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Education and later-life blood pressure: evidence from compulsory schooling laws and college expansion in the United States

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Education and later-life blood pressure: evidence from compulsory schooling laws and college expansion in the United States

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This manuscript is dedicated to

Mark, Harper, Maddie, and Sprite,

without whom I could never have finished this.

And to Hank,

without whom I could have finished this

much more easily

and

probably sooner.

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And the best cohort-mate and friend

I could have asked

to spend the last 7 years with,

Kristina Dang.

Education and later-life blood pressure: evidence from compulsory schooling laws and college expansion in the United States

Amanda Michelle Irish

Abstract

More schooling predicts better cardiovascular disease (CVD) outcomes, but a causal relationship between education and later-life blood pressure (BP) outcomes is less clear. Most prior work also evaluates the effect of education at the mean of BP, giving little insight into effects across the whole outcome distribution. We leveraged natural experiments, variation in compulsory schooling laws (CSLs) affecting K-12 education in the US as well as variation in the availability of colleges by state in the US, to evaluate how increased education impacted later-life BP. We also evaluate the intention-to-treat (ITT) association of college availability with later-life BP. We used quantile regression to evaluate the effects of education along the distribution of BP.

We performed two-sample instrumental variable (IV) analyses using data from the US Census and from the Health and Retirement Study (HRS) to evaluate how variation in CSLs and college availability impacted later-life BP. In these analyses, we used linear regression in the first stage and both linear and quantile regression in the second stage. In the ITT analysis, we used linear and quantile regression. All models were adjusted for individual- and state-level covariates. To evaluate whether the effects of education varied by race and sex, we also generated results with race by sex interaction terms.

We found no evidence of an effect of education on BP in the overall study population for the CSL IV analysis. In the college IV analysis, we found that increasing the number of colleges in a state decreased SBP, although confidence intervals included the null in CQR models. In the overall ITT analysis, we found no evidence of an association for 2-year colleges, but a small decrease in SBP associated with each additional 4-year college.

In subgroup analyses, however, we found important differences in the effect of education. In the CSL IV analysis, among White women, each additional year of K-12 education consistently lowered SBP across the SBP distribution, while among Black men, education consistently elevated SBP across the SBP distribution. In the college IV analysis, among White women, each additional 2- or 4-year college consistently lowered SBP across the SBP distribution while effect estimates in other subgroups all included the null. In ITT analyses, we found that women had decreased SBP at the upper end of the SBP distribution while men had increased SBP over the same range. Black women consistently had decreased SBP associated with additional 2- and 4-year colleges, although estimates were imprecise.

Quantile regression identified differential relationships between education and blood pressure distribution that mean regression is unable to estimate. Race by sex subgroup analyses showed differential impacts of education on later-life BP. Future research should determine if these results are consistent and to understand why the expected education and blood pressure gradient is reversed for Black men.

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Introduction

Cardiovascular disease (CVD) is the leading cause of mortality in the United States, and its burdens are unequally distributed with respect to racial and ethnic groups[1] and sex.[2] A large body of literature shows that increased educational attainment is associated with lower blood pressure (BP),[3–5] the major risk factor for CVD, but most prior work uses either self-reported hypertension or mean BP as outcomes. Because CVD risk increases non-linearly with BP,[6] these measures fail to capture the effects of exposures or interventions on the right tail of the BP distribution, where risk is highest for morbidity and mortality.

In addition, some prior work on the relationship between education and CVD is correlational, providing little insight into causality. Residual confounding is often an insurmountable problem in observational studies, especially for social exposures like education. Quasi-experimental models may be better able to circumvent confounding,[7] although causal inferences from either approach rely on unprovable assumptions.[8] Quasi-experimental studies on the health benefits from education have mostly focused on other, non-CVD outcomes. For example, increased years of schooling improved later-life self-rated physical health,[9] performance on memory tests,[10] and dementia risk.[11] Two studies have found a causal link between increased educational attainment and lower risk of heart disease,[12,13] although there was no observed effect on the risk of self-reported hypertension.[13] We build on the prior literature by examining how education might impact the population distribution of BP. Because of the nonlinear relationship between BP and CVD risk, a larger effect of education in the high-risk tail would disproportionately reduce mortality risk.

Here we leverage a natural experiment, differences in state compulsory schooling laws, in an instrumental variables (IV) analysis to mitigate the confounding bias characteristic of observational studies. Compulsory schooling laws have been used in prior studies[7,9–11,13–15] as instruments for educational attainment. In the second stage of the IV, we use conditional quantile regression to examine the relationship between education and BP across the distribution of systolic and diastolic BP. We also assess whether the relationship between education and CVD differs by race and sex. Because many states - particularly in the South - had segregated schools during the period over which compulsory schooling laws were changing (1900-1964), we hypothesize that white individuals benefited more from increasing education than Black individuals. We also hypothesize that women benefited more than men, because men, and White men in particular, have access to health-promoting alternatives to education, such as earnings, authority, and other power structures that women and other structurally minoritized groups do not, and thus men may not "need" education to achieve good health.[16]

Methods

Sample: We used the 1980 and 1990 US Census 5% state samples for the first stage of the analysis, and US Health and Retirement Study (HRS) data for the second stage. The 5% state sample is a 1-in-20 national random sample of the public-use microdata samples drawn from the persons included in the full census count who completed the Census long-form; these data were made available through IPUMS.[17] HRS is a nationally representative longitudinal survey of individuals over the age of 50 at baseline and their spouses. The first cohort was established in 1992, with new cohorts introduced every 6 years and interviews conducted biennially.[18]

We applied the same inclusion/exclusion criteria to both the Census and HRS samples. Our instrumental variables were compulsory schooling laws between 1900 and 1964. We restricted the samples to those who reported Black or white race and those born between 1900 and 1965, since Hispanic individuals, other race(s), and those born before 1900 comprised small groups. We also excluded those who were not born in the US or with unknown place of birth, and those born in Alaska and Hawaii (which did not become US states until 1959) since CSLs are not relevant to those who did not reside in US states during childhood. We restricted both samples to exclude those with more than 12 years of education because CSLs are most impactful among those who would not have stayed in K-12 schooling, absent these policies.[14] Finally, we excluded those with missing data on educational attainment, blood pressure measures, or covariates.

Of the 23,944,166 individuals in the 1980 and 1990 US census microsamples, 7,496,752 met our initial inclusion criteria. Of those, 217,446 (2.9%) were excluded for missing data. Our final sample size in the first stage of the analysis was 7,379,306 individuals in the US census. In HRS, there were 25,606 participants in our sample with at least one wave of BP data. Of those, 9,428 met our initial inclusion criteria and 3 participants (0.03%) were excluded for missing data. Thus our final sample size from HRS was 20,257 BP observations from 9,425 individuals for systolic and diastolic BP.

Exposure and instruments: Our exposure was the number of years of K-12 education. We used state CSLs as our instrument, which were operationalized in two ways: first, as the difference between the minimum dropout age and enrollment age, and second, as the difference between the minimum age for obtaining a work permit and enrollment age. We used historical information on state CSLs compiled from federal reports by Lleras-Muney[16], Acemoglu and Angrist[2], and Glymour et al.[3] Since federal reports were typically available biennially, we carried forward the previous value of enrollment age, minimum dropout age, and minimum work permit age if a report was not available that year. Many prior studies have used these state CSLs as an instrument for educational attainment.[1–11]

Outcome: Our outcomes were measured systolic and diastolic blood pressure. Beginning in wave 8 (2006), HRS randomly selected half of participants for an in-home visit with blood pressure measurements; the other half of HRS participants received an in-home visit in wave 9. Blood pressure measurements continued in each subsequent wave, alternating between the original randomly selected subgroups. In each wave, blood pressure is measured three times and the average of the second two measurements is reported if all three measurements are valid. If only one or two measurements are valid, the measurement or the average of the two valid measurements, respectively, are reported. We used all available waves of BP measurements for each individual.

Effect modification: We tested whether there were differences in the association of educational attainment and outcome by race and sex together by using multiplicative interaction terms between the numeric years of schooling variable and indicator variables for membership to each group. In other words, we assessed whether years of schooling had differential effects for white men, Black men, and Black women compared to white women. Multiplicative interaction terms were included in both the first and second stages of the IV.

Covariates: We controlled for state-level covariates to rule out common causes of state-level compulsory schooling laws and later-life cardiovascular outcomes, and for individual-level covariates strongly associated with the outcome to increase statistical precision. State-level characteristics were compiled using Statistical Abstracts of the United States and other US Census Bureau reports,[10] and have been used in prior studies evaluating state CSLs and health outcomes.[10,11,13] They included the percent Black, percent urban, and percent foreign-born when respondents were 6 years old, and the state's manufacturing jobs per capita, manufacturing wages (standardized to 2017 US dollars - the most recent year for which data was available) when respondents were 14 years old. We used linear interpolation for state characteristics between years for which federal reports were available.

Individual characteristics included in our overall model were race, sex, birth year as a restricted cubic spline, and indicator variables for the 9 US Census divisions. In the second-stage model, we additionally included an indicator variable for the wave that blood pressure was measured to account for potential wave-to-wave measurement differences.

Analysis: We first tabulated characteristics of US Census and HRS participants. We then used a two-sample instrumental variables (TSIV) approach. We selected an instrumental variable (IV) approach over linear and conditional quantile regression (CQR) analyses of the education-blood pressure relationship because education and blood pressure have common causes that we are unable to fully account for with confounder adjustment, like parental socioeconomic position or childhood health. IV approaches mitigate the concerns for omitted confounding variables or residual confounding and are an established method in epidemiologic research.[19]

The validity of the IV approach relies on several assumptions: that there are no confounders of the instrument-outcome relationship, that the instrument is associated with the exposure, and that the instrument has no effect on the outcome except through the exposure. We address the first assumption by including time-varying state-level demographic and socioeconomic variables, which has been done in previous analyses using state CSLs as an instrument,[10,11,13,20,21] and by including fixed effects for the census division of birth. The second assumption can be demonstrated in results for the first stage. The third assumption is reasonable because there do not seem to be any viable alternative pathways between CSLs and blood pressure outcomes except through increasing years of schooling. One study found no evidence that increasing CSLs also increased school quality, which is perhaps the only other plausible pathway through which CSLs could act.[22]

We used a TSIV in order to improve statistical power and avoid weak instrument bias in the relatively small HRS sample. The first stage used CSLs to predict educational attainment in the 1980 and 1990 US census samples (analysis restricted to

1900 - 1965 birth cohorts); the predictions of educational attainment were matched to HRS participants by state and year of birth, race, and sex. For example, the predicted educational attainment for a Black woman born in Georgia in 1932 would be applied to any HRS participants matching these characteristics. The second stage then evaluated the relationship between these predicted values of educational attainment and BP.

In the second stage of systolic and diastolic BP models we used OLS regression to estimate the effect on the mean BP for comparison, and the CQR method developed originally by Koenker and Bassett[23] and extended for use with clustered data by Parente and Santos-Silva[24] to estimate the effect at each quantile of BP. Quantile regression allows us to examine the association between the exposure and outcome along the entire distribution of the outcome, as opposed to estimating mean effects as is typically done. CQR is also more robust to outliers than OLS.

The first and second stages are summarized as follows:

$$E(Years\ of\ schooling) \ = \alpha_0 + \alpha_1 CSL + \alpha_2 CSL_w + \alpha_3 X + \varepsilon_1 \qquad (1)$$

$$E(blood\ pressure) \ = \beta_0 + \beta_1 Predicted\ years\ of\ schooling + \beta_2 + \varepsilon_2 \qquad (2)$$

$$Q_\tau(blood\ pressure) \ = \gamma_\tau + \gamma_{1\tau} Predicted\ years\ of\ schooling + \gamma_{2\tau} X + \varepsilon_3 \qquad (3)$$

Equation (1) represents the first stage of the TSIV in which the two state compulsory law instruments are used to predict the educational attainment of every individual in the US census sample. CSL indicates the implied years of schooling based on legal dropout age and CSL_w indicates the implied years of schooling based on work permit age. Equations (2) and (3) represent the second stage of the TSIV in which predicted education from the first stage is used to estimate the mean of blood pressure (equation 2), or the shift in the population distribution of blood pressure (equation 2, where Q_τ represents the conditional quantile function). In all equations, X represents a vector of state- and individual-level characteristics as described above, and the subscript τ represents the τth quantile of the outcome distribution, conditional on the covariates.

