Workplace Stratification and Racial Health Disparities*

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Abstract

We provide the first US-based evidence on the relationship between relative workplace rank and health status for the near population of workers in one US state. Using a new linkage of commercial all-payer health insurance data to administrative earnings records for workers in Utah from 2013-2015, we quantify the impact of relative workplace rank on health status, the incidence of specific chronic diseases, and racial health disparities. We show that about 70% of SES-health gradient that is commonly interpreted as an income gradient actually operates through relative rank. For an average worker, moving from the 90th to the 10th percentile of within-firm rank holding fixed income, age, location, and health insurance characteristics is associated with a 16.5% increase in morbidity. The racial segregation of jobs in the US leads minority workers to be overrepresented in lower-ranked jobs within firms, which in turn exacerbates racial health disparities.

JEL Codes: I14, J15, I10

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1 Introduction

An average person at the top of the US income distribution lives over 12 years longer than a person at the bottom of the distribution (Chetty et al., 2016). Similar cross-sectional income-health gradients exist in many other high-income countries, despite substantial differences in social safety nets and healthcare systems. Yet there is compelling evidence that exogenous increases in income have no positive effects on adult physical health.[1] If income does not causally affect physical health, what can explain the strong and widespread cross-sectional income-health gradients?

Canonical economic models tend to emphasize the importance of economic resource levels in affecting health and well-being. Yet it has long been understood by social scientists that people also care deeply about their relative position within society.[2] Deaton (2001) and Deaton and Paxson (2003) propose and evaluate models in which health outcomes are determined by both income levels and differences between one's income and the average income of a reference group. Similar types of ideas have motivated a longstanding empirical literature that has shown that many different dimensions of relative social status, holding fixed income, have substantial impacts on life expectancy, rates of cardiovascular disease, happiness, job satisfaction, and myriad other measures of health and well-being.[3]

What are the aggregate impacts of relative social rank on health outcomes? To what extent does the strong income-health gradient operate through social rank? Do differences in social rank affect disparities in health outcomes across racial or other demographic groups? And which health conditions are affected by relative rank? Questions of broad importance such as these have been challenging to answer, in part because relative position is difficult to measure and is rarely observed alongside individual measures of specific health conditions.

We provide the first US-based evidence on the aggregate relationships between workers' relative ranks within workplaces and health status for the near population of workers in one US state. Using a new dataset that links administrative commercial all-payer health insurance data to earnings records for workers in Utah between 2013 and 2015 we quantify the impact of relative position within the workplace on morbidity, the specific health conditions that drive this relationship, and racial health

[1] Cesarini et al. (2016); Miller et al. (2024); Snyder and Evans (2006); Ruhm (2000); Ruhm (2005)

[2] "men do not desire to be rich, but to be richer than other men." Pigou (1920) attributed this quote to John Stuart Mill.

[3] Anderson and Marmot (2012); Antonovsky (1967); Blázquez et al. (2014); Card et al. (2012); Clark and Oswald (1996); Clark et al. (2008); Cutler et al. (2011); Easterlin (1995); Halliday (2017); Kröger (2016); Luttmer (2005); Ravesteijn (2016)

disparities.

We show that an average worker at the 90th percentile of within-firm rank, holding fixed income, age, location, and health insurance policy characteristics has 0.204 (16.5%) fewer diagnosed chronic conditions than a worker at the 10th percentile. Within-workplace relative rank has an even stronger association with health status for racial minorities, especially American Indian workers, who have 34% more diagnosed chronic conditions at the 10th percentile than at the 90th percentile. Because of the racial segregation of jobs in the US workforce (Hellerstein and Neumark, 2008), which leads Black, Hispanic, and American Indian workers to be overrepresented in lower-ranked jobs within firms, this large impact of within-firm rank on health exacerbates racial health disparities. We also show that the majority (70%) of the SES-health gradient that is commonly interpreted as an income-health gradient actually operates through relative rank within the workplace.

Applying linked administrative health and earnings data to this question allows us to make several important innovations. First, whereas most studies have faced tradeoffs between precise measures of relative position or detailed health records, we use a linkage between rich administrative data containing detailed health claims and diagnosis records and a long panel of quarterly earnings records and relative workplace ranks for nearly all jobs in the state. Because social status is difficult to measure, many previous studies have focused on specific domains that affect famous individuals, using newsworthy deaths as the primary health outcome. For example, Borgschulte and Vogler (2019) estimate that politicians who win closely contested elections live 1-3 years longer than their defeated opponents, Becker et al. (2007) estimate that baseball Hall of Fame inductees live 10% longer than those who narrowly missed induction, Han et al. (2011a) find that winners of Oscar awards live around 4 years longer than nominees, and similar qualitative patterns have been shown for Nobel Laureates (Rablen and Oswald, 2008; Chan, Mixon, Sarkar and Torgler, 2022), academics (Liu et al., 2017), US Navy submarine personnel (Suandi, 2021), and British Civil Servants in the Whitehall studies (Marmot et al., 1991; Anderson and Marmot, 2012).

In contrast to the studies that focus on narrow domains, which tend to find important effects of social position on health, those that have sought more general answers to these questions often find little or no evidence of such a relationship (Boyce and Oswald, 2012; Johnston and Lee, 2013). Deaton and Paxson (2003) explain why this discrepancy should be unsurprising. Broadly representative samples often come at the expense of precise measures that differentiate relative position from income levels, requiring researchers to aggregate measures of relative position across reference groups. This aggregation biases empirical estimates of the relationship between relative position towards zero, consistent with the dichotomous empirical evidence in the literature.[4]

A second important advantage of linked administrative data is that it allows us to use information about the network structure of the labor market to separately estimate the impacts of earnings levels and relative ranks on health, leveraging differences in earnings distributions across firms that cause workers with the same earnings levels to have different relative ranks within the workplace. In contrast to studies that use data from a single organization (including the famous Whitehall studies and Nicholas (2020), who studies a 1930s cohort of workers at General Electric) where earnings levels are highly correlated with workplace rank, in our broader labor market data only 50% of the variance in workers' percentile ranks in the statewide earnings distribution is explained by within-firm rank. We also construct household earnings measures to further help disentangle the effects of relative rank from those of financial resources.

And third, large-scale administrative data allow us to quantify differences in the strength of workplace social determinants of health for understudied minority groups, including Black, American Indian, and Hispanic workers, and to quantify the role of workplace social position in affecting aggregate racial disparities in health outcomes.

We begin by showing a variety of stylized descriptive evidence on the importance of within-workplace rank in explaining disparities in major chronic disease prevalence by race and ethnicity. We show that there is a strong negative association between workplace rank and health status for every demographic group in a parsimonious model that only controls for earnings levels (fixed effects for each percentile of the household earnings distribution) and age. After adding detailed controls including age-by-zip code effects, effects for every health insurance policy contract, gender effects, year effects, firm size, and job tenure, the strength of the average conditional relationship and racial disparities between workplace rank and chronic disease prevalence remain strong, with an average worker at the 10th percentile having 0.204 (16.5%) more chronic conditions than a worker at the 90th percentile, and larger differences for minority workers.

Conversely, we show that the strong association between income levels and chronic disease prevalence shrinks substantially, by 70%, after controlling for workplace rank. Using a symmetric comparison of the rank-health gradient conditional on income percentiles to the income-health gradient conditional on rank percentiles, we show that health disparities by workplace rank are 4.8 times larger than disparities by household income. This pattern holds for every racial and ethnic group. Using detailed medical diagnoses, we show that workers at the bottom of the rank distribution have

[4] See Deaton (2001); Deaton and Paxson (2003).

substantially greater prevalence of 16 out of the 18 major chronic diseases we study.

We then ask whether these patterns of health disparities by workplace rank can be explained by labor market sorting, including the possibility that unhealthy workers are disproportionately assigned to low-ranked jobs (conditional on earnings). We estimate life-cycle age-morbidity patterns for workers at different percentiles of the rank distribution. For men, there are no differences in health status by rank upon entering the labor force in the early to mid-20s, but a gap emerges and widens substantially during a worker's 30s and 40s. Among women, the divergence in health status by rank becomes evident in the mid-30s, and the health gaps are smaller than those among men. This cross-cohort analysis is supplemented by within-worker event study analyses of job changes, which show clear effects of changes in workplace rank on the slope of the age-morbidity profile.

One challenge to interpreting the descriptive evidence as causal effects of rank on health status is that systematic variation in rank and health could be driven by selection in the types of workers assigned to different ranks. This could occur if health status affects labor supply decisions, or if employers discriminate on health in hiring, promotion, or separation decisions. We estimate a model of exposure to workplace social status in which the health effects of exposure are age-specific and accumulative, and use the model to assess this potential endogeneity of workplace rank with respect to health status. We first conduct a comparison of job movers and stayers in the spirit of Chetty and Hendren (2018). In this analysis, we ask whether workers who change jobs and move up the workplace rank distribution experience health effects that are comparable to those of job stayers employed at the same rank. We find that workers who change ranks by moving between jobs have treatment effects on health that are approximately 80%-120% as large as the effects among long-term job stayers at the same rank. This implies that the potential endogeneity in job mobility patterns cannot explain the rank-health gradient that we document.

Supporting this conclusion, we provide three diagnostic tests showing that observational job mobility patterns up and down the distribution of workplace rank are not associated with idiosyncratic differences in health status, conditional on income. Specifically, in every decile of the health status residual workers have similar probabilities of changing jobs (conditional on age), and job movers have similar changes in workplace rank. Future workplace rank after a job move is also not meaningfully predictive of current health status, indicating that assignment to different ranks after job moves is not related to the health status residual. These analyses suggest that nonrandom sorting in the labor market on the basis of unmodeled differences in health status cannot explain the differences in health status that we document.

To further reinforce this point, we construct instrumental variables using firm-level promotion rates, network patterns of coworker flows between firms, and early career rank to isolate variation in exposure to workplace rank that is independent of idiosyncratic variation in health status. The IV estimates are similar to the baseline fixed effects estimates, consistent with the diagnostic evidence that job mobility patterns across ranks are unrelated to residual health status. One exception is very early career workers, for whom the early career rank instrument yields estimates that are moderately larger than the fixed effects estimates.

The comparison between movers and stayers and the IV analyses also help provide information about potential mechanisms that could explain the observed relationship between workplace rank and health status. Specifically, one interpretation challenge is that although income, zip code of residence, age, and race are strong predictors of education, we cannot directly measure education, which could explain part of the disparities in health status by rank. Since we focus on a sample of prime-aged workers, for whom education is generally complete, the evidence that workers who change jobs and move to different ranks experience similar health effects as job stayers at the same rank is inconsistent with education as a mechanism for the results. Corroborating this interpretation, we show that the estimates are similar when isolating within-worker-firm changes in rank and health status as identifying variation.

This is the first known evidence from a broadly representative US population of workers to show that social hierarchy in the workplace is a strong causal determinant of health, even outside of the influence of earnings. We conclude by conducting counterfactual analyses to quantify how the overrepresentation of minority workers in low-ranked jobs compounds income and resource-based inequality with health inequality that operates through social hierarchy in the workplace. We estimate that if Black and American Indian workers had the same workplace rank distribution as White workers, holding income fixed, the health improvements would be similar to completely curing both Ischemic Heart Disease and Chronic Obstructive Pulmonary Disease (COPD).

2 Background

2.1 Mechanisms Connecting Rank and Health

Epidemiologists and medical researchers have long hypothesized that socioeconomic status causally affects health in broad and systemic ways. A key mechanism behind this relationship is that people in lower positions within social hierarchies have persistent exposure to psychosocial stress associated with feeling subordinate and lacking control or power (APA, 2017). This leads to persistent activation of fight-or-flight responses, which suppresses the immune system, reduces immune cell counts and functioning, and causes chronic inflammation (Dhabhar, 2009). Repeated exposure to this process results in an increase in 'allostatic load,' or wear and tear on the body, and the shortening of telomeres, which are a genetic biomarker of aging (McEwan, 1998; Seeman et al., 2001; Epel et al., 2004). The immunosuppressive and inflammatory responses to chronic stress can therefore lead to the premature onset of a wide range of age-related diseases (Albert et al., 2006; APA, 2017; Baum et al., 1999; Braveman and Gottlieb, 2014; Cawthon et al., 2003). The medical literature has identified chronic stress as a risk factor that increases the prevalence of every chronic condition that we study in this paper.

Perhaps the most convincing evidence that social rank causally affects health comes from studies of non-human primates. Shively and Clarkson (1994) ran a study in which they experimentally changed the relative social positions of monkeys while holding fixed all of their material conditions like food and shelter. They found that monkeys moved from dominant to subordinate positions had pronounced increases in atherosclerosis, which causes heart attacks, strokes, and peripheral artery disease. Experimental studies that changed the social hierarchy among male baboons have found similar evidence that lower social status is associated with higher cortisol (a stress biomarker), higher resting blood pressure, and fewer lymphocytes, among other negative health effects (Sapolsky, 1993; Sapolsky and Share, 1994; Sapolsky and Spencer, 1997; Sapolsky and Share, 2004).

2.2 Existing Evidence on the Rank-Health Relationship

The literature on relationships between socioeconomic status (SES) and health is exceptionally large and spans many fields outside of economics and sociology.[5] A positive correlation between SES and health has been extensively documented, but there continues to be debate about both the direction of causality in this relationship and the potential mechanisms that underlie it. One conclusion that appears indisputable is that, to some extent, there are causal forces operating in both directions. Negative health shocks can have lasting impacts on earnings and other measures of SES (Fadlon and Nielsen, 2021; Halla, 2013; Dobkin et al., 2018; Prinz et al., 2018). Similarly, shocks to economic resources can have lasting effects on health status, particularly if they lead to deprivation in early

[5] See Cutler et al. (2011) and Evans et al. (2012) for excellent reviews of this literature.

childhood (Adda et al., 2009; Baird et al., 2013; Adda and Fawaz, 2020; Lenhart and Chey, 2017; Case et al., 2005, 2002a).

Here we focus on a specific component of SES: relative position in one social hierarchy, the workplace. Given the extensive literature on relationships between income, place, and health, our aim specifically is to quantify the relationship between social position in the workplace and health while removing the impacts that operate directly or indirectly through income or location of residence.

The most influential studies of the relationship between relative workplace rank and health were the famous Whitehall Studies (Marmot et al., 1991), which showed a steep negative relationship between employment grade and health status in the British Civil Service. In this study, workers with lower relative positions had worse health behaviors, higher rates of many diseases, and higher mortality rates. Case and Paxson (2011), however, revisit the Whitehall II study and argue that the implication of a causal relationship is problematic because within the Whitehall sample there are differences in the height of workers across employment grades, which is related to childhood nutrition. The authors show that early-life health status is strongly correlated with initial employment grade and with the probability of promotion to a higher grade. Because early-life health status is also associated with later-life health, this confounds the interpretation of the evidence. Anderson and Marmot (2012), however, use the same Whitehall data and apply an IV strategy based on differences in job promotion rates by department to show that at least part of this relationship, the effect of employment grade on heart disease, is causal.

Motivated in part by the findings of the Whitehall studies, social epidemiologists argued that a person's relative position in a social hierarchy is a powerful determinant of health (Adler et al., 1994; Marmot, 2004; Wilkinson, 2005), and that interactions between race and socioeconomic position or power have contributed to the widening of racial health disparities (Williams and Collins, 1995; Williams et al., 1997). The hypothesis is also consistent with aggregate relationships between economic outcomes and health status across countries. Among developed countries, there is only a weak correlation between per capita GDP and life expectancy, but the relationship between income and health is much stronger within countries. Some have argued that greater income inequality within countries is associated with worse health (Marmot, 2004; Wilkinson, 2005), though Deaton and Paxson (2004) argue that evidence does not support this conclusion.

Recent studies in economics have used natural experimental variation to estimate the effects of social status on mortality, and find convincing evidence of a causal relationship in many specific contexts. Borgschulte and Vogler (2019) use a regression discontinuity design to study the effects of winning a political election (state Governor, US Senate, or US House) and find that over the past century, politicians who win closely contested elections live more than two years longer than their opponents, and the effects are even larger (3-4 years) for the more powerful positions of Governor or Senator. However, Link et al. (2013) show that the opposite patterns hold for US presidents and Vice Presidents, though the same study finds significant increases in longevity from winning an Emmy award. The authors make the case that the context of social status also matters, as some positions of high-rank increase stress, which is the opposite of the average impact of social position on stress. This suggests that one may expect to see worse health outcomes among people with low social positions, improvement in health as social position increases, but a potential reversal of patterns at the top of the hierarchy, especially in contexts where positions of power increase stress.[6]

Suandi (2021) studies the effects of promotion on the life expectancy of US Navy submariners in the Second World War. The author constructs an instrument by leveraging defects in the design of the Mark 14 torpedo used in submarines, which were calibrated based on the historical geomagnetic field in Newport, Rhode Island. Depending on differences in the geomagnetic field of different patrol locations, these torpedoes had different probabilities of missing their targets. Sailors on patrol in locations where this design error was smaller were more likely to sink ships and, therefore, more likely to be promoted, and they lived 2.4 years longer than their non-promoted counterparts.

Several other studies have shown that winning prestigious awards reduces mortality rates. Redelmeier and Singh (2001), Sylvestre et al. (2006), and Han et al. (2011b) all estimate the effects of winning an Oscar on life expectancy, and find that winners live on average between 3.5-4.2 years longer than nominees, though the small sample causes some of these estimates to be statistically insignificant. Rablen and Oswald (2008) estimate that Nobel prize winners in the 19th century lived 1-2 years longer than nominees who did not win, and rule out the financial impacts of the prize as the mechanism for this difference. Chan, Mixon Jr, Sarkar and Torgler (2022) also study the effects of winning a Nobel prize, and show that winning the prize at an age that is 10 years lower increases life expectancy by 1 year. Becker et al. (2007) estimate that baseball players elected to the Hall of Fame live 10% longer than those who narrowly miss induction.

The general conclusion that higher social status improves health outcomes is not incontrovertible. While there is a collection of convincing evidence within specific narrow domains, often focusing on famous politicians, entertainers, scholars, or athletes, it has been difficult to replicate similar

[6]Nicholas (2020) also finds higher mortality among senior executives in a cohort of white-collar employees at the General Electric headquarters in 1930.

findings in broadly representative data. Boyce and Oswald (2012) find little evidence that job promotions improve health among respondents to the British Household Panel Survey, and find declines in psychological wellbeing. Johnston and Lee (2013) find some positive effects of promotion on mental health, but little evidence of broader health effects in Australian survey data. These studies have required making important tradeoffs between having clean quasi-random sources of variation in social status versus having broadly representative data. Deaton and Paxson (2001) point out that mismeasurement of relative position is likely to attenuate estimates of the relationship between social status and health, which may explain the conflicting evidence in this literature.

To date, there is no known empirical evidence from a broadly representative population that has documented a causal connection between social rank and health status that operates outside of an income, education, or resource-based channel. Our empirical setting allows us to separate the impacts of income levels from those of relative position in the workplace, and to document the specific disease channels that are likely precursors of a relationship between relative position and mortality. Finally, we quantify the extent to which racial and ethnic minorities are disproportionately impacted by the health effects of workplace rank.

3 Data and Descriptive Statistics

We use two primary data sources: the Utah All-Payer Claims Dataset (APCD) (Utah Office of Healthcare Statistics, 2013-2015) from 2013-2015, linked to a custom aggregated earnings file constructed by the Utah Department of Workforce Services using administrative earnings records for jobs covered by unemployment insurance (UI) (Utah Department of Workforce Services, 2002-2019). We describe these files, the construction of our primary analysis sample, and descriptive statistics below.

3.1 Data

The APCD is an administrative database constructed by the Utah Office of Health Care Statistics. Under Utah state law, all commercial insurance companies are required to submit administrative records containing insurance enrollment plans and dates, individual demographics, and medical, drug, and dental claims to the Office of Health Care Statistics.[7] The APCD is constructed from these

[7]See Utah Health Data Authority Act, Ch 33a. All commercial insurers with at least 2,500 total subscribers statewide were required to comply with this law. Beginning in 2016, the US Supreme Court decision in *Gobeille v. Liberty Mutual*

administrative records. It contains records of virtually all commercial insurance plans, including ACA exchange plans, employer-sponsored insurance plans, individual and small group plans, and Medicaid managed care plans. The APCD excludes traditional Medicare enrollees, though our focus in this paper is on working adults below age 65, who are generally ineligible for Medicare.

In this study, we use the portions of the APCD that include administrative insurance enrollment records, demographic records, and claims records, all of which are submitted to the Office of Health Care Statistics by insurers following standardized submission guidelines. The enrollment file is a person-plan-month file that reports the exact enrollment dates for every commercial insurance plan and every person in each month. Individuals frequently have multiple concurrent insurance plans, for example, separate medical and dental plans or separate medical and prescription drug plans, and each of these plans is included separately in the enrollment file.

The APCD contains basic demographic information about each individual, including an (anonymized) individual identifier, gender, age, race[8], ethnicity[9], and geographic location. The individual identifier, gender, age, and geography are never missing for any individual in the data. However, as is common in administrative health records, race and ethnicity are unknown for some people. In ACA exchange plans sold on healthcare.gov, for example, race and ethnicity are optional demographic fields that enrollees are not required to provide. In the 2013-15 ACPD, race and ethnicity are missing or unknown for a large share of individuals, 65%. Because of the high missing rate, we supplement missing race and ethnicity information by linking individuals in the ACPD to the complete set of hospital emergency discharge records from 1996-2013, hospital inpatient records from 1992-2013, ambulatory hospital discharge records from 1996-2013, and APCD records from 2016-2019, all of which also contain race and ethnicity records. We use the union of all information about race and ethnicity for each person across all files and identify information on race or ethnicity for 74% of individuals who appear in the 2013-15 APCD. The racial and ethnic distributions in our sample are similar to the overall Utah demographic profile based on ACS data.

The enrollment file also includes anonymized plan IDs, group sizes, and employer identifiers for employer-provided plans. The claims components of the APCD contain medical and prescription

Insurance Co. overruled some aspects of all state laws nationwide used to create APCD files by allowing self-insured employers to opt out of the requirements. We use data prior to this law change, which contains nearly all commercially insured individuals in Utah.

[8]The racial classification is the standard OMB system: American Indian/Alaska Native, Asian, Black/African American, Native Hawaiian or other Pacific Islander, White, or Other. If someone is recorded as having multiple races by different insurers, we assume the person is of mixed race.

[9]Ethnicity is defined as whether the individual is of Hispanic, Latino, or Spanish origin

drug claims. Both claims databases contain detailed information on prices and patient cost-sharing components. The medical claims contain service codes, dates, and diagnoses, which are reported for inpatient, outpatient, and professional services following conventions similar to Medicare claims. Prescribed drug claims also include drug codes, fill dates, quantities, refills, days supplied, dispensing fees, and pharmacy identifiers and locations. We run all claims through the Johns Hopkins ACG Software v13.0, which analyzes combinations of medical diagnosis codes, procedure codes, and prescription drug records to construct indicators for medical conditions and chronic diseases, as well as predictive risk scores that summarize expected healthcare spending. We use this ACG software to identify 18 major chronic conditions that are the focus of our analyses.

