies that examined multiple health outcomes, we abstracted the effect size for each health outcome. Fourth, if separate models were conducted for different countries or for men and women separately (without an overall estimate), we abstracted an estimate for each group. Finally, for health outcomes categorized as "other," effect sizes were not abstracted given the tremendous heterogeneity in this outcome.

Study Summary Approach

We first provided a descriptive analysis of the characteristics above. We examined the longitudinal trends in publication, with a focus on trends in the analytic methods employed over time. We cross-tabulated the distribution of health outcomes by country, to identify which areas have been investigated in prior work, as well as the distribution of health outcomes by analytic method.

Given the multiple aspects of heterogeneity we identified, we were only able to conduct quantitative meta-analysis for a handful of outcomes (described below). Nevertheless, for all outcomes we conducted qualitative assessments of the effects of educational attainment. That is, for each of the primary models in the manuscripts in our sample, we documented whether the effect size was associated with a statistically significant effect size at a P threshold of 0.05. For those that were statistically significant at this level, we documented whether educational attainment was associated with improved or worsened health (i.e., because negative coefficients may represent improved health for some outcomes, and worsened health for other outcomes). For fertility, we designated “improvement” as reduced fertility, e.g., fewer children or delayed childbearing, since economic prosperity and increased human capital are generally associated with reduced fertility (26, 27), although for this outcome improvement is subjective. Readers interested in particular outcome or country subgroups are encouraged to interactively examine Supplemental Table 2 which can be sorted by different study attributes and effect estimates.

Study Characteristics Associated with Study Findings

We next carried out analyses to determine the study characteristics that were associated with (1) statistically significant findings (versus non-significant findings); and (2) improvements in the health outcomes of interest (versus worsened health or no effect). These two analyses were conducted using multivariable logistic regressions, adjusting for statistical method, year of publication, whether the model came from a published manuscript, whether gender, race, and education were restricted in the sample, the natural log of the sample size, the type of policy variation employed, and whether more than one country was included in the analysis. This analysis was conducted on the data set that included the point estimates from all primary models (N = 613). Observations with missing point estimates (N = 2) and missing sample size (N = 40) were excluded from this analysis. Due to small cell sizes and subsequent instability in regression estimates, we also excluded those models that used “other” statistical methods (N = 4). The final sample size for this analysis was 576 models, roughly 94% of the full sample. To account for heteroscedasticity and correlated results among models from the same manuscript, robust standard errors were clustered by manuscript.

Meta-analytic Approach

Finally, we selected a subset of manuscripts on which to conduct meta-analysis. To do so, we first identified studies that employed similar measures of the health outcomes in question. For example, studies that examined current smoking status were deemed to be different from those that examined the number of cigarettes per day. Next, we identified studies that employed similar measures of the exposure. For example, studies that examined the effects of a one-year change in educational attainment were deemed to be different from those that examined the effects of completing high school. If there were at least 10 studies that were similar in terms of both the outcome and exposure measure, we conducted a meta-analysis of the point estimates. The outcomes that met these criteria were mortality, current smoking status, obesity, and hypertension. We pooled point estimates using random effects, a model which assumes that health effects are heterogeneous and drawn from a normal distribution. Effect heterogeneity was measured using I-squared, with higher levels indicating heterogeneity and supporting the need for a random effects model. For studies that only presented *P*-values, we calculated standard errors using previously described techniques (28).

In some cases, the same data set was used to answer the same research question across several manuscripts, with each employing slightly different cohorts, inclusion criteria, or model specifications (e.g., Meghir 2011, 2012, and 2013 examined the effects of education on mortality using Swedish register data). When this occurred, we conducted separate meta-analyses, alternatively including each of the overlapping manuscripts. In every case, this resulted in virtually identical meta-analysis results, so we arbitrarily selected one of these several versions to present here.

Analyses were conducted in StataMP 14 (College Station, Texas) and R 3.2.1 (Vienna, Austria).

RESULTS

Search Results

The results of the Google Scholar search resulted in the identification of 20,433 manuscripts (Figure 1). After the preliminary screen, this was narrowed to 215 relevant articles. After a more thorough examination, 94 were deemed irrelevant, and 36 were duplicates. PubMed did not result in the identification of any additional articles, and four more manuscripts were identified through reference lists. The final sample size was 89 manuscripts.

Characteristics of Sample Manuscripts

Supplemental Table 1 lists the studies in the final sample, including relevant characteristics (12, 15, 29–118). The earliest was published in 2002 (29), with a substantial increase in the number of manuscripts per year across the study period. Sample sizes ranged from 128 (105) to 8,887,608 individuals (85), with most studies having sample sizes below 100,000 (median 28,310, Supplemental Figure 1). There was considerable heterogeneity in birth cohorts included, even for studies examining the same country and health outcome. For example, a series of studies examining mortality in the U.S. used differing birth cohorts including 1901–1925 and 1925–1945 (12, 56, 78, 109). Regrettably, about 20% of manuscripts did not provide a first-stage coefficient, limiting the ability to determine the “effective sample size” of two-stage IV analyses.

Table 1 provides summary statistics on manuscripts included in the sample. Approximately half were published in peer-reviewed journals. Over 90% of manuscripts included multiple analytic methods, including standard techniques such as ordinary least squares, logistic, and

Cox regressions, although most also included econometric quasi-experimental methods such as IV, regression discontinuity, and others. While IV analyses have remained the most popular type of quasi-experimental method employed over time, an increasing number have employed other techniques (Supplemental Figure 2).

