Inequality Amplified: Workplace Stratification and Racial Health Disparities*

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DECEMBER 20, 2023

Abstract

To what extent is a worker's relative rank within their workplace a determinant of health status, conditional on income? We provide the first US-based evidence on the aggregate relationship between relative workplace rank and health disparities 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 racial health disparities, and the incidence of specific health conditions that drive these disparities. We show that about 75% of the conditional income-health gradient is explained by relative rank, and only one-quarter is explained by income levels. For an average Black, Hispanic, or American Indian worker, moving from the 90th to the 10th percentile of within-firm rank holding fixed income, age, location, and health insurance policy characteristics is associated with a 23% increase in the average number of diagnosed chronic conditions. This stronger association for racial minorities than for White workers amplifies aggregate inequality: 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, J01

*We are grateful to Norman Thurston and the staff at the Utah Office of Health Care Statistics for their support in providing data for this study. This research was supported financially by the National Institute on Minority Health and Health Disparities (R01MD017619), the Robert Wood Johnson Foundation (Grant 72674), and a pilot grant from the Eunice Kennedy Shriver National Institute of Child Health and Human Development (P2CHD058484).

1 Introduction

An average person at the top of the income distribution in the US lives over 12 years longer than an average person at the bottom of the income distribution. Similar patterns of health disparities by income exist within many other developed countries and within US states and cities. It is perhaps surprising, then, that differences in income levels across developed countries are not strongly associated with differences in health and well-being.

Canonical economic models tend to emphasize the importance of economic resource levels in affecting health and well-being. Yet it has long been understood among economists that people also care deeply about their relative position within society.[1] 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, and myriad other measures of health and wellbeing.[2] For example, politicians who win closely contested elections live 1-3 years longer than their defeated opponents (Borgschulte and Vogler, 2019), baseball Hall of Fame inductees live 10% longer than those who narrowly missed induction (Becker et al., 2007), winners of Oscar awards live around 4 years longer than nomineees (Han et al., 2011a), 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).

What are the aggregate impacts of social status on health outcomes? To what extent does the strong income-health gradient operate through relative social position? Do differences in social position affect disparities in health outcomes across racial or other demographic groups? And which health conditions are affected by relative position? Questions of broad importance such as these have been challenging to answer for at least several reasons. Relative position is difficult to measure, and is rarely observed alongside individual measures of specific health conditions. This is why many researchers have focused on narrow domains that affect famous individuals, using newsworthy deaths as

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

[2] Luttmer (2005); Anderson and Marmot (2012); Antonovsky (1967); Blázquez et al. (2014); Cutler et al. (2011); Halliday (2017); Kröger (2016); Ravesteijn (2016)

the primary health outcome. 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 fail to find evidence. 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.[3]

We provide the first US-based evidence on the aggregate relationship between workers' relative ranks within workplaces and health disparities for the near population of workers in one US state. Using a new database 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 racial health disparities and estimate the incidence of specific health conditions that drive these disparities. We show that about three-quarters of the conditional income-health gradient is explained by relative rank within the workplace, and only one-quarter is explained by income levels. Within-workplace relative rank has an even stronger association with health status for racial minorities than for White workers. For an average Black or American Indian worker, moving from the 90th to the 10th percentile of within-firm rank holding fixed income, age, location, and health insurance policy characteristics is associated with a 33% increase in the average number of diagnosed chronic conditions, compared to a 17% increase for an average White worker. This stronger association for racial minorities amplifies aggregate inequality: the racial segregation of jobs in the US leads minority workers to be over-represented in lower-ranked jobs within firms, which in turn exacerbates racial health disparities.

Applying linked administrative health and earnings data to this question allows us to make several important innovations. First, we 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, such as the famous Whitehall studies, where earnings levels are highly correlated with workplace rank,

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

in our broader labor market data only 54% of the variance in earnings levels is explained by within-firm rank. Second, 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 UI-covered jobs in the state. Third, large-scale administrative data allows 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 parsimoneous model that only controls for earnings levels and age. Moving from the 90th percentile of workplace rank to the 10th percentile is associated with 0.17 additional chronic conditions (16% of the sample mean) for White workers, and substantially larger differences for minority workers. Black, Hispanic, and American Indian workers at the 10th percentile of workplace rank have 0.23 (22%), 0.25 (24%), and 0.33 (32%) more diagnosed chronic diseases than workers of the same race or ethnicity employed at the 90th percentile within their firm. After adding detailed controls from the administrative data, 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 between workplace rank and chronic disease prevalence becomes even stronger.