We calculated effect estimates and confidence intervals for the mean, the 10th-90th quantiles of systolic and diastolic BP, and the probabilities of the binary outcomes using bootstrapping. By drawing a random sample from the census population 500 times, we produced 500 values of predicted education for each individual. We then used these to estimate β_1 (equation 2) and γ_1 (equation 3). The estimates presented here are the means of these 500 estimates, and the 95% confidence intervals are the estimates at the 2.5th and 97.5th percentiles.

Sensitivity analyses: We evaluated several other outcome specifications for BP by dichotomizing the HRS sample at selected quantiles of the outcome and at clinically important values. Specifically, we dichotomized the sample into whether participants had greater than or equal to the systolic and diastolic BP values at the 10th, 20th, 30th, 40th, 50th, 60, 70th, 80th, and 90th quantiles, and dichotomized the sample into whether participants had greater than or equal to the systolic BP values of 130, 140, 160, and 180mmHg, and diastolic BP values of 80, 90, 100 and 120.[25,26] We then used linear regression to model the probability of having BP greater than or equal to these cut points.

We used Stata version 16.1, R version 4.0.4, and RStudio version 1.4.1106 to conduct the first stage of the IV analysis, and Stata version 17.0 for the second stage.

Analyses were conducted using the University of Michigan's secure server for HRS data. Ethics approval was provided by UC San Francisco (#13-11886). All HRS respondents gave verbal informed consent for their participation in the study, and data collection was approved by the Health Sciences and Behavioral Sciences Institutional Review Board at the University of Michigan.

Results

Table 1.1 shows the demographic characteristics of the analytic sample used for the second stage of the IV analysis. Black individuals tended to have more recent birth years, lower educational attainment, and were more likely to have been born in the South compared to white individuals. Black individuals were more likely to live in states with higher percentages of Black residents, lower urbanicity, lower percentages of foreign-born residents, lower manufacturing jobs per capita, and lower manufacturing wages compared to white individuals.

In the first stage of the overall IV model conducted in the US Census microsample data, we found that increasing the required years of schooling by changing the school dropout age increased Census respondent's schooling by an average of 0.0056 years (0.0029, 0.0084) while changing the work permit age increased schooling by an average of 0.055 years (0.053, 0.058). The F-statistic for the two instruments was 1220. See Table 1.4 for additional first-stage results for models with effect modification.

In our overall OLS model for the second stage of the IV, the effect size for SBP was small and we could not rule out the null hypothesis that additional education had no effect (estimate: 0.60 mmHg, 95% CI: -1.05, 2.19; Table 1.4, Figure 1.2). The overall

CQR models similarly were largely null: we found that the effect estimates had wide confidence intervals and only a few of the estimates had 95% confidence intervals that excluded 0 (Table 1.4, Figure 1.2). Effect sizes for CQR models ranged approximately from -1 to +2 mmHg.

In our models evaluating effect modification, we found that additional schooling was beneficial for white women above about the 60th percentile of SBP, with the largest decreases in SBP at the highest quantiles. For example, each year of education was associated with a decrease of 1.62 mmHg (95% CI: -3.84, 0.53) at the 10th percentile but a decrease of 5.05 mmHg (95% CI: -9.00, -1.12) lower SBP at the 90th percentile.

Interaction terms for the effect of education on systolic BP (Table 1.4) showed that both Black and white men had increased SBP relative to white women consistently over the distribution of SBP. There was little variation in the difference in SBP for white men relative to white women over the distribution of SBP (10th percentile interaction term: 3.31 mmHg, 95% CI: 2.08, 4.68; 90th percentile interaction term: 3.16 mmHg, 95% CI: 0.85, 5.46), while for Black men, the magnitude of the increase in SBP relative to white women also increased at higher percentiles of SBP (10th percentile interaction term: 3.38 mmHg, 95% CI: 1.50, 5.60; 90th percentile interaction term: 7.08 mmHg, 95% CI: 3.70, 10.8). Black women also tended to have increased SBP relative to white women at higher percentiles of SBP, but no evidence of a difference at lower percentiles of SBP (10th percentile interaction term: -0.098 mmHg, 95% CI: -1.45, 1.51; 90th percentile interaction term: 3.87 mmHg, 95% CI: 1.06, 6.62).

Results for diastolic BP showed that in the overall model, increased years of schooling increased DBP by an average of 1.14 mmHg (95% CI: 0.24, 2.06). This increase occurred across the entire distribution of DBP in CQR models. In interaction analyses, we found that there was no evidence of an effect of education on DBP among white women. White men, Black women, and Black men all had increased DBP relative to white women. Full results for DBP are provided in the appendix.

In sensitivity analyses, results for binary quantile-based cut points in the overall model were consistent with the null hypothesis of no effect of education on the probability of having BP over any of the quantiles we assessed. In interaction models, white women (our reference group) had decreased probability of having BP over each quantile we assessed, although not all estimates excluded the null of no effect. Effect sizes were smallest at the tails of the distribution (10th percentile estimate: -0.019, 95% CI: -0.052, 0.011; 90th percentile estimate: -0.015, 95% CI: -0.049, 0.020) and largest for the 40th percentile, which corresponds to a systolic BP of 125 mmHg (40th percentile estimate: -0.068, 95% CI: -0.12, -0.014). Interaction terms for the other groups showed that for the most part, the effect of education for white men, Black women, and Black men was different for the 60th percentile of systolic BP and below.

Results for BP-based cut points in the overall model were consistent with the null hypothesis of no effect of education on the probability of having systolic BP over any of the cut points. In the interaction model, effects of education on systolic and diastolic BP were consistent with the null for white women, and interaction terms for the other groups were largely null as well.

Discussion

We used US census data and HRS participants to examine the relationship between education and later-life CVD outcomes, leveraging variation in CSLs by state and over time. We found no evidence of an overall effect of education on BP outcomes at the population level, but found that increasing education lowered SBP among white women, especially at higher levels of blood pressure, and increasing education increased SBP among Black men across the entire distribution of SBP.

The subgroup findings may explain why our findings in the overall study population were null, and why previous IV analysis found that education did not appear to affect the probability of hypertension.[13] Among the race-by-gender subgroups, only one group, white women, consistently benefited from education and in the other three, education had mixed or deleterious effects which make the overall results mostly null. White women may have been able to uniquely benefit from increased education among the subgroups we studied through a combination of resource substitution and structural racism. Resource substitution posits that education is more beneficial for disadvantaged groups with fewer alternative resources compared to more advantaged groups (cite).[16] In this sense, women are disadvantaged compared to men - especially during the time period over which the women in HRS were completing their education.

Structural racism and intersectionality, on the other hand, may explain why Black participants either did not benefit or were harmed. There are several potential mechanisms: firstly, the quality and quantity of education for Black children was vastly different from that of white children over this period, most of which was during the *de jure* and *de facto* eras of school segregation, particularly in the South where most Black HRS participants were born.[27] School segregation has been shown to have substantial adverse health effects.[28–30] Other studies have also shown that benefits of education were not accessible for Black men in particular.[27,31,32] Secondly, Black men may experience interpersonal racism in school settings and highly educated Black men may encounter more interpersonal racism outside of school if their educational attainment challenges racial hierarchies. The health harms of increases in interpersonal racism associated with additional schooling may outweigh potential health benefits of additional education, at least during the years of the present study.

There were several limitations to our study. For the two-sample approach, we rely on the assumption that the relationship between education and the covariates that we used to match predictions from the first-stage census sample to the second-stage HRS sample is the same for both groups. A limitation common to all IV analyses is that the untestable assumption of no confounding between the instrument and outcome association conditional on covariates. We attempted to mitigate this by adjusting for state-level characteristics. IV analyses are typically interpreted as the “local average treatment effect” under a monotonicity assumption that increases in CSLs do not decrease anyone’s educational attainment. In the case of quantile regression, this becomes a “local quantile treatment effect” that is also subject to the monotonicity assumption.[33] Under this assumption, the local quantile treatment effect in this case applies specifically to the group of “compliers”--individuals whose educational attainment increased because of CSL implementation. Effects of increases in education may differ for these individuals than others in the population. These findings may not be generalizable to individuals not identifying as Black or white, or to people in more recent birth cohorts, since the most active period of increases in implied length of schooling occurred prior to the Civil Rights era.

The primary strengths of our study are that we used a quasi-experimental study design to generate rigorous estimates of the effect of education on BP and CVD; and that we were able to perform subgroup analyses to assess how the relationship between education and outcomes varied by race and sex. In addition, we used conditional quantile regression to explore whether the exposure-outcome relationship differed across the distribution of BP in our study sample. We used measured BP for our primary outcomes which is not subject to the same type of measurement biases that affect self-reported outcome measures. Finally, we used a large and diverse sample of older US adults.

Our findings suggest that a focus on increased educational attainment in early life may have substantial cardiovascular health benefits in later life for white women and may have harmful effects in Black men. These results highlight the importance of both subgroup analyses and quantile regression to explore how an exposure reshapes the outcome distribution. The effects of early education are particularly relevant at this time, as multiple US states are weakening child labor protections,[34] which will adversely impact educational attainment. Future work should establish the mechanisms through which white women benefit, and through which Black men are harmed, so that future educational policy can achieve equity across all population subgroups.

Table 1.1 Health and Retirement Study participant characteristics for measured blood pressure analyses. State-level characteristics are lagged so that the year of the state-level data corresponds to the year in which the respondent was either 6 or 14 years old, depending on the characteristic. Characteristics thought to be important for school enrollment are lagged to 6 years of age and those thought to be important for leaving school are lagged to 14 years of age.

Sample characteristic	**Total**	**White women**	**White men**	**Black women**	**Black men**
N	9425	4176	2876	1400	973
Individual-level covariates					
Birth year, median (IQR)	1941 (1932, 1952)	1939 (1931, 1949)	1939 (1931, 1949)	1950 (1938, 1957)	1948 (1937, 1956)
Range	1905-1965	1905-1965	1907-1965	1906-1965	1913-1965
Years of schooling, median (IQR)	12 (11, 12)	12 (11, 12)	12 (11, 12)	12 (10, 12)	12 (10, 12)
US census region, %					
Northeast	19.1	22.9	22.2	9.1	8.5
Midwest	30.4	36.4	39.3	8.8	9.8
South	44.9	33.4	32.4	80.4	80.0
West	5.6	7.4	6.2	1.6	1.8
State-level covariates					
Percent Black in state of residence at age 6, median (IQR)	7.5 (4.2, 23.3)	6.0 (3.5, 14.0)	6.0 (3.3, 12.6)	25.7 (13.7, 34.3)	25.1 (12.8, 33.4)
Percent urban in state of residence age 6, median (IQR)	58.5 (40.5, 72.6)	62.7 (43.1, 73.4)	63.1 (44.3, 73.6)	47.3 (35.0, 66.9)	50.2 (36.7, 67.3)
Percent foreign-born in state of residence age 6, median (IQR)	4.3 (0.7, 9.7)	5.9 (1.8, 11.5)	6.0 (1.9, 11.2)	0.6 (0.4, 3.7)	0.6 (0.4, 4.0)

Sample characteristic	Total	White women	White men	Black women	Black men
Manufacturing jobs per 1000 population in state of residence at age 14, median (IQR)	68.6 (46.1, 92.0)	69.5 (46.2, 94.2)	70.7 (47.5, 94.4)	63.2 (45.0, 77.9)	62.9 (42.5, 81.6)
Manufacturing wages (2017 USD) in state of residence at age 14, median(IQR)	31,700 (24,700 , 39,800)	31,400 (24,900 , 39,500)	31,900 (25,500 , 41,100)	31,200 (23,200 , 37,900)	32,900 (24,200 , 38,700)

