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# Social inequalities, debt, and health in the United States

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ABSTRACT

*Background:* Household financial debt has more than tripled since the 1980s in the United States. The experience of indebtedness is socially structured and there is mounting evidence that debt is linked to decrements in health. However, it is unclear whether debt contributes to social disparities in health.
*Objective:* We examined whether household debt, measured by debt in excess of income and wealth, mediated education-based social inequalities in health, including cardiovascular risk factors (hypertension) and chronic conditions (diabetes, coronary heart disease, and psychiatric problems).
*Method:* We used longitudinal data from a sample of over 10,500 adults aged 18 years and older surveyed biennially between 1999 and 2015 as part of the Panel Study of Income Dynamics (PSID). We estimated the total effect of education on our health outcomes. To assess mediation by levels of household debt, we then estimated the controlled direct effect of education through pathways not mediated by levels of household debt, after accounting for lagged time-varying confounders and loss to follow-up using marginal structural models.
*Results:* Compared to respondents with at least a high school education, respondents with less than a high school education reported higher household debts in excess of income and wealth; they also reported a higher incidence of hypertension [risk ratio (RR) = 1.25, 95%CI = 1.13, 1.39), coronary heart disease (RR = 1.42, 95%CI: 1.25, 1.62), diabetes (RR = 1.50, 95%CI: 1.34, 1.68), and psychiatric problems (RR = 1.39, 95%CI: 1.24, 1.56). Compared to the total effects, the controlled direct effects of education on health were attenuated, particularly for death or first onset of hypertension and coronary heart disease, after fixing levels of household debt-to-income and debt-to-wealth.
*Conclusion:* Our results provide early evidence that household debt in excess of wealth partly mediates education-based inequalities in hypertension and coronary heart disease in the United States, with less consistent evidence for other chronic conditions.

## 1. Introduction

Access to diverse forms of credit, including mortgages, credit cards, and student loans, is viewed as essential to social mobility in the United States, and has greatly expanded in recent decades, including to previously underserved groups (Dwyer, 2018; Krippner, 2017). This "democratization" of credit has been accelerated by the deregulation of the consumer credit system, the entry of private market institutions, and the spread of predatory lending to disadvantaged populations (Fourcade & Healy, 2013; Immergluck, 2011). As a result, the United States has seen a dramatic increase in overall consumer financial debt, with total debt reaching an all-time high of $14.15 trillion in 2019 and over 80% of households reporting indebtedness (Federal Reserve Bank of New York, 2020). Additionally, as an unwanted concomitant of credit access, there has been a widening of debt disparities across socioeconomic groups, including higher rates of high-interest payday and short-term borrowing and increased unsecured debt burdens (relative to income and wealth) among socially disadvantaged populations (Bourke et al., 2012; Houle, 2014; Williams, 2004). For many households, credit and debt are important tools for easing the strain of growing economic inequality; the wage gap between those with and without a college degree, for instance, has widened in recent decades, making credit an essential resource for lower-educated Americans facing declining incomes (Rugaber, 2017). Indeed, a recent Pew Research Report found that while college educated

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young households (under 40) have higher debt to income ratios (as well as higher median wealth and income), this is due largely to mortgage debt, while non-college-educated households have higher burdens of unsecured debts (Fry, 2014).

These growing unsecured debt burdens, especially among disadvantaged groups, have implications for health and health disparities. A growing body of literature documents that household debt is a predictor of poorer health across a range of mental and physical health outcomes, from depression and psychological distress to hypertension and inflammation (Berger et al., 2016; Hojman et al., 2016; Sweet et al., 2013). Unsecured debt in particular, such as from credit cards, student loans and payday loans, appears to pose a unique risk to health, compared to secured mortgage debt, possibly because the higher interest rates and aggressive repayment structures are more stressful (Hojman et al., 2016; Sweet et al., 2018; Zurlo et al., 2014). Because the relative burden of these unsecured debts is rising among socioeconomically disadvantaged groups, there is a question of how they might contribute to other socioeconomic disparities in health.

Considering that several health inequalities have widened in recent years, including those by educational attainment (Zimmerman & Anderson, 2019), and that better understanding the mechanisms underlying these trends is a key priority in contemporary social epidemiology (Kawachi & Subramanian, 2018), it is critically important to examine how factors like debt burden might mediate disparities in health. In this study we used longitudinal data from the Panel Study of Income Dynamics (PSID) to: (1) measure education-based health disparities, including cardiovascular risk factors (hypertension) and chronic conditions (coronary heart disease, diabetes, and psychiatric problems); (2) assess the patterning of financial debt (based on unsecured debt-to-wealth and unsecured debt-to-income measures) by education and the relation between unsecured debt and health; and (3) examine whether unsecured debt mediates education-based inequalities in health.

## 2. Methods

### 2.1. Data and sample

The PSID survey began in 1968 with a sample of 4,802 households, including an oversampling of 1,872 low-income households to facilitate research on social inequalities (Institute for Social Research et al., 2019). Families were interviewed annually until 1997 and biennially thereafter (Insolera & Freedman, 2017). Any individual born to or adopted by a sample member enters the sample and is subsequently followed. Additionally, as adult members leave their households (for example, as children transition to adulthood and establish their own households) they become their own "family units" and are surveyed at each wave. As such, the PSID effectively augments its sample with new birth cohorts. Since 1969, the PSID has maintained wave-to-wave response rates over 96% (Rank & Hirschl, 2009). Weights to address unequal probabilities of selection and attrition are provided by the PSID and the weighted sample is representative of the US population over time (Gouskova et al., 2008). Information collected cover employment, income, wealth, expenditures, health, marriage, education, and numerous other topics (Institute for Social Research & University of Michigan, 2017a; McGonagle et al., 2012).