The second primary data source is a custom earnings file produced by the Utah Department of Workforce Services. This earnings file is constructed in a way that aggregates information to balance privacy while still providing informative longitudinal quarterly data on earnings from jobs in Utah between 2002-2019. To construct the file, the Utah DWS calculated each worker's total earnings from all jobs in each quarter. In each quarter-year they then group workers into permilles (1000-quantiles) according to the worker's total earnings from all jobs, and report the average total quarterly earnings level for the permille. Throughout this paper, we refer to this measure as 'individual earnings'. Grouping workers into permilles introduces a small amount of measurement error to protect privacy, but average permille earnings levels are nonetheless very informative. Specifically, between 2013-2015 the average width of the permille bin (which is the extreme upper bound on measurement error in the bin) for the median earner was $17, or 0.2% of median quarterly earnings of $7,288.[10] The measurement error introduced by binning is much smaller than typical measurement error in earnings measures that come from surveys. For example, Abowd and Stinson (2007) link reported earnings in the SIPP to administrative W-2 records and find that the average discrepancy is 10-30%, and measurement error is correlated with education.

The DWS defines a dominant firm for a multi-job worker in each quarter as the firm that provides the highest quarterly earnings. We use this information to statistically estimate firm boundaries over time using a procedure that we describe in Appendix B. For workers with employer-sponsored health insurance, which is 81% of our analysis sample, firm boundaries are known because we observe employer IDs in the APCD.

[10] At the lower tail of the distribution, the bins have smaller widths ($11.5 at the 5.0th permille) while in the upper tail there is greater variation in earnings. At the 95.0th permille, the bins are $223 wide, or about 0.8% of average quarterly earnings of $27,859.

3.2 Preparation of the Estimation Sample

Our population of interest is employees aged 18 to 65 who have health insurance coverage at any point between 2013 and 2015. We start by using statewide earnings records to calculate the total income percentiles of each worker in each quarter. Similarly, we compute the quarterly within-firm ranks by using the earnings distribution within each firm. We exclude firms with fewer than five employees (3.5%) so that within-firm ranks are reasonably measured.

The monthly enrollment files in the APCD record the zip code of residence for each month of insurance enrollment. We assign individuals in the sample to their primary zip code of residence in each quarter, which we use to control for residential location. The enrollment file also contains information on whether a worker is enrolled in any health insurance policy each month, as well as anonymized insurance policy IDs. In models in which we include fixed policy ID effects to control for differences in health insurance characteristics, we assign each individual to their primary source of medical insurance.[11]

The earnings records are linked to the APCD by anonymized individual identifiers. Of the individuals in the earnings file, about 15% are not included in our population of interest because they do not ever hold commercial health insurance in Utah between 2013-2015, so it is not feasible to observe health information.

3.3 Descriptive Statistics

Table 1 shows descriptive statistics of the data. In Column 1 we report summary statistics for the population of interest, individuals who have observed earnings in Utah between 2013-2015 and any commercially-insured medical or drug claims during this period. Column 2 restricts the sample to workers employed at firms with at least 5 workers, with observed race or ethnicity, who had positive earnings and health insurance coverage in the same quarter. This is our main analysis sample, which contains 644,171 unique individuals and 5,291,167 person-quarter observations.

The population and main sample have similar racial and ethnic distributions as the state, based on comparisons to Census data. 1.5% of our main sample is Black, slightly lower than the 2.05% share in the Census, and 20% of our sample is Hispanic (any race), higher than the 15% in the Census.[12] These race and ethnicity summary statistics are non-mutually exclusive, so individuals who report

[11] In the case of multiple medical plan IDs we assign the individual to the plan in which they were enrolled the longest within the quarter. In the case there are still exact ties we randomly select assignment.

[12] See https://data.census.gov

Table 1: Descriptive Statistics

	(1) Employed With Insurance	(2) Main Sample
Demographics		
Age	38.9	39.5
Male	0.53	0.48
Race/Ethnicity		
White, Non-Hispanic	0.71	0.71
Black	0.014	0.015
American Indian	0.016	0.016
Other	0.079	0.079
Hispanic (Any Race)	0.20	0.20
Health Status		
At least 1 chronic condition	0.43	0.48
Number of chronic conditions	1.04	1.24
Health Insurance Type		
Employer-Sponsored	0.78	0.75
Non-Group	0.07	0.06
Medicaid	0.04	0.06
Earnings		
Average Quarterly Individual Earnings	12,151	11,549
Average Quarterly Household Earnings	22,306	21,435
Firm		
Average Number of Employees	3,389	3,800
Number of Persons	1,050,160	644,171
Number of Person-Quarters	8,676,633	5,291,167

Notes: In Column (1) summary statistics for Race/Ethnicity are conditional on non-missing values.

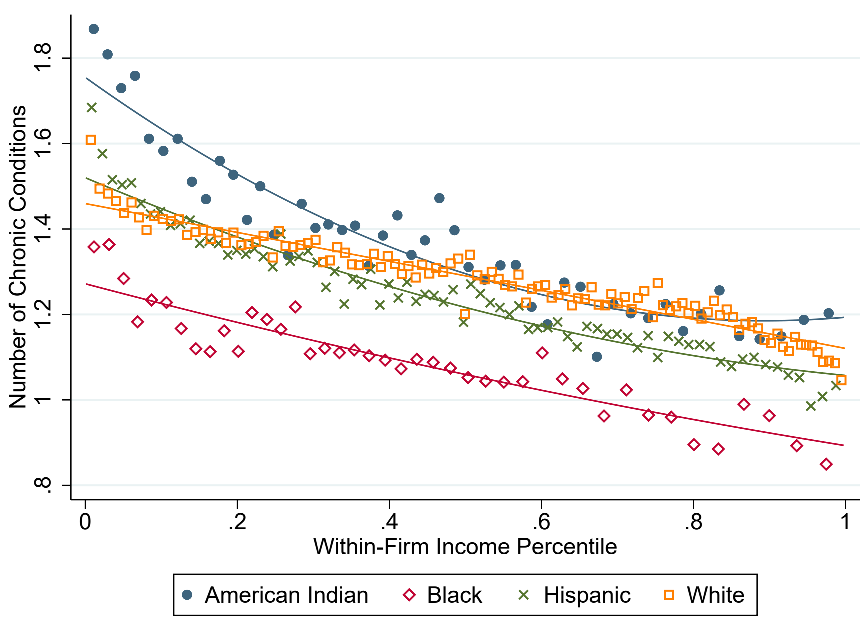
multiple racial or ethnic groups are included in multiple categories, with the exception of the White Non-Hispanic category. For this reason, the race and ethnicity shares do not sum to 100%.

Although Utah is a primarily White Non-Hispanic state, the share of the population that is neither White nor Hispanic is higher than states like Ohio and Wisconsin. Utah ranks 36th in the nation in the US Census Bureau's state-level Race and Ethnicity Diversity Index in 2020. The composition of minority groups in Utah is different than the US population distribution, which is important for contextualizing the racial disparities that we discuss. In the 2020 Census, Utah had the 13th largest state-level share of Hispanic residents. Utah is also home to 8 different tribal nations and had the 15th largest share of American Indian residents in the 2020 Census. Utah has a relatively small Black population, ranking 47th among US states. Column (2) shows that the main analysis sample is similar to the population sample in column (1) in other dimensions as well, except that the average number of chronic conditions is moderately higher in the analysis sample.

4 Descriptive Evidence on Within-Firm Rank and Health

We begin by documenting the conditional relationship between within-firm relative rank and health status. In estimating this relationship, we improve on the previous literature in several ways. First, we observe workers at about 30,000 firms with different earnings distributions, along with the relative positions of each worker in each firm over time. In contrast, the Whitehall studies focused on civil servants' employment grade, which was defined on the basis of salary. Second, since relative position and earnings levels are very highly correlated within any single organization, it is usually difficult to separate the impacts of workplace social status from other channels that connect earnings and health status, such as financial and housing stability.[13] We are able to exploit variation in rank while holding income fixed, and do so within geographic areas where such income controls are meaningful.[14] Combining both within and across-firm variation allows us to compare workers with the same earnings but different relative ranks.

Figure 1: Number of Chronic Conditions vs Within-Firm Income Percentile
Conditional on Age and Earnings Levels



Notes: This figure shows the joint distribution of the number of chronic conditions workers have been diagnosed with and their percentile earnings rank within their primary employer. Each dot is the race-specific conditional mean chronic condition count in a given bin of within-firm ranks, controlling for the full set of age indicators and fixed effects for each percentile of household earnings. The conditional means are constructed using the semiparametric partial linear regression approach developed in Cattaneo et al. (2023).

[13] Anderson and Marmot (2012) use an IV approach to overcome this challenge using the original Whitehall data.

[14] For example, holding income fixed across states with vast differences in housing costs would potentially conflate health status and housing stability.

Figure 1 shows the joint distribution of the number of chronic conditions workers have been diagnosed with and their percentile earnings rank within their primary employer. Each dot is the race-specific conditional mean chronic condition count in a given bin of within-firm ranks, controlling for the full set of age indicators and fixed effects for each percentile of household earnings, which allows earnings to have potentially nonlinear effects on health. The conditional means are constructed using the semiparametric partial linear regression approach developed in Cattaneo et al. (2023). For every demographic group, there is a negative relationship, suggesting that workers at higher ranks within the firm have fewer chronic health conditions than lower-ranked workers with the same age and earnings. On average, across all racial groups, workers in the top percentile of the within-firm earnings rank have 0.49 (35.8%) fewer chronic health conditions than workers in the bottom percentile.[15]

While Figure 1 shows an interesting relationship between rank and the prevalence of chronic conditions, there are many excluded factors that influence health status and may be correlated with relative rank, even after conditioning on age and income. For example, workers with higher employment ranks could live in different neighborhoods and be exposed to different environmental risks, they could have longer job tenure and less employment instability over their careers, or, perhaps as a consequence, they could have different health insurance plans and seek treatment at different sets of healthcare providers, among other possibilities.

To assess the influence of these other factors, in Figure 2 we include controls for as many of these factors as feasible. We condition on fixed effects for each percentile of household earnings, the full set of interactions between each year of age and each zip code of residence (to allow the age profile to interact with place effects)[16], gender, year, fixed effects for each health insurance policy, employer size, and job tenure.[17] Health insurance policy effects are constructed using anonymized group policy identifiers that link sets of individuals enrolled in the same insurance contract. For example, workers who choose different plan options from the same employer have different group

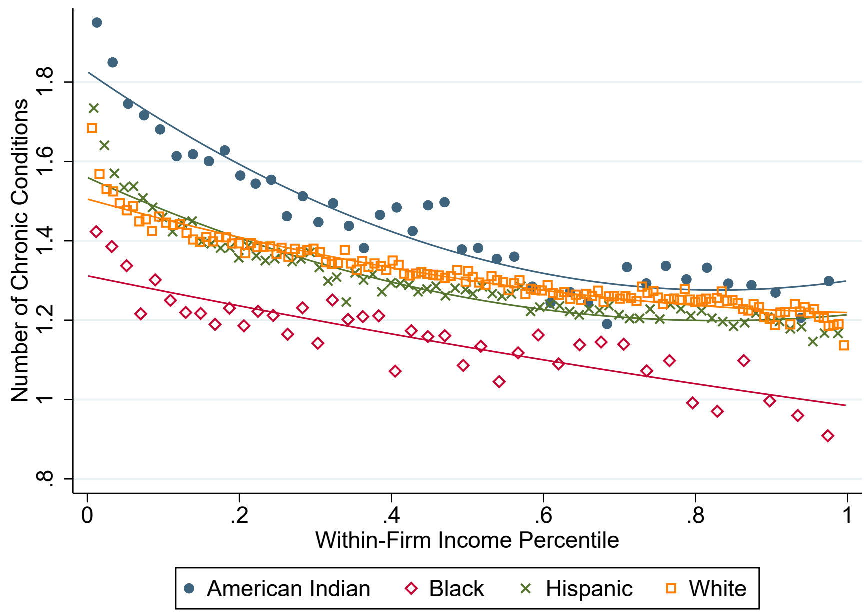
[15] Figure 1 also shows that the average level of chronic conditions is lower for Black workers than it is for other races. This is different than overall patterns of health status by race in the US. The majority of the difference in chronic condition levels between Black and White workers in our sample is explained by lower rates of mental health diseases (bipolar, schizophrenia, and depression) among Black workers, as we show in Appendix Table A1.

[16] There are 47 age groups, 345 zip codes, and 14,936 age by zip code interactions.

[17] Job tenure may capture differences in the duration of exposure to workplace ranks. For now, we focus on presenting static conditional means, and in practice job tenure has only a modest impact on health status conditional on the other controls. In Section 5, we investigate the dynamics of the health production process, including the dynamics of exposure over time. See also Fang and Gavazza (2011) for more on the impact of job tenure on health status (that operates through health insurance policy design).

policy numbers. Group policy effects capture differences in a wide set of contract features including deductibles, cost-sharing rules, provider networks, out-of-pocket maximums, or other insurance features that may influence either health status or disease diagnosis rates.

Figure 2: Number of Chronic Conditions vs Within-Firm Income Percentile
Full Set of Controls



Notes: This figure shows the joint distribution of the number of chronic conditions workers have been diagnosed with and their percentile earnings rank within their primary employer. Each dot is the race-specific conditional mean chronic condition count in a given bin of within-firm ranks, controlling for fixed effects for each percentile of household earnings, age × zip code fixed effects, gender, year, plan ID fixed effects, average firm size, and average tenure within a firm. The conditional means are constructed using the semiparametric partial linear regression approach developed in Cattaneo et al. (2023).

Figure 2 shows that the relationship between poor health and employment rank remains strongly negative after conditioning on all of these factors. We estimate that White workers at the 10th percentile of employment rank have on average 0.229 more diagnosed chronic conditions than White workers at the 90th percentile, or about 18% of the overall mean chronic condition prevalence. The 90-10 rank-health percentile gaps are larger for all of the racial and ethnic minority groups, 0.422 (34%) for American Indians, 0.277 (22%) for Hispanics, and 0.262 (21%) for Black workers. We also show in Appendix Figure A1 that these patterns are very similar if we control for percentile effects of individual earnings instead of household earnings.

To understand the roles of the control variables in the relationship, we decompose the changes in the gradients between Figure 1 and Figure 2 by quantifying the average marginal impact on the

90-10 percentile rank-health gradient of adding sets of control variables to the model.[18] Controlling for location through zip code effects has fairly small effects on the rank-health gradient for Black, Hispanic, and White workers. Place effects appear to be more substantial for American Indians, and removing them shrinks the gradient by 8%. Controlling for gender and year shrinks all of the gradients moderately. Adding fixed effects for each health insurance policy has modest but disparate impacts across races. These controls increase the health-rank gradient for American Indian and White workers by 0.013 to 0.014 (2.9% to 5.2% of the baseline gap), respectively, but narrow it substantially, by 0.027, for Hispanic workers. Controlling for firm size and tenure has relatively small effects on the gradient.

On aggregate, the controls reduce the rank-health gradient modestly, but come far short of explaining the relationship. The conditional rank-health gradient ranges from 6% smaller than the baseline gradient for American Indian workers to 25% smaller for Hispanic workers. The complete decomposition results are shown in Appendix Table A2.

4.1 The Relative Importance of Workplace Rank

There is extensive evidence of large disparities in health outcomes associated with socioeconomic factors like earnings and education. For example, Chetty et al. (2016) estimates that the gap in average life expectancy between people in the top and bottom 1% of household earnings in the US is over 12 years. The gap in average life expectancy by education is also large. A four-year college degree graduate has six years greater life expectancy than those without a degree (Case and Deaton, 2021). Large gaps in a wide range of health status measures and disease prevalence precede both of these disparities in mortality (Bhattacharya et al., 2023; Kennedy et al., 2007; Wagstaff and Van Doorslaer, 2000; Pickett and Wilkinson, 2015; Marmot, 2003; Molina, 2016; Cutler and Lleras-Muney, 2006; Grossman, 2006; Case and Deaton, 2021).

Although far less is known about the association between health and workplace rank in the US, it is worth asking first whether such an association could possibly be of first-order importance in explaining health disparities relative to the enormous gaps associated with earnings levels and education. In this section, we show that the strength of the association between workplace rank and health status is not only substantial—it is stronger than the association between earnings and health

[18] These measures are average marginal impacts in the sense that the magnitude of the effect of adding controls is order-dependent, so we calculate the marginal effects for every permutation of orderings and present the average marginal effect across all permutations. Consequently, the sum of the average marginal effects may differ slightly from the difference between the baseline and conditional gaps.

status.

Figure 3: Comparing Health Gradients: Within-Firm Rank vs. Earnings Percentile

Notes: This figure shows the joint distribution of the number of chronic conditions workers have been diagnosed with and their percentile earnings rank within their primary employer (the solid dots) and their percentile in the distribution of total household earnings (the hollow diamonds). Each dot is the conditional mean chronic condition count in a given bin of within-firm ranks (or total earnings), controlling for fixed effects for each percentile of household earnings levels (or within-firm ranks), age × zip code fixed effects, gender, year, plan ID fixed effects, average firm size, and average tenure within a firm. Conditional means are constructed using the semiparametric partial linear regression approach developed in Cattaneo et al. (2023).

In Figure 3 we pool the four demographic groups together and show the semiparametric relationships between chronic condition prevalence and percentiles of the total household earnings distribution (hollow diamonds) and percentiles of the within-firm rank distribution (solid circles), conditional on the same controls as Figure 2. To parallel the design of Figure 2, when estimating the rank-health relationship we control for household earnings percentile effects, and when estimating the income-health relationship we control for rank percentile effects.

Figure 3 shows that the conditional rank-health gradient is clearly steeper than the household earnings-health gradient for most of the distribution, with the exception of the extreme upper tail of the earnings distribution. The conditional 90-10 gap in chronic disease prevalence by earnings is 0.035, while the 90-10 rank-health gap is 0.204.

If we estimate the same income-health relationship without controlling for within-firm rank the 90-10 gap in chronic disease prevalence by total earnings is 0.115. This suggests that, by this measure of health status, 70% (0.08/0.115) of the conditional relationship between earnings and health operates through the relationship between earnings and workplace rank. This statistic is of course influenced by the control variables included in the model. If only age controls are included in the model, we estimate that the 90-10 gap in chronic disease prevalence by total earnings falls by 77%, from 0.142 to 0.033, when within-firm rank is added to the model. In Appendix Figure A2 we show the earnings-health relationships separately for each racial group. We find that the income-health gradient is flatter for all racial groups.

Of course, this evidence does not imply that earnings levels are not an important determinant of health status. Part of the well-documented association between health status and earnings includes the impact of workplace rank on health because rank and earnings are correlated. Specifically, in our analysis sample 50.4% of the variance in workers' within-firm percentile ranks is explained by their overall individual earnings percentile rank. Figure A2 is suggestive that a substantial portion of the well-documented relationship between earnings and health status is contained within the portion of earnings variation that is explained by workplace rank.

This finding can help improve the understanding of why there are such large differences in the literature between conditional income-health correlations, which consistently show large income-health gradients, and experimental or quasi-experimental evidence that shows no causal effects of income on physical health, or even negative effects (Cutler et al., 2011; Miller et al., 2024; Snyder and Evans, 2006; Ruhm, 2000, 2005).

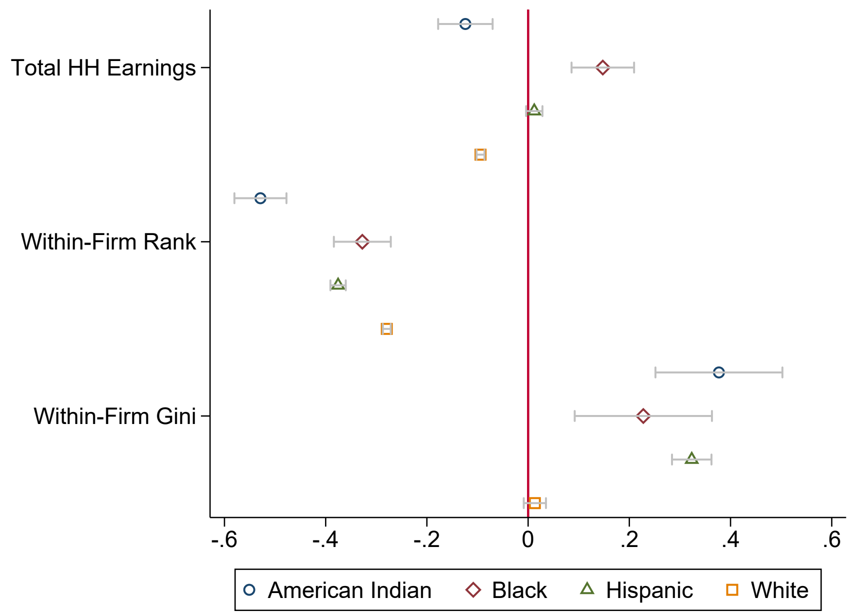
To corroborate this finding in yet another way, we run a horse-race style regression to directly compare the relative explanatory power of earnings and workplace rank within the same model. We estimate a fixed effects specification that includes the same set of control variables from the descriptive semiparametric analyses above:

$$y_{it} = \sum_r \beta_{1r(i)} c_{it} + \sum_r \beta_{2r(i)} c_{it}^2 + \gamma x_{it} + \theta_{r(i)} + \theta_{a(i,t)z(i,t)} + \theta_{p(i,t)} + \varepsilon_{it} \tag{1}$$

in addition to interactions between race indicators and a vector c_{it} of socioeconomic variables including percentiles of the household earnings distribution, within-firm earnings percentile rank, and within-firm inequality measured using the Gini coefficient. $\beta_{1r(i)}$ is the corresponding vector of race-specific coefficients (on the linear c_{it} term). Our interest is in understanding the relative importance of these socioeconomic factors in explaining chronic disease prevalence.

Figure 4 shows the estimated average marginal effects of each element of c_{it} on y_{it}, by race. The

Figure 4: Strength of Relationships between Work Outcomes and Health Status, by Race



Notes: The figure plots the estimated average marginal effects of each outcome by race from Equation 1. Bands show the 95% confidence intervals. In the regression, we also control for age × zip code, gender, year, and plan ID fixed effects, average firm size, and average tenure within a firm.

average marginal effect of c_{it} in Equation 1 is equal to $\beta_{1r} + 2 * \beta_{2r}\overline{c_{it}}$.[19] The coefficients on earnings percentiles are scaled to represent the marginal effect of moving between the lowest and highest percentiles of the corresponding distribution. The figure shows that the conditional relationship between household earnings and health is mixed and heterogeneous across racial and ethnic groups. For American Indian and White workers, those in the top percentile of the household earnings distribution have, respectively, 0.124 (10%) and 0.094 (7.6%) fewer chronic conditions than those in the bottom percentile. The relationship has the opposite sign for Black workers, with workers at the top of the distribution having 0.148 (11.9%) more chronic conditions. Consistent with the literature on the "Hispanic paradox," we find a very small and statistically insignificant relationship between income and health for Hispanic workers, 0.012 (1%).[20] This mixed evidence is perhaps unsurprising given the evidence discussed in Cutler et al. (2011) that the direction of causality from income to health is 'far from clear,' and recent evidence from an RCT of guaranteed income finding little to no causal effect on health status (Miller et al., 2024).