Half of studies restricted analyses to participants of only one gender. This was often a restriction to female participants because the outcome was fertility, although several studies also restricted analyses of gender-neutral health outcomes to men. A smaller percentage restricted analyses by race or educational attainment. A third of studies also conducted subgroup analyses by gender, race, or educational attainment. About 14% of studies included data on more than one country, with a maximum of 13 countries in a single study. About half of studies examined only one health outcome, while several manuscripts included over a dozen outcomes (mean 2.6, SD 2.5). Finally, there was substantial heterogeneity in the type of CSL variation that studies exploited. While about half used school dropout age or overall years of compulsory schooling, roughly 5–10% of studies employed other types of variation such as quarter/month-of-birth or child labor laws.

Manuscript Distribution by Country and Health Outcome

Studies of CSLs and health have been conducted in 25 countries, although the vast majority took place in Europe (Figure 2). The inclusion of different countries reflects not just a difference in geographical or political contexts, but also a fundamental difference in the type of exogenous variation that studies exploit. For example, in the United Kingdom investigators typically take advantage of a single educational reform that took place in 1947 as a source of variation (46, 80), while those in the United States take advantage of many smaller changes across states and time (64, 91). Even within a given country, analyses often differed substantially. For example, as mentioned above, different birth cohorts were included in the analyses, potentially resulting in inconsistent findings across studies. Alternately, authors leveraged CSL variations in different ways. For example, one U.S. study exploited differences in minimum school drop-out age as an IV for educational attainment (56), while another U.S. study used drop-out age, enrollment age, and child labor laws (63).

The studies examined over 25 different health outcomes (Figure 3), with the “Other” category representing a heterogeneous group of miscellaneous outcomes such as “back pain” and “multivitamin use.” Even within categories, there was considerable variation in how outcomes were measured, as described below and in Supplemental Table 2. For example, for the most common outcome of “fertility,” studies used varied constructs including number of children, any children, age at first birth, and others (47, 110, 116).

We also examined the distribution of studies jointly across countries and health outcomes (Figure 4), since combining studies across country settings presents challenges given possible differences in the types of CSL policies implemented, as well as variation in historical and political contexts. Several outcomes—including fertility, self-rated health, obesity, smoking, and mortality—have each been examined in multiple studies in a single country. For many combinations of country and health outcome, however, the number of prior studies on CSLs and health is sparse or non-existent. Also, as discussed below, even for studies in the same country examining the same health outcome, model specifications differed substantially, which may limit the interpretability of meta-analyses presenting composite estimates of the effects of educational attainment on health.

Details of Study Results

We abstracted 613 point estimates from the 89 studies in our sample (Supplemental Table 2), with as many as 31 point estimates abstracted for a single study (112).

Heterogeneity in Outcomes and Analytic Methods.—We cross-tabulated the type of outcome with the type of analytic method for the primary models from each study. While most models employed IV analyses, suggesting that results for a given health outcome may be comparable (Supplemental Figure 3), a substantial degree of heterogeneity was apparent when examining studies by the type of health outcome included. For example, studies categorized as addressing “alcohol-related” outcomes examined the number of drinks (56), probability of drinking (65), probability of drinking over the weekly limit (40), probability of currently drinking (50), probability of being a moderate drinker (77), and probability of drinking all seven days in the last week (114). Similarly, for fertility, measures included the probability of first birth by a given age (38, 47, 49, 58, 74, 88, 101), time until first birth (44, 52), probability of ever giving birth or of childlessness (49, 72, 76), and number of children (ever, or by a given age) (47, 58, 61, 72, 76, 88). Even within each of these subcategories, heterogeneity complicates the ability to produce a summary effect measure. For example, among those manuscripts measuring probability of first birth by a given age, some studies examined different teenage age ranges (38, 58, 101) while others examined birth at older ages (49).

In some cases, heterogeneity in the analytic methods created the challenges to meta-analysis. For example, for mortality in the United States, several manuscripts examined identical data sets using similar mortality measures, but because of differences in covariate adjustment or sample restrictions, produced different estimates (12, 109). In other cases, authors presented study statistics in formats that cannot be easily compared with other studies, such as probit estimates (36, 38, 42, 95). Roughly 6% of models present only a *P*-value, without a confidence interval or standard error.

The Effects of Educational Attainment on Health.—To qualitatively assess the effect of educational attainment on health, we tabulated the number of primary models that found improvements, no effect, or worsening for each outcome category (Figure 5). For several outcomes—i.e., infant mortality, biomarkers, height, lung disease, and heart disease—all of the existing literature fails to reject the null that there is no effect (at a *P* threshold of 0.05). For cancer and alcohol, the weight of the literature seems to suggest that educational attainment worsens outcomes. For the remaining 17 outcome categories, the literature suggests that educational attainment improves health, although for many of these a substantial portion of the literature also fails to reject the null that there is no effect. Even if increased (rather than reduced) fertility is considered an improvement in health, the weight of the literature continues to suggest that education improves health across multiple domains.

Study Characteristics Associated with Study Findings.—Study characteristics that were associated with statistical significance included larger sample size and the use of child labor laws as the source of variation (Table 2). Statistical methodology, year, publication status, sample restrictions, and number of countries analyzed were not associated with statistical significance.

Study characteristics that were associated with improvements in health outcomes included use of a regression discontinuity analysis and larger sample size. Year, publication status, sample restrictions, type of policy variation, and number of countries analyzed were not associated with improvements in health outcomes.