For the purposes of these analyses, only the head and his/her partner of a household surveyed biennially between 1999 and 2015 were included, since the health module was added to the PSID in 1999 (McGonagle & Schoeni, 2006). Additionally, families were excluded if the head was below 25 years of age or in an institution at the time of the interview (around 10% of households at each wave) to give time for respondents to complete education (i.e., college), which is often the case by age 25 (Montez et al., 2012; Teachman, 1987). A dataset was constructed for each outcome, based on a sample of respondents who were free of the condition at baseline (e.g., diabetes free), and therefore at risk for first onset of the event. Respondents were followed until they experienced the health condition, died, or were lost to follow up, whichever came first.

### 2.2. Exposure

Educational attainment marks social status at the beginning of adulthood and often precedes and influences other markers of social status, including occupational status, earnings, personal and household income, and wealth (Matthews & Gallo, 2010; Mirowsky, 2017). Educational attainment, defined as the number of completed years of education, was dichotomized at 12 years (less than high school versus high school or greater education).

### 2.3. Mediators

We measured debt as the levels of household unsecured debt over wealth and household unsecured debt over income. The construction of these measures is described in the PSID documentation (Institute for Social Research & University of Michigan, 2017b). Briefly, unsecured debt is constructed as the sum of family credit card debt, student loan debt, medical debt, legal debt and loan debt. Wealth is constructed as a sum of values of several asset types (farm or business, savings, real estate, stocks, vehicles, annuity and other assets), net of debt value plus value of home equity. Income represents the total family income and can contain negative values due to a net loss. Values were adjusted for inflation using data from the US Bureau of Labor Statistics (Bureau of Labor Statistics, 2018). Estimates of total net worth are similar between PSID and the Survey of Consumer Finances, with the largest differences between the two surveys concentrated among the wealthiest 1–2 percent of households (Pfeffer et al., 2016).

### 2.4. Outcomes

Four outcomes were investigated including: (1) death or first onset of hypertension; (2) death or first onset of coronary heart disease; (3) death or first onset of diabetes, and (4) death or the first onset of any emotional, nervous, or psychiatric problems. While the first onset of outcomes was based on self-reported doctor's diagnosis, the year of death was obtained from the PSID restricted mortality data. Self-reported information on other acute health outcomes such as stroke and myocardial infarction was available. However, since we lacked data on cause-specific mortality, these events were treated as potential confounders.

### 2.5. Covariates

Time-fixed confounders [C], measured the first time the participant entered the sample included sex, year of birth, self-rated childhood health before 17 years of age, childhood poverty and race. Time-varying covariates [R] included region of residence, marital status, employment status, health insurance coverage (yes/no), number of household members under 18 year of age, self-reported height, weight and health behaviors (smoking, drinking and physical activity), and history of stroke, and heart attack. Body Mass Index (BMI) was computed after applying a correction to self-reported height and weight (Keith et al., 2012; Stommel & Schoenborn, 2009).

### 2.6. Statistical analysis

Analyses were conducted separately for each outcome specific dataset. Multiple imputation was used to impute missing covariate data (Berglund & Heeringa, 2014; Van Buuren, 2007). Childhood poverty (7.5%), self-reported physical activity (5.7%), prior health (5.4%), weight (2%), drinking behaviors (1.7%) and height (1%) were the covariates with the highest proportions of missing data.

We estimated total and controlled direct effects (CDE) using inverse probability weighted marginal structural models (MSMs) (Nandi et al., 2012). To estimate direct effects, MSMs were preferred over standard regression adjustment methods to mediation (regressing the outcome on education, the mediator, and potential confounders and chronic disease risk factors), because the latter is likely biased when there is time-varying confounding (Baron & Kenny, 1986; Kaufman et al., 2004; Nandi et al., 2012). In this case, characteristics such as employment status, health behaviors, and cardiovascular events can affect and be affected by levels of debt (i.e., to act as mediators and confounders), motivating the MSM approach.

Fig. 1 describes the estimation of inverse probability weights. We first computed inverse probability weights for censoring to account for loss to follow-up, $w_{it}^{L}$ (Miguel A Hernán et al., 2006; Robins et al., 2000). Second, we estimated inverse probability of treatment weights for the relation between education and our health outcomes, $w_{i}^{A}$. Third, weights for the total effect were obtained by multiplying censoring and inverse probability of treatment weights, $w_{it}^{TE} = w_{i}^{L} * w_{i}^{A}$. Fourth, we computed a weight, $w_{it}^{M}$, to account for fixed and time-varying confounders of the relation between our mediator, either unsecured debt over wealth or income measured at the prior survey wave $t-1$, $M_{t-1}$, and outcomes $Y_{it}$ measured at wave t. Since our mediators were continuous measures, we used a quantile binning approach to construct the $w_{it}^{M}$ weights (Naimi et al., 2014). Finally, CDE weights were computed using the product of the three weights, $w_{it}^{CDE} = w_{i}^{L} * w_{i}^{A} * w_{it}^{M}$.

The weights were stabilized, truncated (1st and 99th percentiles), and computed using pooled logistic and generalized logit regressions. We assessed the balanced of covariates using standardized mean differences and absolute weighted correlations (Austin, 2019).

After computing weights, the total effects of education on our outcomes were estimated using a stabilized inverse probability weighted marginal structural model of the form (1) $E[Y] = \beta_0 + \beta_{Education}$. CDEs were estimated assuming homogeneity in the relation between education and health across levels of household unsecured debt over wealth/income by fitting equation (2): $E[Y_{AM}] = \beta_0 + \beta_{Education} + \beta_{Mediator,}$ with the CDE $= \beta_{Education}$. We also allowed for an interaction between education and household debt (3), $E[Y_{AM}] = \beta_0 + \beta_{Education} + \beta_{Mediator} + \beta_{Education*Mediator,}$ with the CDE $= (\beta_{Education} + \beta_{Education*Mediator})$ (VanderWeele, 2009b).