In contrast, the coefficients on within-firm percentile ranks show clear and consistent patterns that are substantially larger in magnitude. In this model, we estimate that moving from the top to the

[19]Note that in this model the marginal effect at the means is equal to the average marginal effect.

[20]The full set of coefficients and standard errors are reported in Appendix Table A3.

bottom of the within-firm earnings distribution, conditional on earnings levels and other controls, is predicted to increase the number of chronic conditions by between 0.529 (43%) for American Indian workers to 0.279 (23%) for White workers.

We also show the relationships between workplace earnings inequality and health status, although this is not a central focus of the current paper. Here we find important differences between the impacts of inequality on health for White workers relative to all minority groups. The average marginal effect of an increase in the firm-level Gini coefficient from 0 (perfect equality) to 1 (perfect inequality) increases the number of chronic conditions by 0.377 (30%), 0.323 (26%), and 0.228 (18%) for American Indian, Hispanic, and Black workers, respectively. In contrast, we find a small and statistically insignificant relationship for White workers, 0.013 (1%).

4.2 Which Health Conditions Contribute to the Rank-Health Gradient?

In the preceding analyses we focus on the number of diagnosed chronic conditions as our primary measure of health status. This aggregate measure has the potential to mask the outsized influence of a subset of conditions that may be driving the observed patterns. Disaggregating the analysis by medical condition could also be useful for ruling out some of the potential mechanisms through which workplace social status might influence health status.

In Table 2 we show that the negative relationship between workplace rank and health status holds for nearly all of the 18 chronic medical conditions we study. In column 1 we report the 90-10 disease prevalence gap by chronic condition in levels. To compare the relative sizes of the gaps, accounting for differences in baseline population disease prevalence by condition, in Column 2 we report the ratio of the average prevalence for workers at the 90th percentile divided by the prevalence at the 10th percentile. The rows in the table are ranked according to this ratio. All of the estimates in the table are based on analyses pooled across all races and ethnicities. While the 90-10 prevalence ratio exceeds one for 17 of the 18 conditions, there is substantial heterogeneity in the ratio. Half of the conditions have ratios exceeding 1.25, while 4 have ratios below 1.10.

The patterns of differences in disease rates by workplace rank are even more striking when visualized. In Figure 5 we show the relationships between disease rates and rank for a sample of diseases using the same method as in Figure 2, controlling for fixed household earnings percentile effects, age-by-zip code fixed effects, gender, year, plan ID fixed effects, average firm size, and average tenure within a firm. In Appendix Figure 1 we show the full set of figures for all 18 diseases.

Table 2: Within-Firm 90-10 Percentile Gap by Chronic Condition

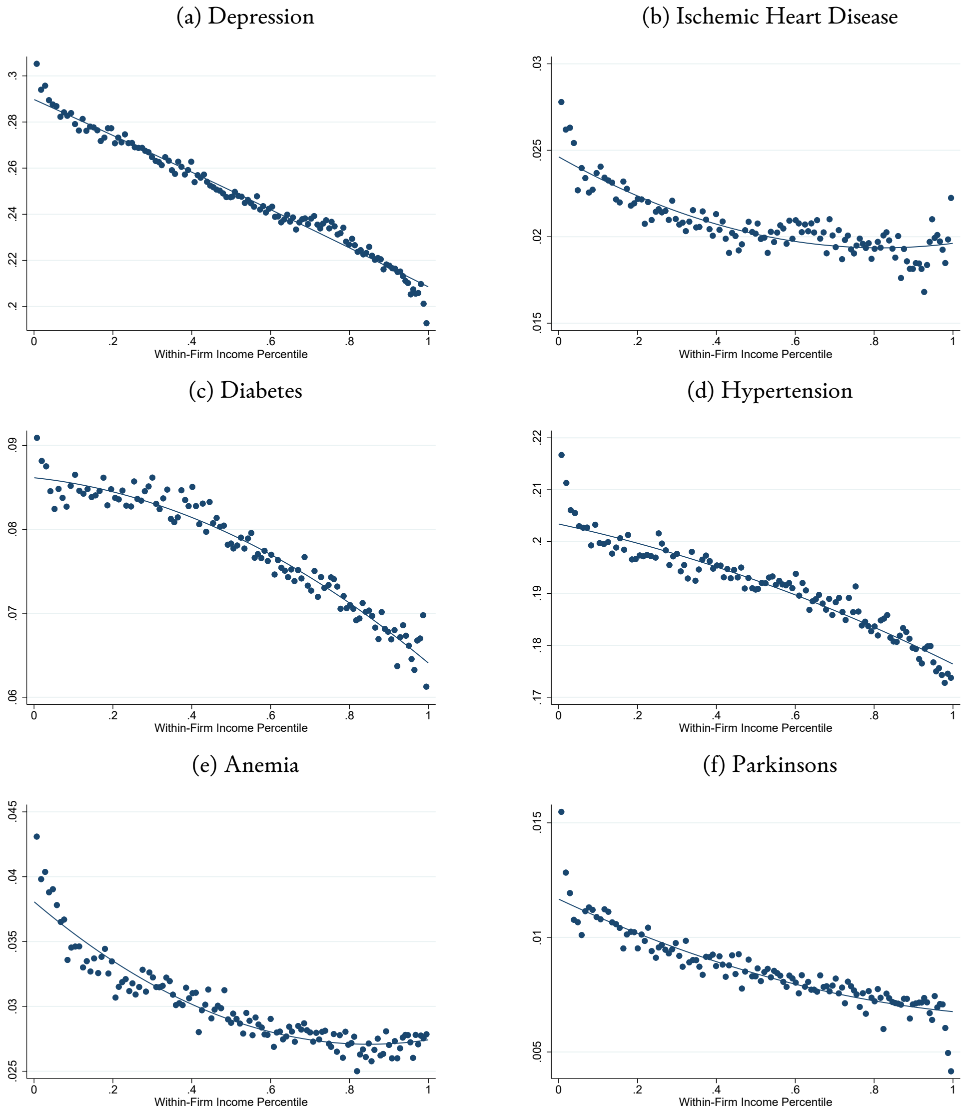
	$p_{10} - p_{90}$	p_{10}/p_{90}
Schizophrenia	0.004	2.456
Bipolar	0.010	1.860
Congestive Heart Failure	0.002	1.712
Parkinsons	0.004	1.564
Seizures	0.018	1.557
Chronic Obstructive Pulmonary Disease	0.006	1.328
Anemia	0.009	1.314
Depression	0.065	1.300
Diabetes	0.018	1.261
Ischemic Heart Disease	0.004	1.206
Arthritis	0.002	1.189
Hyperthyroidism	0.013	1.156
Hypertension	0.022	1.120
Lower Back Pain	0.023	1.111
Osteoporosis	0.001	1.090
Macular Degeneration	0.000	1.040
Asthma	0.003	1.017
Glaucoma	-0.001	0.945

Notes: For each disease we estimate the semiparametric partial linear regression model from Cattaneo et al. (2023), similar to Figure 2, and control for household income percentile effects, age × zip code effects, gender, year, and plan ID effects, average firm size, and average tenure within a firm. We obtain the 10th and 90th percentiles from the quadratic fitted polynomials and calculate their differences and ratios.

These results help corroborate the breadth of the effects of workplace social position on health status. As discussed in Section 2.1, the hypothesized mechanism behind such broad-based effects on chronic disease rates is that exposure to chronic stress leads to immunosuppression that accelerates the onset of a wide range of medical conditions. Psychologists and economists have also documented that self-reported stress levels are higher among people with lower incomes, lower education, and other dimensions of low SES (APA, 2017; Baum et al., 1999).

In Appendix Figure A15 we provide complementary evidence on differences in healthcare utilization patterns by workplace rank. Consistent with lower-ranked workers having higher rates of morbidity, we also find a strong negative relationship between workplace rank and the number of emergency room (ER) visits, inpatient hospital stays, and professional office visit relative value units (RVUs are a measure of resource-based utilization intensity). In contrast, there is a slightly positive relationship between workplace rank and preventative office visits, and no clear evidence of a difference in the use of other preventative care like vaccinations (flu, HPV, Hepatitis A, and Hepatitis B) between high versus low-ranked workers.

Figure 5: Diagnosis Rates by Condition vs Within-Firm Income Percentile
Full Set of Controls



Notes: This figure shows the joint distribution of chronic condition prevalence and workers' percentile earnings rank within their primary employer. Each dot is the conditional mean chronic condition rate in a given bin of within-firm ranks, controlling for fixed household earnings percentile effects, age × zip code fixed effects, gender, year, plan ID fixed effects, average firm size, and average tenure within a firm. The conditional means are constructed using the semiparametric partial linear regression approach developed in Cattaneo et al. (2023).

4.3 Panel Evidence from Job-Movers

The preceding evidence comes from comparisons across individuals. Although these models control for a wide set of observed and unobserved factors that might influence health status outside of the workplace rank channel, it is also possible to examine short-term changes in health status within individuals before and after moving to jobs with higher or lower ranks. These analyses help demonstrate that the relationship we document between workplace rank and health cannot be explained by static unobserved factors that differ across individuals, such as education or early-life health status.

We use the sample of workers who changed jobs exactly one time during the panel period. To ensure we have pre- and post-move data, we also restrict the sample to workers who moved between Q3 2013 and Q4 2014. To restrict the identifying variation to include only the within-person and between-job components, we calculate the average rank on the origin job, prior to the move, the average rank on the destination job, following the move, and the change in average ranks between jobs. We then estimate the following model:

$$y_{it} = \beta \Delta wp_i * t + \theta_i + \theta_{e(i,t)} + \theta_t + \varepsilon_{it} \tag{2}$$

where $\Delta wp_i * t$ is the change in average rank across jobs, which is static within workers in this sample, interacted with a linear quarterly time trend. β is the conditional rate at which y_{it} changes per quarter of exposure to the change in rank. θ_i is a fixed worker effect, $\theta_{e(i,t)}$ contains fixed effects for each percentile of household income, and θ_t is a quarter-year effect.

Table 3: Within-Person Changes in Rank around Job Moves

	Base controls (1)	Full controls (2)	Donut hole (3)	Symmetry (4)
$\Delta wp \times t$	-0.021	-0.021	-0.018	-0.019
	(0.005)	(0.007)	(0.006)	(0.008)
$\Delta wp > 0$				-0.002
				(0.004)
Person FEs	Yes	Yes	Yes	Yes
Base controls	Yes	Yes	Yes	Yes
Full controls	No	Yes	No	No
Observations	234,991	202,119	129,802	234,991

Notes: Column 1 reports results from Equation 2. Column 2 adds fixed employer, zip code, and health insurance policy effects. Column 3 excludes the quarter of the job change and the two quarters before and after. Column 4 adds an interaction term between $\Delta wp_i * t$ and an indicator that equals 1 if the change in rank is positive. Standard errors are clustered at the individual level.

Estimates from this model are shown in Table 3. In column 1 we estimate $\widehat{\beta} = -0.021$, which implies that the rate at which chronic conditions increase with age slows by 0.021 per quarter when workers move from the lowest-ranked job to the highest-ranked job. Of course, there are no job changes in the data that are this extreme (the average job change is 0.21 percentile points in absolute value), but this is a convenient way of scaling the estimates that is consistent with the scaling in the main results. In column 2 we show that $\widehat{\beta}$ is similar if we also include fixed employer effects, zip code effects, and health insurance policy effects.

Handel et al. (2024) show evidence that there are disruption effects in healthcare utilization around the time of job changes. To remove the potential impact of these disruptions, we also estimate a model that is similar to a long-difference specification, in which we drop quarters shortly before and after the job change and use the remaining quarters to estimate the effect, similar to a donut hole specification. Column 3 shows that the estimates are robust to this disruption effect: omitting the quarter of the job change and the two quarters before and after decreases $\widehat{\beta}$ slightly to -0.018.

In column 4 we add an interaction term between $\Delta wp_i * t$ and an indicator that equals 1 if the change in rank is positive. The base $\widehat{\beta}$ remains similar, -0.019, and the interaction term (-0.002) is small and insignificant, suggesting that there is no evidence of an asymmetric effect of moving to higher-ranked jobs relative to moving to lower-ranked jobs.

Although we have a short panel relative to the generally slow-moving evolution of chronic disease counts, a stock variable, we also estimate an event study model to document the immediate-term dynamics of y_{it} around these job changes using the same sample.

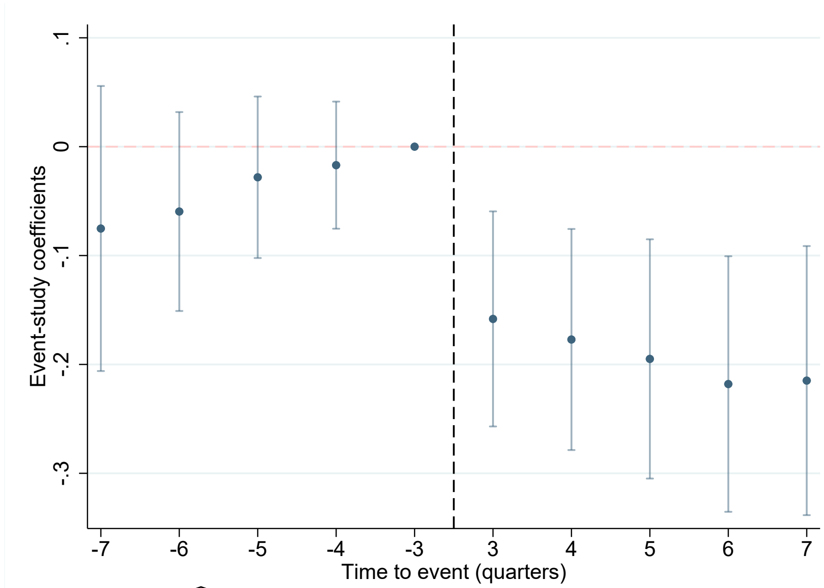
$$y_{it} = \sum_{k=-7}^{-3} \beta_k \Delta wp_i + \sum_{k=3}^{7} \beta_k \Delta wp_i + \theta_i + \theta_{e(i,t)} + \theta_t + \varepsilon_{it} \tag{3}$$

Following the specification in column 3 of Table 3, Equation 3 is a long-difference (or donut hole) specification that excludes the impacts of utilization disruptions in the quarters immediately around the job change. Specifically, we exclude data from the quarter of the job change and the two quarters before and after the change, and normalize the event time coefficient to zero in period $k = -3$.

The estimates, shown in Figure 6, indicate a modest upward trend in chronic conditions between the seventh to third quarters before the job move. After the job change, this trend reverses sharply. By quarter 3 the coefficient $\widehat{\beta_3} = -0.16$. For an average upward job change, which has a change in rank of 0.21, the worker is predicted to have 0.03 (0.03 = 0.16 * 0.21) fewer chronic conditions than would otherwise have been predicted given the passage of time. The estimates increase gradually over time, and by quarter 7 the coefficient $\widehat{\beta_7} = 0.215$.

Equation 3 uses all of the intensive margin changes in rank from both positive and negative changes in rank to estimate β_k. We can also estimate models using binary indicators for large positive or negative changes in rank. In Figure 7, we show estimates of $\widehat{\beta_k}$ from Equation 3 in which Δwp_i

Figure 6: Event Study Estimates around Job Moves



Notes: This figure shows estimates of $\widehat{\beta_k}$ from Equation 3. Standard errors are clustered at the individual level.

is replaced with a binary indicator for changes in rank that exceed 0.25 in absolute value.

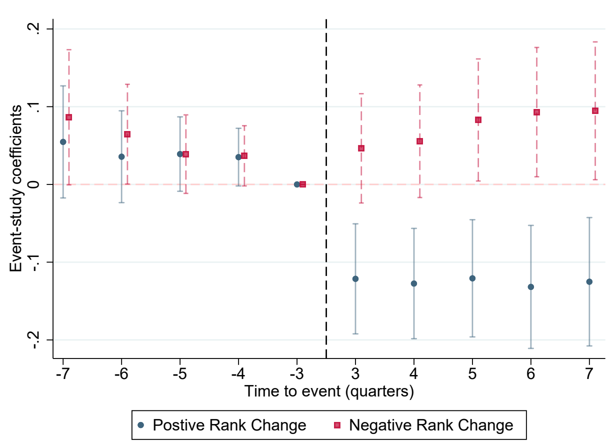
Figure 7a shows estimates separately by the direction of the change in rank. We find a clear divergence in y_{it} between workers who move to lower-ranked jobs relative to those who move to higher-ranked jobs, despite very similar trends for the two groups prior to moving. For workers who move to lower-ranked jobs, the slight downward trend in the pre-period reverses after the move, and $\widehat{\beta_k}$ increases gradually over time, contributing to an expanding gap between the two groups.

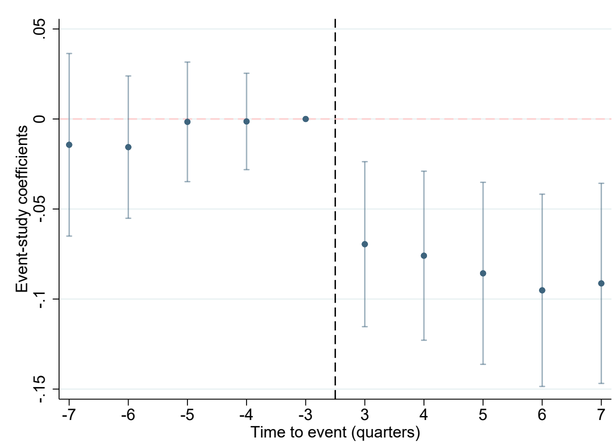
In Figure 7b we combine the binary positive and negative changes so that Δwp_i takes values -1, 0, or 1. The estimates show large declines in y_{it} for upward movers, relative to a counterfactual in which y_{it} increases gradually with age. In quarter 3 following the move, the coefficient $\widehat{\beta_3} = -0.07$. This corresponds to the effect for an average mover. Conditional on the move exceeding 0.25 percentile points in rank, the average change in rank is 0.42 percentile points. Dividing $\widehat{\beta_3}$ by 0.42 gives -0.167, which is consistent with the intensive margin estimate of $\widehat{\beta_3}$ from Figure 6. In Appendix Figures A3 and A4, respectively, we show that the estimates are similar using the imputation method from Borusyak et al. (2024) and without the donut hole.

5 Firm Rank and Health Production over the Lifecycle

In this section, we ask: at what stage in the life-cycle does the relationship between workplace rank and health status become evident? The literature on social determinants of health suggests that health

Figure 7: Event Study Estimates around Job Moves:
Binary Model of Changes > 0.25





Notes: This figure shows estimates of $\widehat{\beta_k}$ from Equation 3 in which Δwp_i is replaced with a binary indicator for change in rank that exceed 0.25 in absolute value. Figure 7a shows estimates separately by the direction of change in rank. Figure 7b combines positive and negative changes. Standard errors are clustered at the individual level.

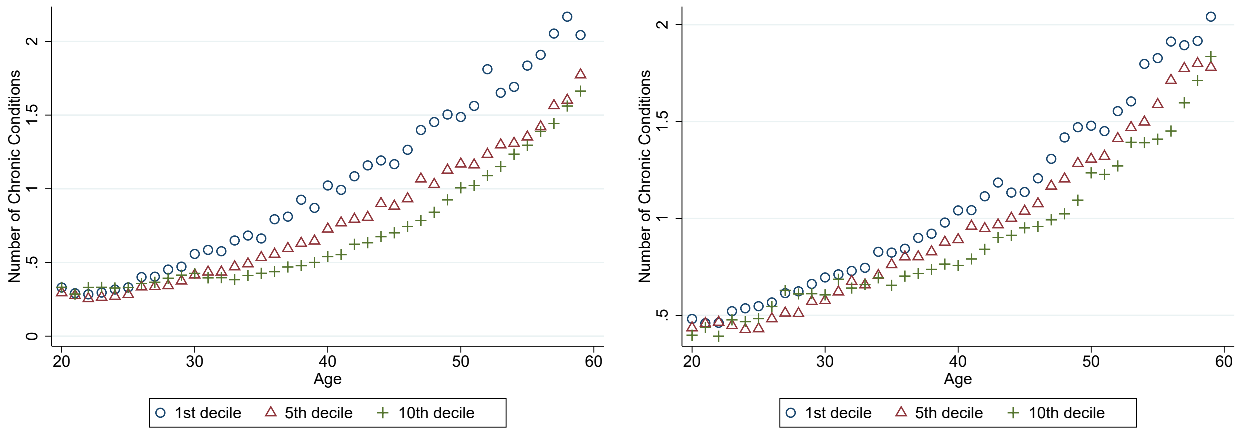
effects occur over prolonged exposure to disparities in social status. This implies that one should expect to see a widening gap in health status over the lifecycle for workers at different points in the rank distribution. Alternatively, a gap in health status by workplace rank among workers in their 20s who are just entering the labor force could potentially indicate a form of labor market selection in which health affects workplace rank via job assignment, even after conditioning on earnings.

Figure 8 shows the life-cycle health production patterns of workers at different deciles of the workplace rank distribution.[21] For male workers in Figure 8a, the average number of chronic conditions is approximately the same for workers in their 20s at different deciles of workplace rank. The disparity in health status grows sharply during a worker's 30s, and a clear ordered relationship between workplace rank decile and chronic conditions becomes evident. In the bottom portion of the workplace rank distribution, the gap between the 1st and 5th deciles emerges in a worker's early 30s and continues growing through their 50s. In the top portion of the distribution, the health of workers in the 10th and 5th deciles begins diverging in the mid-30s and continues diverging through the 40s, before narrowing by the time a worker reaches age 60. An alternative way of expressing the disparities in the lifecycle evolution of health is that it takes a middle-aged male worker about 11 additional years to reach one expected chronic condition if they work in the top decile of workplace

[21]The figures control for average firm tenure, log firm size, and fixed household earnings percentile effects, year, zip code, and policy ID effects.

Figure 8: Evolution of Chronic Conditions by Age across Firm Ranks

(a) Men (b) Women



Notes: The figures show the life-cycle health production patterns of workers at different deciles of the workplace rank distribution for men and women, separately. Each dot is a conditional mean estimated by regressing the number of chronic conditions on fixed household income percentile effects, zip code effects, year, plan ID effects, average firm size, and average tenure within a firm, and then recentering the residuals around the mean chronic condition count.