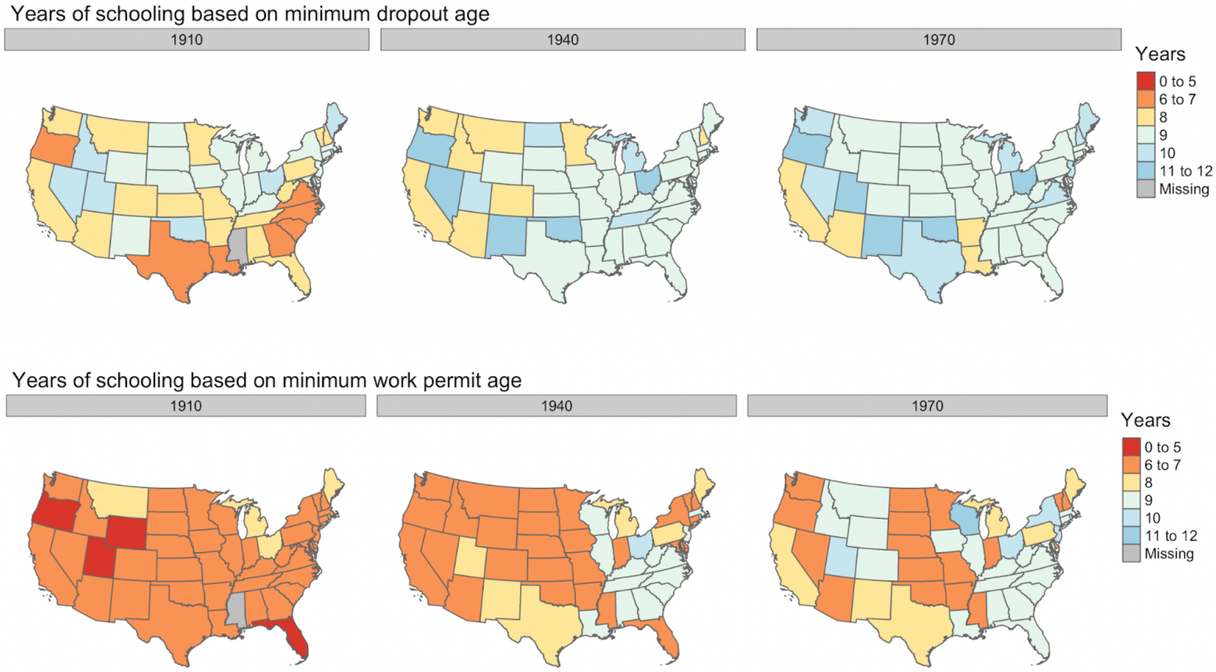

Figure 1.1 Years of schooling implied by state compulsory schooling laws.

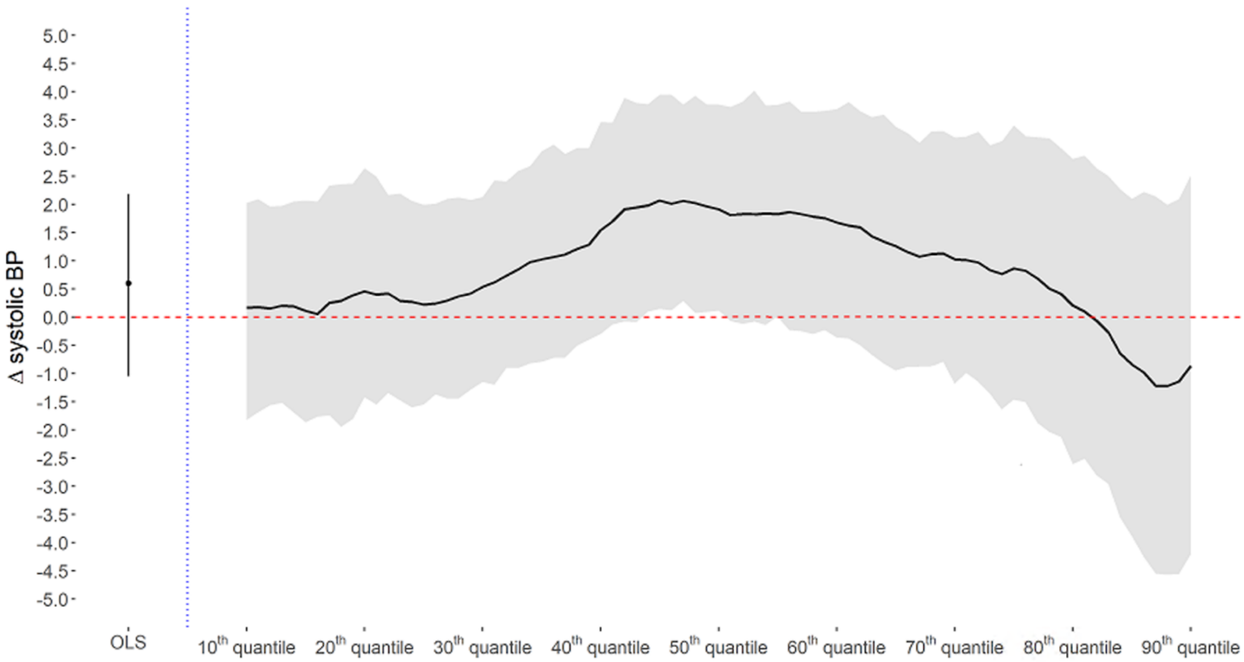

Figure 1.2 Linear and conditional quantile regression effect estimates for change in systolic blood pressure in the overall sample. OLS = ordinary least squares. N=9,425 individuals with 20,257 DBP measurements. Effects are measured in mmHg. Overall model includes both instruments (implied years of schooling according to legal dropout age and work permit age) and is adjusted for birth year, sex, race, census division of birth, year of BP measurement, and state characteristics: percentage Black, urban, and foreign-born residents, manufacturing jobs per capita, and manufacturing wages.

Appendix

Diastolic BP results

In our overall OLS model for the second stage of the IV (Table 1.5, Figure 1.3), we found that each additional year of education increased mean diastolic BP by 1.14 mmHg (95% CI: 0.24, 2.06). Similarly, each additional year of education increased diastolic BP across all quantiles in the CQR model (Table 1.5, Figure 1.3), with quantiles between approximately the 25th and 80th showing the largest increases. These were similar in magnitude to the OLS estimate.

In our models evaluating effect modification of race and sex (Table 1.5), results for white women were consistent with no effect of education on diastolic blood pressure in both the OLS model and generally across quantiles of the CQR models. Interaction terms (Table 1.5) for the difference in effect of education for white men, Black women, or Black men were all consistent with increasing diastolic BP for these groups relative to white women in the OLS and CQR models. The magnitude of the additional effect was similar across quantiles for white and Black men. Results for Black women show an increasing magnitude of the effect from lower to upper quantiles, but the effect estimates across quantiles were not statistically significantly different from one another.

Table 1.2 US Census 1980 and 1990 5% microsample characteristics. State-level characteristics are lagged so that the year of the state-level data corresponds to the year in which the respondent was either 6 or 14 years old, depending on the characteristic. Characteristics thought to be important for school enrollment are lagged to 6 years of age and those thought to be important for leaving school are lagged to 14 years of age.

Sample characteristic	**Total sample**	**White women**	**White men**	**Black women**	**Black men**
N	7,379,306	3,578,180	2,914,333	490,005	396,788
Individual-level covariates					
Birth year, median (IQR)	1934 (1921, 1949)	1932 (1920, 1947)	1935 (192, 1949)	1938 (1924, 1951)	1940 (1926, 1952)
Range	1900-1965	1900-1965	1900-1965	1905-1965	1901-1965
Years of schooling, median (IQR)	12 (10, 12)	12 (10, 12)	12 (9, 12)	11 (9, 12)	11 (8, 12)
US census region, %					
Northeast	24.5	27.2	26.0	8.5	8.6
Midwest	30.7	33.4	33.9	9.0	9.5
South	37.5	31.4	32.0	80.7	79.8
West	7.3	8.0	8.1	1.9	2.1
State-level covariates					
Percent Black in state of residence at age 6	6.3 (2.9, 16.5)	5.3 (2.5, 11.3)	5.6 (2.5, 12.4)	26.8 (12.8, 35.5)	26.3 (12.6, 35.1)
Percent urban in state of residence at age 6	59.6 (39.0, 72.3)	61.7 (40.9, 73.1)	61.9 (41.3, 73.1)	41.2 (29.5, 63.2)	43.1 (30.5, 65.1)
Percent foreign-born in state of residence at age 6	5.8 (1.3, 12.7)	6.9 (2.2, 14.2)	6.4 (2.0, 13.2)	0.8 (0.4, 4.3)	0.9 (0.4, 4.4)
Manufacturing jobs per 1000 population in state of residence at age 14	69.5 (44.6, 92.1)	71.1 (45.5, 93.8)	70.5 (45.5, 93.0)	58.7 (38.2, 78.5)	59.1 (38.5, 79.7)
Manufacturing wages (2017 USD) in state of residence at age 14	27,400 (19,100 , 37,900)	26,900 (18,900 , 37,600)	28,100 (19,500 , 38,200)	25,400 (16,900 , 35,300)	27,000 (18,100 , 36,700)

Table 1.3 Stage 1 linear estimates for effect of instruments on additional educational attainment in the US Census 1980 & 1990 microdata sample. Effects are measured in additional years of education. All models use data from the 1980 and 1990 US census 5% microsample. Overall model includes both instruments (implied years of schooling according to legal dropout age and work permit age) and is adjusted for birth year, sex, race, census division of birth, year of BP measurement, and state characteristics: percentage Black, urban, and foreign-born residents, manufacturing jobs per capita, and manufacturing wages. Interaction model sets white women as the reference group and include interaction terms for implied years of schooling according to legal dropout age x race x sex and implied years of schooling according to work permit age x race x sex. Overall models by race-sex subgroups are the same as the overall model except do not include race and sex as covariates since they are limited to a single level of those variables.

	Years of schooling implied by legal dropout age	**Years of schooling implied by work permit age**	**F-statistic**
Overall model N = 7,379,306	0.0056 (0.0029, 0.0084)	0.055 (0.053, 0.058)	1220
Interaction model with white women as reference group N = 7,379,306	-0.047 (-0.050, -0.044)	0.0090 (0.0061, 0.012)	420
Overall model, white women N = 3,578,180	-0.017 (-0.022, -0.014)	0.058 (0.055, 0.061)	646
Overall model, white men N = 2,914,333	0.00040 (-0.0042, 0.0050)	0.043 (0.039, 0.047)	259
Overall model, Black women N = 490,005	0.042 (0.030, 0.053)	0.026 (0.015, 0.037)	63
Overall model, Black men N = 396,788	0.053 (0.038, 0.067)	0.025 (0.011, 0.039)	53

Table 1.4 Stage 2 Linear and conditional quantile regression estimates for effect of one additional year of schooling on systolic blood pressure. N=9,425 individuals with 20,257 SBP measurements. Effects are measured in mmHg. Overall model includes both instruments (implied years of schooling according to legal dropout age and work permit age) and is adjusted for birth year, sex, race, census division of birth, year of BP measurement, and state characteristics: percentage Black, urban, and foreign-born residents, manufacturing jobs per capita, and manufacturing wages. Interaction model sets white women as the reference group and includes interaction terms for predicted x race x sex.

Model	**Estimated Effect on Mean (OLS Coefficients)**	**Estimated Effect on Quantiles (Conditional Quantile Regression Coefficients)**				
		10th percentile	**25th percentile**	**50th percentile**	**75th percentile**	**90th percentile**
Overall	0.60 (-1.05, 2.19)	0.17 (-1.82, 2.02)	0.22 (-1.55, 1.99)	1.92 (0.12, 3.78)	0.86 (-1.47, 3.40)	-0.86 (-4.20, 2.52)
Interaction model						
White women (ref)	-2.73 (-4.53, -1.01)	-1.62 (-3.84, 0.53)	-2.03 (-4.10, -0.13)	-1.34 (-3.43, 0.59)	-2.70 (-5.87, 0.33)	-5.05 (-9.00, -1.12)
Interaction term for white men	3.04 (2.11, 4.02)	3.31 (2.08, 4.68)	3.48 (2.35, 4.56)	2.51 (1.37, 3.74)	2.77 (1.19, 3.69)	3.16 (0.85, 5.46)
Interaction term for Black women	1.71 (0.63, 2.85)	-0.098 (-1.45, 1.51)	0.87 (-0.50, 2.20)	1.78 (0.50, 3.04)	2.43 (0.49, 4.30)	3.87 (1.06, 6.62)
Interaction term for Black men	4.90 (3.32, 6.34)	3.38 (1.50, 5.60)	4.56 (2.68, 6.33)	4.44 (2.87, 6.34)	5.31 (2.94, 7.52)	7.08 (3.70, 10.8)

Table 1.5 Stage 2 Linear and conditional quantile regression estimates for effect of one additional year of schooling on diastolic blood pressure. N=9,425 individuals with 20,257 DBP measurements. Effects are measured in mmHg. Overall model includes both instruments (implied years of schooling according to legal dropout age and work permit age) and is adjusted for birth year, sex, race, census division of birth, year of BP measurement, and state characteristics: percentage Black, urban, and foreign-born residents, manufacturing jobs per capita, and manufacturing wages. Interaction model sets white women as the reference group and includes interaction terms for predicted x race x sex.