As a sensitivity analyses, we repeated the analysis with PSID sampling weights, and considering death as artificial censoring (eFig. 1) (Miguel A. Hernán et al., 2006). We reported results on the relative risk and risk difference scales and computed robust variances to obtain 95% confidence intervals. All analyses were performed in SAS software, version 9.4 (SAS Institute, Inc., Cary, North Carolina) and the code is available in the **Supplement**.

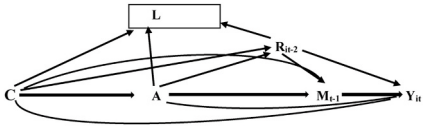


**Fig. 1.** Direct acyclic graphs and weights formulas.
Censoring due to loss to follow-up $w_{it}^{L} = \prod_{t-2}^{t-1} \frac{\Pr[L_{it+1}=0 \mid L_{it}=0, A=a_i]}{\Pr[L_{it+1}=0 \mid L_{it}=0, A=a_i, C=ci, R=r_{it-2}]}$
Inverse probability of treatment weights $w_{i}^{A} = \frac{P(A=a_i)}{P(A=a_i \mid C=c_i)}$
Mediator-outcomes weights $w_{it}^{M} = \prod_{t-2}^{t} \frac{\Pr(M=m_{it-1} \mid A=a_i]}{\Pr(M=m_{it-1} \mid A=a_i, C=ci, R=r_{it-2}, M=m_{it-2}]}$
Total effect weights: $w_{it}^{TE} = w_{it}^{L} * w_{i}^{A}$
Controlled direct effect weights: $w_{it}^{CDE} = w_{it}^{TE} * w_{it}^{M}$
L(t) an indicator of censoring by loss to follow-up at the survey wave t (1: censored, 0: uncensored). A represents education, C time fixed confounders, R time varying confounders and M is the mediator.

## 3. Results

### 3.1. Study population

A total of 9,244; 10,831; 10,677, and 10,553 individuals were included in our analyses for hypertension, coronary heart disease, diabetes, and psychiatric problems, respectively. The incidence of outcomes ranged from 9.7% in 2001 to 6% in 2015 for death or first onset of hypertension, 4.8%–2.3% for death or first onset of coronary heart disease, 4.3%–3.3% for death or first onset of diabetes, and 4.9%–4.1% for death or first onset of psychiatric problems. The average follow-up for each respondent was 5 waves (approximately 10 years). The distribution of socio-economic characteristics, health behaviors, and health insurance coverage was similar across the four samples, as was the proportion of respondents lost to follow-up (Table 1). Around 20% of families had an unsecured debt higher than their wealth; for roughly one-half of these families, this was a consequence of having a negative value for household wealth. Only 5% of families reported an unsecured debt in excess of their total income level (Fig. 2). The proportion of households with unsecured debt surpassing wealth or income decreased with age but increased over the study period (eFig. 2).

### 3.2. Total effects

Balance of time-fixed baseline confounders (year of birth, sex, childhood poverty and race) between those with less than versus at least a high school education was achieved after applying the total effects weights $w_{it}^{TE}$, as seen by comparing the standardized mean differences before and after weighting (Fig. 3a). Additionally, we did not find evidence of misspecification of our propensity score models or non-positivity, as indicated by stabilized weights with means approximately equal to one and a lack of extreme values (eTable 1) (Austin & Stuart, 2015; Cole & Hernán, 2008).

Participants with less than a high school education had a higher risk ratio (RR) of death or first onset of hypertension (RR = 1.25, 95%CI = 1.13, 1.39), death or first onset of coronary heart disease (RR = 1.42, 95%CI = 1.25, 1.62), death or first onset of diabetes (RR = 1.50, 95%CI = 1.34, 1.68), and death or first onset of psychiatric problems (RR = 1.39, 95%CI = 1.24, 1.56) (Tables 2–5). Results were similar when only considering first onset of health outcomes with deaths artificially censored and were less precise when adding PSID sampling weights (eTable 2 and eTable 3).

### 3.3. Education and households unsecured debts

Across the four samples, participants with less than a high school versus more than a high school education reported higher levels of unsecured debt over wealth and unsecured debt over income, in the range of $260,000 and $40,000, respectively.

### 3.4. Households unsecured debts and health outcomes

Participants in the highest quintile of unsecured debt to wealth had 54% (95%CI = 0.96, 2.48) and 69% (95%CI = 1.16, 2.49) increased risks of death or incident hypertension and death or coronary heart disease, respectively, compared to the lowest quintile. However, we did not observe consistent evidence that unsecured debt to wealth increased risks of diabetes or psychiatric problems. Additionally, unsecured debt over income was not consistently associated with the risk of investigated health outcomes (Tables 2–5).