Conversely, we show that the strong association between income levels and chronic disease prevalence shrinks substantially, by 77%, after controlling for workplace rank. Using a symmetric comparison of the nonparametric distribution of disease prevalence by workplace rank percentile conditional on income percentile relative to income percentile conditional on workplace rank percentile, we show that health disparities by workplace rank are several times larger than health disparities by income. This pattern holds for every racial and ethnic group. Using detailed medical diagnoses, we show that workers at the bottom of the workplace rank distribution have substantially greater prevalence of every one of the 18 major chronic diseases that 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, even after conditioning on earnings levels. For men, there are no differences in health status by workplace rank upon entering the labor force in the early to mid-20s. The gap in disease prevalence by workplace rank widens substantially during men's 30s and 40s, and remains similar through their 50s. Among women, there are some slight differences in health status by workplace rank immediately upon entering the labor force in their 20s. The divergence among women is more salient in the top of the workplace rank distribution, which has much lower chronic disease prevalence than the middle or lower end of the distribution even by age 40, after which the lower tail diverges substantially from the middle of the distribution.

Finally, we estimate a model of exposure to workplace social status in which the health effects of exposure are age-specific and accumulative. We construct network-based instrumental variables using patterns of coworker flows between firms to isolate variation in exposure to workplace rank that is independent of idiosyncratic variation in health status. The IV estimates are slightly larger than the descriptive fixed effects estimates. We also 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 unexplained differences in health status cannot explain the differences in health status that we document.

This is the first known evidence from a broadly representative population of workers to show that social hierarchy in the workplace is a strong determinant of health, even outside of the influence of earnings. The health impacts of differences in relative workplace rank are strongest for minority workers. The overrepresentation of minority workers in low-ranked jobs amplifies racial inequality, compounding income and resource-based inequality with health inequality that operates through social hierarchy in the workplace.

2 Literature

The literature on relationships between socioeconomic status (SES) and health is exceptionally large and spans many fields outside of economics and sociology. 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 childhood (Adda et al., 2009; Baird et al., 2013; Adda and Fawaz, 2020; Lenhart and Chey, 2017; Case et al., 2005, 2002).

Here we focus our attention on a specific component of SES: relative position in one social hierarchy, the workplace. Given the extensive literature on relationships between income 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.

Even within this relatively narrow question, substantial prior research has been conducted. The most influential studies of the relationship between social status 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, worse diets, higher rates of disease across many categories of illness, and higher mortality rates. Case and Paxson (2011), however, revisit the Whitehall II study and show that the implication of a causal relationship between employment grade and health is problematic, because within the Whitehall sample there are substantial differences in the height of workers in different employment grades, which is strongly 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 known to be associated with later-life health, this confounds the interpretation of the evidence by impacting both sides of the equation of interest. 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 (Marmot, 2004; Wilkinson, 2005). This hypothesis has broader empirical support. Shively and Clarkson (1994) ran a study in which they experimentally changed the relative social position of monkeys while holding fixed all of the 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.[4] 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) show that the evidence does not support this argument.

More recently, studies in economics have used natural experimental variation to estimate the effects of social status on mortality, and have found convincing evidence of a causal relationship in many specific contexts. Borgschulte and Vogler (2019) uses a regression discontinuity design to study the effects of winning a political election (state Governor, US Senate, or US House) and finds that over the past century, US 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.[5]

Suandi (2021) studies the effects of promotion on life expectancy of US Navy sub-

[4]Studies of 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).

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

mariners 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 torpedos 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, and they attribute this to negative effects of narrowly missing induction.[6]

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 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 between specific and broad domains in this literature.

To date, there is no known empirical evidence from a broadly representative (human)

[6] Abel and Kruger (2010) compare Hall of Fame inductees to all other players and find a negative association between induction and life expectancy.

population that has documented a clear connection between social status and health status that operates outside of an income 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) from 2013-2015, linked to a custom aggregated earnings file constructed by the Utah Department of Workforce Services using administrative unemployment insurance earnings records. We describe these files in detail, 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 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 dates of

[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 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.

enrollment 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. This file contains dates of coverage for all plans regardless of whether the insurer covered any claims under the plan.