Meta-analysis.—When pooling estimates from multiple studies, educational attainment was found to have a beneficial effect on mortality (effect size: −0.05; 95%CI: −0.09, −0.01), with the I-squared ranging from 30% to 60% depending on which of several overlapping studies was included. Increased educational attainment was also associated with decreased probability of being a current smoker (effect size: −0.01; 95%CI: −0.02, −0.002; I-squared 0%). and reduced risk of obesity (effect size: −0.20; 95%CI: −0.40, −0.02; I-squared 75%). Greater educational attainment was associated with reduced hypertension (effect size: −0.01; 95%CI: −0.04, 0.02; I-squared 70–75%), although 95% confidence intervals for this estimate included the null. Each of these coefficients represents the percent change in the outcome as a result of a one-year increase in educational attainment (e.g., a 5% reduction in mortality or a 1% reduction in the risk of smoking).

DISCUSSION

In the present study, we conducted a systematic review of the literature on compulsory schooling and health and subsequently conducted meta-analysis of the literature on the effects of CSLs for specific health outcomes. The literature on compulsory schooling aims to examine the causal effects of education on health, and our study aimed to explain persistent controversy regarding whether the observed associations of educational attainment with health are in fact causal (15, 16). We identified 89 manuscripts in this literature across a range of country settings, half of which were unpublished. A prior systematic review focusing on Europe identified 22 manuscripts on this topic (17), while our study identified 24 manuscripts about the United Kingdom alone, perhaps because of the more comprehensive search strategy. Our study therefore represents the most complete systematic review of this field to date, which spans disciplines including epidemiology, economics, and public policy.

We first documented substantial heterogeneity across a range of study features, including the type of policy examined, the analytic approach, the cohorts included, and the measurement of the health outcome of interest. This heterogeneity—discussed in more detail below—may explain the persistent disagreement in the CSL literature regarding the causal effects of educational attainment on mortality and morbidity. For example, some studies employing nearly identical data sets and measurements of exposures demonstrated different findings due to differences in the analytic approach (12, 15, 109). Another possible explanation for the disagreement in the previous literature is that individual studies may have been underpowered, particularly for IV studies that are characterized by additional imprecision as a result of the two-stage estimation. The meta-analysis that we conduct here overcomes this latter challenge.

We next qualitatively assessed where the weight of the literature fell for each health outcome category. For the majority of health outcomes, the extant literature seems to suggest the positive effects of educational attainment, although we cannot rule out systematic bias across studies. Nevertheless, these findings suggest education's beneficial effects on health across a variety of different types of outcomes. The implications are that multiple mechanistic pathways may link educational attainment and health. For example, this qualitative analysis suggests that education results in improvements in cognition and mental health (perhaps due to psychosocial benefits), increased healthcare utilization (perhaps representing improved healthcare access due to greater income or employment), and improvements in nutrition and obesity (perhaps due to improved income or health behaviors) (119).

For the handful of outcomes for which health outcome and exposures measures were comparable, meta-analyses suggested that increased educational attainment reduces mortality,smoking, and obesity, and may also reduce the prevalence of hypertension. This suggests that education may lead to improvements in multiple cardiovascular risk factors, perhaps due to improvements in diet, stress, or modifiable health behaviors that are associated with improved education and higher income. A prior meta-analysis also identified possible reductions in obesity but no statistically significant effects for mortality (17); although this prior study was limited to a smaller number of manuscripts in Europe. While this means that the prior meta-analysis may have been underpowered, this may also reflect possible heterogeneity in findings across country settings. Given the small number of studies, we were unable to conduct subgroup analyses by country. Moreover, there were unfortunately insufficient numbers of studies examining other non-cardiovascular health outcomes to conduct additional meta-analyses to explore other mechanistic pathways linking education and health. Epidemiologic theory acknowledges that educational attainment may be expected to have different effects on different types of health outcomes, depending on the mediating pathways that predominate (2). Future studies could focus on these non-cardiovascular health outcomes in order to support meta-analysis to address these other domains.

Of note, the results of the meta-analysis suggest that a year of education is associated with a 20% reduced risk of obesity, but a small reduction (1–5%) in the risk of mortality, smoking, and hypertension. While this represents a small effect at the individual level, these effects are more meaningful at the population level. Future studies could incorporate these estimates into cost-benefit analyses to determine whether the added investment in an additional year of education is worth the accrued health benefits.

It is interesting to note that the results of our qualitative synthesis were not consistent with those of our quantitative meta-analysis in all cases, e.g., the results for smoking seemed equivocal in the qualitative analysis but were beneficial in the quantitative analysis, while the results for mortality appeared beneficial in both analyses. This may be because the qualitative analysis cannot leverage the combined power of multiple (potentially null) studies. Qualitative analyses akin to "vote-counting" also have poor properties as statistical tools (120), in addition to the focus on significance testing and indifference to studies of different sample sizes (121, 122). Alternately, the quantitative analysis could only consider a subset of studies with comparable measures of the outcome and exposure, and therefore may not be as comprehensive as the qualitative synthesis. Future work should attempt to use similar measures as those used in prior studies, in order to allow for future meta-analyses to produce quantitative pooled estimates.

For those outcomes for which all studies fail to reject the null that there is no effect, this does not necessarily imply that there is no effect of educational attainment on these outcome categories. For example, each of these studies individually may have been underpowered. Our cut-off using a P of 0.05 in the qualitative assessment was dependent on the results available in the literature. Unfortunately, the heterogeneity of study features for many of these outcomes precludes a meaningful meta-analysis to improve precision of these estimates. Using the same constructs for these health outcomes in future work could allow for meta-analysis with the previous literature. Alternately, there may be no effects only in specific country or historical contexts; future studies could examine different country settings or cohorts to establish generalizability of these findings. Finally, future studies could examine other constructs to determine whether there is an effect for different aspects of these health outcomes.