### 3.5. Controlled direct effects

Balance in the distributions of fixed and time-varying characteristics, measured by the absolute weighted correlations with levels of debt-to-

**Table 1**
Population characteristics.

| Variables[a] | Hypertension dataset (N = 9,244) | Coronary heart disease dataset (N = 10,831) | Diabetes dataset (N = 10,677) | Psychiatric problems dataset (N = 10,533) |
|---|---|---|---|---|
| Number of households | 8405 | 8928 | 8885 | 8770 |
| Lost to follow-up | 1263 (13.7) | 1652 (15.3) | 1629 (15.3) | 1565 (14.9) |
| Death | 478 (5.2) | 943 (8.7) | 993 (9.3) | 1146 (10.9) |
| First onset of the health outcome[b] | 2795 (30.2) | 981 (9.1) | 1191 (11.1) | 1336 (12.7) |
| **Education** | | | | |
| Less than high school | 1846 (20.0) | 2312 (21.4) | 2231 (20.9) | 2205 (20.9) |
| **Age at entry (SD)** | 37.2 (14.3) | 38.9 (15.4) | 39.1 (15.6) | 40.0 (16.1) |
| **Sex** | | | | |
| Male | 4198 (45.4) | 4864 (44.9) | 4826 (45.2) | 4838 (45.9) |
| **Race** | | | | |
| White | 5367 (58.1) | 6126 (56.6) | 6177 (57.9) | 5942 (56.4) |
| Black | 2928 (31.7) | 3644 (33.6) | 3490 (32.7) | 3566 (33.9.5) |
| Others | 949 (10.2) | 1061 (9.8) | 1010 (9.4) | 1025 (9.7) |
| **Childhood poverty** | | | | |
| Poor | 2421 (26.2) | 3040 (28.9) | 2988 (28.0) | 3103 (28.7) |
| Average | 4058 (43.9) | 4513 (42.8) | 4613 (43.2) | 4626 (42.7) |
| Pretty well | 2765 (29.1) | 2980 (28.3) | 3076 (28.8) | 3102 (28.6) |
| **Marital Status** | | | | |
| Married | 5244 (56.7) | 6062 (56.0) | 6015 (56.3) | 6010 (57.1) |
| Single | 2724 (29.5) | 3053 (28.2) | 3003 (28.1) | 2834 (26.9) |
| Widowed | 279 (3.0) | 443 (4.1) | 446 (4.2) | 497 (4.7) |
| Divorced | 681 (7.4) | 876 (8.1) | 837 (7.8) | 826 (7.8) |
| Separated | 316 (3.4) | 397 (3.7) | 376 (3.5) | 366 (3.5) |
| **Smoking status at entry** | | | | |
| Never | 5165 (55.9) | 5983 (55.2) | 5862 (54.9) | 5879 (55.8) |
| Former | 2208 (23.9) | 2556 (23.6) | 2533 (23.7) | 2373 (22.5) |
| Current | 1871 (20.2) | 2292 (21.2) | 2282 (21.4) | 2281 (21.7) |
| **Drinking behavior at entry** | | | | |
| None | 3588 (38.8) | 4390 (40.5) | 4267 (40.0) | 4314 (41.0) |
| Moderate | 4021 (43.5) | 4600 (42.5) | 4577 (42.9) | 4529 (43.0) |
| Heavy | 1635 (17.7) | 1841 (17.0) | 1833 (17.2) | 1690 (16.0) |
| **Physical activity at entry** | | | | |
| No activity | 3414 (36.9) | 4329 (40.0) | 4222 (39.5) | 4261 (40.5) |
| Once a week | 1156 (12.5) | 1305 (12.1) | 1282 (12.0) | 1296 (12.3) |
| Twice a week | 1040 (11.3) | 1153 (10.6) | 1142 (10.7) | 1105 (10.5) |
| 3-5 times a week | 2135 (23.1) | 2337 (21.6) | 2332 (21.8) | 2226 (21.1) |
| ≥5 times a week | 1499 (16.2) | 1707 (15.8) | 1699 (15.9) | 1645 (15.6) |
| **Insurance coverage at entry** | 8175 (88.4) | 9618 (88.8) | 9500 (89.0) | 9320 (88.5) |
| **Body Mass Index** | | | | |
| Normal weight (18.5–25) | 2065 (22.3) | 2248 (20.8) | 2246 (21.0) | 2165 (20.6) |
| Overweight (25–30) | 2922 (31.6) | 3322 (30.7) | 3322 (31.1) | 3221 (30.6) |
| Low obesity (30–35) | 2455 (26.6) | 2892 (26.7) | 2848 (26.7) | 2831 (26.9) |
| Medium obesity (35–40) | 1140 (12.3) | 1418 (13.1) | 1383 (13.0) | 1397 (13.3) |
| Extreme obesity (>40) | 662 (7.2) | 951 (8.8) | 879 (8.2) | 919 (8.7) |
| **Other family member < 18 year of age at entry** | 4147 (44.9) | 4686 (43.3) | 4598 (43.1) | 4510 (42.8) |

Data are shown as means (standard deviations) for continuous variables or counts (percentages) for categorical variables.
[a] Results from one imputed dataset, other datasets have similar distributions.
[b] Health outcome according to each dataset.

income and debt-to-wealth, was achieved after applying the controlled direct effect weights, $w_{it}^{CDE}$ (Fig. 3b and c). The means of different weights were close to one, with some extreme values before the truncation (eTable 1). When considering unsecured debts over wealth as a mediator, the controlled direct effect of education on the risk of death or first onset of hypertension (RR = 1.07, 95%CI = 0.89, 1.28) was substantially attenuated compared to the total effect; the proportion eliminated on the excess relative risk scale was 72% (Suzuki et al., 2014). Similar attenuation was observed for death or first onset of coronary heart disease (CDE = 1.10, 95%CI = 0.87, 1.40; proportion eliminated = 76%). However, there was less evidence that unsecured debt over wealth explained a substantial proportion of the total effect of education on death or diabetes (CDE = 1.38, 95%CI = 1.10, 1.73; proportion eliminated = 24%), or death or psychiatric problems (CDE = 1.24, 95% CI = 0.98, 1.57; proportion eliminated = 38%). We also found less evidence with unsecured debt over income as the mediator (Tables 2–5). Loosening the assumption of homogeneity (i.e., fixing household unsecured debts at each level of its quantile distribution) led to similar conclusions (Fig. 4).