Model	OLS estimate	CQR estimates				
		10th percentile	25th percentile	50th percentile	75th percentile	90th percentile
Overall	1.14 (0.24, 2.06)	0.21 (-1.03, 1.47)	1.31 (0.35, 2.25)	1.28 (0.20, 2.48)	1.65 (0.15, 3.10)	0.37 (-1.98, 2.49)
Interaction model						
White women (ref)	-0.26 (-1.34, 0.72)	-0.33 (-1.69, 1.12)	0.33 (-0.83, 1.60)	-0.44 (-1.75, 0.81)	0.090 (-1.45, 1.74)	-0.85 (-3.19, 1.32)
Interaction term for white men	1.01 (0.49, 1.55)	1.15 (0.39, 2.02)	1.17 (0.53, 1.80)	1.44 (0.82, 2.04)	1.04 (0.24, 1.90)	0.21 (-0.92, 1.33)
Interaction term for Black women	1.35 (0.71, 2.01)	0.81 (-0.086, 1.71)	0.88 (0.17, 1.65)	1.23 (0.49, 2.00)	1.30 (0.27, 2.22)	1.93 (0.61, 3.23)
Interaction term for Black men	2.47 (1.64, 3.40)	2.06 (0.86, 3.36)	2.12 (1.03, 3.30)	2.80 (1.86, 3.83)	2.41 (1.11, 3.61)	2.28 (0.53, 4.01)

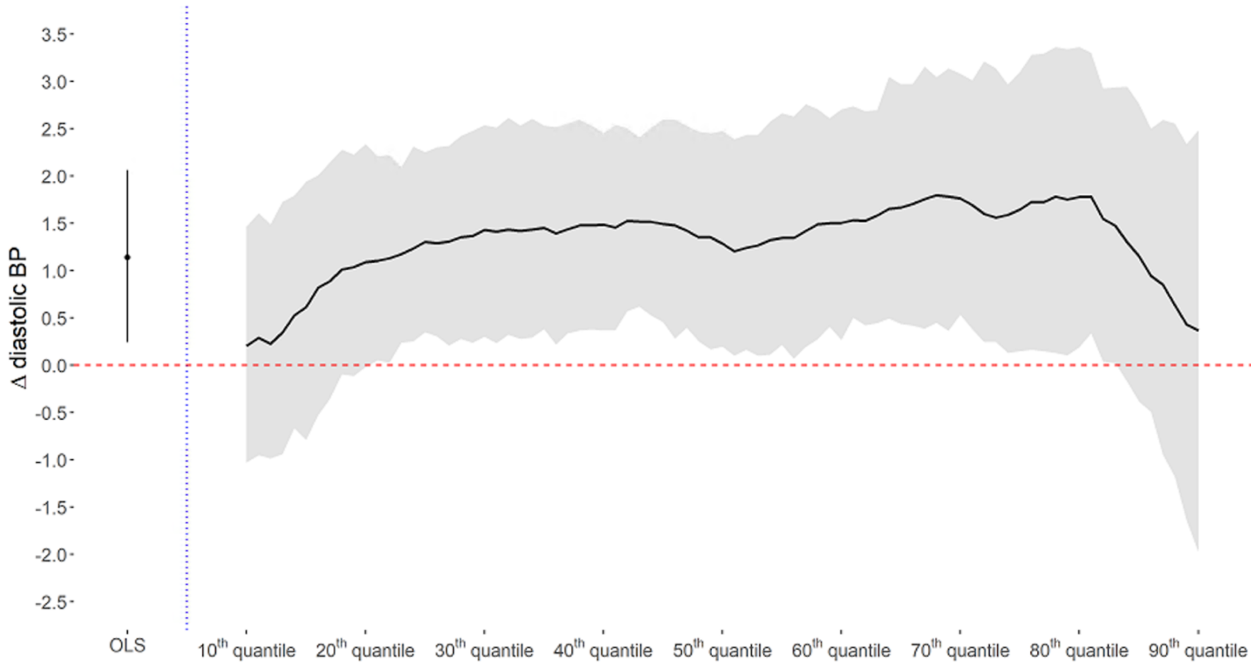

Figure 1.3 Linear and conditional quantile regression effect estimates for change in diastolic blood pressure. N=9,425 individuals with 20,257 DBP measurements. Effect estimates are in units of mmHg. Models adjusted for birth year, sex, race, census division of birth, year of BP measurement, and state characteristics: percentage Black, urban, and foreign-born residents, manufacturing jobs per capita, and manufacturing wages.

Chapter 2: Impact of increased years of college education on cardiovascular risk: An instrumental variable quantile analysis of a natural experiment

Introduction

Hypertension is a leading modifiable risk factor for coronary heart disease (CHD) and stroke.[6,35] Almost half of the adult population in the US has hypertension (47%, 116 million people).[35] Risks are strongly patterned according to sociodemographic characteristics: racially minoritized individuals experience higher incidence and poorer control of hypertension compared to white peers.[36–38] Risks also increase over the range of blood pressure: the risk for progression to coronary heart disease, stroke, and other sequelae increases nonlinearly with blood pressure.[6] This has important implications for designing interventions: interventions differentially impacting the higher end of the blood pressure distribution (e.g. impacting those at highest risk of progression to CHD) may have larger public health effects.

Prior work has shown an inverse relationship between education and cardiovascular disease (CVD) outcomes including blood pressure (BP), CHD, and stroke: more education is associated with lower blood pressure and lower risk of CHD and stroke in later life.[13,39–45] Confounding is a barrier to estimating the causal effect of education on health outcomes, so some recent literature has emphasized quasi-experimental approaches. Most quasi-experimental studies on the health effects of education have leveraged changes in compulsory schooling laws or improvements in the quantity of K-12 education.[7,10,11,14,20,27,39,46] These studies have shown mixed effects for hypertension and no effect for heart disease.[46] The quasi-experimental literature focusing on the health effects of college education (in this paper, we will use the term "college" interchangeably with "postsecondary" to refer to education beyond a high school degree at a 2- or 4-year degree-granting institution) is sparser. No prior quasi-experimental studies to our knowledge have examined the effects of college education on blood pressure although studies have shown decreases in mortality,[47] improved birth outcomes,[48] and improved cognition at older ages.[49]

Most work on blood pressure has focused on mean blood pressure values or on a binary measure of hypertension. Focusing on effects of education on mean blood pressure is reasonable if the relationship between education and blood pressure is homogeneous over the distribution of blood pressure values, e.g., if the effect of education on the mean equals the effect of education on the median or the 90th percentile of blood pressure. However, this assumption of homogenous effects is not necessarily true and overlooks potentially important variation. For example, if education increased BP at the upper end of the distribution and decreased BP at the lower end of the distribution, this might result in the same effect at the mean BP as if education decreased BP at the upper end of the distribution and increased BP at the lower end of the distribution, but these two scenarios would have very different population health effects.

Risks are also patterned along sociodemographic lines, but little work has investigated whether there are differential effects of education according to subgroups defined by both race and gender. One study found that increased education was associated with lower BP in white adults, but Black men had increased BP with more education and Black women had no association.[43] Other studies suggest that Black women may benefit more from education than other groups: one study found that additional education may benefit Black women's mental health more compared to the effect in white men.[50] Another found that longer school term lengths were associated with lower BP measurements for Black women, but not others.[27] Because of the structural and interpersonal racism, Black individuals routinely experience discrimination, including in school settings, which has in turn been associated with negative health effects.[51,52] Women may have a stronger relationship between education and decreased BP compared to men, regardless of race.[45]

In this paper, we use an instrumental variables study design coupled with quantile regression to investigate whether the effect of education on blood pressure is heterogeneous along the distribution of blood pressure. We also use interaction terms to investigate whether there are differential effects for race and gender-specific groups (specifically, white men, white women, Black men, and Black women). Testing for heterogeneity of effects will inform future policy decisions to target implementation towards at-risk groups.

Methods

Source population and analytical sample: We used the US Census 1990 5% sample in the first stage of the analysis. The 5% state sample is a 1-in-20 national random sample of the public-use microdata samples drawn from the persons included in the full census count who completed the Census long-form. Data were obtained through IPUMS.[17]

The Health and Retirement Study (HRS) is a nationally representative population-based cohort study of non-institutionalized US adults aged 50 and over and their spouses of any age.[18] Demographic and health information has been collected by the University of Michigan via interview every 2 years since 1992. We used the HRS tracker data, longitudinal HRS data processed by the RAND Corporation,[53] and data from the Life History Mail Survey of HRS in this study.

Our analytical sample consisted of US-born residents born between 1920 and 1962 for both the Census and HRS samples. In addition, HRS respondents were included if they met our inclusion criteria of 1) providing data in the education section of Life History Mail Survey in 2015 or 2017, which collected more detailed K-12 educational history, including the city and state of the respondent's school, 2) had at least one instance of valid SBP and DBP measurement between 2006-2018. Our final analytical sample size was 5,502,951 respondents from the US census and 7,358 respondents from HRS with 20,860 BP measurements.

Exposure: Years of schooling is our exposure of interest, which we instrumented with the number of 2- and 4-year colleges open in each year in each US state and the District of Columbia, standardized to the estimated number of 18- to 22-year-olds for each combination of state and year. To construct the original county-level dataset, Currie and Moretti[48] collected data on all postsecondary schools open in 1996. The authors included both geographic (US county where school was located) and temporal (years the school was open) information to construct a dataset with undergraduate-level schools open between 1940-1996. For details regarding the construction of the county-level college dataset, see Currie and Moretti.[48] Because county-level microdata is not consistently available for all counties in the US Census, we aggregated the county-level college data up to the state level. We only included years which matched the birth year range of our HRS sample, 1940-1982. This state-level dataset was matched with US census respondents by year and state, so that each individual in the census was associated with the number of 2- and 4-year colleges open in the state in which they were born in the year they turned 18.

Outcome: Our outcomes of interest were measured systolic and diastolic BP (measured in mmHg) in the RAND longitudinal data. Beginning in 2006, a subsample of HRS respondents were selected for an enhanced in-person interview which included physical measurements. In the next wave, the remaining respondents were selected for the enhanced interview. HRS respondents were not able to complete the face-to-face interview if they were living in a nursing home, had a proxy respondent, or had to conduct their interview by telephone. BP was measured three times; the RAND measurements are the mean of the second and third BP measurements if all three were valid. If there were two valid measurements, the mean was reported. If there was one valid measurement, that number was reported. We used all available BP measurements in the RAND data for BP outcomes between 2006-2018.

Covariates: State-level covariates were percentage of Black residents, percentage of residents living in an urban area, and percentage of foreign-born residents, manufacturing jobs per capita, and manufacturing wages (standardized to 2017 US dollars - the most recent year for which data was available) for each state in the state of the respondent's birth (US census respondents) or when the respondents were 18 years of age (HRS respondents). The state characteristics were compiled previously by Glymour,[10] Lleras-Muney,[54] and updated for the current analysis using Statistical Abstracts of the United States and federal manufacturing employment data.[55] State characteristics were linearly interpolated for the years between reports.

Individual-level covariates were also included for characteristics strongly associated with the outcome to increase statistical precision, and to account for the possibility of unmeasured confounders associated with these individual characteristics that are common causes of the instrument and outcomes. We adjusted for race (Black, White, Hispanic, other), sex (female, male), indicator variables for the 9 US census divisions, and birth year (operationalized as a restricted cubic spline).