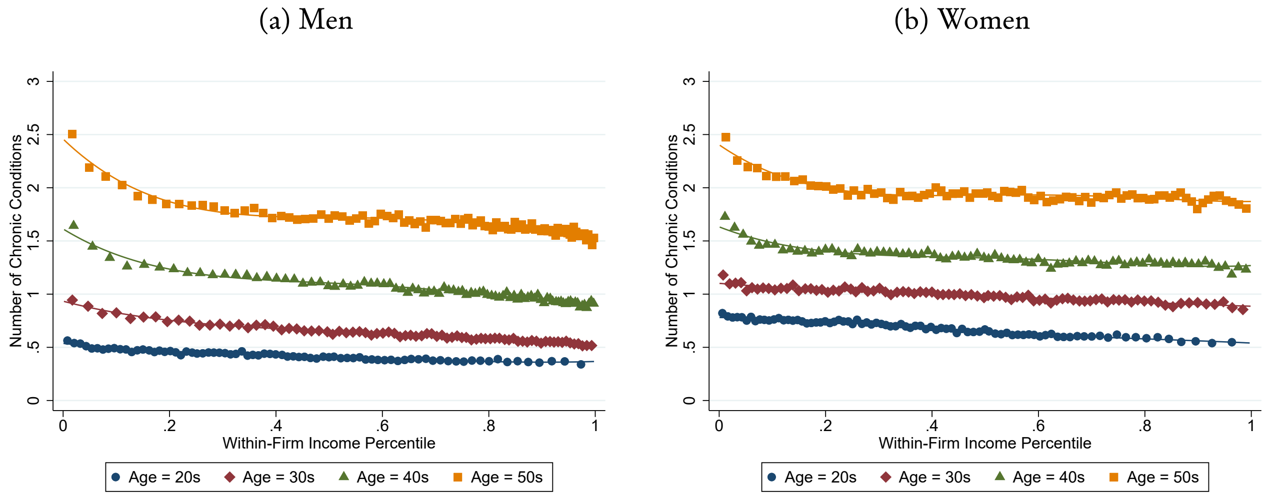
rank, relative to a man with the same covariates who works in the bottom decile of workplace rank. This 11-year gap is comparable in magnitude to the 14.6-year male life expectancy gap across earnings quantiles documented by Chetty et al. (2016). Because minority workers disproportionately hold lower-ranked jobs, this gap in age-morbidity profiles contributes to racial health disparities, as we discuss in Section 8.[22]

For women (Figure 8b), there is slightly more variation in health status by rank among young workers in their mid to late 20s, though the differences are not monotonic in rank. By the mid-30s a clear, ordered relationship emerges and continues expanding through workers' 50s. The gap in health status between the top and bottom of the workplace rank distribution is smaller for women than it is for men.

In Figure 9 we show the semiparametric conditional means across the distribution of workplace rank for each decade of age. Again we find a very little relationship between within-firm rank and health status among young men in their 20s. The rank-health gradient increases through exposure to workplace rank during workers' careers. Among young women in their 20s (Figure 9b), there is again some evidence of a relationship between chronic conditions and within-firm rank. This contributes to a somewhat weaker expansion of the health gap by workplace rank among women

[22] Russo et al. (2024) estimate that at age 55, Black men and women have frailty levels that are similar to White men and women 13 and 20 years older, respectively.

Figure 9: Number of Chronic Conditions vs Within-Firm Income Percentile
By Age in Decades, By Gender



Notes: This figure shows estimates that are constructed similarly to Figure 2 by age categories, including controls for fixed household income percentile effects, age × zip code effects, year, plan ID effects, average firm size, and average tenure within a firm. Fitted lines are 4th-order polynomials.

relative to men.

In Figure 9, the gap in chronic conditions for men at the 10th percentile of workplace rank relative to those at the 90th percentile increases from 0.129 among workers in their 20s, to 0.274 in the 30s, 0.428 in the 40s, and 0.482 in the 50s. For women, the same gaps are 0.205, 0.164, 0.221, and 0.259.[23]

6 Model of Exposure to Workplace Social Status

Interpreting this descriptive evidence is challenging because systematic variation in rank and health could be driven by selection in the types of workers assigned to different ranks. In this section, we consider a model of exposure over workers' careers to different workplace rank treatments. We use this model to characterize the conditional effects on health status of exposure to treatment for a given duration using a movers design similar to Chetty and Hendren (2018). We then describe the research design used to estimate these effects, results, and diagnostic tests to support the identification assumptions. Finally, we describe a corroborative IV-based estimation approach along with findings.

[23]In Appendix Figure A7 we show similar figures separately for each race and gender combination. To more precisely visualize the slopes of these relationships, in Appendix Figure A5 we show the average marginal effect of workplace rank on health by gender, race, and quintile of the workplace rank distribution.

6.1 Variation in Health Status among Job Stayers

Following the focus on chronic health conditions in the preceding sections, we consider a measure of health status y_{it} that deteriorates with age, where a higher y_{it} indicates worse health status. Our interest is in estimating how the accumulation of exposure to a given quantile of workplace rank wp_{it} over a time period impacts health status deterioration. We begin by considering the model:

$$y_{it} = \beta_r wp_{it} + \theta_{e(i,t)} + \theta_{r(i)z(i,t)c(i,t)} + \psi_{j(i,t)} + \theta_{p(i,t)} + \theta_t + \theta_{g(i)} + \varepsilon_{it} \tag{4}$$

where wp_{it} is worker i's within-firm percentile rank in quarter t, with race-specific coefficient β_r. $\theta_{e(i,t)}$ is a fixed effect for each percentile of the household earnings distribution, $\theta_{r(i)z(i,t)c(i,t)}$ is a fixed race by zip code by cohort (year of birth) effect, $\psi_{j(i,t)}$ is a fixed employer effect, where $j(i,t)$ indexes firm j at which worker i is employed in quarter t. $\theta_{p(i,t)}$, θ_t, and $\theta_{g(i)}$ are fixed health insurance plan contract, year, and sex effects, respectively.

We estimate Equation 4 on the set of job stayers who work at the same employer throughout the entire panel, and who remain at a similar workplace rank within the firm such that $wp_{it}^{max} - wp_{it}^{min} < k$, where k is a cutoff threshold. Because the model includes $\psi_{j(i,t)}$, the error term is mean zero for each employer, allowing for the possibility that workers with different health statuses sort nonrandomly into jobs at different employers and, in this sample, stay there.

Using this model we estimate the predicted component of the variation in y_{it} explained by $\widehat{\beta}_r wp_{it}$. This prediction, which we define as $\overline{y}_{rwp}$ varies by race and by percentile of the wp distribution. Figure 10a presents estimates of $\widehat{\beta}_r$ from Equation 4. The estimated coefficients imply that among job stayers, American Indian workers at the bottom percentile of the wp distribution have 0.79 more chronic conditions than those at the top of the wp distribution. The corresponding gaps for Hispanic and White workers are smaller, 0.37 and 0.30, respectively. The estimates for Black and American Indian workers in this job-stayer subsample have large standard errors, and we cannot reject that the estimate is significantly different from zero for Black workers.

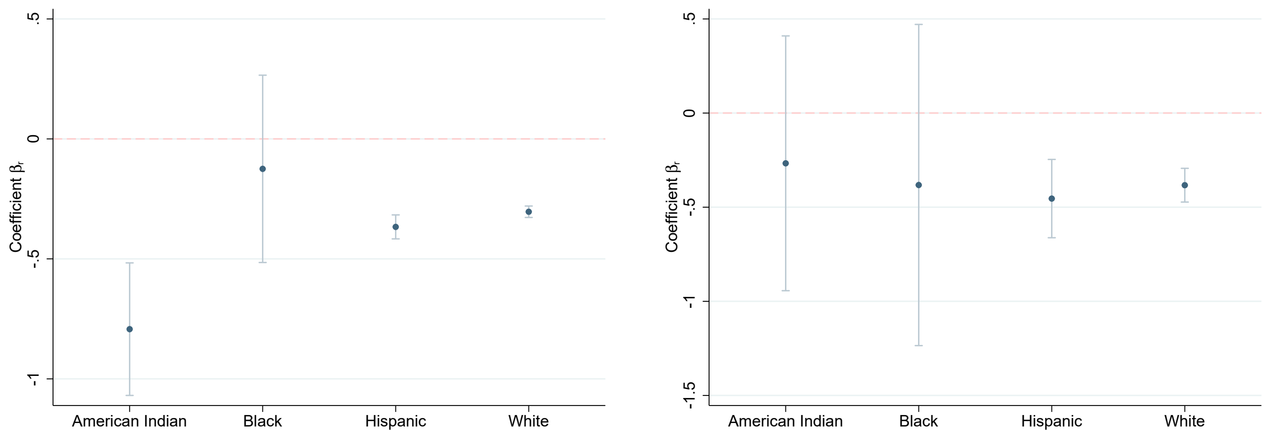
In Figure 10b we present estimates from a version of Equation 4 that also includes fixed person effects[24], so that all of the identifying variation comes from differences in the rates of within-person

[24] We omit person effects from the main specification for several reasons. First, the person effects model becomes conceptually challenging when comparing outcomes of movers and stayers, which is the primary aim of this model. Handel et al. (2024) show using the same data that when workers change jobs there is a short-term disruption to their healthcare utilization. This disruption complicates the timing of the measurement of chronic conditions for movers relative to stayers, and the person effects model is identified solely based on this within-person timing. Given the relatively short panel length in this sample, the person effects model is also somewhat imprecise, especially for the

Figure 10: Estimates of $\widehat{\beta_r}$ from Equation 4

(a) Main Estimates, Equation 4

(b) Equation 4 with Fixed Person Effects



Notes: These figures show estimates of $\widehat{\beta_r}$ from Equation 4 without (10a) and with (10b) fixed person effects. Estimates are based on the sample of job stayers at the same firms for whom $wp_{it}^{max} - wp_{it}^{min} < 0.15$ during the three-year panel spanning 2013-2015.

changes in y_{it} over time for workers at different ranks. These estimates are similar for White and Hispanic workers, and for each group the 95% confidence interval contains the corresponding group point estimate from Figure 10a. This suggests that static unobserved person-level differences, such as education or early childhood health, that are not already absorbed by the included controls are not driving the overall qualitative patterns. In the model that includes person effects, the average $\widehat{\beta}$ pooling all race groups together is -0.39 (0.042).

6.2 Defining and Estimating Exposure Effects

Our next objective is to estimate how the health outcomes of a job mover would change if they were exposed to a level of treatment wp such that job stayers' average health outcomes y_{it} are one unit higher. To answer this question, we consider a sample of workers who move between jobs exactly one time. Using this sample, we estimate:

$$y_{it} = \alpha_m + \beta_m \overline{y}_{rwp} + \xi_{it} \tag{5}$$

where α_m is a common effect on health of moving to any different job at age m, and β_m captures the impact on y_{it} of moving at age m to a job such that the outcomes of job stayers predict that worker

smallest race groups. Nonetheless, we cannot reject that the estimates are different across the two specifications within each race group.

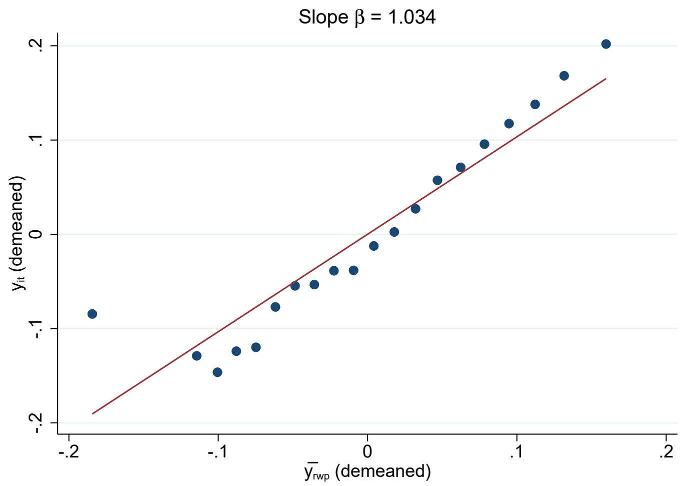
i would have one additional chronic condition due to exposure to wp_{it}. ξ_{it} is the error term, which captures all other person-level factors that affect health status.

Notice that if workers were randomly assigned to jobs, then ξ_{it} would be orthogonal to $\overline{y}_{rwp}$, and β_m would identify the mean impact of a permanent move at age m to a job with workplace rank wp. Of course, job assignment is unlikely to be random, which we discuss below. However, we first present estimates of β_m. The magnitude of β_m tells us the degree to which differences in health status by workplace rank are due to exposure to wp versus selection effects driven by nonrandom job mobility. If the health impact of exposure among job stayers is not predictive of health impacts among movers, $\beta_m = 0$ would imply the relationship between workplace rank and health status shown in Section 3 is driven by nonrandom assignment of workers into jobs with different levels of wp on the basis of the unmodelled component of health status. That is, it would suggest the descriptive patterns are due to selection rather than treatment effects. If $\beta_m = 1$, this would imply that all of the variation in health status reflects the treatment effect of exposure to workplace rank. In this case, in observational data the process of selection by which workers are reassigned to new jobs has no impact on the relationship between health status and wp, implying that job selection is conditionally unrelated to the error term in the health status model. Of course, other values are also possible. In general, β_m tells us how much of the difference in health status associated with wp among job stayers is imparted on job movers. If $0 < \beta_m < 1$ this suggests job movers are less affected than stayers. $\beta_m > 1$ is also possible, and this result could occur if ξ_{it} is systematically positive for low-ranked jobs and systematically negative for high-ranked jobs in the mover sample. This outcome could suggest that job mobility is endogenous to health status, with sicker workers systematically moving to lower-ranked jobs, and vice versa. We evaluate this possibility empirically in diagnostic tests of job mobility patterns below.

In Figure 11 we show the conditional joint distribution of y_{it} and $\overline{y}_{rwp}$, combining information about movers of all ages. This figure is constructed by demeaning y_{it} and $\overline{y}_{rwp}$ by fixed α_m effects and displaying a binscatter of the residuals, motivated by the Frisch-Waugh-Lovell theorem. The figure shows that the relationship between y_{it} and $\overline{y}_{rwp}$ is reasonably linear over most of the distribution and has an average slope of 1.034.

Estimates of $\widehat{\beta_m}$ from Equation 5 are shown in Figure 12. We find that $\widehat{\beta_m}$ is slightly below 1 for workers in their late 20s to early 30s, and continues increasing gradually with age, while remaining close to 1 throughout the prime-aged years of a worker's career. The modest differences between $\widehat{\beta_m}$ and 1 suggest that there are some potential impacts of nonrandom selection that may cause the

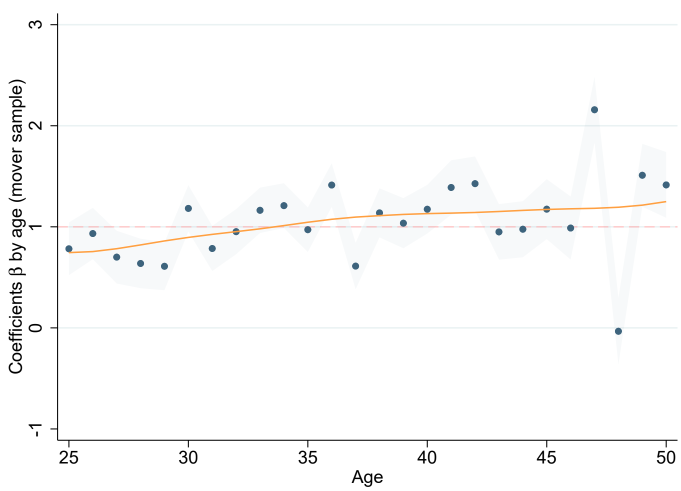
Figure 11: Conditional Joint Distribution of y_{it} and $\overline{y}_{rwp}$ among Movers



Notes: This figure shows the joint distribution of y_{it} and $\overline{y}_{rwp}$ conditional on age at move. Each dot is the conditional mean value of y_{it} for job movers in a given bin of the predicted value of $\overline{y}_{rwp}$, controlling for fixed age at move, α_m, effects. The conditional means are constructed using the semiparametric partial linear regression approach developed in Cattaneo et al. (2023).

patterns of job assignment of movers to differ from that of stayers. However, the overall conclusion from this analysis is that there are substantial treatment effects of exposure to workplace rank, and that movers experience a treatment effect that is comparable to the health differences by workplace rank of stayers.

Figure 12: Estimated $\widehat{\beta_m}$ from Equation 5



Notes: The dots in the figure are estimates of $\widehat{\beta_m}$ from Equation 5 for each age at move. The solid line is a smoothed local linear function of the age-specific estimates.

These estimates come from observational data, in which the error term ξ_{it} may be correlated with $\overline{y}_{rwp}$. For example, the literature on job lock suggests sicker workers have lower rates of job mobility in part because of stronger attachment to employer-provided health insurance. This would suggest that stayers have, on average, higher ξ_{it} than movers, creating a potential source of selection bias that leads β_m to be below 1.

Specifically, in observational data, estimates of $\widehat{\beta}_m$ are equal to:

$$\widehat{\beta}_m = \beta_m + \frac{cov(\xi_{it}, \overline{y}_{rwp})}{var(\overline{y}_{rwp})}$$

which is the sum of the treatment effect of interest and a selection effect.

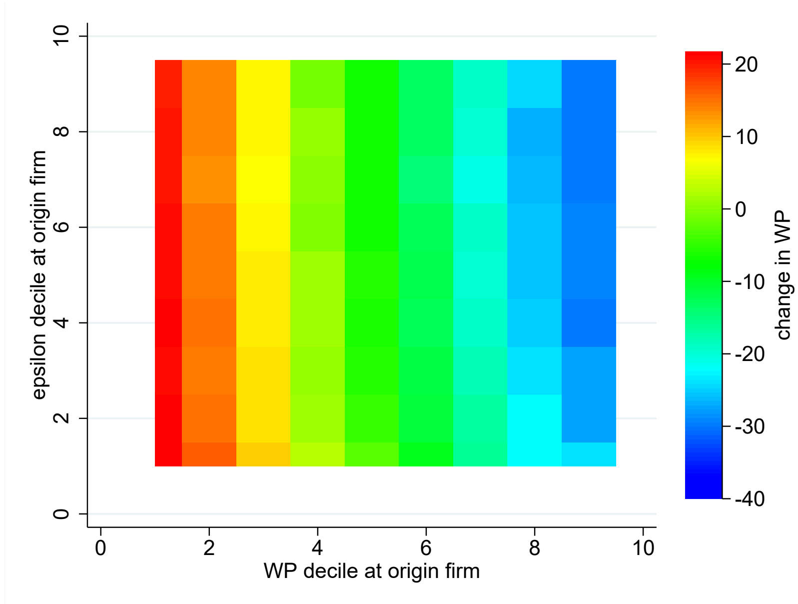
What do we know about this selection term? First, suppose it is reasonable to assume that $\frac{cov(\xi_{it}, \overline{y}_{rwp})}{var(\overline{y}_{rwp})}$ is the same on average for workers who have nearly the same age. That is, even if there is non-random sorting in the labor market that may be related to health, the health-based component of the sorting process is conditionally the same for a 35 year old as it is for a 36 year old. This allows for the possibility that people with chronic conditions may sort differently in the labor market, that older workers may sort differently, but assumes the marginal impact of a chronic condition on sorting does not vary within narrow age bands.

Under this assumption, we can use Equation 5 to estimate $\widehat{\beta_m} - \widehat{\beta_{m+1}} = \beta_m + \frac{cov(\xi_{it}, \overline{y}_{rwp})}{var(\overline{y}_{rwp})} - \beta_{m+1} - \frac{cov(\xi_{it}, \overline{y}_{rwp})}{var(\overline{y}_{rwp})} = \beta_m - \beta_{m+1}$. This assumption therefore implies that the slope of the age-based function in Figure 12 is locally unbiased.

We are also interested in understanding how potential selection effects impact the level of $\widehat{\beta_m}$. One way to assess the severity of nonrandom selection is to test whether there is a systematic pattern between workers' pre-move health status, $\widehat{\xi_{it}}$, and the change in wp_{it} associated with the job move, conditional on the origin job wp_{it-1}.

Figure 13 shows this empirical joint distribution. In the figure, the horizontal axis depicts deciles of the origin job wp_{it-1}, the vertical axis depicts deciles of the pre-move $\widehat{\xi_{it}}$, and each cell is color-coded by the magnitude of the change in workplace rank corresponding to the job move, $wp_{it} - wp_{it-1}$, where red cells indicate upward moves of 0.2 (20 percentile points), dark blue cells indicate downward moves of -0.4, and green-yellow cells indicate moves with no change in wp. If job mobility patterns were not associated with health status, we would expect to see that the colors of the heatmap are the same within each column of the figure, indicating that the magnitudes of changes in wp are uncorrelated with $\widehat{\xi_{it}}$ conditional on the origin job rank, wp_{it-1}. This is broadly consistent with the patterns shown in the figure—the colors are similar within each column, but vary across columns.

Figure 13: Change in Workplace Rank for Job Movers
by $\widehat{\xi_{it}}$ and Origin Job Rank



Notes: We recommend viewing this figure in color. It shows the empirical joint distribution of mobility probability given the within-firm rank deciles of the origin job and deciles of the pre-move health conditions, measured by $\widehat{\xi_{it}}$.

If sicker workers were disproportionately less likely to move into high-ranked jobs, we would expect to see smaller (or more negative) values in the top row of the figure (sickest workers) than we do in the bottom row (healthiest workers). There is little evidence of such systematic variation based on health status. The overall distribution provides diagnostic evidence suggestive that the selection component of the observational patterns of job mobility is likely to have little impact on $\widehat{\beta}_m$.

A second diagnostic test to quantify the impact of potential selection is to restrict the job-mover sample to only the pre-move period and estimate:

$$y_{i\tau-} = \beta^{-} wp_{i\tau-} + \beta^{+} wp_{i\tau} + \theta_{e(i\tau-)} + \theta_{r(i),z(i,\tau-),c(i)} + \psi_{j(i,\tau-)} + \theta_{p(i,\tau-)} + \theta_{\tau-} + \theta_{g(i)} + \varepsilon_{i\tau-} \quad (6)$$

where τ is the quarter of the job change and $\tau-$ refers to all prior quarters. β^{-} is the coefficient on workplace rank prior to changing jobs, and β^{+} measures the relationship between future workplace rank and pre-move health, $y_{i\tau-}$. Since this model is estimated using only pre-move data it is not possible for future rank to impact current health unless there is selection; $\widehat{\beta^{+}}$ is a direct measure of the selection effect.