Sensitivity analyses, considering death as artificial censoring yield similar results for unsecured debt over wealth, with lower estimates of the proportion of the total effect eliminated. However, there was some evidence that unsecured debt over income attenuated the total effect of education on coronary heart disease (proportion eliminated = 76%) (eTable2).

## 4. Discussion

### 4.1. Main findings

Our study found education-based inequalities in health outcomes, with respondents with less than 12 years of education having a higher risk of death or onset of hypertension, diabetes, coronary heart disease and psychiatric problems. Marginal structural model used to compute controlled direct effects indicated that the relation between education and the health outcomes investigated are partially mediated by household unsecured debts over wealth, particularly for hypertension and coronary heart disease. The total effects of education on first onset of death or diabetes, and first onset of death or psychiatric problems were not substantially attenuated by levels of unsecured debt over wealth or income. Using mediation analyses to quantify the contribution of debt to education-based inequalities in health is not prevalent in the literature. One study in the United Kingdom has identified household debts as a mediator of the relation between low income and mental disorders

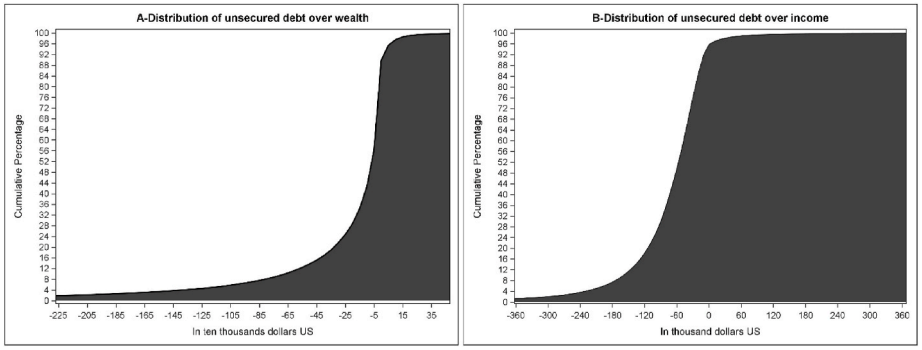


**Fig. 2.** Distributions of mediators*
*Results from the hypertension dataset, other datasets have similar distributions.

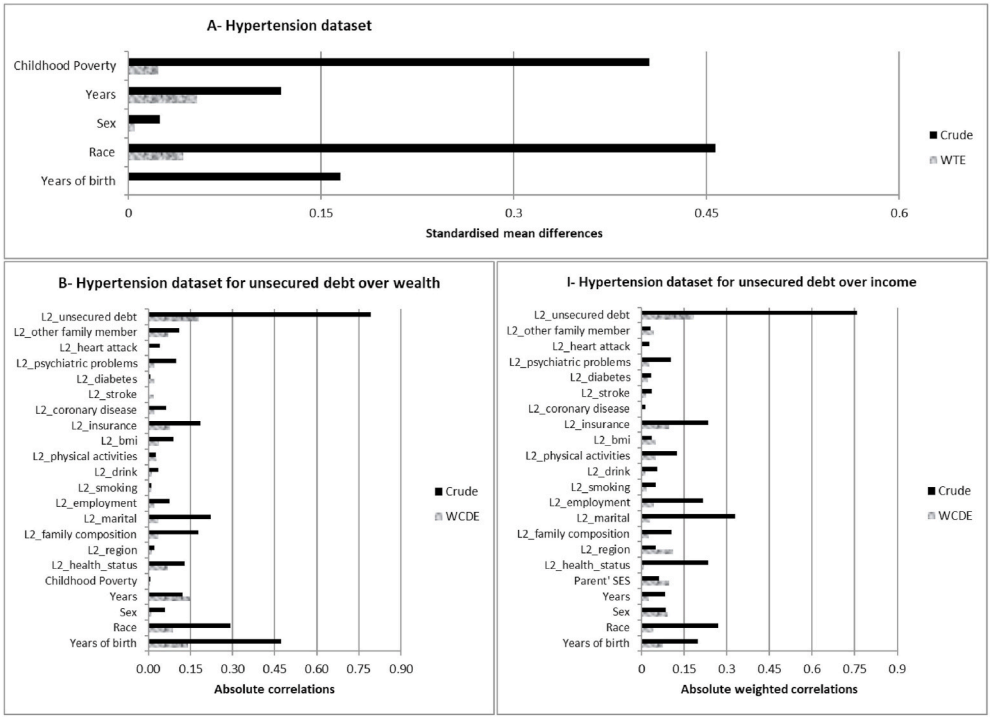


**Fig. 3.** Assessment of balance of fixed and time-varying confounders*
*Results from one imputed dataset, other datasets have similar distributions.