For cancer and alcohol, our qualitative synthesis suggests that educational attainment leads to worsened outcomes. Future studies could attempt to validate these findings in other samples, or could examine the mechanisms of these findings. For example, increased cancer may be due to increased access to healthcare and subsequent diagnostic bias, or increases in certain types of cancer (e.g., increased breast cancer due to decreased fertility).

More generally, future work that attempts to synthesize additional outcomes from the CSL literature may do well to consider the heterogeneity we identified in this review, as well as differences in the historical and political contexts of each study. For example, the authors of one study of the effects of CSLs on mortality in Europe appropriately included a discussion of the observed effect heterogeneity longitudinally and across countries (60). Studies in different country settings may be affected by differences in geographical and political contexts as well as differences in the type of CSL variation that is exploited, e.g., one-time country-wide reforms versus numerous state-level reforms.

Yet even within studies of the same country, investigators exploited different types of CSL variation, e.g., school entry age versus drop-out age. While conceptually this might be because authors were more interested in increased educational attainment in early or late childhood as opposed to overall number of years of schooling—i.e., a difference in the endogenous variable of interest—this was rarely acknowledged as the reason for a given identification strategy (111). Indeed, differences in the source of variation might result in differences in the effect on educational attainment, which may then lead to different effects on health outcomes. The results of a study that employs one type of variation are not necessarily generalizable to other settings in which educational attainment varies due to some other source of variation. For instrumental variables analysis, this concept is referred to as a local average treatment effect. Moreover, several studies employed quarter- or month-of-birth as an instrument, despite criticism that this source of variation is driven by endogenous biological or socioeconomic differences rather than exogenous policy changes (123).

Finally, the studies in this sample employed a range of analytic methods, primarily econometric techniques such as IV analysis. All of the studies presented a variety of models and sensitivity analyses within the same paper (up to 31 primary findings in a single paper). Not only does this make it difficult to identify which should be considered the "primary" model, but the differences across studies in analytic methodology in some cases prevent the calculation of a summary statistic without numerous (possibly incorrect) assumptions. Moreover, sample sizes varied by a factor of almost 70,000, despite the fact that small samples are more likely than large samples to be underpowered and biased in IV analyses, all else being equal (124). Notably, these sample sizes are not reflective of the "effective sample size" for IV studies, in which the strength of the first stage plays an important part; unfortunately, the first stage was not consistently provided in sample manuscripts.

Each of these types of heterogeneity may preclude meaningful meta-analysis, or even direct comparison of studies conducted in similar settings on similar outcomes. Thus, meta-analyses that attempt to aggregate such heterogeneous studies (such as those included in our review) should be interpreted cautiously, (17). This heterogeneity may also help to explain the inconsistent findings across studies that seemingly address similar research questions, such as conflicting findings on the effect of educational attainment on mortality in the U.S. (12, 109). Indeed, we found that differences in several of these study features were associated with the probability of statistical significance and with the probability of finding a beneficial effect of educational attainment on health.

If possible, future studies should also look to the manuscripts included in this review and attempt to use similar analytic methods or variable constructs, to allow for maximum comparability of estimates across studies. Consortia such as the Core Outcome Measures in Effectiveness Trials (COMET) Initiative have called for a greater standardization of the measurement of health outcomes, since the current approach to research in the medical and social sciences has made it difficult to synthesize results meaningfully (125).

The final contribution of this study has been the identification of knowledge gaps in the existing literature for specific country settings. For example, while self-rated health has been studied numerous times in the U.S. and several European countries, it has not been examined at all in other country settings. Since CSL implementation differed in each country, and because educational attainment may have different effects in different historical or political contexts, findings in one country for a particular outcome are not necessarily generalizable to other settings. Our review thus identifies specific country-outcome combinations where further studies would add new knowledge to the literature to guide policy in different country settings.

To conclude, this systematic review and meta-analysis of the effects of CSLs on health suggests that educational attainment has mixed but largely beneficial effects on health across numerous outcome categories. It characterizes substantial heterogeneity across most key study design features, which may complicate the ability to conduct meaningful meta-analysis, or even to compare studies with seemingly similar research questions. Future work may focus on knowledge gaps identified by this study, or review the effects of other types of educational policies that may have different effects on health and health disparities.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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HIGHLIGHTS

- Studies disagree on whether educational attainment causally affects health.
- We systematically review quasi-experimental studies of compulsory schooling.
- There is substantial heterogeneity across outcomes, settings, and analytic methods.
- Education has mixed—but largely beneficial—effects on a range of health outcomes.