Analysis: First, we tabulated characteristics of US census and HRS respondents. Then, we then used a two-sample instrumental variables (TSIV) approach to estimate the effect of education on BP. We selected an instrumental variable (IV) approach over adjusting for known confounders because education and blood pressure have common causes that are not measured in our dataset or cannot be fully controlled for with the variables we do have (for example, parental socioeconomic position or childhood health). IV approaches mitigate the concerns for omitted confounding variables or residual confounding.[19]

The validity of the IV approach relies on several assumptions: that there are no confounders of the instrument-outcome relationship, that the instrument is associated with the exposure, and that the instrument has no effect on the outcome except through the exposure. We address the first assumption by including time-varying state-level demographic and socioeconomic variables, which has been done in previous studies using number of colleges as an instrument[48] as well as in studies using state compulsory schooling laws to instrument for education,[10,13,54] and by including fixed effects for the census division of birth. The second assumption can be demonstrated in results for the first stage. The third assumption may be violated if increasing the number of 2- and 4-year colleges affects blood pressure through mechanisms other than an individual's own education: for example, through increasing a spouse's education. This seems less likely in this study sample, as most couples in HRS had similar educational backgrounds.

We used a TSIV approach rather than an IV in just the HRS sample to avoid weak instrument bias in our relatively small sample.[56] The first stage used the number of 2- and 4-year colleges and ordinary least squares (OLS) regression to predict educational attainment in 1990 US census samples (analysis restricted to 1922 - 1962 birth cohorts). Then the predictions of educational attainment from census data were matched to HRS participants by state and year of birth, race, and sex. The first stage is summarized in the following equation:

$$E[Years\ of\ schooling \mid C, 2yr\ colleges, 4yr\ colleges] = \alpha_0 + \alpha_1 2yr\ colleges + \alpha_2 4yr\ colleges + \alpha_3 C + \varepsilon_1 \quad (1)$$

In equation (1), the two instruments for college education are used to predict the educational attainment of every individual in the US census sample. *C* represents a vector of state- and individual-level characteristics as described above.

The second stage then evaluated the relationship between these predicted values of educational attainment and BP using OLS regression and conditional quantile regression. In systolic and diastolic BP models we used the CQR method developed originally by Koenker and Bassett[23] and extended for use with clustered data by Parente and Santos-Silva[24] to estimate the effect at each quantile of BP. Unlike linear regression, quantile regression allows us to examine the association between the exposure and outcome along the entire distribution of the outcome. It is also more robust to outlying values of the outcome than OLS.

$$E[Blood\ pressure \mid C, Predicted\ years\ of\ schooling] = \beta_0 + \beta_1 Predicted\ years\ of\ schooling + \beta_2 C + \varepsilon_2 \quad (2)$$

$$Q_\tau(Blood\ pressure \mid C, Predicted\ years\ of\ schooling) = \delta_{0\tau} + \delta_{1\tau} Predicted\ years\ of\ schooling + \delta_{2\tau} C + \varepsilon_3 \quad (3)$$

Equations (2) and (3) represent the second stage of the TSIV in which predicted education from the first stage is used to estimate the mean of blood pressure (equation 2), or the shift in the population distribution of blood pressure (equation 3, where Q represents the conditional quantile function). In all equations, *C* represents a vector of state- and individual-level characteristics as described above, ε represents an error term, and the subscript τ represents the τth quantile of the outcome distribution, conditional on the covariates.

We bootstrapped the effect estimates and confidence intervals for the mean and the 10th-90th quantiles of systolic and diastolic BP. By drawing a random sample from the census population 500 times, we produced 500 different values of predicted education for each individual. We then used these to estimate β_1 (equations 2), and γ_1 (equation 3) 500 separate times. The estimates presented here are the means of these 500 estimates, and the 95% confidence intervals are the estimates at the 2.5th and 97.5th percentiles.

We also conducted interaction analyses for race and sex subgroups, but only reported results for Black men and women and white men and women, as the sample size for Hispanic and “other” racial groups were too small to draw meaningful conclusions. White men were used as the reference group for interaction terms because in the context of structural racism and intersectionality, they are the most advantaged group. To obtain effects of education in each subgroup, we rotated the reference group in the interaction analysis. For example, to obtain the effect of education in Black women, we set Black women as the reference group and included interaction terms for Black men, white women, and white men.

We conducted a sensitivity analysis in which we adjusted for state of birth (census) or state of residence at age 18 (HRS) instead of census division. We dropped observations from states with fewer than 50 participants from this analysis; a total of 228 participants (3.1% of the original 7,358-participant sample) from 14 states and the District of Columbia were excluded.

We conducted the first stage of the analysis in R version 4.1.3 and RStudio version 2023.03.1+446 using the `lm` function. We conducted the second stage in Stata version 17.0 using the `regress` and `quantreg2` commands. Ethics approval was provided by UC San Francisco (#13-11886). All HRS respondents gave verbal informed consent for their participation in the study, and data collection was approved by the

Health Sciences and Behavioral Sciences Institutional Review Board at the University of Michigan.

Results

Compared to US census respondents (Table 2.1), HRS respondents were younger (mean birth year 1946 vs. 1940), had completed more years of schooling (13.8 years vs. 12.7 years), and were more likely to be female (59.8% vs. 51.6%). HRS participants were also more likely to identify as Black (16.0% vs. 9.6%), which is expected since HRS oversamples Black individuals.

Table 2.2 shows that the first stage results indicated the instruments were strong (F-statistic = 1051) and that having one additional 2-year college increased educational attainment by 0.37 years (95% CI: 0.35, 0.39) on average, while one additional 4-year college increased educational attainment by 0.48 years (95% CI: 0.45, 0.51) on average.

Findings from the second-stage OLS and CQR models (Table 2.3, Figure 2.1) for systolic BP were consistent with each other in direction and magnitude. The OLS estimate indicated a decrease of 1.79 mmHg (95% CI: -3.54, -7.70e-5) in SBP for each additional year of education, while the CQR estimate at the median was a decrease of 1.08 mmHg (95% CI: -3.18, 1.06). The CQR estimates were larger in magnitude at the two extremes of the distribution: at the 10th quantile, there was a decrease of 2.74 mmHg (95% CI: -5.20, -0.38); at the 90th quantile, there was a decrease of 3.11 mmHg (95% CI: -7.09, 1.06). However, confidence intervals were wide and the estimates are not different enough from one another across the distribution of SBP to provide conclusive evidence of a pattern.

Subgroup analysis showed decreases in SBP were primarily driven by effects in white women. Decreases in systolic blood BP for white women were greatest at the upper end of the systolic blood pressure distribution, at the 70th, 80th, and 90th quantiles. In the HRS sample among white women, these percentiles correspond to systolic BPs of 134, 140, and 150 mmHg, respectively. White women were the only subgroup with consistent decreases across the SBP distribution, and decreases were largest at the highest quantiles: for example, at the 10th quantile, there was a decrease of 2.34 mmHg (95% CI: -4.59, -0.027), while at the 90th quantile, there was a decrease of -4.93 mmHg (95% CI: -8.34, -1.73) for each additional year of college education.

Findings for other subgroups were largely null across the distribution of SBP. In Black women, there were small increases in SBP across the distribution, but point estimates at all quantiles included the null. For example, at the 10th quantile there was a 0.31 mmHg increase in SBP (95% CI: -2.99, 3.45) and at the 90th quantile there was a 0.028 mmHg increase (95% CI: -4.55, 4.80). White men had a similar pattern to Black women. For example, at the 10th quantile there was a 0.89 mmHg increase in SBP (95% CI: -1.45, 3.26) and at the 90th quantile there was a 1.02 mmHg increase (95% CI: -2.56, 4.60). In Black men, the direction of point estimates was mixed, but all point estimates included the null. At the lower end of the distribution, SBP decreased with each additional year of college education, while at the middle and upper ends of the distribution, SBP increased. For example, at the 10th quantile there was a 1.55 mmHg decrease in SBP (95% CI: -4.74, 1.13) and at the 90th quantile there was 0.90 mmHg increase (95% CI: -3.67, 5.72).

Interaction terms estimated the difference in the effect of education on BP for white women, Black women, and Black men compared to white men. Interaction terms for white women were negative compared to white men and statistically significant across the entire distribution of SBP. Interaction terms for Black women were null over the entire distribution of SBP, indicating no significant difference of effect of education compared to white men. The interaction terms for Black men compared to white men were negative at the lower end of the distribution of SBP and null over the remainder of the quantiles.

Results for DBP were qualitatively similar to those for SBP and are detailed in the appendix. Results from a sensitivity analysis in which we adjusted for state of birth (census) or state of residence at age 18 (HRS) instead of census division were similar in direction and magnitude to results for our main analysis; however, confidence intervals were very wide.

Discussion

We found evidence that increases in college education decrease systolic BP in later life among white women but not for white men, Black men, or Black women using national data from the 1990 US census and HRS and sophisticated analytic methods. Decreases in systolic blood BP for white women were greatest at the upper end of the systolic blood pressure distribution. With effect estimates in the -4 to -5 range at higher systolic BP percentiles, the effect of an additional year of college education is about one-third to half that of a typical response to antihypertensive medication. Given that 47% of white women in our US census sample have completed one year of college or more, these results represent a huge population-level effect among those at highest risk for adverse cardiac outcomes. As such, these findings have implications for health equity and future policy directions.

Larger decreases for white women at the highest systolic BPs indicate a reshaping of the BP curve for this demographic group, pulling in the tail of the distribution to one of lower risk. In addition, decreases along the entire distribution of BP for white women indicate a shifting of the curve to the left. These benefits are consistent with the type of population approach to addressing public health problems (“addressing the determinants of incidence”) discussed by Rose in his seminal paper[57] - except here, we only found evidence that these benefits extend to white women.

Results from interaction analyses are consistent with prior work showing that women and white people have benefited more from education in other health outcomes: women had a greater decrease in depression score[16] and self-rated mental health[50] for each additional year of education compared to men, and white people with additional education were less likely to be obese[58] or current smokers[59] than Black people with additional education. Other prior work, however, has shown that Black women’s mental health[50] and BP measures[27] may benefit more from education than other groups. While our study did not confirm these findings, it was underpowered to detect effects in smaller groups so our results also do not provide evidence to contradict these prior results in Black women.

Our study has several strengths. We used a natural experiment to generate rigorous estimates of the effects of education on BP, which were robust to alternative covariate specification. We used objectively measured outcomes rather than self-reported ones. Using quantile regression allowed us to assess for heterogeneity of effects along the distribution of measured BP. Finally, we conducted interaction analyses by race and gender subgroups to assess for heterogeneity of effects for these important sociodemographic factors.

Our study also has limitations. For the two-sample approach to be valid, we rely on the assumption that the relationship between education and the covariates that we used to match predictions from the first-stage census sample to the second-stage HRS sample is the same for both groups. The census and HRS samples have similar covariate values, which lends support that we met this assumption. A limitation common to all IV analyses is the untestable assumption of no confounding between the instrument and outcome association conditional on covariates. IV analyses are typically interpreted as the “local average treatment effect” under a monotonicity assumption that increases in number of colleges do not decrease anyone’s educational attainment. In the case of quantile regression, this becomes a “local quantile treatment effect” that is also subject to the monotonicity assumption. Under this assumption, the local quantile treatment effect in this case applies specifically to the group of “compliers” (individuals whose educational attainment increased because of the increase in the availability of college education). Effects of increases in education may differ for these individuals than others in the population; however, this population is relevant for assessment of policy and planning future policy, since they are the ones affected by the policy.

As noted previously, there are several untestable assumptions for the IV approach to be valid: that there is no confounding of the relationship between the instrument and outcome conditional on covariates, and that the instrument does not affect the outcome except through the exposure. In this case, we addressed the potential confounding issue by controlling for as many potential confounders as we were able. It is possible that increasing the number of colleges could influence BP through other mechanisms, such as by increasing a spouse's education or another family member's education.