**Table 2**
Total and controlled direct effects of education on death or first onset of hypertension[a].

| | Unsecured debt over wealth as mediator | | Unsecured debt over income as mediator | |
|---|---|---|---|---|
| | RR (95% CI) | RD (95% CI) | RR (95% CI) | RD (95% CI) |
| TE | 1.25 (1.13–1.39) | 2.02 (1.04, 3.00) | 1.25 (1.13–1.39) | 2.02 (1.04, 3.00) |
| Education → Mediators[b] | – | 256,771 (205,963–307,001) | – | 40,495 (35,747–45,243) |
| Mediators → Outcome[c] | 2 Vs 1: 1.26 (0.82–1.93) | 2 Vs 1: 1.85 (−1.15, 4.84) | 2 Vs 1: 1.19 (0.75–1.90) | 2 Vs 1: 1.12 (−2.36, 4.62) |
| | 3 Vs 1: 1.24 (0.76–2.00) | 3 Vs 1: 1.68 (−1.96, 5.31) | 3 Vs 1: 0.98 (0.63–1.51) | 3 Vs 1: 0.18 (−3.40, 3.04) |
| | 4 Vs 1: 1.29 (0.82–2.04) | 4 Vs 1: 1.92 (−1.44, 5.29) | 4 Vs 1: 1.11 (0.70–1.75) | 4 Vs 1: 0.91 (−2.58, 4.40) |
| | 5 Vs 1: 1.54 (0.96–2.48) | 5 Vs 1: 3.85 (−0.02, 7.72) | 5 Vs 1: 1.20 (0.75–1.91) | 5 Vs 1: 1.47 (−2.16, 5.11) |
| CDE | 1.07 (0.89–1.28) | 0.57 (−1.10, 2.25) | 1.11 (0.93–1.33) | 0.90 (−0.62, 2.41) |

TE = Total effects; CDE= Controlled direct effects using truncated weights (1st and 99th percentiles); RR = relative risks, and RD = difference in percentage points.
[a] Rubin's rule was used to combine estimates across imputed datasets.
[b] Risk differences were obtained through a linear model using total effect weights $w_{it}^{TE}$, with high school or greater education as the reference.
[c] Results were obtained through a log-binomial regression with CDE weights $w_{it}^{CDE}$. Mediators were split in quintiles, and the lowest quintile (least indebted) was the reference.

**Table 3**
Total and controlled direct effects of education on death or first onset of coronary heart disease[a].

| | Unsecured debt over wealth as mediator | | Unsecured debt over income as mediator | |
|---|---|---|---|---|
| | RR (95% CI) | RD (95% CI) | RR (95% CI) | RD (95% CI) |
| TE | 1.42 (1.25–1.62) | 1.23 (0.73–1.73) | 1.42 (1.25–1.62) | 1.23 (0.73–1.73) |
| Education → Mediators[b] | – | 254,414 (211,870–296,959) | – | 39,624 (35,471–43,777) |
| Mediators → Outcome[c] | 2 Vs 1: 1.52 (1.04–2.23) | 2 Vs 1: 0.93 (0.10, 1.77) | 2 Vs 1: 0.73 (0.33–1.61) | 2 Vs 1: 1.14 (−3.98, 1.69) |
| | 3 Vs 1: 2.18 (1.30–3.67) | 3 Vs 1: 2.15 (0.36, 3.95) | 3 Vs 1: 0.79 (0.37–1.66) | 3 Vs 1: 0.90 (−3.66, 1.86) |
| | 4 Vs 1: 1.67 (1.10–2.54) | 4 Vs 1: 1.14 (0.07, 2.21) | 4 Vs 1: 0.83 (0.39–1.74) | 4 Vs 1: 0.66 (−3.47, 2.15) |
| | 5 Vs 1: 1.69 (1.16–2.49) | 5 Vs 1: 1.14 (0.25, 2.03) | 5 Vs 1: 0.78 (0.38–1.62) | 5 Vs 1: 0.97 (−3.71, 1.78) |
| CDE | 1.10 (0.87–1.40) | 0.37 (−0.42, 1.17) | 1.25 (1.01–1.54) | 0.73 (0.03, 1.44) |

TE = Total effects; CDE= Controlled direct effects using truncated weights (1st and 99th percentiles); RR = relative risks, and RD = difference in percentage points.
[a] Rubin's rule was used to combine estimates across imputed datasets.
[b] Risk differences were obtained through a linear model using total effect weights $w_{it}^{TE}$, with high school or greater education as the reference.
[c] Results were obtained through a log-binomial regression with CDE weights $w_{it}^{CDE}$. Mediators were split in quintiles, and the lowest quintile (least indebted) was the reference.

**Table 4**
Total and controlled direct effects of education on death or first onset of diabetes[a].

| | Unsecured debt over wealth as mediator | | Unsecured debt over income as mediator | |
|---|---|---|---|---|
| | RR (95% CI) | RD (95% CI) | RR (95% CI) | RD (95% CI) |
| TE | 1.50 (1.34–1.68) | 1.81 (1.23–2.39) | 1.50 (1.34–1.68) | 1.81 (1.23–2.39) |
| Education → Mediators[b] | – | 274,429 (229,101–319,757) | – | 40,617 (36,252–44,918) |
| Mediators → Outcome[c] | 2 Vs 1: 1.13 (0.75–1.69) | 2 Vs 1: 0.52 (−0.94, 1.99) | 2 Vs 1: 0.66 (0.35–1.24) | 2 Vs 1: 2.06 (−5.34, 1.21) |
| | 3 Vs 1: 1.27 (0.81–2.00) | 3 Vs 1: 1.17 (−0.70, 3.03) | 3 Vs 1: 0.62 (0.34–1.14) | 3 Vs 1: 2.09 (−5.33, 1.15) |
| | 4 Vs 1: 0.94 (0.61–1.46) | 4 Vs 1: 0.32 (−1.83, 1.19) | 4 Vs 1: 0.76 (0.40–1.43) | 4 Vs 1: 1.20 (−4.60, 2.20) |
| | 5 Vs 1: 1.38 (0.81–2.34) | 5 Vs 1: 1.61 (−0.87, 4.09) | 5 Vs 1: 0.75 (0.41–1.37) | 5 Vs 1: 1.51 (−4.81, 1.79) |
| CDE | 1.38 (1.10–1.73) | 1.61 (0.45–2.77) | 1.32 (1.08–1.61) | 1.25 (0.31, 2.18) |

TE = Total effects; CDE= Controlled direct effects using truncated weights (1st and 99th percentiles); RR = relative risks, and RD = difference in percentage points.
[a] Rubin's rule was used to combine estimates across imputed datasets.
[b] Risk differences were obtained through a linear model using total effect weights $w_{it}^{TE}$, with high school or greater education as the reference.
[c] Results were obtained through a log-binomial regression with CDE weights $w_{it}^{CDE}$. Mediators were split in quintiles, and the lowest quintile (least indebted) was the reference.