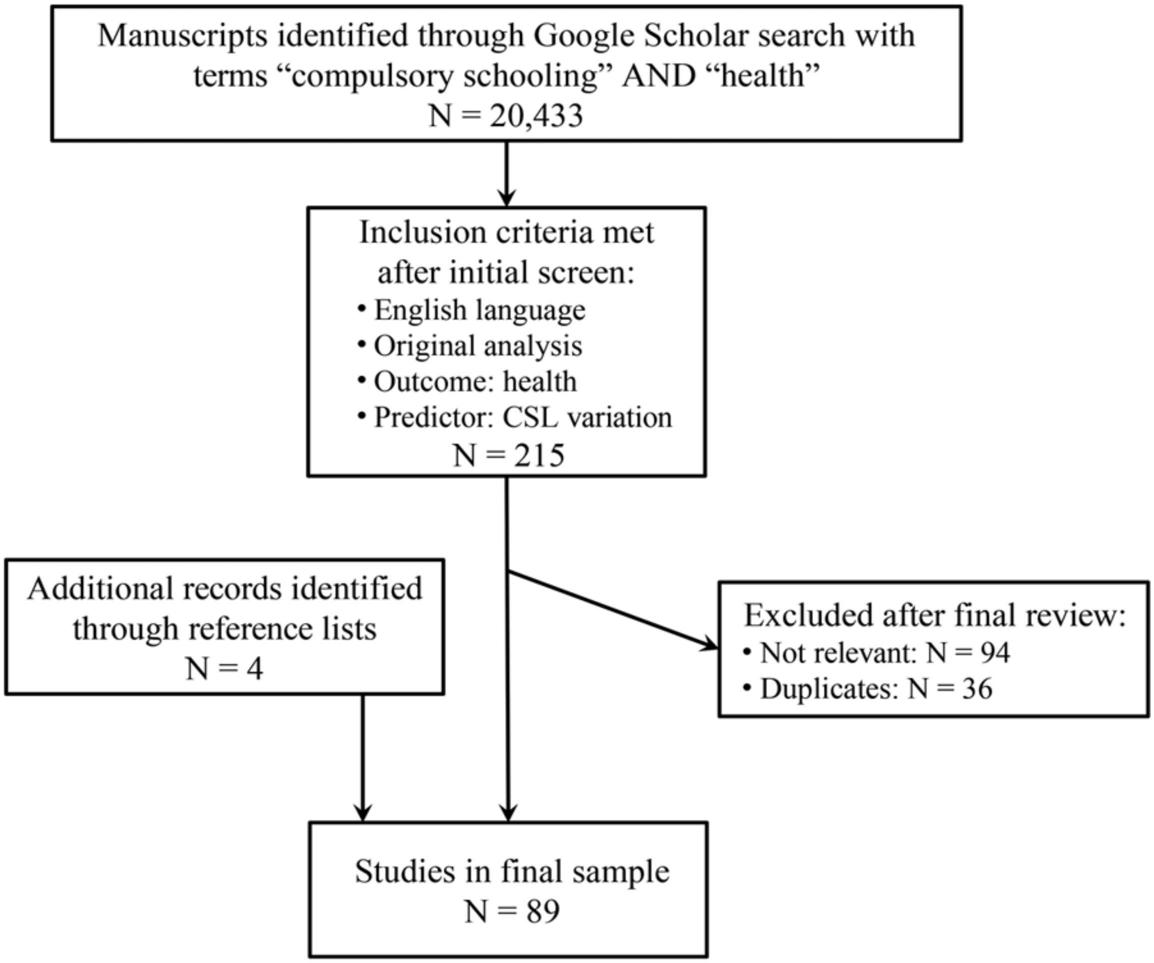

Figure 1.
Study Flowchart

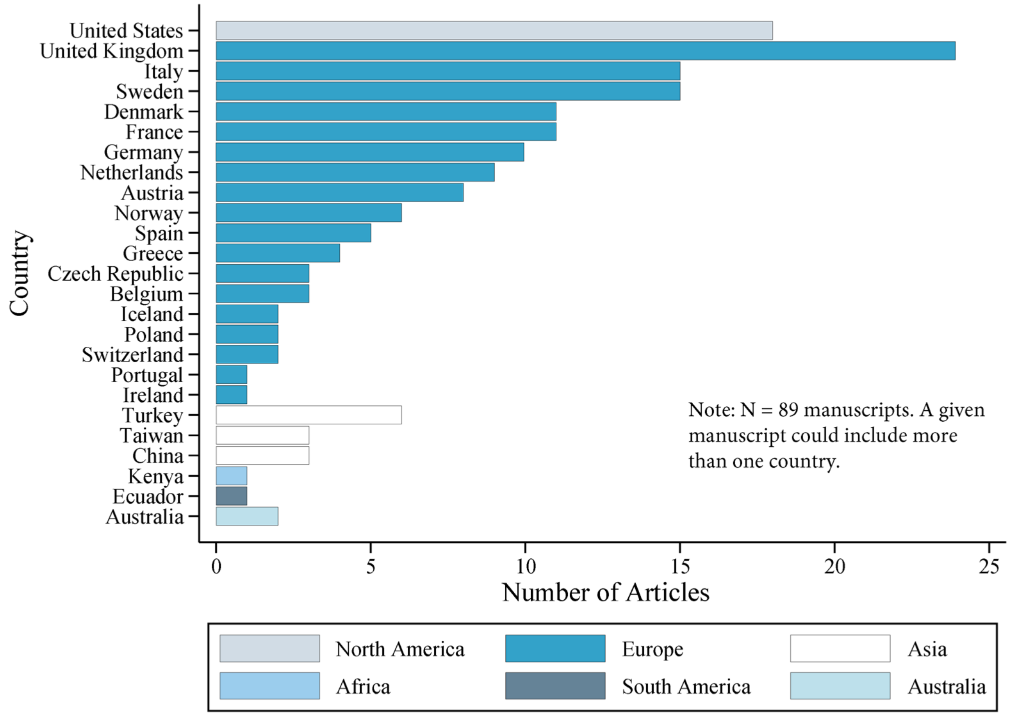

Figure 2.

Countries Examined in Study Manuscripts. Note: N = 89 manuscripts. A given manuscript could include more than one country.