There may be selection bias in our HRS sample since BP was only measured beginning in 2006: especially in older cohorts, participants needed to survive to 2006 to be included in our study. In addition, participants who required proxies did not have physical measurements, including BP, performed. These participants are more likely to have poorer overall health so our sample may be healthier than the target population. In our interaction analyses, Hispanic and "other" racial and ethnic groups were too small to draw meaningful conclusions, so we did not report results for these groups. As is typical in IV analyses, our estimates were imprecise and we were unable to rule out the null for any population subgroup except white women. Finally, these results may not generalize to the present day due to changes in the educational system that have occurred since 1982, the last year of college data that matched with our dataset.

This study used a natural experiment and quantile regression to estimate the effect of education on BP and assess for effect heterogeneity. Our findings support the expected inverse relationship between educational attainment and later-life BP, but this was only detected among white women. The relationship is less clear in white men, Black men, and Black women. Future research should establish the magnitude and direction of the relationship in studies powered appropriately to detect these differences, and should include more comprehensive racial and ethnic groups.

Table 2.1 Characteristics of US Census Microsample Participants and Health and Retirement Study participants included in quantile and linear regression analyses of measured blood pressure

Sample characteristic	**US Census (1990) Median (IQR) or %**	**HRS Median (IQR) or %**
N	5,502,951	7,358
Individual-level variables		
Years of schooling	12 (12, 14)	13 (12, 16)
Systolic blood pressure (mmHg)	NA	126 (115, 139)
Diastolic blood pressure (mmHg)	NA	78 (71, 85)
Birth year	1947 (1935, 1955)	1947 (1939, 1954)
Range	1922-1962	1922-1962
Female	51.6	59.8
Race		
Black	9.6	16.0
White	85.9	78.5
Hispanic	3.3	4.0
Other	1.2	1.7
US census region		
Northeast	24.2	21.9
Midwest	29.6	34.0
South	33.9	34.6
West	12.3	9.6
State-level variables for state of birth (Census) or state of residence at approximately age 18 (HRS)		
Percent Black	8.5 (5.1, 13.5)	8.5 (5.3, 13.8)
Percent urban	70.4 (57.3, 81.6)	71.3 (59.0, 81.9)

Sample characteristic	US Census (1990) Median (IQR) or %	HRS Median (IQR) or %
Percent foreign-born	4.0 (1.6, 8.7)	4.6 (1.7, 8.7)
Manufacturing jobs per 1000 population	69.3 (49.6, 90.2)	70.0 (51.1, 89.9)
Manufacturing wages (2017 USD)	38,900 (31,200 , 45,500)	39,200 (32,900 , 45,700)

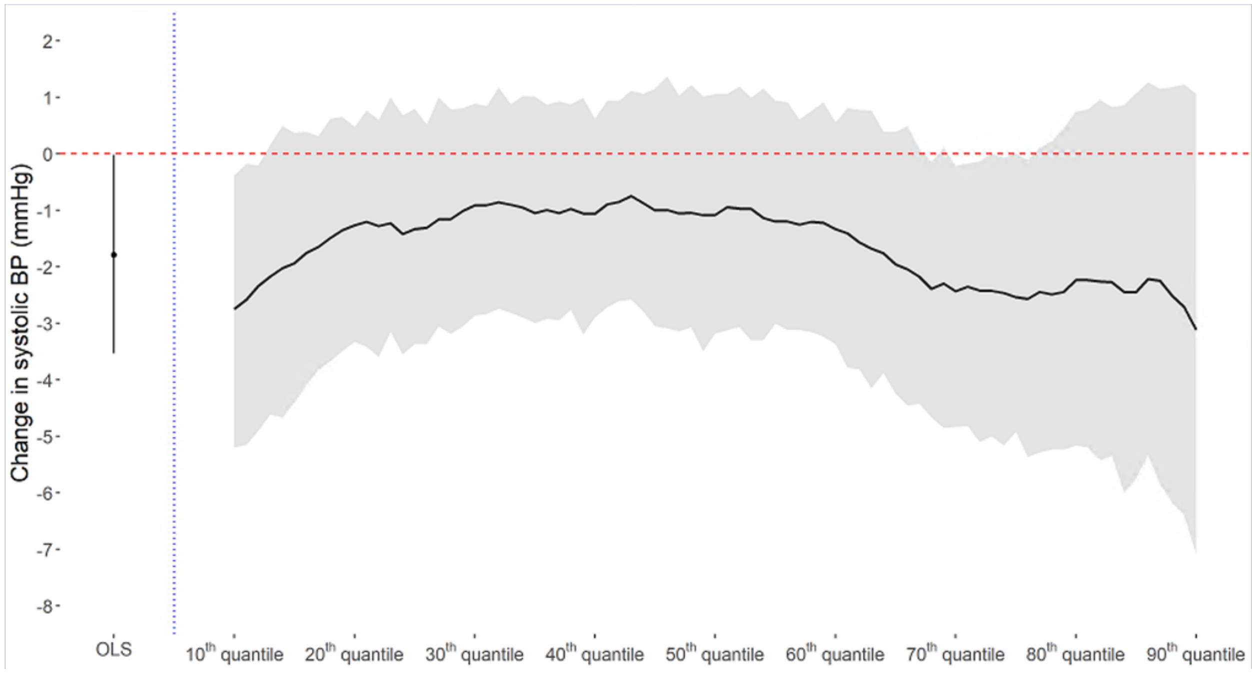

Figure 2.1 OLS and CQR effect estimates for change in systolic BP in the HRS sample. Models used data from HRS (N = 7,358) and were adjusted for birth year, sex, race, census division of birth, year of BP measurement, and state characteristics: percentage Black, urban, and foreign-born residents, manufacturing jobs per capita, and manufacturing wages. OLS = ordinary least squares and CQR = conditional quantile regression.

Appendix

Diastolic BP results

In our overall OLS model for the second stage of the IV (Table 2.4, Figure 2.3), we found no evidence that additional education had an effect on mean diastolic blood pressure (DBP) (b = -0.16 mmHg, 95% CI: -1.21, 0.98). Similarly, there was no evidence of an effect at any quantile in the CQR models (Table 2.4, Figure 2.3); for example, at the 10th quantile, the effect estimate was -0.33 mmHg (95% CI: -2.03, 1.22) and at the 90th quantile, it was -0.19 mmHg (95% CI: -2.35, 1.64).

In our models evaluating effect modification of race and sex (Figure 2.4), results were similar to those for SBP. White women consistently had decreased DBP compared to white men in the OLS model and generally across quantiles of the CQR models. However, in interaction models setting white women as the reference group, confidence intervals for the effect of education on DBP included the null. Black women, Black men, and white men all had increased DBP in OLS models and generally across quantiles of the CQR models where each respective subgroup was set as the reference group. Again, confidence intervals were wide and most included the null. No subgroup other than white women had significantly different effects of education on DBP compared to white men.

Table 2.2 Stage 1 OLS estimates for effect of instruments on additional educational attainment. Effects are measured in additional years of education. Interaction model sets white men as the reference group, so effect sizes presented are those for white men. All models use data from the 1990 US census 5% sample. Overall model includes both instruments (2- and 4-year colleges) and is adjusted for birth year, sex, race, census division of birth, year of BP measurement, and state characteristics: percentage Black, urban, and foreign-born residents, manufacturing jobs per capita, and manufacturing wages. Interaction model includes interaction terms for 2-year colleges x race x sex and 4-year colleges x race x sex. Overall models by race-sex subgroups are the same as the overall model except do not include race and sex as covariates since they are limited to a single level of those variables.

	Number of 2-year colleges per 100,000 18 - 22-year-olds	**Number of 4-year colleges per 100,000 18 - 22-year-olds**	**F-statistic**
Overall model N = 5,502,951	0.37 (0.35, 0.39)	0.48 (0.45, 0.51)	1051
Interaction model with white men as reference group N = 5,502,951	0.18 (0.16, 0.20)	0.47 (0.44, 0.51)	507
Overall model, white men N = 2,308,503	0.52 (0.49, 0.56)	0.43 (0.38, 0.49)	566
Overall model, white women N = 2,415,894	0.37 (0.36, 0.42)	0.50 (0.45, 0.54)	563
Overall model, Black men N = 232,558	0.42 (0.33, 0.51)	0.20 (0.033, 0.38)	45
Overall model, Black women N = 294,881	0.12 (0.052, 0.20)	0.26 (0.12, 0.41)	10

Table 2.3 Stage 2 OLS and CQR estimates for effect of one additional year of schooling on systolic blood pressure. N= 7,358 with 20,860 BP measurements for all models. Models used data from HRS and were adjusted for birth year, sex, race, census division of birth, year of BP measurement, and state characteristics: percentage Black, urban, and foreign-born residents, manufacturing jobs per capita, and manufacturing wages. Interaction terms represent the difference in the effect of education for each respective population subgroup compared to white men. OLS = ordinary least squares and CQR = conditional quantile regression.

	Estimated Effect on Mean (OLS Coefficients)	**Estimated Effect on Quantiles (Conditional Quantile Regression Coefficients)**				
		10th percentile	**30th percentile**	**50th percentile**	**70th percentile**	**90th percentile**
Systolic BP at the mean and specified quantiles (mmHg)	128	106	117	126	136	152
Overall model	-1.79 (-3.54, -0.000077)	-2.74 (-5.20, -0.38)	-0.91 (-2.86, 0.89)	-1.08 (-3.18, 1.06)	-2.43 (-4.83, -0.21)	-3.11 (-7.09, 1.06)
Interaction model						
White men (ref)	0.97 (-2.65, 4.63)	0.89 (-1.45, 3.26)	1.06 (-0.56, 2.86)	1.51 (-0.59, 3.43)	0.23 (-1.80, 2.31)	1.02 (-2.56, 4.60)
Interaction term for white women	-3.65 (-7.57, 0.11)	-3.16 (-4.50, -1.88)	-3.49 (-4.43, -2.49)	-3.51 (-4.63, -2.31)	-4.02 (-5.15, -2.85)	-5.83 (-7.58, -4.17)
Interaction term for Black women	0.45 (-2.93, 3.71)	-0.53 (-3.28, 2.22)	-0.67 (-2.87, 1.74)	-0.15 (-1.77, 1.63)	0.25 (-1.59, 2.21)	-0.82 (-4.13, 2.60)
Interaction term for Black men	0.49 (-2.50, 4.06)	-2.41 (-4.84, -0.31)	-1.10 (-3.08, 0.87)	0.92 (-1.46, 3.14)	1.43 (-1.79, 4.18)	-0.074 (-3.52, 4.35)

Table 2.4 Stage 2 OLS and CQR estimates for effect of one additional year of schooling on diastolic blood pressure. N= 7,358 individuals with 20,860 BP measurements for all models. Models used data from HRS and were adjusted for birth year, sex, race, census division of birth, year of BP measurement, and state characteristics: percentage Black, urban, and foreign-born residents, manufacturing jobs per capita, and manufacturing wages. Interaction terms represent the difference in the effect of education for each respective population subgroup compared to white men. OLS = ordinary least squares and CQR = conditional quantile regression.