**Table 5**
Total and controlled direct effects of education on death or first onset of psychiatric problems[a].

| | Unsecured debt over wealth as mediator | | Unsecured debt over income as mediator | |
|---|---|---|---|---|
| | RR (95% CI) | RD (95% CI) | RR (95% CI) | RD (95% CI) |
| TE | 1.39 (1.24–1.56) | 1.59 (0.98–2.20) | 1.39 (1.24–1.56) | 1.59 (0.98–2.20) |
| Education → Mediators[b] | – | 281,213 (234,623–327,803) | – | 39,437 (34,998–43,876) |
| Mediators → Outcome[c] | 2 Vs 1: 1.29 (0.74–2.26) | 2 Vs 1: 1.14 (−1.20, 3.48) | 2 Vs 1: 0.68 (0.35–1.29) | 2 Vs 1: 1.75 (−4.77, 2.27) |
| | 3 Vs 1: 1.09 (0.62–1.90) | 3 Vs 1: 0.26 (−2.01, 2.52) | 3 Vs 1: 0.75 (0.40–1.40) | 3 Vs 1: 1.39 (−4.38, 1.60) |
| | 4 Vs 1: 1.17 (0.67–2.05) | 4 Vs 1: 0.60 (−1.72, 2.91) | 4 Vs 1: 1.01 (0.54–1.91) | 4 Vs 1: 0.15 (−3.04, 3.33) |
| | 5 Vs 1: 1.45 (0.82–2.57) | 5 Vs 1: 1.78 (−0.76, 4.33) | 5 Vs 1: 0.99 (0.54–1.81) | 5 Vs 1: 0.20 (−3.21, 2.81) |
| CDE | 1.24 (0.98–1.57) | 1.12 (−0.10, 2.33) | 1.23 (1.01–1.50) | 0.89 (−0.05, 1.84) |

TE = Total effects; CDE= Controlled direct effects using truncated weights (1st and 99th percentiles); RR = relative risks, and RD = difference in percentage points.
[a] Rubin's rule was used to combine estimates across imputed datasets.
[b] Risk differences were obtained through a linear model using total effect weights $w_{it}^{TE}$, with high school or greater education as the reference.
[c] Results were obtained through a log-binomial regression with CDE weights $w_{it}^{CDE}$. Mediators were split in quintiles, and the lowest quintile (least indebted) was the reference.

(Bridges & Disney, 2010). However, that study used the traditional direct effect estimation approach, regressing health outcomes on the exposure, the mediator and potential confounders, which requires stronger assumptions to produce unbiased estimates (Kaufman et al., 2004).

Our finding that debt burden partially explains educational disparities in hypertension and coronary heart disease, but with less evidence for the other health outcomes tested, could indicate that blood pressure is a particularly sensitive (and/or early) biomarker of debt-related financial strain. This finding is also in line with other recent studies that have documented associations between financial debt (measured as household unsecured debt over wealth and income) and cardiovascular risk factors like hypertension, body mass, and inflammation (Sweet et al., 2013, 2018). Elevated blood pressure is known to be associated with psychological stress (Cohen et al., 2007; McEwen & Gianaros, 2010), and other research suggests that being in debt is a potent psychosocial stressor (Richardson et al., 2013). Further studies should investigated this stress pathway. Altered care seeking behavior could also be a factor, particularly as it relates to hypertension treatment. Recent research shows that those in debt regularly skip medical care and treatments to avoid incurring more debt (Sweet, 2020), and this could be especially true for those with lower education and fewer other financial resources.

Socially disadvantaged borrowers, including racial and ethnic minorities and those of lower socioeconomic standing (including lower educational attainment), face a toxic and burdensome debt landscape. They are more likely to be denied loans or constrained by unfavorable loan terms, including higher interest rates and fees and more punitive

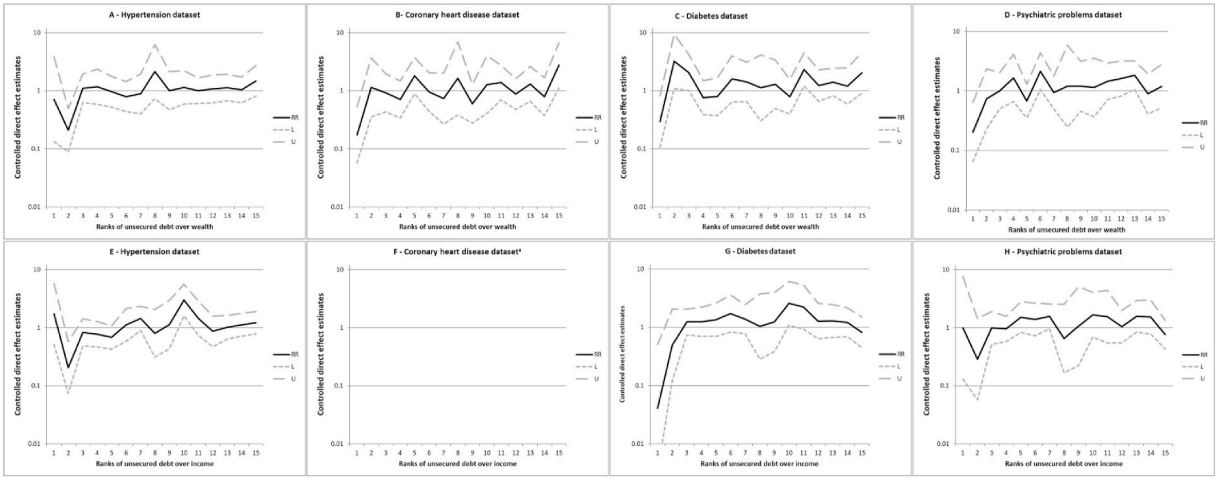


**Fig. 4.** Controlled direct effects of education on health outcomes when mediator values range from the first to the fifteen quantiles*
*Rubin's rule was used to combine estimates across imputed datasets.
[a] The model did not converge.