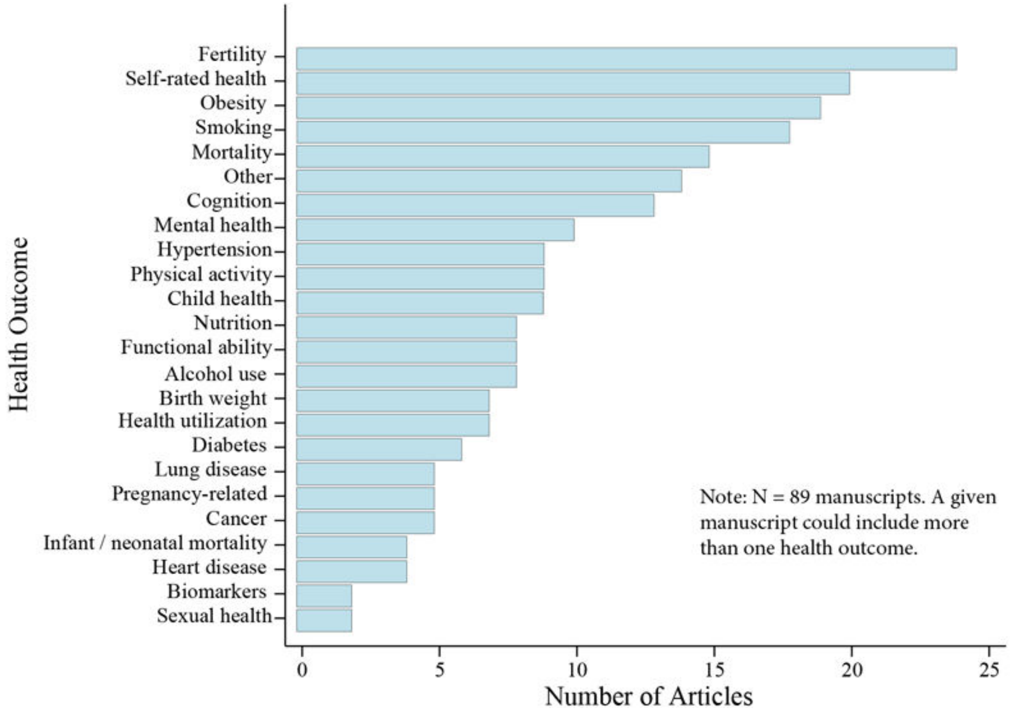

Figure 3.

Health Outcomes Examined in Study Manuscripts. Note: N = 89 manuscripts. A given manuscript could include more than one health outcome.

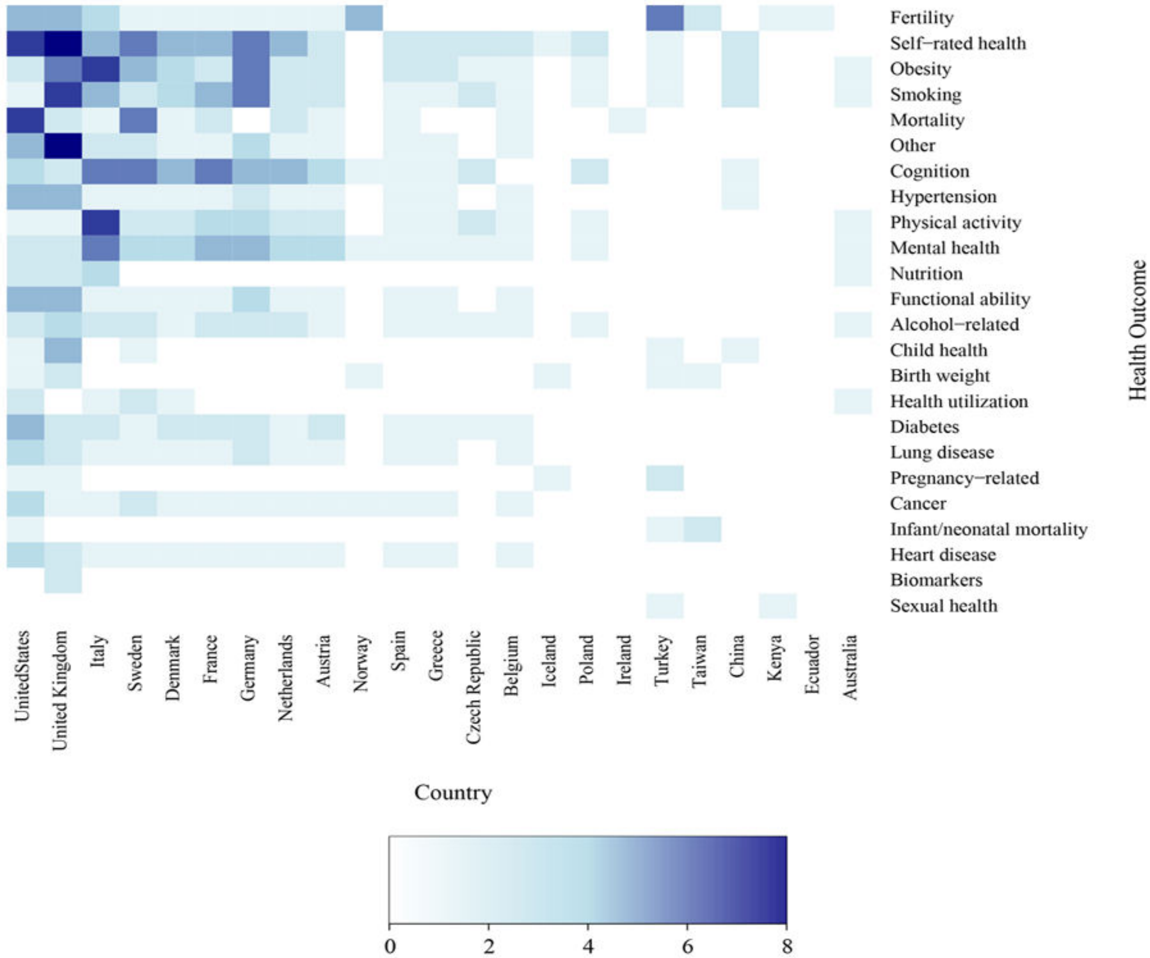

Figure 4.