Table 4 reports estimates from Equation 6 in Column 1. The estimated coefficient β^{+} on future rank is small and statistically insignificant. The coefficient on prior rank, β^{-}, is -0.298, consistent

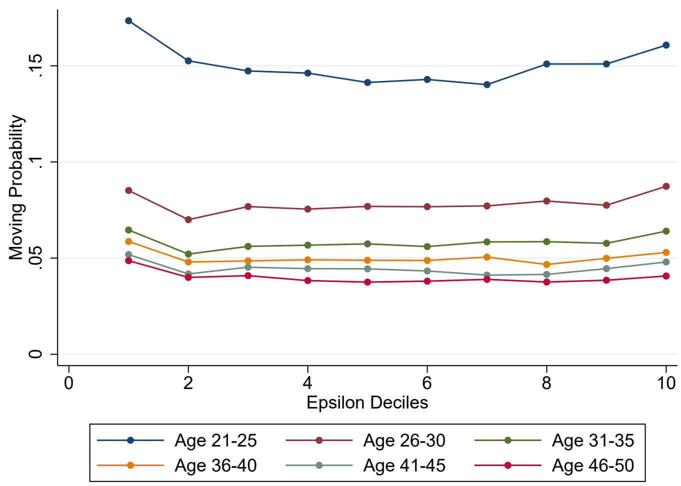
Table 4: A Test of Selection on Future Workplace Rank

	(1)	(2)
Prior Rank ($\widehat{\beta^-}$)	-0.298***	-0.275***
	(0.025)	(0.017)
Future Rank ($\widehat{\beta^+}$)	0.033	
	(0.027)	
Observations	244106	244106

Notes: The table shows the estimates from Equation 6, where we control for within-firm ranks and household earnings percentile effects, and also for fixed race $\times$ cohort $\times$ zipcode, gender, year, policy ID, and firm effects. In Column 1, we additionally control for the post-move future ranks.

with the broad evidence discussed in Section 4. Additionally, Column 2 shows that β^- remains similar when future workplace rank $wp_{i\tau}$ is excluded from the model, suggesting that the dimensions upon which workers sort when changing jobs are not related to the error term.

Figure 14: Job Mobility Rates by Age and Health Status



Notes: This figure shows the probability of moving across deciles of the distribution of the health status residual $\widehat{\varepsilon_{it}}$ from Equation 4, separately by 5-year age bands.

A final selection diagnostic is motivated by the potential for nonrandom sorting caused by job-lock. This could occur if sicker workers are less likely to change jobs because they depend more strongly on employer-provided health insurance. In Figure 14 we show that there is very little evidence of job mobility rates being correlated with residual health status. To construct this figure, we re-estimate Equation 4 using the full sample of job movers and stayers, estimate $\widehat{\varepsilon_{it}}$, and bin the residual estimates into deciles. We then plot the average (unconditional) probability of moving jobs by decile of $\widehat{\varepsilon_{it}}$ for each 5-year age band.

We find that within each age band the probability of changing jobs is very similar across deciles of $\widehat{\varepsilon_{it}}$. Of course, there are large differences in job mobility rates across age groups, particularly among very young workers (age 21-25). There is also slight evidence that the healthiest workers in the first decile of $\widehat{\varepsilon_{it}}$ are about 1-3 percentage points more likely to change jobs than those in the second decile, but the probability of changing jobs is largely flat across the rest of the distribution of $\widehat{\varepsilon_{it}}$.[25] This evidence also helps corroborate the assumption discussed above that within narrow age bands there appears to be very minimal evidence that job mobility is correlated with the error term ε_{it}, supporting the identification assumption discussed in Section 6.2.

6.3 Longer-Term Accumulation of Treatment Exposure

The preceding analyses are based on the main analysis sample. However, the earnings data go back further in time than our measure of chronic conditions, which makes it possible to also evaluate how the accumulation of exposure to workplace rank in earlier years affects health status measures in 2013-2015. By examining exposure to workplace rank over a longer time period, we can more effectively incorporate differences in both the duration and intensity of exposure to workplace rank to quantify how the health effects of treatment accumulate over time.

To do this, we again consider a comparison of job stayers and movers. We define long-term job stayers as those who remain employed at the same firm and remain at a similar rank within the firm (k=0.15) for a spell of at least seven consecutive years that occurs between 2006-2015. Using this job stayers sample we estimate the following model:

$$y_{iT} = \beta_c \sum_{s=T-28}^{T} wp_{is} + \eta_c \theta_{e(i,1 \ldots T)} + \theta_{r(i),z(i,T)c(i,T)} + \psi_{j(i,T)} + \theta_{p(i,T)} + \theta_t + \theta_{g(i)} + \varepsilon_{iT} \qquad (7)$$

where y_{iT} is health status at a terminal endpoint T, which is either the end of the job spell or the end of the sample window if the job spell continues beyond 2015. $\sum_{s=T-28}^{T} wp_{is}$ is the sum of workplace rank wp during the 28 quarters of the focal portion of the job spell. Since the sample window is fixed at 7 years for all workers, this captures the accumulation of exposure to workplace rank. β_c is a cohort-specific coefficient on the cumulative exposure measure, where cohorts are defined by 5-year bins based on year of birth. Analogously, we construct percentiles of the sum

[25] To be clear, this evidence is not necessarily contrary to the job-lock hypothesis because we focus only on the unexplained component of health status, while there could be non-random job mobility based on the explained variation in health status.

of household income during the same 7-year period, and include fixed effects for each percentile of this distribution, which are denoted by $\theta_{e(i,1...T)}$. η_c denotes a cohort-specific coefficient on each of the household income percentile effects. The interaction $\eta_c\theta_{e(i,1...T)}$ is incorporated by including cohort-by-income percentile effects. The remaining control variables are the same as Equation 4.

Estimates of β_c from Equation 7 capture the impacts of both the duration and intensity of treatment exposure, as well as how the effects of exposure vary by cohort. The estimates suggest that among older workers, born in 1952-56 cohorts, each quarter of exposure to the bottom percentile of workplace rank increases the expected number of chronic conditions by about 0.023, relative to a worker in the same cohort group who is exposed to the top percentile of workplace rank. In younger cohorts, the same exposure is estimated to have smaller impacts on health, ranging from 0.006 in the youngest cohort, 1977-81, to 0.018 in the 1962-66 cohort. The full set of estimates are shown in Appendix Figure A16. In general, the effects are larger in older cohorts, consistent with the age-based heterogeneity shown in Figure 9.

Using this model we estimate the predicted component of the variation in y_{iT} explained by $\widehat{\beta_c}\sum_{s=T-28}^{T} wp_{is}$ among long-term stayers. This prediction, which we denote by $\overline{y_{cwp}}$, varies by cohort of birth and by percentile of cumulative exposure to workplace rank. We then estimate how the health status of workers who moved jobs is related to that of long-term job stayers by estimating the best linear prediction of job movers outcomes based on the outcomes of long-term stayers:

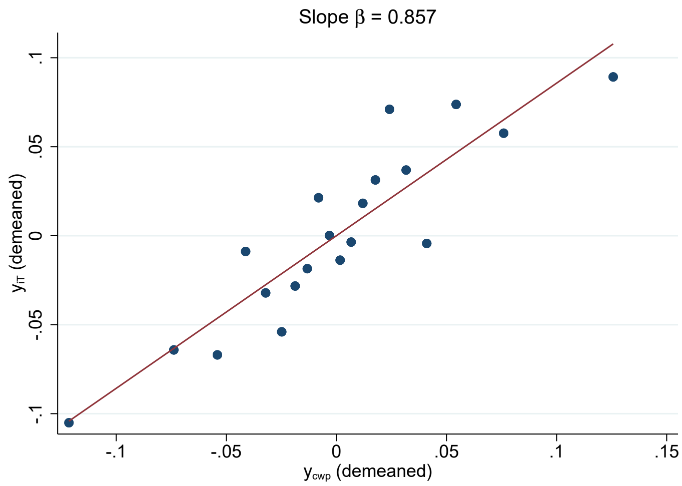
$$y_{iT} = \alpha_{cez} + \sum_{m=1}^{10} \beta_m \mathbb{I}\{m_i = m\}\ \overline{y_{cwp}} + \varepsilon_{iT} \tag{8}$$

where α_{cez} are fixed cohort by household income percentile by zip code effects. $\mathbb{I}\{m_i = m\}$ is an indicator function for the decile of the predicted exposure effect $\overline{y_{cwp}}$, which allows for the possibility of nonlinear heterogeneity in the relationship between $\overline{y_{cwp}}$ and y_{iT}.

Figure 15 shows the joint distribution of y_{it} and $\overline{y}_{rwp}$, conditional on α_{cez}. Consistent with Figure 11, we find that the long-term exposure effects of job movers are very similar to the predicted treatment effects from the long-term stayers sample. The aggregate relationship is reasonably linear and has an average slope of 0.86.

The estimates of $\widehat{\beta_m}$ from Equation 8 (which are shown in Appendix Figure A17) suggest that the treatment effect for job movers is slightly lower than the effect among long-term job stayers. In the lowest two deciles of $\overline{y}_{rwp}$, $\widehat{\beta_m}$ is about 0.5, while in the remaining deciles the ratio is between 0.71 to 1.0. The sample of workers who remain at the same job and similar workplace rank for 7

Figure 15: Conditional Joint Distribution of y_{iT} and $\overline{y_{cwp}}$ among Movers



Notes: This figure shows the joint distribution of y_{iT} and $\overline{y_{cwp}}$ conditional on fixed cohort by household income percentile by zip code effects. Each dot is the conditional mean value of y_{iT} for job movers in a given bin of the predicted value of $\overline{y_{cwp}}$, controlling for α_{cez} effects. The conditional means are constructed using the semiparametric partial linear regression approach developed in Cattaneo et al. (2023).

or more years is relatively small, however, so these estimates are fairly imprecise. We view these results as corroborative evidence that the conclusions from Section 6.1 remain similar when taking a longer-term perspective on the accumulation of exposure to workplace rank.

6.4 IV Estimates

The analyses in the preceding sections show that when workers move between jobs there is little evidence that changes in relative workplace rank, conditional on income and other controls, are correlated with unmodeled idiosyncratic differences in health status. The estimated exposure effect of workplace rank for job movers appears similar to that among stayers. However, there is some potential scope for the nonrandom assignment of workers to jobs, or job mobility patterns, to be correlated with the residual component of health status, creating endogeneity.

In this section, we describe the construction of instrumental variables that we use to further assess the potential impacts of endogeneity in workplace rank on our estimates. To motivate the construction of our instruments, note that one can decompose the variation in wp_{it} observed in the panel into three components: changes in wp_{it} that occur within worker-firm pairs over time, changes in wp_{it} caused by workers moving across firms, and a level component that could be measured at any point in time, for example an initial starting point wp_{i0} or the level that immediately precedes a job move. We consider three classes of instruments that capture all of these components, allowing us to use the total variation in workplace rank for identification.

The objective in constructing instruments is to find variation that is informative about components of the variation in wp_{it}, but conditionally uncorrelated with unmodeled differences in individual health status contained in ε_{it}. There are two key features of Equation 4 and the data generating process that motivate our construction of the instruments. The first feature is that Equation 4 contains a fixed employer effect, $\psi_{j(i,t)}$. This allows for the possibility that different employers arbitrarily select (or discriminate against) different sets of workers, potentially based on ε_{it}. The inclusion of $\psi_{j(i,t)}$ in the model makes ε_{it} mean zero for every firm in the sample. The second feature is that because wp_{it} is a within firm-quarter percentile, the distribution of wp_{it} is the same in every firm, in every quarter, and in every firm-quarter.

Promotion Rates

The first instrument builds upon the intuition developed by Anderson and Marmot (2012), who use differences in average job promotion rates by department of the British Civil Service in the Whitehall sample to instrument for workers' realized ranks. In our setting, instead of using variation across 20 departments, we construct firm-level instruments for the nearly 70,000 firms in our data. To do this, we estimate the following model using the full panel of earnings data for all workers between 2002 through 2019:

$$wp_{it} = \alpha_j^P + \rho_j^P tenure_{it} + \xi_{it}^P \tag{9}$$

where the superscript P refers to the promotion equation, α_j^P is a firm-specific intercept, ρ_j^P is a firm-specific coefficient that captures the average relationship between job tenure and workplace rank within jobs at firm j, and ξ_{it}^P is the residual. Firms with high average upward rank mobility have larger positive ρ_j^Ps, while firms with more stagnant ranks have ρ_j^P closer to zero.[26]

This constructed promotion instrument ρ_j^P is highly informative of changes in workplace rank within jobs over time (as we will show). But, because it is a firm-level statistic calculated from all workers over a long term, and because ε_{it} is mean zero in each firm, ρ_j^P is not likely to be correlated with any particular worker's idiosyncratic ε_{it}.

Job Mobility Network

In a sample of job movers who change jobs one time, the variation in wp_{it} consists of across-job

[26]We find that a small minority of firms (1.5% of total observations) have negative ρ_j^Ps, which could occur if these firms tend to hire externally for higher-ranked positions rather than promoting from within. For the construction of the instrument we excluded firms with negative ρ_j^P from the analysis, though the results are not sensitive to this decision.

variation from the job change, and within-job variation. One can write the total variation in wp_{it} for these job movers as:

$$wp_{it}^D - wp_{it}^O = wp_{i\tau+}^D + \left(wp_{it}^D - wp_{i\tau+}^D\right) - wp_{i\tau-}^O - \left(wp_{it}^O - wp_{i\tau-}^O\right)$$

where wp_{it}^D and wp_{it}^O are worker i's ranks at their destination (D) and origin (O) jobs, respectively. $wp_{i\tau+}^D$ is worker i's rank in the first quarter worked at the destination firm, where τ is the quarter in which the job change occurred. Similarly, $wp_{i\tau-}^O$ is the worker's rank in the final quarter at the origin job, preceding the move. $\left(wp_{it}^D - wp_{i\tau+}^D\right)$ is the within-job variation in rank at the destination firm, relative to the worker's rank in the first quarter of the job, and $\left(wp_{it}^O - wp_{i\tau-}^O\right)$ captures similar variation within the origin job relative to the final quarter of work.

To construct an instrument for the first component, $wp_{i\tau+}^D$, we use information about other workers who left the same origin firm O prior to worker i changing jobs and moved to any other firm (not necessarily the same destination firm as worker i). Using this sample of former coworkers, denoted $-i$ to indicate that this set excludes the focal worker i, who exited the origin firm prior to worker i exiting, we estimate:

$$wp_{-it}^D = \nu_{j(i,t)}^O + \omega_{j(i,t)}^O \; wp_{-it}^O + u_{-it}^D \tag{10}$$

where wp_{-it}^D and wp_{-it}^O are the workplace ranks of coworker $-i$ at their destination and origin firms, respectively, $\nu_{j(i,t)}^O$ is a firm-specific intercept for the origin firm O, and $\omega_{j(i,t)}^O$ is a firm-specific coefficient that captures the average relationship between origin and destination ranks of workers who exited origin firm $j(i,t)$ at which worker i was employed. In general, $\omega_{j(i,t)}^O$ differs from zero because workers departing some firms are systematically upwardly (or downwardly) mobile in rank.

We estimate Equation 10 on the former coworkers of worker i to predict wp_{-it}^D for a worker who left origin firm O at the median wp rank: $\widehat{wp_{-it}^D} = \widehat{\nu_{j(i,t)}^O} + \widehat{\omega_{j(i,t)}^O} * 0.5$. We estimate the prediction at the median quantile to account for differences in the probability of changing jobs as a function of origin job rank, which we show in Appendix Figure A8. We use $\widehat{wp_{-it}^D}$ as an instrument for $wp_{i\tau+}^D$ in the sample of job movers.

This instrument is valid because worker i and their former coworkers $-i$ who departed from the same origin firm are likely to share similarities in their labor market opportunities, so that $\widehat{wp_{-it}^D}$ is informative of worker i's destination rank. However, since ε_{it} is mean zero in every firm, the idiosyncratic differences in the unexplained future residual component of health status of worker i's former coworkers after their job moves (to different destination firms) should be unrelated to worker i's residual health status after their move. In other words, worker i's future health status after moving is not affected by the future health of their former coworkers, suggesting that the instrument is excludable from the model.

Following the same intuition, we construct an instrument for $wp^O_{i\tau-}$ using coworker network flows in the opposite direction. We use information about workers who arrived at the same destination firm as worker i, but came from different origin firms to estimate:

$$wp^O_{-it} = \nu^D_{j(i,t)} + \omega^D_{j(i,t)}\ wp^D_{-it} + u^O_{-it} \tag{11}$$

This instrument is similarly motivated. It is valid because worker i and their coworkers $-i$ who moved to the same destination firm share similar labor market opportunities, so $\widehat{wp^O_{-it}}$ is strongly informative of worker i's origin workplace rank. However, worker i's health prior to moving should not be affected by the past health of their future coworkers, suggesting that the instrument is excludable from the model. As with the first network instrument, we estimate the prediction at the median destination rank $wp^D = 0.5$. We then use $\widehat{wp^O_{-it}}$ as an instrument for $wp^O_{i\tau-}$.

When estimating the model on the movers sample using these two job mobility network instruments, we also instrument for within-job changes in rank $\left(wp^D_{it} - wp^D_{i\tau+}\right)$ and $\left(wp^O_{it} - wp^O_{i\tau-}\right)$ using the respective promotion IVs for the destination and origin firms so that there are instruments for all of the components of the variation in wp_{it}.

Labor Market Entry Rank

In Section 5 we showed that the divergence in health status by employment rank occurs largely in workers' 30s and 40s, and there is little evidence of health disparities by rank for younger workers entering the labor force in their 20s. Because the earnings panel is 17 years long, we have a relatively large sample of workers in their 30s and mid-40s for whom it is feasible to observe their initial wp_{it} rank upon entering the labor force. Since the vast majority of workers in their 20s are very healthy, even those with latent factors that may predispose them to greater risk of poor health later in life, the potential for correlation between unmodeled variation in health status, ε_{it}, and wp_{it} is likely to be small among young workers. At the same time, we show that a worker's initial wp_{it} rank in their late 20s is informative of their future ranks.

To operationalize this intuition, we calculate the average wp_{it} rank for all quarters of work for workers aged 25-30, which we denote $\overline{wp_i}^Y$, where Y denotes young workers. Then, for the sample of workers aged 31-46, we use $\overline{wp_i}^Y$ as an instrument for the initial starting rank on their first observed job between 2013-2015. We combine this level-based instrument with the promotion instrument to capture variation in ranks that occurs within jobs over time.

Estimation and Results

We estimate a 2SLS model that has a similar specification as Equation 4:

$$y_{it} = \beta_{IV} wp_{it} + \theta_{e(i,t)} + \theta_{r(i),z(i,t)c(i,t)} + \psi_{j(i,t)} + \theta_{p(i,t)} + \theta_t + \theta_{g(i)} + \zeta_{it} \tag{12}$$

where we use the instruments described above for wp_{it}. Because we use only one instrument in some cases, we estimate a combined β_{IV} instead of race-specific coefficients. The remaining model structure and control variables are the same as Equation 4.

Table 5: Comparison of IV Estimates

Instrument(s)	Firm Promotion Rates		Mobility Network & Promotion Rates		Entry Rank & Promotion Rates	
	IV	OLS	IV	OLS	IV	OLS
$\widehat{\beta}$	-0.287	-0.340	-0.375	-0.367	-0.609	-0.340
(se)	(0.006)	(0.004)	(0.040)	(0.006)	(0.057)	(0.013)
1st-stage F-stats	2,696,594		31,195		19,403	
Average y	1.207		1.056		1.031	
Observations	4,029,377		1,371,632		394,248	

Notes: This table presents estimates from Equation 12. In Column 1 the instrument is firm average promotion rates. In Column 3 the instruments are the two job mobility network instruments and the firm promotion instrument. In Column 5 the instruments are the labor market entry rank instrument and the firm promotion instrument. Columns 2, 4, and 6 present corresponding OLS estimates from Equation 12 for the same samples.

Table 5 reports results from these IV models. In column 1 we use the firm promotion rate instrument and estimate that workers in the top percentile of the rank distribution have 0.29 fewer chronic conditions than those in the bottom percentile. This corresponds to a 24% difference relative to a mean of 1.21. The OLS estimate from the same sample is slightly larger than the IV estimate, at 0.34. A substantial advantage of using the promotion instrument is that it can be calculated for a large sample, whereas the mobility network and entry rank instruments can only be calculated on smaller subsets of the data.

In column 3 we report IV estimates that combine the two job mobility network instruments with the firm promotion instrument. This specification includes instruments for all of the within and between-job components of the variation in rank, but a drawback is that the estimation sample is smaller because the network instruments cannot be calculated for every firm in the data. We find again that the IV estimate (-0.375) is similar to the OLS estimate (-0.367).

Finally, in column 5 we report results from the IV specification in which we instrument for initial job rank using $\overline{wp_i}^Y$, the worker's average rank between ages 25-30. We again combine this instrument with the promotion instrument to capture all of the within and across-job variation in ranks. We find in this case that the IV estimate is substantially larger (-0.609) in absolute value than both the corresponding OLS estimate (-0.340) and the other IV estimates. This is perhaps unsurprising, because younger workers have the highest job mobility rates, so there is more potential for endogeneity in job mobility. It is also consistent with evidence from Figure 12 showing that exposure effects for young job movers are smaller than effects among stayers. We find that young male workers drive much of this larger IV estimate.

We view these IV results as complementary to the diagnostic tests, which help corroborate the interpretation of results from the movers analysis.

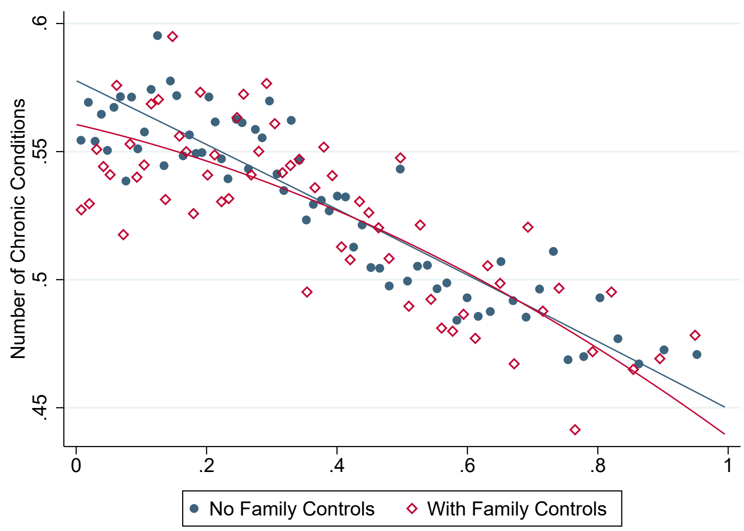
7 Early-Life Health: A Sibling Comparison

One potential challenge to interpreting workplace rank-health gradients among adults is that early life factors can affect both socioeconomic status and health later in adulthood (Case et al., 2002b). This is particularly salient in cases of early-life nutritional deprivation, which affects physical and cognitive development, affects earnings in adulthood, and is associated with the onset of chronic health conditions in adulthood (Case and Paxson, 2011). One manifestation of this relationship documented by Case and Paxson (2008) is that taller workers have higher earnings, and this height-earnings premium can be explained by taller workers having higher cognitive test scores when they were children. Case and Paxson (2011) discuss the importance of the pattern for interpreting results from the Whitehall II study, in which taller workers held higher employment grades on average.

An advantage of our empirical setting that partially alleviates this concern is with nearly 30,000 firms in our main analysis sample, we have a large amount of variation in within-workplace rank among workers with the same earnings levels, in contrast to the Whitehall setting. To the extent early life factors affect earnings levels, we already control for these differences in our main estimates. However, we can further assess the robustness of the results to differences in household resources during childhood by comparing the rank and health outcomes of adult siblings in the labor force.