repayment structures, than are those in higher status positions. Therefore, disadvantaged groups are more likely to be caught in the debt trap of the 'poverty industry', having higher debt burdens (relative to wealth and income), a harder time paying down debts, and likely suffer greater financial and material consequences (such as damaged credit) and more stress (Soederberg, 2014; Williams, 2004). It is unsurprising then that debt appears to play a role in health disparities, as a mode through which social disadvantage gets under the skin.

Our analyses focus on health disparities across levels of educational attainment, which has consistently been one of the most potent socioeconomic predictors of health and longevity in the US(Zajacova & Lawrence, 2018). Data also suggest that these disparities are widening; while overall US population health has improved in recent years, the health of lower educated groups has declined (Montez et al., 2019), underscoring the need to better understand the full suite of factors involved in these health patterns. Our finding that relative debt burdens are highest among those with the lowest education, and that those debt burdens helped to explain higher rates of hypertension and coronary heart disease in that group, contributes to this needed work. Future research should investigate whether unequal debt burdens mediate other forms of health inequalities and disparities.

Our findings also reinforce the need to consider the broad political economic contexts that embed and connect many of the social inequities that matter for health. A range of factors have been proposed to explain health disparities by education, including greater access to health-related knowledge, material resources and opportunities, but scholars also point to the important role that education plays in reproducing social inequity and the ways in which educational disparities are embedded in broader structures of neoliberal society (Walsemann et al., 2013; Zajacova & Lawrence, 2018). These same neoliberal structures, which degrade public institutions in favor of private options and seek to transform all aspects of civil society into a competitive marketplace, also shape the inequitable distribution of debt in the US as well as other social and economic inequalities (Collins et al., 2008; Harvey, 2007). Piecing together the varied ways in which landscapes and pathways of social disadvantage produced by these political power structures intersect to impact health will give us a more complete picture of how health inequalities work and could potentially be addressed at multiple junctures.

### 4.2. Limitations

For our approach to be valid, we first assumed that measured fixed and time-varying confounders were sufficient and that our marginal structural models were correctly specified to assure conditional exchangeability between the respondents with different levels of education and unsecured debts (Hernán & Robins, 2006). However, there may be errors in the measurement of education, household debts, potential confounders, and health outcomes, as well as unmeasured confounders. Health outcomes, in particular, may suffer for measurement error as respondents must have access to a doctor to get screened and receive a diagnosis. Given that those with higher education would generally have more access to a physician, the total effects are likely an underestimate of the true education-based inequalities in the health outcomes investigated. We attempted to minimize unmeasured confounding by including relevant variables available in the PSID and accounting for potential time-varying confounding using inverse probability weights; nonetheless, unmeasured confounding of the total and mediator-outcome associations is possible. Second, we should assume that our exposure (education) and mediators (household debts) map to a well-defined intervention (VanderWeele, 2009a). Although is possible to think about interventions on education and household debt, these measures should be considered proxies for a more complex socioeconomic construct (Diemer et al., 2013). In addition, dichotomizing education may have led to residual confounding as its relations with mediators and outcomes are not constant within each stratum (less than versus high school). Finally, we assumed positivity (i.e., non-zero probabilities of the exposure, mediator, and loss to follow up at every level of our measured confounders) (Cole & Hernán, 2008; Westreich & Cole, 2010). Even if this assumption probably holds for the estimation of total effects, we conducted analyses with truncated CDE weights to address extreme values, which may be indicative of positivity violations.

In addition to the caveats mentioned above, we may have been underpowered to detect controlled direct effects, given that adjusting for lagged values of time-varying confounders excluded the first two years of follow-up of all participants. During our study period, some participants (around 15% in the four datasets) returned to school and therefore changed their education status. However, removing these respondents and repeating the analyses (including the weight computation), yielded similar conclusions (eTable 4). In terms of external validity, our results only apply to our study samples since PSID sampling weighted estimates may not be representative of the general United States population after excluding families where the head was less than 25 years of age, excluding family members other than the head and his spouse, and creating separate samples for each outcome that excluded prevalent cases.

## 5. Conclusion

Using marginal structural models adjusting for time-varying measured confounders, we provided some evidence for the hypothesis that education-based inequalities in health are mediated by levels of unsecured household debt. Further studies are needed, using other measures of household debts, measures of stress pathways as potential intermediaries, and cause-specific mortality.

## Author statement

All authors contributed to the development of research objectives and drafted part of the manuscript and approved the final version. BB conducted data analysis.

## Ethical approval

This study received ethics approval from the McGill University Faculty of Medicine Research Ethics committee (IRB Study Number A03-E06-18 A).

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## Declaration of competing interest

The authors declare that they have no competing interests.

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## Abbreviations:

| | |
|---|---|
| **BMI** | Body Mass Index |
| **CDE** | Controlled direct effects |
| **MSM** | Marginal structural model |
| **PSID** | the Panel Study of Income Dynamics |

## Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.ssmph.2021.100736.

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