	OLS estimate	**CQR estimates**				
		10th percentile	**30th percentile**	**50th percentile**	**70th percentile**	**90th percentile**
Diastolic BP at the mean and specified quantiles (mmHg)	78	65	72	78	84	93
Overall	-0.16 (-1.21, 0.98)	-0.33 (-2.03, 1.22)	-0.17 (-1.22, 1.06)	0.14 (-1.10, 1.23)	0.081 (-1.23, 1.56)	-0.19 (-2.35, 1.64)
Interaction model						
White men (ref)	1.08 (-1.30, 3.50)	0.41 (-1.31, 1.90)	1.06 (-0.099, 2.24)	0.69 (-0.34, 1.83)	0.71 (-0.60, 2.07)	0.47 (-1.29, 2.31)
Interaction term for white women	-1.85 (-4.23, 0.36)	-1.39 (-2.14, -0.63)	-1.59 (-2.28, -0.93)	-1.46 (-2.19, -0.76)	-1.29 (-2.05, -0.50)	-1.73 (-2.86, -0.67)
Interaction term for Black women	-1.05 (-2.91, 1.01)	0.59 (-0.96, 2.46)	-0.24 (-1.29, 0.84)	0.57 (-0.45, 1.62)	0.64 (-0.63, 1.89)	2.00 (-0.30, 3.94)
Interaction term for Black men	-0.93 (-2.85, 1.21)	0.19 (-1.86, 2.54)	-0.0022 (-1.55, 1.41)	-0.28 (-1.85, 1.04)	0.51 (-0.97, 1.78)	1.79 (-1.01, 3.98)

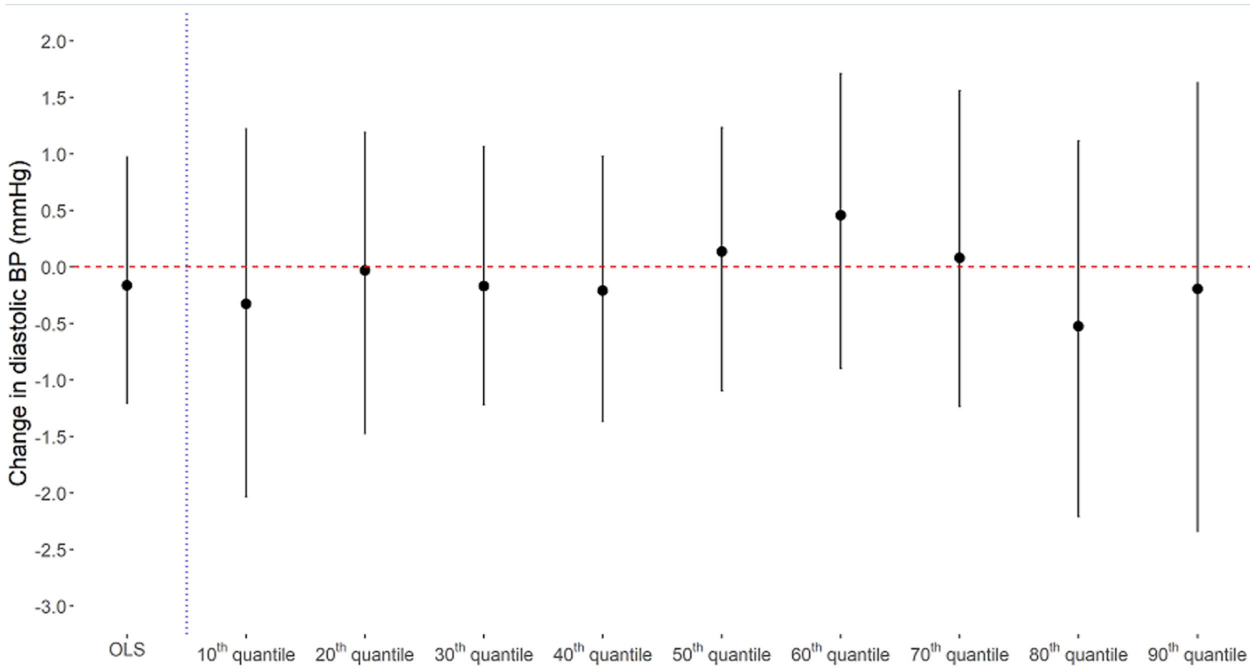

Figure 2.2 OLS and CQR effect estimates for change in diastolic BP. N=7,358 individuals with 20,860 BP measurements. Effect estimates are in units of mmHg. Models adjusted for birth year, sex, race, census division of birth, year of BP measurement, and state characteristics: percentage Black, urban, and foreign-born residents, manufacturing jobs per capita, and manufacturing wages.

Chapter 3: Association of college expansion with later-life blood pressure in the United States

Introduction

Hypertension affects nearly 120 million adults in the US and is an important modifiable risk factor for heart disease, the leading cause of death in 2022.[35,60] While any persistently elevated blood pressure (BP) value >120/80 mmHg is associated with increased risk of heart disease and stroke, the risk increases with increasing BP, and the relationship is nonlinear.[6] This implies that public health interventions aimed at the high-risk tail of the BP distribution may have a larger impact on population health. Population distributions of BP also vary by race and sex, with men having higher average BP than women and Black adults having higher average BP compared to White and Mexican American adults.[61]

Education is a key social determinant of health that has been linked with BP over numerous associational and quasi-experimental studies.[13,27,29,36,37,39,43–46,62,63] Policies to increase educational attainment may play a role in lowering BP at the population level through their effect on education. Existing literature on the impact of education policies on health have focused on the impact of compulsory schooling laws. The weight of evidence from these studies shows that increasing educational attainment through compulsory schooling laws improves later-life health,[11,13,14,20,46,56] although some studies show mixed or no effects.[46]

Less work has evaluated health impacts of post-secondary education. The role of the federal government in postsecondary education policy expanded significantly after

World War II, which expanded the number of 2- and 4-year colleges. The Truman Commission Report called for a national community college system and increases in federal spending on financial aid for college, which manifested in a slew of formal legislation.[64] Between 1949 and 1970, fall enrollment increased from 2,444,900 to 8,004,660 and the number of colleges increased from 1,851 to 2,525.[65] This period of growth peaked in the 1960s, when on average 1 new 2-year college opened every week.[66] Little work has evaluated how this dramatic increase in college geographic accessibility impacted subsequent health outcomes,[47,48,67] or evaluated differential effects by race and gender. Our paper helps to fill this critical gap.

The impact of college expansion on blood pressure disparities is, *a priori*, unclear. On the one hand, resource substitution theory suggests that people with less access to established social hierarchies and power structures may benefit more from additional resources, such as higher educational attainment, compared to those with more social advantages, like white men.[16] Resource multiplication theory suggests the inverse: that the more socially advantaged would benefit more from additional education.[16] The pervasiveness of sexism and structural racism means that policies that increased college enrollment of women and minoritized individuals would not necessarily translate into equal long-term health gains for all gender and racial/ethnic groups. The limited prior work evaluating differential relationships between education and health by sociodemographic group suggests that White and Black women benefit more from each year of education than White men, while findings for Black men are inconsistent.[50,68–71]

In this study, we focus on the potential heterogeneity of the association between education and BP in two ways: firstly, we assess for heterogeneity of the association at different points of the BP distribution. To do this, we use conditional quantile regression to examine the relationship between the number of 2- and 4-year colleges with BP across the distribution of systolic and diastolic BP. Secondly, we conduct interaction analyses by race/ethnicity and gender to assess for heterogeneity by sociodemographic group.

Methods

Source population and analytical sample: The HRS is a nationally representative population-based cohort study of US adults aged 50 and over and their spouses of any age.[18] Detailed demographic and health information has been collected every 2 years since 1992. Beginning in 2006, a random subsample of HRS respondents was selected for an enhanced in-person interview which included physical measurements. In the next wave, the remaining respondents were selected for the enhanced interview. Physical measures, including BP, were taken every 4 years for each eligible respondent. HRS respondents were not able to complete the face-to-face interview if they were living in a nursing home, had a proxy respondent, or had to conduct their interview by telephone.

We combined the core demographic HRS data collected by the University of Michigan with longitudinal BP data measured by HRS and processed by the RAND corporation spanning the 2006-2018 waves of data collection.[53] HRS does not directly collect county-level geographic location data for residents' childhood years, so to match

HRS respondents with the county-level college availability data, we used the education-focused Life History Mail Survey of HRS. This survey collected more detailed K-12 educational history including city and state of the respondents' schools. We used the last school the respondent attended and restricted to those who were between 16 and 19 years of age; the majority of these respondents were 17 or 18 years old and listed their final grade completed as grade 12. We used the city and state location data to match the respondents to their probable county of residence in high school. Because we were working backwards from high school data, we assume that the respondent's location at the time preceded their decision to attend (and potentially move to) college.

A dataset with the number of colleges and universities open by county and year was previously compiled by Currie and Moretti.[48] The authors included both geographic (US county where school was located) and temporal (years the school was open) information to construct a dataset with 2- and 4-year undergraduate-level schools open between 1940-1996. They also used US census data to estimate the number of 18 to 22-year-olds in each county. This dataset was matched with HRS respondents by year and US county, so that each HRS respondent was associated with the number of 2- and 4-year colleges open in the county of their high school at approximately age 18.

Our analytical sample consisted of HRS respondents who met our inclusion criteria of 1) being born in the US, 2) providing a valid US city and state combination (i.e., one that could be matched unambiguously to a county) for the last school the respondent attended in the Life History Mail Survey in 2015 or 2017, 3) being between 16 and 19 years of age when last attending school, and 4) having at least one instance of valid systolic BP (SBP) and diastolic BP (DBP) measurement between 2006-2018.

Respondents were followed until death, dropout, or the 2018 study wave. Of the respondents who matched our inclusion criteria, we dropped 52 due to missing data for state-level covariates (0.8% of eligible sample). Our final analytical sample size was 6,145 unique respondents contributing 17,497 blood pressure measurements.

Exposure: Our exposures of interest were the number of 2- and 4-year colleges open in each year between 1940-1982 in each US county and the District of Columbia, divided by the estimated number of 18 - 22-year-olds in each county each year in order to standardize the measurement (and then multiplied by 100,000, to give a measure of number of colleges per 100,000 18 - 22-year-olds). We estimated the association between the number of 2- and 4-year colleges and BP separately such that each analytic model had two independent variables, one for the number of 2- and the number of 4- year colleges.

Outcome: Our primary outcomes were repeated measures of SBP and DBP (measured in mmHg) in the RAND longitudinal data. BP was measured three times at each face-to-face interview; the RAND measurements are the average of the second and third BP measurements if all three were valid. If there were two valid measurements, the mean of those two measurements was reported. If there was one valid measurement, that number was reported. We used all BP measurements in the RAND data for BP outcomes between 2006-2018.

Effect Modifiers: In addition to overall models, we also evaluated differential relationships in subgroups defined by race and sex.

Covariates: We included state- and individual-level covariates to control for confounding of the exposure-outcomes relationship. State-level covariates included percentage of the population identifying as Black, percentage of the population living in an urban area, and percentage of the population that was foreign-born for each state when the respondents were 18 years of age. State-level characteristics were compiled using Statistical Abstracts of the United States and other US Census Bureau reports spanning 1899-1980,[55] and were linearly interpolated for years between reports. These statistics have been used in prior studies evaluating state compulsory schooling laws and health outcomes.[10,11,13]

Individual-level covariates included race and ethnicity (Black, White, Hispanic, other), sex (female, male), census division at birth (New England, Middle Atlantic, South Atlantic, East North Central, East South Central, West North Central, West South Central, Mountain, and Pacific), mother's educational attainment (in years), father's educational attainment (in years), and missing indicators for mother's and father's educational attainment. There were 529 respondents (8.6%) with missing information for mother's education, 867 respondents (14.1%) with missing information for father's education, and 403 respondents (6.6%) with missing information for both mother's and father's education. We assigned the mean value of mother's or father's education to those with missing values, and used both the value of mother's and father's education as well as the missing indicators as control variables in the analysis.

Analysis: We first described sample characteristics overall and by race and sex. We then estimated the association of the number of 2- and 4-year colleges per 100,000 18 - 22-year-olds with quantiles of SBP and DBP using conditional quantile regression. We estimated this association at deciles of SBP and DBP from the 10th to the 90th. Next, we estimated the association between the number of colleges and mean SBP and

DBP using linear regression. For both linear and conditional quantile regression, we clustered standard errors at the person level to account for respondents with multiple BP measurements. We produced estimates and 95% confidence intervals for all models by bootstrapping. We resampled with replacement and estimated beta coefficients 500 times; the estimates presented here are the mean of those 500 coefficients and the 95% confidence intervals were constructed by taking the values at the 2.5th percentile and 97.5th percentile.

We also conducted analyses to evaluate whether the association between the number of 2- and 4-year colleges per 100,000 18 - 22-year-olds varied by race and sex subgroups. We created a 3-way interaction between the 2-year college variable, race, and sex; and between the 4-year college variable, race, and sex. We used white men as the reference group to estimate the differences in the association of 2- and 4-year colleges with SBP and DBP for white women, Black women, and Black men. White men were set as the reference group as they are the most structurally privileged members of society.