To do this, we construct sibling linkages for a subsample of the adult workers who were observed as child dependents to the same parent(s) in the health insurance enrollment records. Since the insurance enrollment panel only goes back to 2013, we can only link siblings who are age 28 or younger in the earnings records. We observe 84,110 workers for whom we can match the worker to at least one adult sibling working in the earnings records.

Figure 16: Within-Sibling Comparison: Chronic Conditions vs Workplace Rank



Notes: This figure shows the joint distribution of the number of chronic conditions workers have been diagnosed with and their percentile earnings rank within their primary employer. Each dot is the conditional mean chronic condition count in a given bin of within-firm ranks, controlling for fixed household effects, individual earnings percentile effects, age × zip code fixed effects, gender, year, plan ID fixed effects, average firm size, and average tenure within a firm. The conditional means are constructed using the semiparametric partial linear regression approach developed in Cattaneo et al. (2023).

With this sample of siblings, we re-estimate Figure 2 and compare the results to a model that includes a fixed 'household effect,' so that the identifying variation is restricted to comparisons between siblings.[27] The estimates are shown in Figure 16. Consistent with evidence discussed above, we find a relatively modest downward-sloping relationship for these workers in their late 20s. The levels of chronic condition counts are of course much lower in this young sample. The key finding is that adding household effects as controls has negligible impact on the estimated relationship, suggesting that the included controls in our baseline specification already capture the impacts of differences in early-life household resources quite well.

[27]There are two differences in the control variables used to estimate this model, relative to Figure 2. First, because we have a relatively small sample of individuals that can be matched to siblings, we pool all racial groups together. Second, because these individuals are young and have high geographic mobility rates, we control for fixed individual earnings percentile effects rather than household earnings effects.

8 Implications for Health Disparities

To what extent can aggregate disparities in health across racial and ethnic groups be explained by workplace rank? In this section we conduct two counterfactual exercises to quantify the importance of workplace rank for aggregate disparities.

There is a high level of racial and ethnic segregation across jobs in the US labor market generally (Hellerstein and Neumark, 2008), and we find similar patterns in our data. For example, in Appendix Figure A18 we show that there are 2.7 times more Black workers in the lowest vigintile of ranks relative to the share of Black workers in the top vigintile of ranks. Similarly, American Indian and Hispanic workers are 2.3 times and 1.5 times, respectively, over-represented in jobs in the lowest vigintile of ranks. Conversely, White workers are under-represented in every vigintile of the rank distribution relative to their share in highly ranked jobs. Because minority workers are far more likely to hold lower-ranked jobs, this labor force segregation contributes to health disparities.

To quantify the contribution of differences in exposure to job ranks across groups, we start with the estimated conditional relationship between workplace rank and chronic conditions from Figure 2. Because the relationships shown in Figure 2 include level differences in average disease rates across racial groups, we impose a normalization to remove these differences and isolate the contribution of workplace rank. We do this by assuming that the predicted chronic condition count for a worker at the median workplace rank (50) is the same for all groups. Under this normalization assumption, we then estimate the counterfactual expectation of chronic condition counts by group if each group had the same workplace rank distribution as White workers, without changing the race-specific rank-health gradient functions.

The middle panel of Table 6 shows estimates from this counterfactual exercise. We estimate that if American Indian workers had the same workplace rank distribution as White workers they would have 0.054 fewer chronic conditions, about 3.9% of the American Indian mean. The estimates are similar for Black workers (0.054, 4.1%) and somewhat smaller for Hispanic workers (0.018, 1.3%).

While these may seem like modest improvements relative to average health levels, they are substantial from the perspectives of health improvements and health disparities. For example, relative to American Indian and Black workers, the magnitude of the health advantage of White workers that operates through workplace rank is similar to completely curing both Ischemic Heart Disease *and* Chronic Obstructive Pulmonary Disease (COPD) in the population of White workers only. Moreover, relative to the normalized expected chronic condition count of White workers (1.314),

Table 6: Counterfactual Estimates

	American Indian	Black	Hispanic
$\mathbb{E}$[Chronic Conditions], Figure 2	1.448	1.158	1.303
$\mathbb{E}$[Chronic Conditions], Normalizing Levels	1.386	1.327	1.348
Counterfactual 1: All Groups Have White Rank Distribution			
$\mathbb{E}$[Chronic Conditions]	1.332	1.273	1.330
Δ $\mathbb{E}$[Chronic Conditions], Levels	-0.054	-0.054	-0.018
Δ $\mathbb{E}$[Chronic Conditions], %	-3.897%	-4.091%	-1.333%
Counterfactual 2: All Groups Have White Rank-Health Gradient			
$\mathbb{E}$[Chronic Conditions]	1.333	1.339	1.326
Δ $\mathbb{E}$[Chronic Conditions], Levels	-0.053	0.012	-0.023
Δ $\mathbb{E}$[Chronic Conditions], %	-3.844%	0.882%	-1.680%

0.054 chronic conditions is equal to 75% of the normalized American Indian-White morbidity gap, and exceeds the total Black-White normalized morbidity gap.

The second counterfactual exercise we conduct, shown in the bottom panel of Table 6, estimates the effect of changing the slope of the rank-health gradient to match the White slope, while holding the race-specific distributions of ranks fixed. We find that this would have the largest effects for American Indian workers, reducing morbidity by 0.053 conditions (3.8% of the mean), with smaller effects for Hispanic workers (0.023 and 1.7%). For Black workers, this counterfactual would increase morbidity slightly.

9 Conclusion

This paper provides the first population-level US evidence of the relationship between a worker's relative status within their workplace and health status. The headline finding is that relative workplace rank has a stronger association with the number of chronic health conditions than does income. In particular, about 70% of the conditional income-health gradient is explained by relative rank *within* the workplace, and only 30% is explained by income levels. Importantly, this relationship is robust to detailed controls for income, age, location, and specific health insurance policy characteristics. The rank-health gradient, however, varies significantly by race, with stronger associations between rank and health status for racial minorities than for White workers.

We show that this rank-health gradient exists for 16 of the 18 major chronic diseases that we study. Exploiting the panel structure of the data, we also find that these health disparities emerge over time. At the time of labor market entry, there are few health differences by rank. By the time workers reach their forties, however, the relationship between within-workplace rank and the number of chronic health conditions is quite pronounced. By focusing on diseases that affect working-age adults we provide new details into the health channels that are strongly correlated with the patterns of mortality other studies have documented. Future work will further investigate possible mechanisms for this emergence. This evidence is consistent with the broad literature on social determinants of health, which includes workplace environment and relative social position as factors that affect chronic exposure to stress, which has been shown to inhibit the immune system and impact the development of many chronic health conditions.

Building on this finding, we estimate a model of exposure to workplace status where the health effects of exposure are age-specific and accumulate over time. We derive network-based instrumental variables using coworker flows between firms to isolate variation in exposure to workplace rank that is unrelated to variation in health status. We find the IV estimates are similar to the fixed effects estimates for most workers, and somewhat larger for younger workers. After showing that job mobility up and down the workplace rank distribution is not itself related to health status, we conclude that nonrandom sorting in the labor market on the basis of unobserved differences in health status cannot explain the differences in health status by within-firm rank that we document.

Given that racial sorting in workplaces leads to a disproportionate number of racial minorities at the lower end of the within-workplace distribution, existing racial disparities are exacerbated by the within-firm status relationship to health that we document.

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Online Appendices

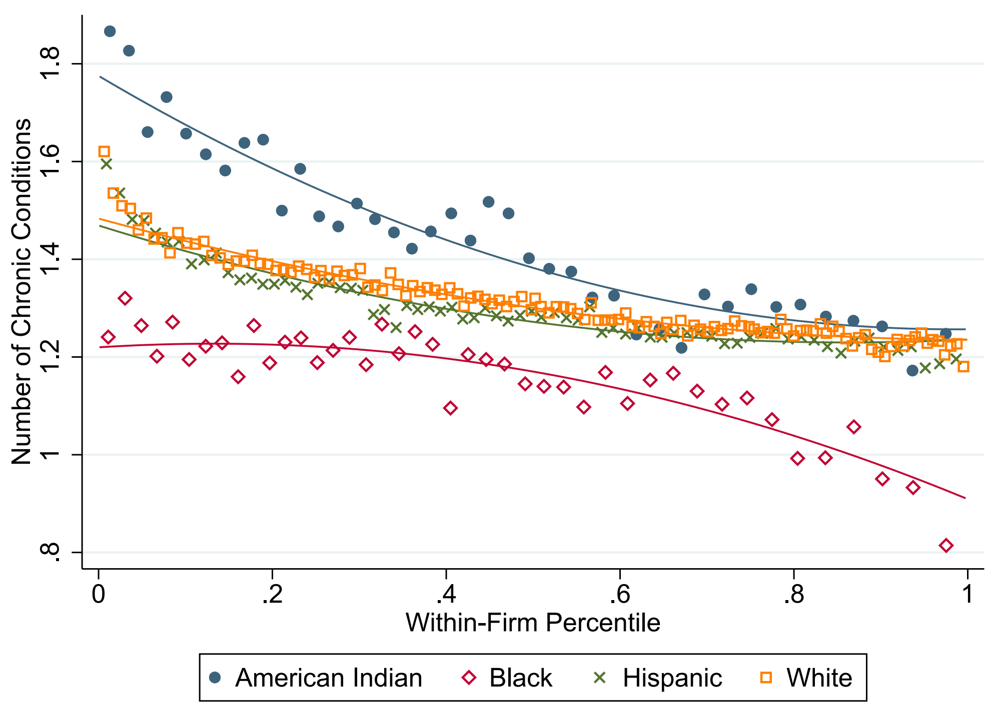
Appendix A Additional Tables and Figures

Appendix Table A1: Average Chronic Condition Rates by Condition Category and Race

	American Indian	Black	Hispanic	White
Mental Health Diseases	0.316	0.216	0.295	0.299
Cardiovascular Diseases	0.255	0.233	0.223	0.222
Other Diseases	0.536	0.447	0.493	0.490

Notes: 'Mental Health Diseases' include bipolar, schizophrenia, and depression. 'Cardiovascular Diseases' include congestive heart failure, ischemic heart disease, and hypertension. 'Other Diseases' include asthma, anemia, arthritis, COPD, diabetes, glaucoma, hyperthyroidism, lower back pain, macular degeneration, osteoporosis, Parkinson's, and seizures.

Appendix Figure A1: Number of Chronic Conditions vs Within-Firm Income Percentile
Full Set of Controls, Individual Income Percentile Effects



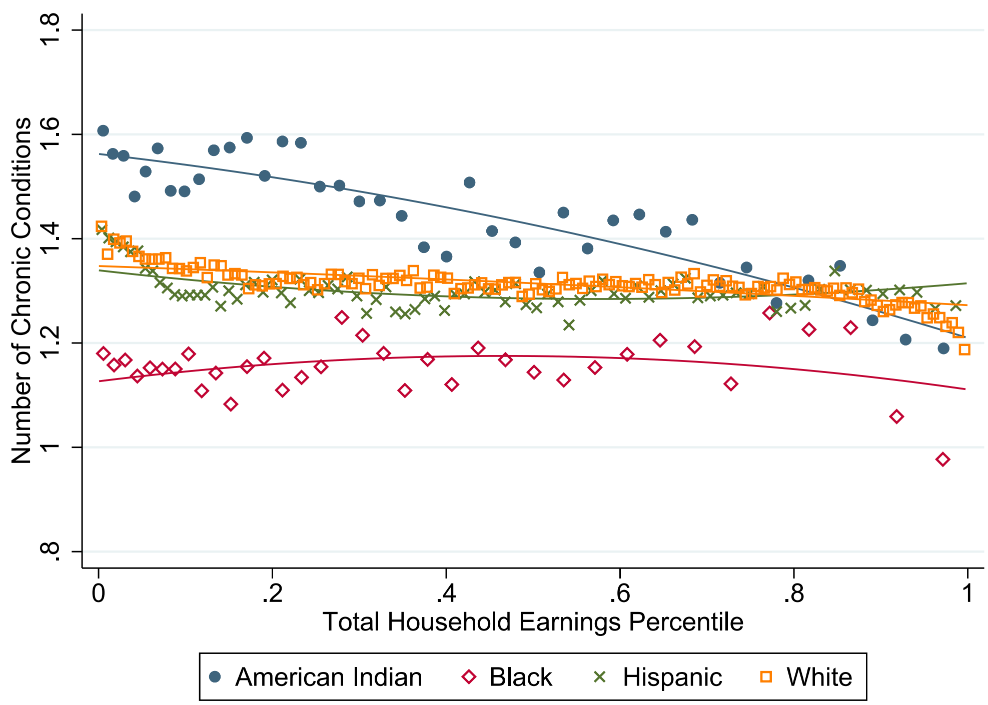
Notes: This figure shows the joint distribution of the number of chronic conditions workers have been diagnosed with and their percentile earnings rank within their primary employer. Each dot is the race-specific conditional mean chronic condition count in a given bin of within-firm ranks, controlling for fixed effects for each percentile of individual earnings, age × zip code fixed effects, gender, year, plan ID fixed effects, average firm size, and average tenure within a firm. The conditional means are constructed using the semiparametric partial linear regression approach developed in Cattaneo et al. (2023).

Appendix Table A2: Impacts of Control Variables on the 90-10 Percentile Gap in Chronic Conditions

	American Indians	Black	Hispanic	White
Baseline 90-10 Gap (Figure 1):	0.449	0.303	0.370	0.271
Location	-0.037	-0.011	0.002	-0.012
Social and Demographic Controls	-0.021	-0.022	-0.068	-0.046
Health Insurance Policy FE	0.013	-0.010	-0.027	0.014
Firm/Job Characteristics	0.018	0.001	-0.001	0.001
Fully Conditional 90-10 Gap (Figure 2):	0.422	0.262	0.277	0.229

Notes: This table reports differences in chronic disease rates between the 90th and 10th percentiles of employment rank by race. 'Baseline' estimates condition on age effects and household income percentile effects. The 'Location' row reports to average marginal impact on the 90-10 gap of adding zip code effects. 'Social and Demographic Controls' include gender and year effects. 'Health Insurance Policy FE' includes controls for group policy ID effects. 'Firm/Job Characteristics' includes average firm size and firm average tenure. Each of these four rows report average marginal effects of adding the respective controls, where the average is taken over all possible sequential orderings of the sets of controls. 'Fully Conditional 90-10 Gap' corresponds to Figure 1, and reports the 90-10 percentile gap including all sets of controls.

Appendix Figure A2: Number of Chronic Conditions vs Total Earnings Percentile



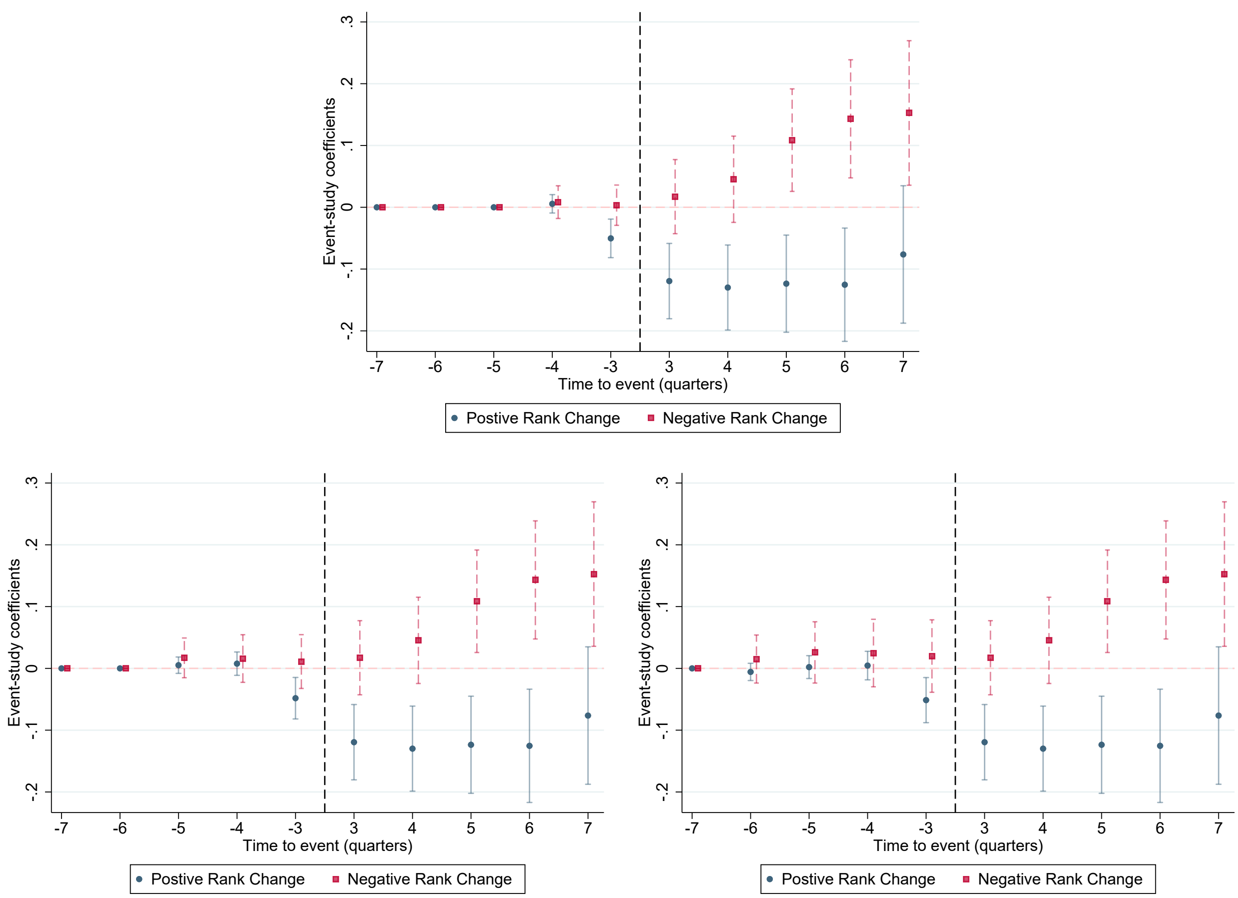
Notes: This figure shows the joint distribution of the number of chronic conditions workers have been diagnosed with and their percentile in the distribution of household earnings in Utah. Each dot is the race-specific conditional mean chronic condition count in a given bin of household earnings, controlling for within-firm ranks, age × zip code fixed effects, gender, year, plan ID fixed effects, average firm size, and average tenure within a firm. The conditional means are constructed using the semiparametric partial linear regression approach developed in Cattaneo et al. (2023).

Appendix Table A3: Strength of Relationships between Work Outcomes and Health Status, by Race

	American Indians	Black	Hispanic	White
Total HH Earnings	-0.066	-0.040	-0.035	-0.126
	(0.038)	(0.040)	(0.012)	(0.007)
Within-Firm Rank	-0.544	-0.252	-0.342	-0.238
	(0.035)	(0.036)	(0.010)	(0.006)
Within-Firm Gini	0.364	0.036	0.247	-0.051
	(0.071)	(0.076)	(0.021)	(0.012)

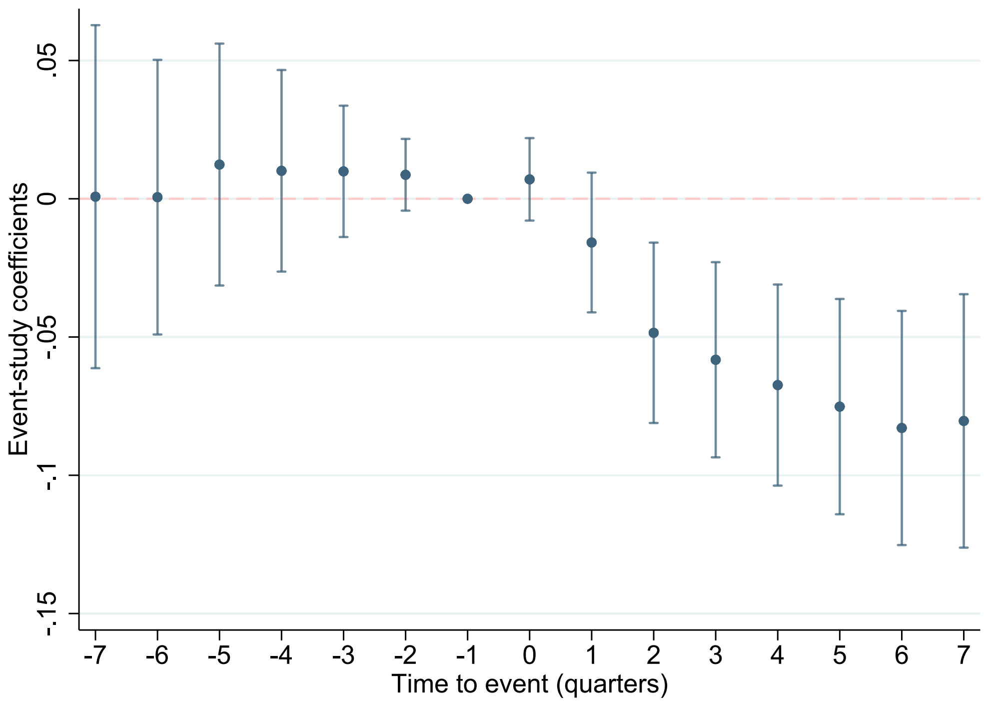
Notes: This table reports estimates from Equation 1, as shown in Figure 4.

Appendix Figure A3: Event Study Robustness: Borusyak et al. (2024) Model



Notes: This figure is similar to Figure 7a but is estimated using the imputation model developed in Borusyak et al. (2024) under different normalization assumptions. Standard errors are clustered at the individual level.

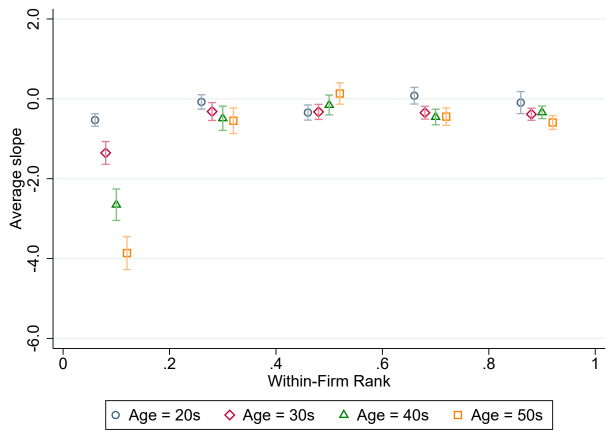
Appendix Figure A4: Event Study Estimates without Donut Hole



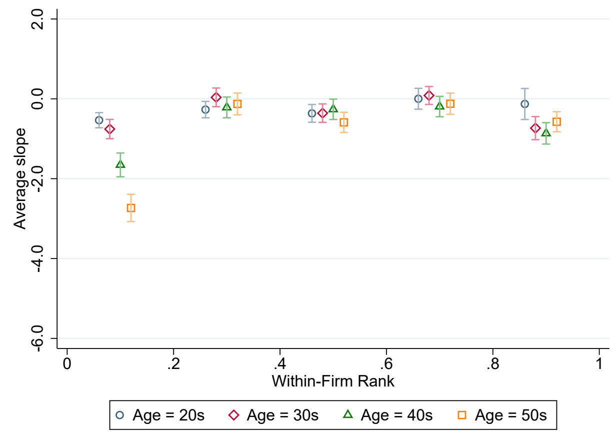
Notes: This figure shows estimates of $\widehat{\beta_k}$ from Equation 3, and is similar to Figure 6 but without a donut hole around the time of the job move. Standard errors are clustered at the individual level.