Distribution of Studies, by Country and Health Outcome. Note: N = 89 manuscripts. A given manuscript could include more than one health outcome or country.

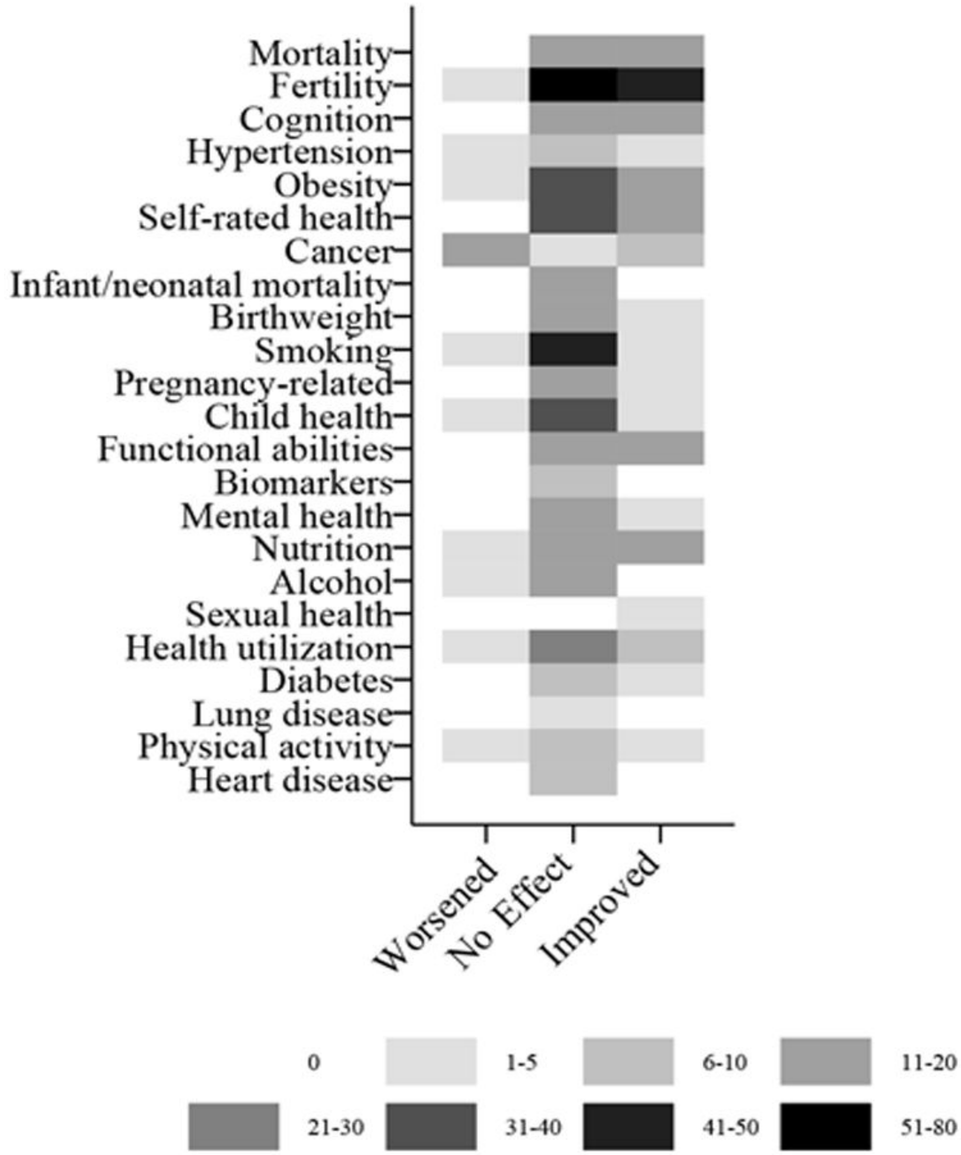

Figure 5.

Number of Models Showing Effects of Education by Health Outcome. Note: N = 613 models from 89 manuscripts. "No effect" indicates a confidence interval that includes 0 or $p \geq 0.05$.