In addition, we rotated the reference group among the other sociodemographic groups to obtain point estimates for the association between the number of 2- and 4-year colleges and BP for white women, Black women, and Black men. We used this approach to retain sample members and produce more precise estimates rather than stratifying by sociodemographic subgroups due to relatively small sample sizes, particularly for Black men. All effect modification analyses were conducted using both linear and conditional quantile regression. As described for the overall analysis, we used bootstrapping to obtain point estimates and 95% confidence intervals for the associations of the number of 2- and 4-year colleges with SBP and DBP.

Analyses were conducted using Stata version 17.0. Ethics approval was provided by UC San Francisco (#13-11886). All HRS respondents gave verbal informed consent for their participation in the study, and data collection was approved by the Health Sciences and Behavioral Sciences Institutional Review Board at the University of Michigan.

Sensitivity analysis: We conducted a sensitivity analysis in which we limited the sample to respondents with birth years spanning from 1932-1952. These respondents would be 18 years old in the years 1950-1970, which is when the greatest changes in the number of 2-year colleges occurred. We also estimated the association between 2- and 4-year colleges with likelihood of attending college in our sample, and the association between at least some college and BP to better understand what effect size might be reasonable or expected in our study sample.

Results

Respondent characteristics are shown in Table 3.1. Black men and women had a higher median population-adjusted number of 2-year colleges in their county of residence at approximately age 18 compared to white men and women, although the interquartile ranges were similar for all 4 groups. Black men had the highest median population-adjusted number of 4-years colleges in their county of residence at approximately age 18 compared to all other groups. Black men also had the highest BP values of any group. Black respondents tended to be younger, have lower parental educational attainment, were more likely to be born in the South, and less likely to live in urban settings at about age 18 compared to white respondents.

Overall results: In linear and conditional quantile regression models (Table 3.2), we found that each additional 2-year college per 100,000 18 - 22-year-olds per county was not associated with systolic blood pressure. Coefficients for the difference in SBP were very small and 95% confidence intervals included the null. For example, linear models showed that each additional 2-year college was associated with a decrease in SBP of 0.044 mmHg (95% CI: -0.37, 0.27). This association was consistent over the distribution of SBP in CQR models. For example, at the 10th quantile, each additional 2-year college was associated with a decrease in SBP of 0.033 (95% CI: -0.41, 0.38) while at the 90th quantile there was a decrease of 0.070 mmHg (95% CI: -0.70, 0.65).

We found that each additional 4-year college in the county predicted slightly lower blood pressure at the mean (b = -0.10 mmHg, 95% CI: -0.19, -0.015); this relationship was consistent over the SBP distribution, although estimates were less precise: at the 10th quantile, each additional 4-year college was associated with a decrease in SBP of 0.052 (95% CI: -0.18, 0.061) while at the 90th quantile there was a decrease of 0.14 mmHg (95% CI: -0.34, 0.048).

Interaction results: Interaction models assessing heterogeneity by sex suggested differential impacts at higher quantiles such that each additional 2-year college in the county predicted higher SBP for men at the 90th quantile (b = 0.75 mmHg, 95% CI: -0.55, 1.88), but lower SPB for women at the 90th quantile (b = -0.61 mmHg, 95% CI: -1.32, 0.076; interaction b = -1.36 mmHg, 95% CI: -2.79, 0.19), although estimates were somewhat imprecise.

Interaction models assessing heterogeneity by race and sex suggested that each additional college (2- or 4-year) in the county predicted lower SBP for Black women across the distribution, although estimates were imprecise. Results for other subgroups were mixed: for white women, each additional 2-year college was associated with no change in SBP except at the 90th quantile, where there was a slight decrease in SBP. Each additional 4-year college, however, was associated with decreased SBP at lower quantiles and no change in SBP at upper quantiles.

For Black men, each additional 2-year college predicted decreased SBP at lower quantiles and no change at upper quantiles, except at the 90th where the point estimate suggested an increased SBP, but was very imprecise. Each additional 4-year college predicted no change in SBP at lower quantiles and decreased SBP at upper quantiles, although again estimates were imprecise. For white men, each additional 2-year college predicted no change in SBP at all quantiles except the 90th, where similar to Black men, the point estimate suggested an increase in SBP but was imprecise. Each additional 4-year college predicted no change in SBP across all quantiles.

Results for all models (overall and interaction) were qualitatively similar with DBP as the outcome. Results from the sensitivity analysis limiting birth years to 1932-1952 were qualitatively similar to those above. Coefficients tended to be slightly larger, but most confidence intervals included the null of 0. We also found that additional 2- or 4-year colleges were not associated with an increased odds of having attended college in our sample.

Discussion

In pooled analyses, we found no evidence of an association between the number of 2-year colleges open per 100,000 18 - 22-year-olds in the respondent's county and the respondent's SBP, and a statistically significant but very small decrease in mean SBP for 4-year colleges. Findings were similar across all quantiles in the conditional quantile regression, and were similar in magnitude to results for linear regression. Interaction analyses suggest that increases in 2-year colleges may have been more beneficial for women than for men at the high-risk right tail of the blood pressure distribution. Increases in 4-year colleges were generally beneficial, although the magnitude of the association was small, with little subgroup variation. Black women were most consistent in having a decrease in SBP for both 2- and 4-year colleges across the distribution of SBP, although estimates were imprecise.

These results are largely consistent with sensitivity analyses showing null results for the association between 2- and 4-year colleges and at least some college attainment in the HRS sample. This is in contrast with prior studies that show an effect of college openings on college attendance.[47,48,72] The association between education and SBP in the HRS sample is in the expected direction and is consistent with findings from previous literature.[43–45] This means that any associations we found between the number of colleges in a county and SBP are likely due to other factors that are common causes of colleges and BP (e.g., confounders that we were unable to control for), chance, or a different mechanism of action than increasing an individual's own education (e.g., perhaps the causal sequence is number of colleges - county median income - BP, rather than number of colleges - education - BP).

Possible reasons for our null findings can be divided into 2 broad categories: that there truly is no association between the number of colleges and SBP, and that there is an association that we failed to detect in our sample. This could be due to residual confounding, measurement bias, or selection bias. Measurement bias seems unlikely since our outcome was measured and the exposure and outcome are from different data sources. Selection bias is possible since BP measures began in 2006 and 2008, while HRS itself began in 1992: respondents needed to survive to at least 2006 or 2008 to be included in this study. In addition, respondents who require proxies to participate in HRS did not have their BP measured. Because those requiring proxies are more likely to be ill, they may have a different relationship between the exposure and outcome. Finally, because our study is an intention-to-treat analysis, the effect of number of colleges on BP could be obscured by those who did not attend college. This seems less likely since we did not find an association between the number of colleges and odds of college attendance.

Our study has several strengths: we use an objectively measured outcome, which is less likely to be subject to measurement bias than self-reported outcomes. We use quantile regression, an under-utilized method in the epidemiologic literature that allows us to assess whether there is heterogeneity of the exposure-outcome relationship along the distribution of the outcome. We also were able to match HRS participants with the number of colleges at the county level. This relatively fine geographic scale lends credibility to our approach because it is likely that high school students are factoring in geographic proximity to a college when making their decisions about whether and where to attend college (especially 2-year colleges, which are primarily responsible for the increase in colleges between 1950-1970).

Our study also has several limitations: this analysis is not causal, so associations should not be interpreted as causal effects. There is likely to be residual confounding given the complex socio-demographic variables that may influence both where governments or private entities decide to open new colleges and health, including BP.[48] We were unable to assess whether there were heterogenous associations of the number of colleges and BP for US-born Hispanic and “other” racial and ethnic groups because these groups were too small to draw meaningful conclusions. Our findings also may be specific to the time period in which HRS participants were deciding whether to attend college and may not be applicable to the present day. As discussed above, selection bias may also have influenced our results.

Few other studies assess the association of college education on BP specifically, or even the association of college education on health, so our study adds to this nascent literature despite our null findings. Future work should establish the optimal functional form of the exposure, and assess whether these results are replicable in another dataset.

Table 3.1 HRS study participant characteristics for measured BP analyses, overall and by race and sex subgroups reported in interaction analyses.

Sample characteristic	**Total sample**	**White men**	**White women**	**Black men**	**Black women**
N	6,145	1,967	2,797	349	638
Individual-level variables					
Number of 2-year colleges per county per 100,000 18 - 22-year-olds when respondent was approx. age 18, median (IQR)	0.18 (0, 0.51)	0.17 (0, 0.53)	0.08 (0, 0.45)	0.21 (0, 0.52)	0.21 (0, 0.52)
Number of 4-year colleges per county per 100,000 18 - 22-year-olds when respondent was approx. age 18, median (IQR)	0.53 (0, 1.22)	0.54 (0, 1.25)	0.53 (0, 1.25)	0.52 (0, 1.12)	0.61 (0, 1.38)
Systolic blood pressure in mmHg, median (IQR)	127 (116, 139)	129 (119, 141)	124 (112, 137)	133 (121, 146)	128 (116, 142)
Diastolic blood pressure in mmHg, median (IQR)	78 (71, 85)	78 (71, 85)	77 (70, 84)	81 (74, 89)	80 (73, 88)
Birth year, median (IQR)	1946 (1938, 1953)	1943 (1936, 1952)	1944 (1937, 1953)	1952 (1944, 1956)	1950 (1942, 1955)
Years of mother's schooling, median (IQR)	12 (8, 12)	12 (10, 12)	12 (9, 12)	10 (8, 12)	10 (8, 12)
Years of father's schooling, median (IQR)	10 (8, 12)	12 (8, 12)	11 (8, 12)	10 (8, 12)	10 (8, 11)
US census region at approx. age 18 (%)					
Northeast	22.3	25.4	24.3	17.3	13.4
Midwest	35.0	41.8	41.8	14.2	13.7
South	33.5	23.9	25.0	67.3	70.9

Sample characteristic	Total sample	White men	White women	Black men	Black women
West	9.3	8.8	8.9	1.1	2.0
State-level covariates at approx. age 18					
Percent of population identifying as Black, median (IQR)	8.5 (5.3, 13.0)	7.3 (3.8, 11.5)	7.8 (4.6, 12.1)	18.7 (11.7, 27.5)	19.8 (12.1, 28.9)
Percent of population living in urban setting, median (IQR)	70.9 (57.3, 81.2)	70.9 (59.3, 82.1)	71.2 (58.8, 81.4)	60.9 (46.2, 79.5)	59.7 (46.3, 77.0)
Percent foreign-born population, median (IQR)	4.6 (1.7, 8.7)	4.9 (2.2, 8.8)	4.9 (2.2, 8.7)	1.3 (0.6, 5.7)	1.2 (0.5, 4.8)
Manufacturing jobs per 1000 population, median (IQR)	71.7 (52.2, 91.8)	75.9 (52.7, 93.0)	76.1 (53.6, 92.2)	71.5 (56.3, 89.5)	69.8 (55.1, 86.4)
Manufacturing wages in 2017 USD, median (IQR)	38,300 (32,000 , 45,000)	38,700 (31,700 , 45,300)	38,800 (32,100 , 45,100)	35,600 (31,500 , 43,100)	35,100 (30,800 , 42,700)

Table 3.2 Linear and conditional quantile regression estimates for association of one additional 2- or 4-year college per 100,000 18 - 22-year-olds in the respondent's county with measured systolic blood pressure. Models used data from HRS (N = 6,145) and were adjusted for birth year, sex, race, mother's education, father's education, missing indicators for mother's and father's education, census division of birth, year of BP measurement, and state characteristics: percentage Black, urban, and foreign-born residents, manufacturing jobs per capita, and manufacturing wages. OLS = ordinary least squares.

Variable	Estimated Association at Mean (OLS Coefficients)	Estimated Association at Quantiles (Conditional Quantile Regression Coefficients)				
		10th percentile	30th percentile	50th percentile	70th percentile	90th percentile
Number of 2-year colleges	-0.044 (-0.37, 0.27)	-0.033 (-0.41, 0.38)	-0.020 (-0.37, 0.25)	0.0027 (-0.40, 0.45)	0.060 (-0.35, 0.51)	-0.070 (-0.70, 0.65)
Number of 4-year colleges	-0.10 (-0.19, -0.015)	-0.053 (-0.18, 0.061)	-0.073 (-0.16, 0.021)	-0.090 (-0.21, -0.0076)	-0.061 (-0.23, 0.10)	-0.14 (-0.34, 0.048)

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