Appendix Figure A5: Slope of the Workplace Rank-Health Gradient by Rank Quintile
By Age in Decades, By Gender

(a) Men



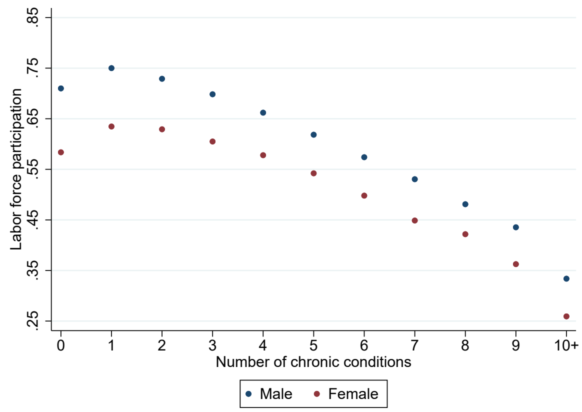
(b) Women



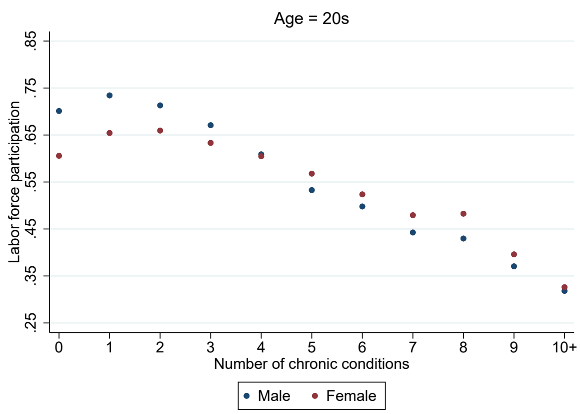
Notes: The figures show how the average marginal effect of workplace rank on health by gender, race, and quintile of the workplace rank distribution.

Appendix Figure A6: Labor Force Participation and Health

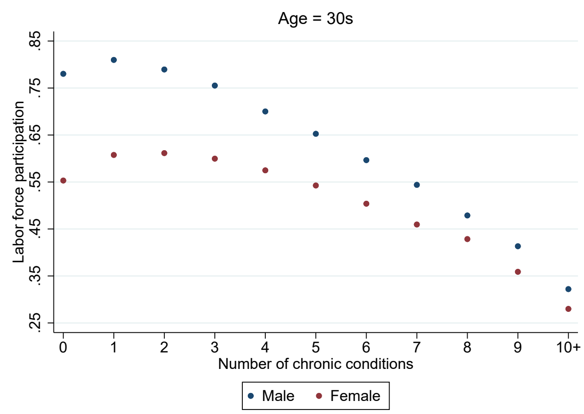
(a) Overall



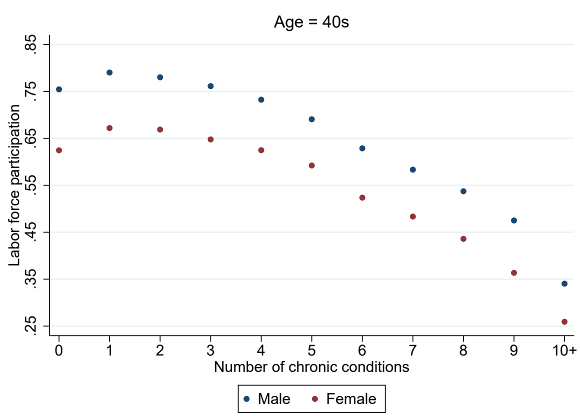
(b) Age 20s



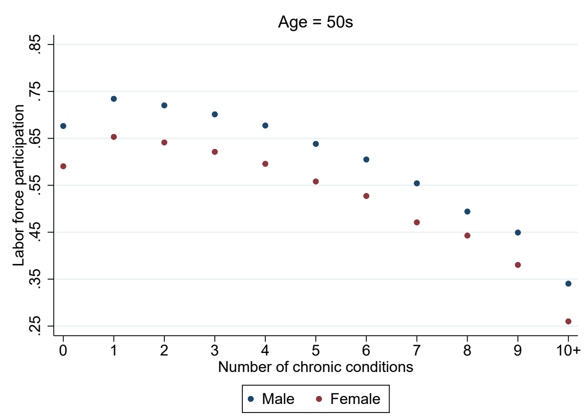
(c) Age 30s



(d) Age 40s

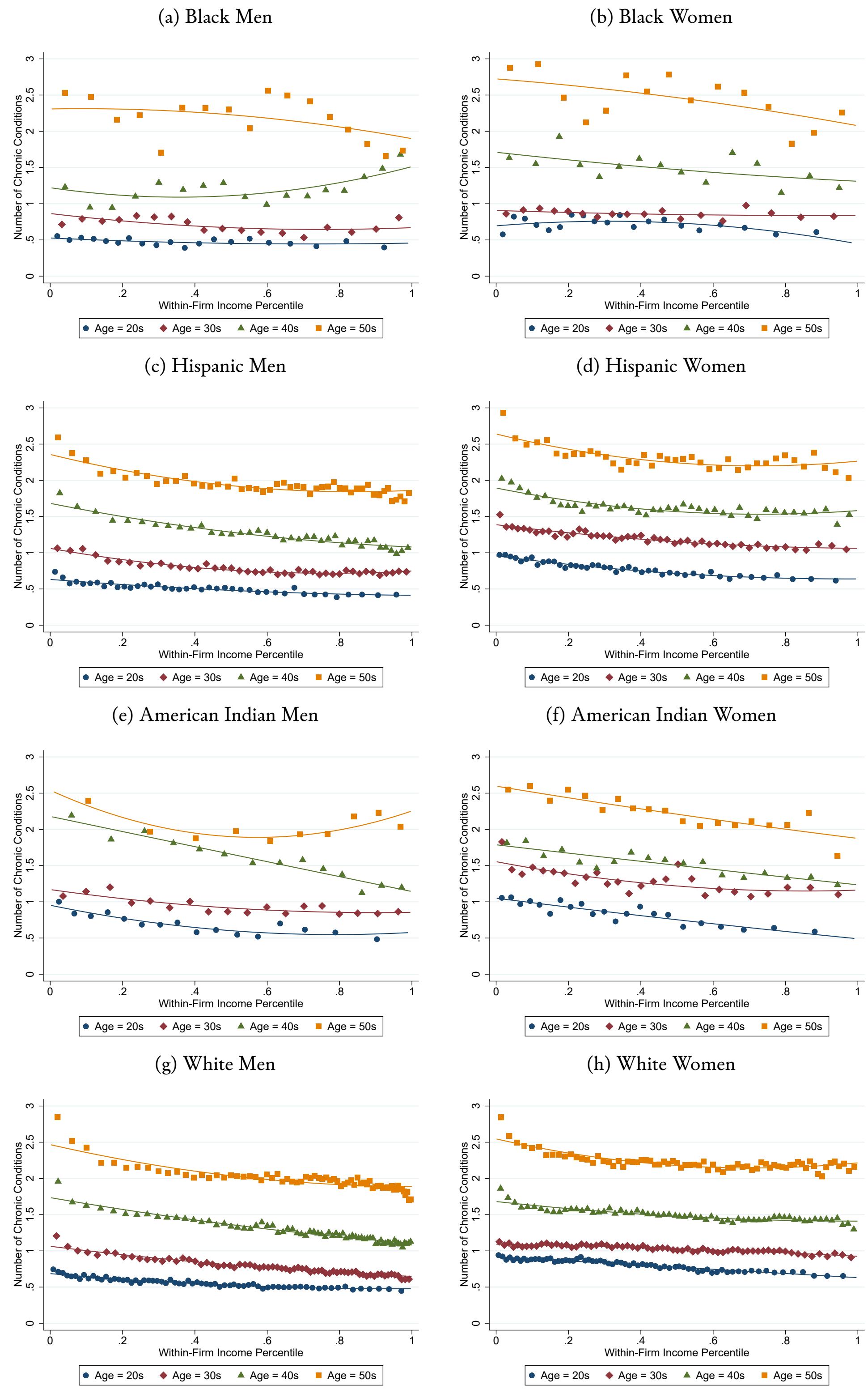


(e) Age 50s



Notes: These figures report labor force participation rates by sex, health status, and age. Labor force participation is defined as the quarterly share of workers with any positive earnings, conditional on the individual being between ages 18 and 65 in the quarter and having health insurance coverage at any point between 2013 and 2015. Subfigure A6a shows the overall labor force participation rate, and subfigures A6b through A6d show rates by age categories.

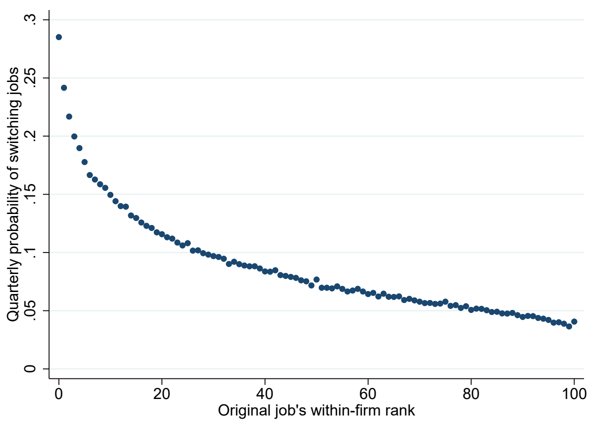
Appendix Figure A7: Number of Chronic Conditions vs Within-Firm Income Percentile By Age in Decades, By Race-Gender



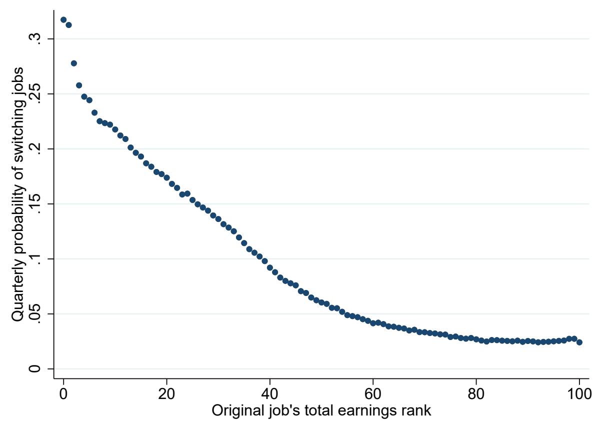
Notes: We conduct the same semiparametric method used in Figure 2 by age categories, where we control for fixed household income percentile effects, age × zip code effects, year, plan ID effects, average firm size, and average tenure within a firm. Fitted lines are 4th-order polynomials.

Appendix Figure A8: Quarterly Probability of Switching Jobs
by Origin Job Within-firm Rank and Earnings Percentile

(a) Within-firm Rank

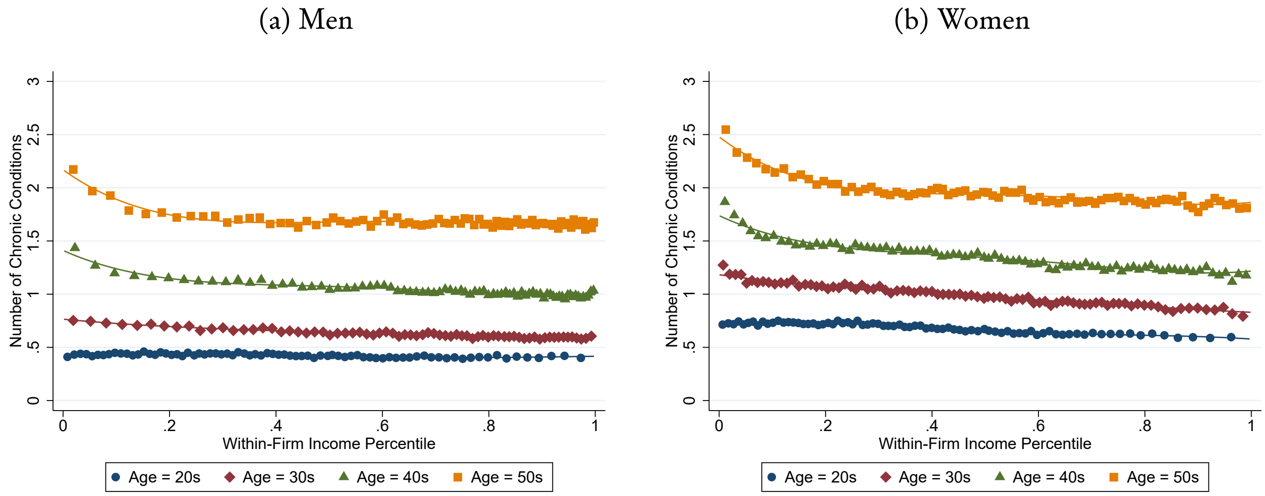


(b) Earnings Percentile



Notes: These figures report the quarterly probability of switching jobs conditional on the origin job's within-firm rank (panel a) and total earnings rank (panel b).

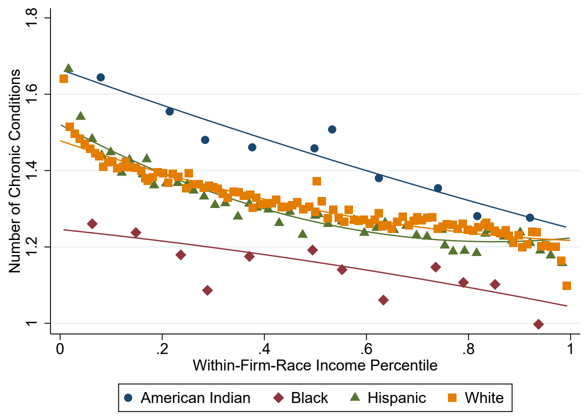
Appendix Figure A9: Number of Chronic Conditions vs Within-Firm Income Percentile
By Age in Decades, By Gender
Individual Earnings Percentile Fixed Effects



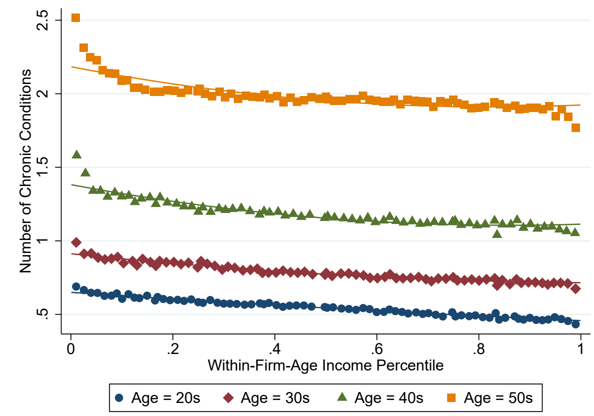
Notes: We conduct the same semiparametric method used in Figure 2 by age categories, where we control for fixed individual income percentile effects, age × zip code effects, gender, year, plan ID effects, average firm size, and average tenure within a firm. Fitted lines are 4th-order polynomials.

Appendix Figure A10: Alternative Social Status Measures

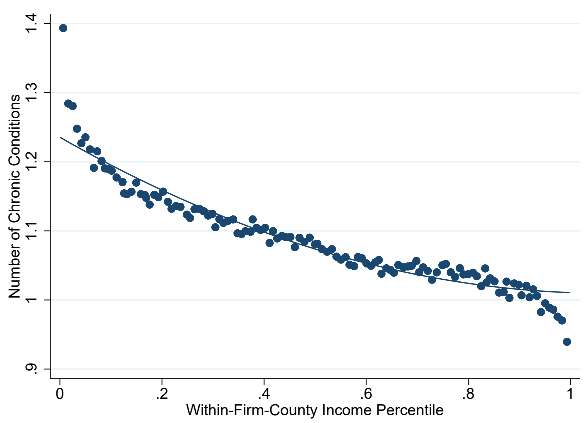
(a) Rank within Firm-Race Bin



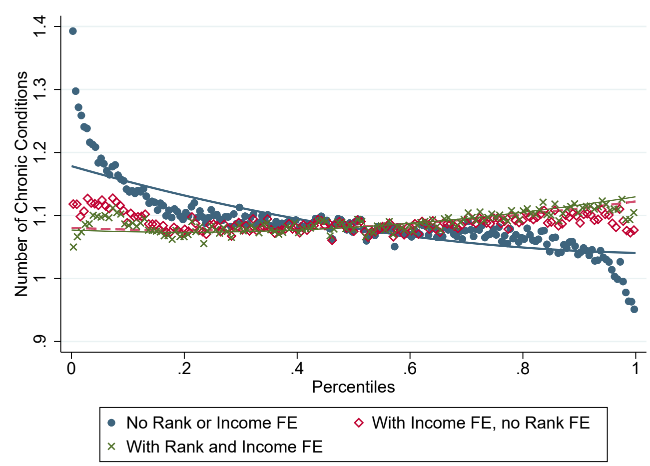
(b) Rank within Firm-Age Bin



(c) Rank within Firm-County Pairs



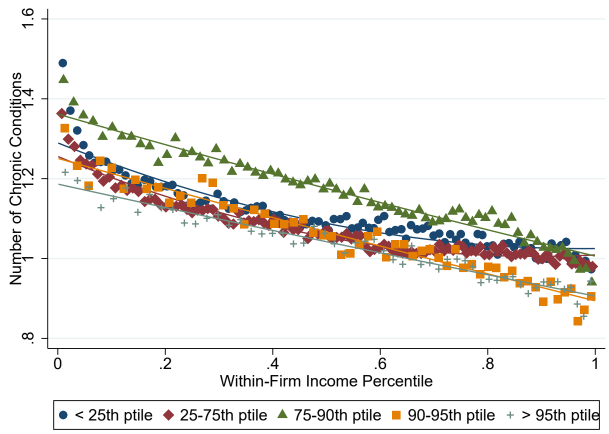
(d) Rank within Zip Code of Residence



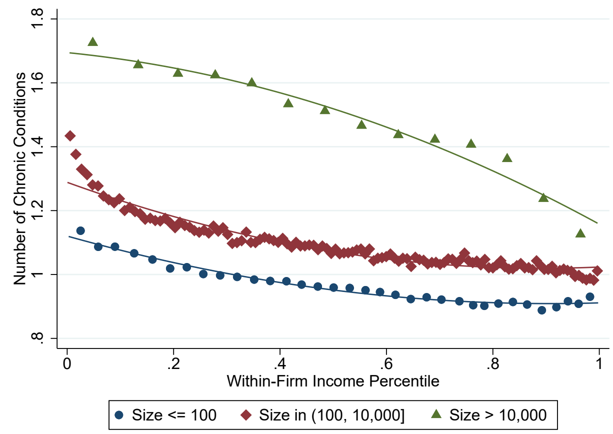
Notes: In panels (a), (b), and (c) the horizontal axes are (a) the worker's percentile rank among workers with the same race within their firm, (b) percentile rank among workers with the same age (in decades) within their firm, and (c) percentile rank within the firm-county pair. In panel (d) we calculate each worker's percentile rank of the earnings distribution within their zip code of residence and show the rank-health gradient with an without controlling for fixed within-firm rank effects and income percentile effects. All figures are constructed using the semiparametric method from Figure 2, and include fixed household income percentile effects (unless otherwise indicated), age × zip code effects, gender, year, plan ID effects, average firm size, and average tenure within a firm. Fitted lines are 2nd-order polynomials.

Appendix Figure A11: Heterogeneity in the Rank-Health Gradient

(a) Rank Gradient by Gini Index



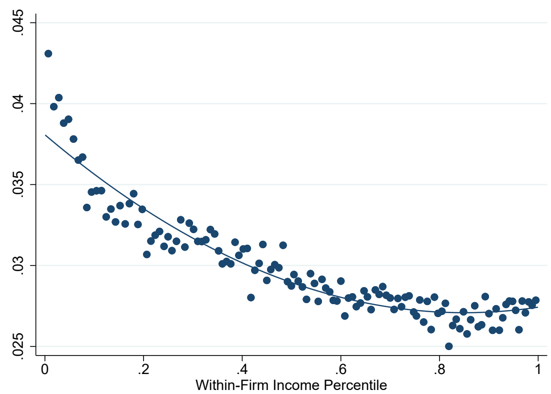
(b) Rank Gradient by Firm Size



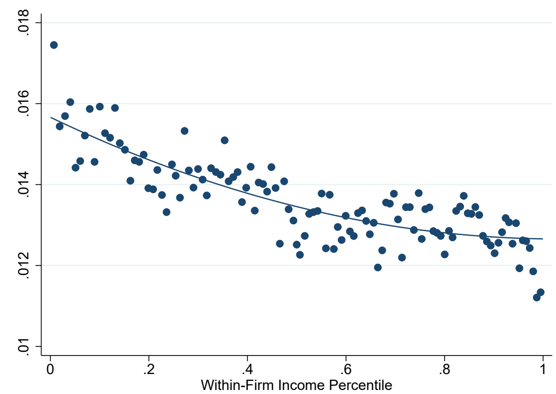
Notes: In panel (a) we calculate the Gini Index for each firm and show heterogeneity in the rank-health gradient by percentile bins of the Gini Index. In panel (b) we show heterogeneity in the rank-health gradient by different firm sizes: small firms (size ≤ 100), larger firms (size in between 100 and 10,000), and very large firm with more than 10,000 workers. Both figures are constructed using the semiparametric method from Figure 2, and include fixed household income percentile effects, age $\times$ zip code effects, gender, year, plan ID effects, average firm size, and average tenure within a firm. Fitted lines are 2nd-order polynomials.