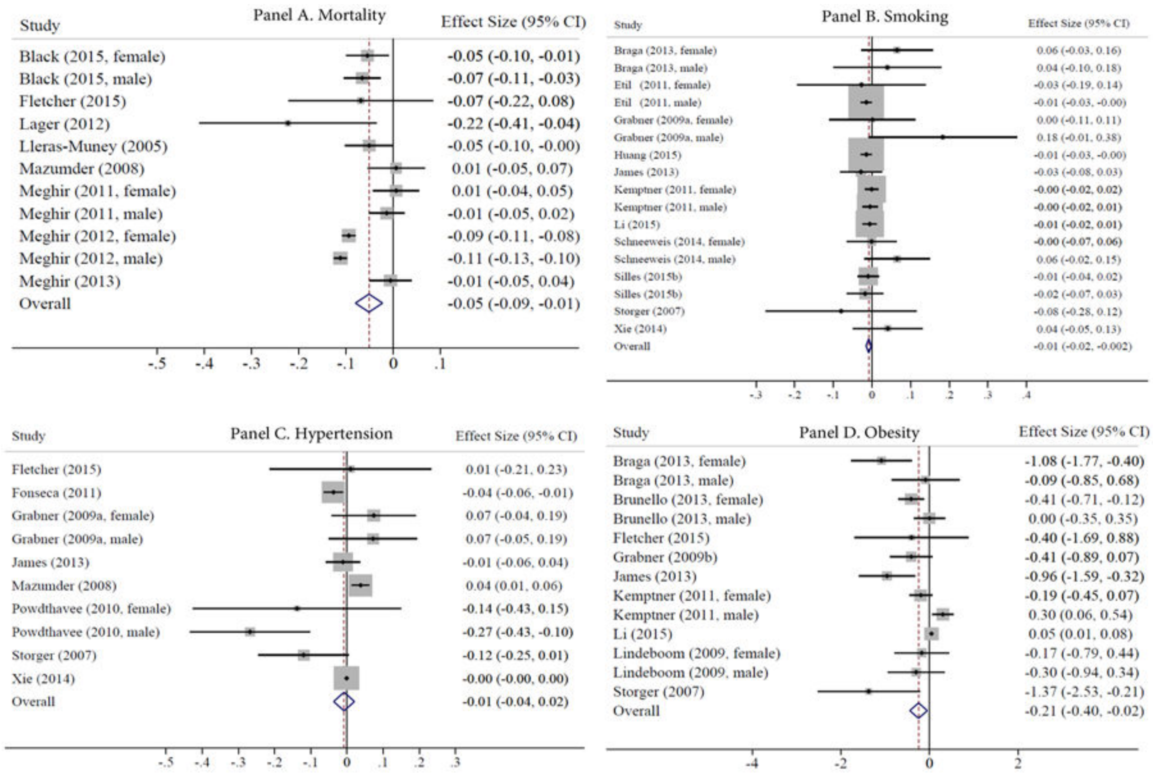

Figure 6.

Meta-Analysis of the Effects of Educational Attainment on Health. Note: Manuscripts were included in the meta-analysis if they employed similar measures of the health outcomes and the exposure. Coefficients represent the percent change in the health outcome of interest as a result of a one-year increase in educational attainment, e.g., a 5% reduction in mortality. Estimates were pooled using random effects models. Note that Braga (2013) estimates were reported in the manuscript as a change per 3 years of education; this estimate was divided by 3 to represent an annual change, for the purposes of this meta-analysis.

Table 1.

Manuscript Characteristics (N = 89)

Characteristic	Value
Published in peer-reviewed journal, N (%)	52 (58.4)
Multiple analytic methods, N (%)	82 (91.0)
Standard[1]	89 (100.0)
Instrumental variables	67 (75.3)
Regression discontinuity	25 (28.1)
Other[2]	8 (9.0)
Sample restricted, N (%)	48 (53.9)
By gender	42 (47.2)
By race	6 (6.7)
By educational attainment	5 (5.6)
Sub-group analyses conducted, N (%)	31 (34.8)
By gender	26 (29.2)
By race	1 (1.1)
By educational attainment	4 (4.5)
Countries included per article	
More than one country, N (%)	12 (13.5)
Mean ± SD	1.9 ±2.5
Health outcomes included per article	
More than one health outcome, N (%)	45 (50.6)
Mean ± SD	2.6 ±2.5
Variation in compulsory schooling law, N(%)	
Quarter/month of birth	6 (6.7)
School start age (i.e., enrollment age)	11 (12.4)
School end age (i.e., dropout age)	50 (56.2)
Child labor laws	5 (5.6)
Overall years of compulsory schooling	34 (38.2)
Other	4 (4.5)

Note: Each manuscript might employ multiple analytic methods, multiple types of CSL variation, etc., so that totals may add up to more than 100%.

[1]"Standard" analytic methods include non-quasi-experimental techniques: ordinary least squares, logistic regression, and Cox proportional hazards models.

[2]"Other" analytic methods include difference-in-differences, quantile regression, instrumental variables quantile regression, instrumental variables quantile treatment effect, and others.

Table 2.

Association between Study Characteristics and Study Findings (N=576)

	Odds Ratio [95% CI]	
	Statistically Significant Finding	**Health Outcome Improved**
Statistical Method (ref: IV)		
Regression discontinuity	5.18	11.1 *
	[0.58, 46.0]	[0.95, 129]
Standard regression	1.87	1.42
	[0.63, 5.55]	[0.47, 4.26]
Year	0.95	0.92
	[0.77, 1.17]	[0.77, 1.10]
Published	0.80	1.27
	[0.32, 2.00]	[0.52, 3.08]
Gender restricted	1.12	1.21
	[0.54, 2.33]	[0.60, 2.43]
Race restricted	2.58	2.35
	[0.37, 17.8]	[0.24, 23.0]
Education restricted	1.04	0.65
	[0.15, 7.04]	[0.055, 7.65]
Ln(s ample size)	1.24 *	1.21 *
	[0.97, 1.58]	[0.97, 1.50]
Type of Policy Variation		
Quarter/month of birth	0.60	0.63
	[0.11, 3.36]	[0.12, 3.38]
School start age	3.02	2.36
	[0.47, 19.5]	[0.28, 19.6]
School end age	1.02	0.83
	[0.19, 5.43]	[0.18, 3.72]
Child labor laws	0.053 **	0.11
	[0.0034, 0.82]	[0.0067, 1.84]
Overall years of school	0.73	1.09
	[0.14, 3.88]	[0.25, 4.83]
Other	0.36	0.24
	[0.067, 1.88]	[0.036, 1.55]
More than 1 country analyzed	1.75	1.62
	[0.73, 4.21]	[0.70, 3.74]

*** p<0.01,

** p<0.05,

* p<0.1

Analysis involved multivariable logistic regression, with robust standard errors clustered by manuscript. Observations with missing point estimates and sample sizes were excluded (N = 33), and observations using “other” statistical methods were excluded due to unstable estimates (N = 4). IV = instrumental variables.