Appendix Figure A12: Diagnosis Rates by Condition vs Within-Firm Income Percentile
Full Set of Controls

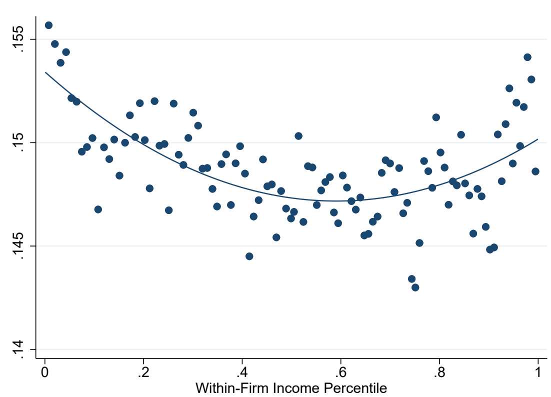
(a) Anemia



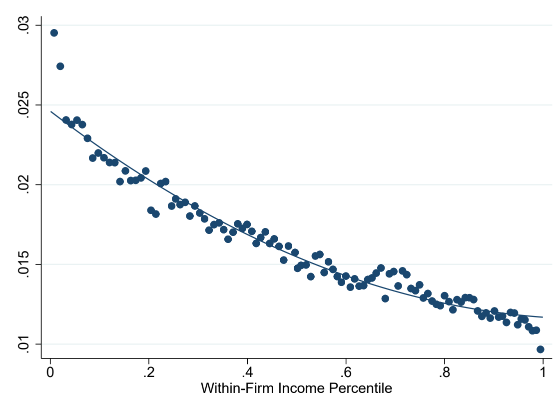
(b) Arthritis



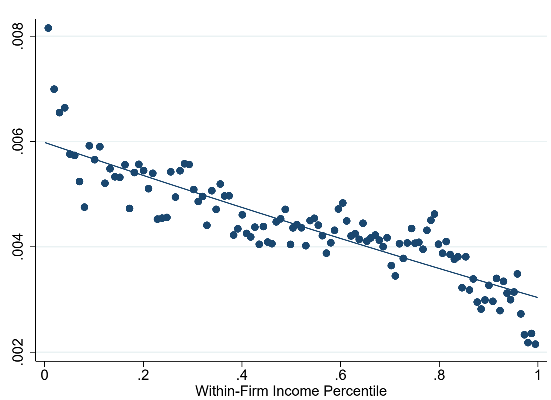
(c) Asthma



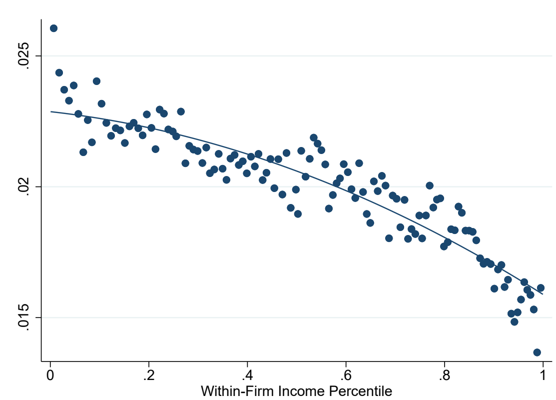
(d) Bipolar



(e) Congestive Heart Failure

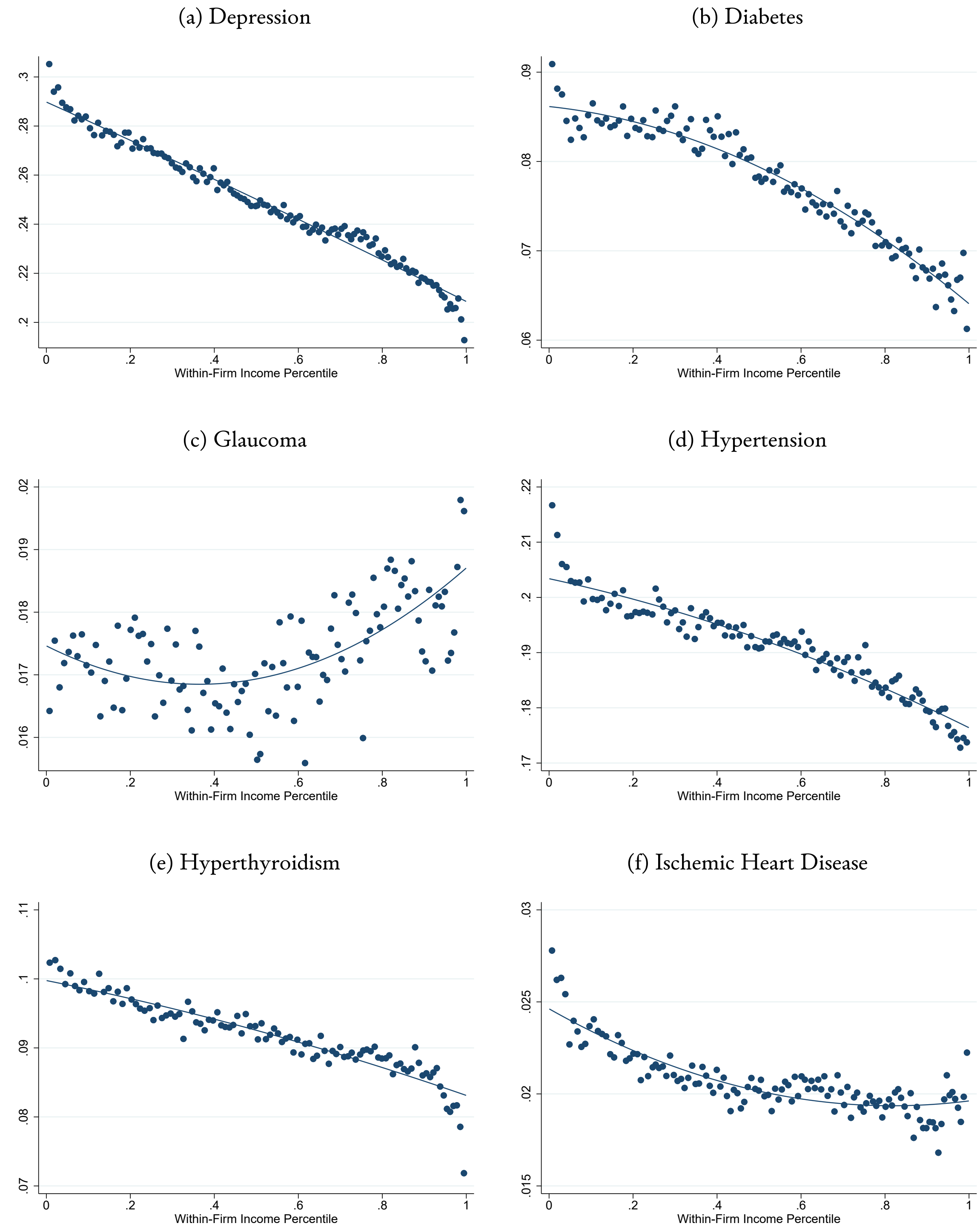


(f) COPD



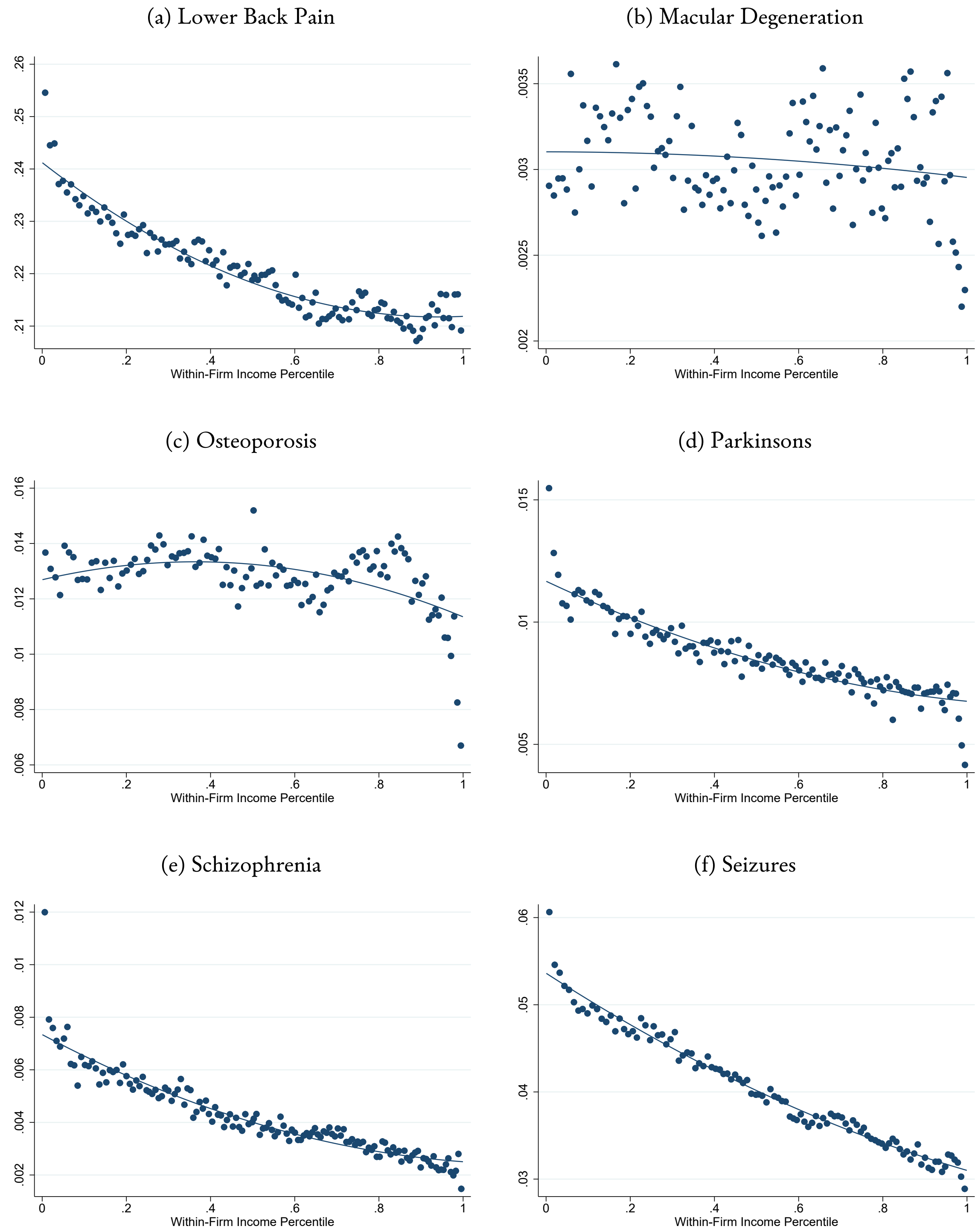
Notes: The y-axis in each figure shows the prevalence of each disease. All figures are constructed using the semiparametric method from Figure 2, and include fixed household income percentile effects, age × zip code effects, gender, year, plan ID effects, average firm size, and average tenure within a firm. Fitted lines are 2nd-order polynomials.

Appendix Figure A13: Diagnosis Rates by Condition vs Within-Firm Income Percentile
Full Set of Controls



Notes: The y-axis in each figure shows the prevalence of each disease. All figures are constructed using the semiparametric method from Figure 2, and include fixed household income percentile effects, age × zip code effects, gender, year, plan ID effects, average firm size, and average tenure within a firm. Fitted lines are 2nd-order polynomials.

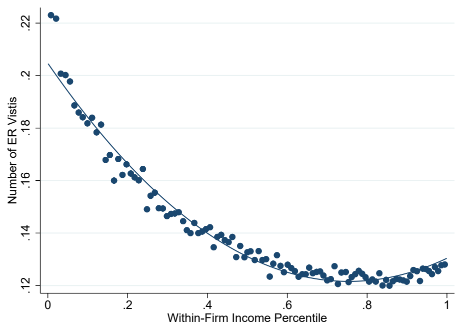
Appendix Figure A14: Diagnosis Rates by Condition vs Within-Firm Income Percentile
Full Set of Controls



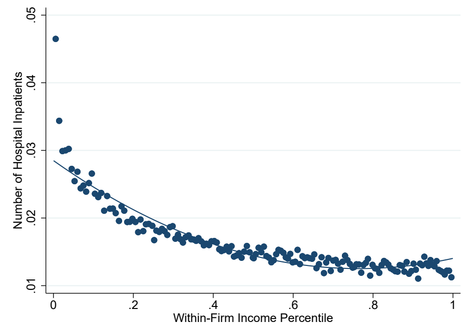
Notes: The y-axis in each figure shows the prevalence of each disease. All figures are constructed using the semiparametric method from Figure 2, and include fixed household income percentile effects, age × zip code effects, gender, year, plan ID effects, average firm size, and average tenure within a firm. Fitted lines are 2nd-order polynomials.

Appendix Figure A15: Healthcare Utilization vs Within-Firm Income Percentile
Full Set of Controls

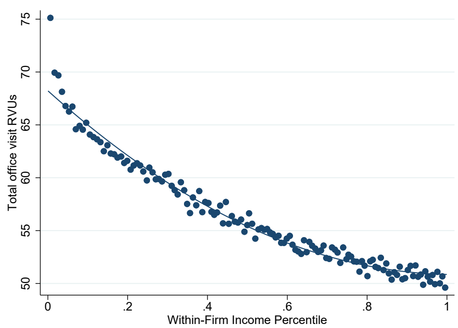
(a) Number of ER Visits



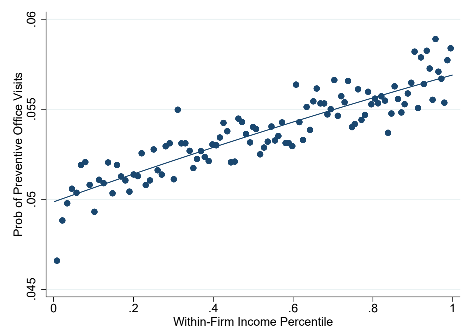
(b) Number of Inpatient Visits



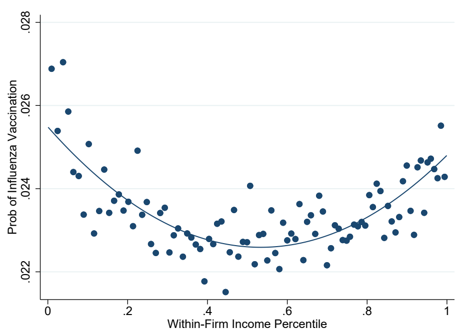
(c) Professional Office Visit RVUs



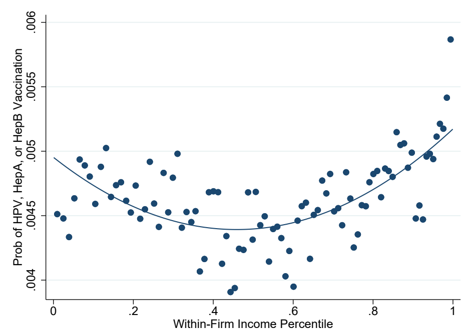
(d) Quarterly Probability of Having at Least One Preventative Care Visit



(e) Quarterly Probability of Recieving Flu Vaccine

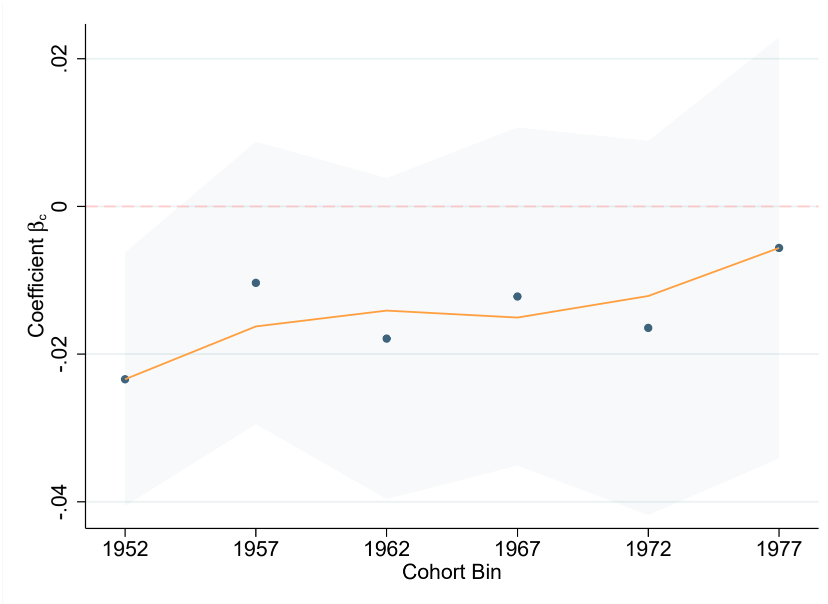


(f) Quarterly Probability of Recieving HPV or Hepatitis Vaccine



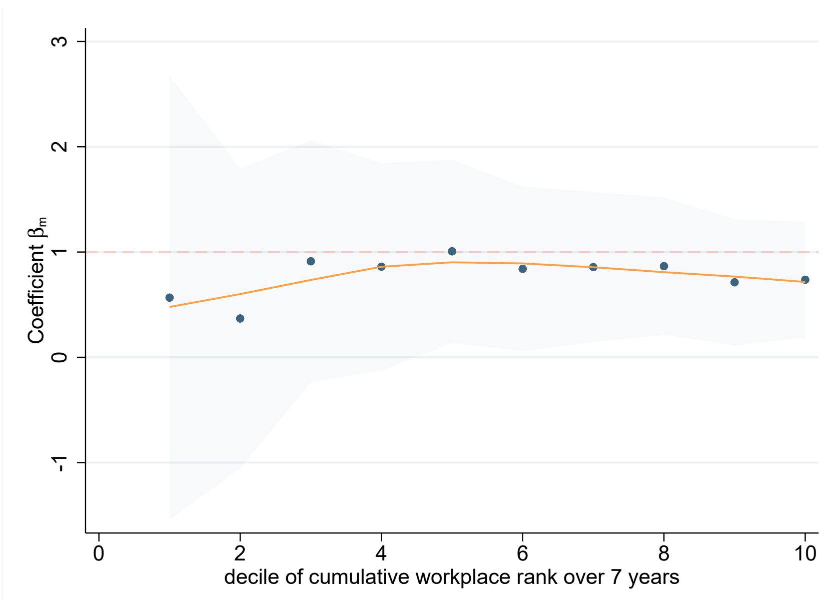
Notes: Panels a, b, and c report total number of visits by visit type. The number of ER and inpatient visits is calculated using the Johns Hopkins ACG software. Office visit RVUs are defined as office or outpatient visits coded CPT codes 99211-99215 and 99201-99205. Relative value units are defined as the non-facility fee paid by Medicare for these procedures in 2014. Panels d, e, and f report the probability of having one of three preventive services. We define a preventive health visit as a comprehensive preventive medicine appointment (CPT codes 99381-99387, 99391-99397), individual counselling for preventive medicine (CPT codes 99401-99404), or a behavioral change intervention (CPT codes 994063-99420). Flu vaccines are defined according to the CPT codes: 90630, 90653-90658, 90660-90662, 90664-90668, 90672-90674, 90682, 90685-90687, 90756, Q2034-Q2039. An HPV or Hepatitis vaccine includes CPT codes 90649, 90650, 90632-90634, 90636, 90739, 90740, 90743, 90744, 90746, 90747, G0010. All figures are constructed using the semiparametric method from Figure 2, and include fixed household income percentile effects, age $\times$ zip code effects, gender, year, plan ID effects, average firm size, and average tenure within a firm. Fitted lines are 2nd-order polynomials.

Appendix Figure A16: Estimated $\widehat{\beta_c}$ among Long-Term Stayers by Cohort



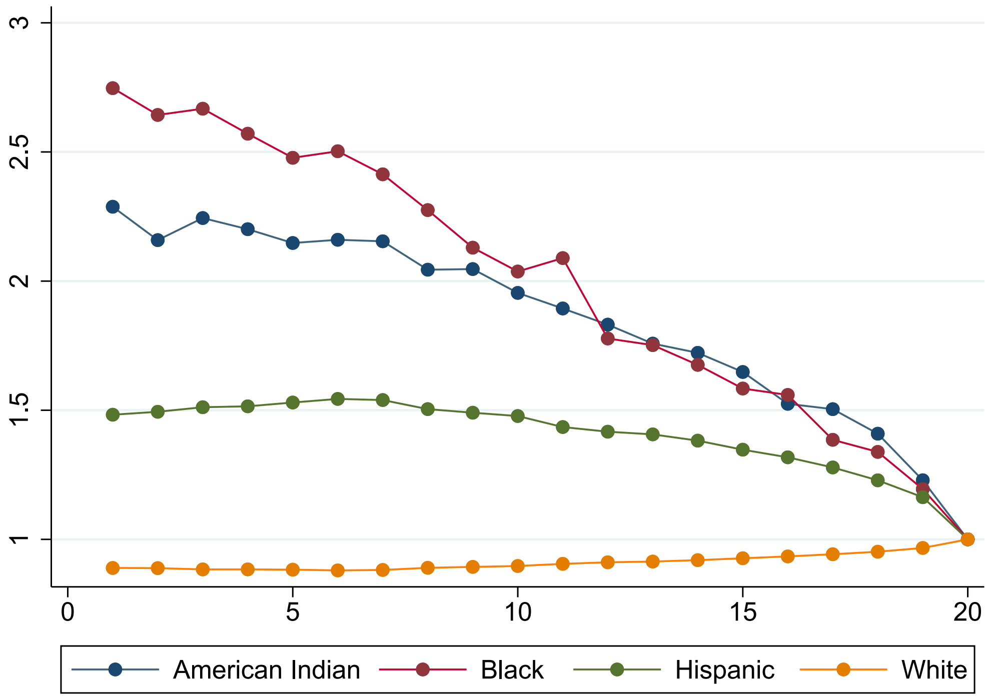
Notes: This figure shows estimates of $\widehat{\beta_c}$ from Equation 7 for 5-year bins of birth cohort. Estimates are based on the sample of job stayers for whom $wp_{it}^{max} - wp_{it}^{min} < 0.15$ during the 7+ year job spell.

Appendix Figure A17: Estimated $\widehat{\beta_m}$ from Equation 8



Notes: The dots in the figure are estimates of $\widehat{\beta_m}$ from Equation 8 for each decile of $\overline{y}_{rwp}$. The solid line is a smoothed local linear function of the age-specific estimates.

Appendix Figure A18: Racial and Ethnic Segregation of Jobs by Workplace Rank



Notes: This figure shows the factor by which workers are over or under-represented in jobs by vigintile of the workplace rank distribution, relative to the share of workers of the same race or ethnicity in the top vigintile of job ranks. White workers are under-represented in every vigintile of the rank distribution, relative to the share of White workers in the top vigintile. For all other racial and ethnic groups, workers are over-represented in lower ranked jobs relative to their shares in the top vigintile.

Appendix B Defining Firm IDs

The earnings file does not include firm IDs, but it does contain information about the network structure of the labor market that can be used to statistically estimate firm boundaries over time. The file contains a variable that reports the average pay at the employer-quarter level for all workers for whom that employer was the primary employer. This average pay variable is infused with white noise to preserve privacy. The noise-infused variable is uniform at the firm-quarter level.

For large firms, the data contains large blocks of workers with the same sequence of quarterly firm average pay. Given the precision with which average pay is recorded, the probability of this happening by statistical chance is infintesimal. Using similar logic, we compute the statistical probabilities that different combinations of workers are coworkers with each other in each quarter over the panel. Using a simulated model we estimate that in a sample of approximately 60,000 firms, the probability that two or more firms share the exact same average pay by statistical chance in one quarter is less than 4%. The probability that this happens by chance in two consecutive quarters is less than 0.001%. These error probabilities are over the entire set of all firm boundaries, not the boundaries of a particular firm. Therefore for firms that exist for multiple quarters, especially firms that have five or more workers (as in our main analysis sample), the statistical estimation of firm boundaries is precise. Moreover, the APCD contains firm IDs for the 81% of the sample that has employer-provided insurance, which we also use to validate the statistical estimation procedure.