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. Missing information suggests that insurers do not know this information. Data on race come from a set of three race variables in the data, allowing enrollees to report multiple races. Ethnicity is similarly recorded. The data also contain a separate field to indicate whether the individual is of Hispanic, Latino, or Spanish origin. 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 we 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 very similar to the overall Utah demographic profile based on ACS data.

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

[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 also include drug codes, fill dates, quantities, refills, days supplied, dispensing fees, and pharmacy identifiers and locations.

We run all of the available claims data 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, which we will use as a measure of the underlying health of individuals.

The second primary data source used for this study is a custom earnings file produced by the Utah Department of Workforce Services, and linked by anonymized individual identifiers to the ACPD. This earnings file is constructed in a way that aggregates information to balance individual privacy while still providing informative longitudinal quarterly data on earnings from all UI-covered 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 grouped workers into permilles (1000-quantiles) according to the worker's total earnings from all jobs, and then reported 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. 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. This 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) find that linked survey responses in the SIPP indicate earnings levels that are on average 10-30% different than administrative W-2 earnings for the same person, and differences in average measurement error rates are correlated with demographic characteristics like 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 A. 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.

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 the within-firm ranks are reasonably measured.

The monthly enrollment files in the APCD record the zip codes 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 in models in which we 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, and 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 of exact ties we randomly select assignment.

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.

In some analyses we focus on variation from job changes. For these analyses, we define a sample of job movers, which includes workers who experience an employer-to-employer move with no missing quarters between the jobs.[10] The job mover sample is restricted to workers employed at the origin firm and at the destination firm for at least three months each. To avoid censored job durations at the endpoints of our study period we use additional years of earnings records from 2012 and 2016 to determine compliance with this sample restriction.

[10]Since we do not observe the exact date of the move, it is possible that a worker experiences an unemployment spell of no more than two quarters.

3.3 Descriptive Statistics

Table 1 shows descriptive statistics of the data. In Column 1 we report summary statistics for the full population of interest, individual who held a UI-covered job in Utah between 2013-2015 and also held commercial health insurance at some point during this period. Column 2 reports statistics for our primary analysis sample, after imposing the sample restrictions described above. In column 3, we report summary statistics for the job mover subset of the analysis sample.

Table 1: Descriptive Statistics

	(1) Employed With Insurance	(2) Main Sample	(3) Mover Sample
Demographics			
Age	38.7	38.9	34.5
Fraction Male	0.54	0.53	0.54
Race/Ethnicity			
White, Non-Hispanic	0.76	0.77	0.76
Black	0.015	0.014	0.016
American Indian	0.017	0.016	0.015
Other	0.081	0.080	0.079
Hispanic (Any Race)	0.18	0.17	0.17
Fraction with Recorded Race/Ethnicity	0.74	0.74	0.76
Health Status			
At least 1 chronic condition	0.43	0.43	0.38
Number of chronic conditions	1.07	1.04	0.82
Health Insurance Type			
Employer-Sponsored	0.70	0.81	0.80
Employer-Sponsored as Subscriber	0.44	0.54	0.49
Non-Group	0.067	0.059	0.057
Medicaid	0.039	0.038	0.036
Earnings			
Average Quarterly Earnings	11,378.6	12,269.1	10,405.1
Firm			
Average Number of Employees	3,098.8	3,481.3	2,430.1
Number of Persons	1,188,992	1,021,974	147,215
Number of Person-Quarters	10,835,595	8,304,220	1,471,707

The population of interest has similar racial and ethnic distributions as the state overall, based on comparisons to Census data. For example, 1.4% of our sample is Black,

compared to 1.2% in the U.S. census in 2010. While this is a minority of the Utah population, the black population in our study still exceeds 13,000 individuals. In column (2), the main analysis sample has a slightly higher income, in part due to the sample restrictions that ensure a minimum labor force attachment, is more likely to be the primary beneficiary on their employer-sponsored insurance plan, and is slightly more likely to be White. The job movers sample in column (3) is younger, healthier, and has lower quarterly earnings than our main sample, consistent with general patterns of job mobility in the literature.

4 Descriptive Evidence on Within-Firm Rank and Health Disparities

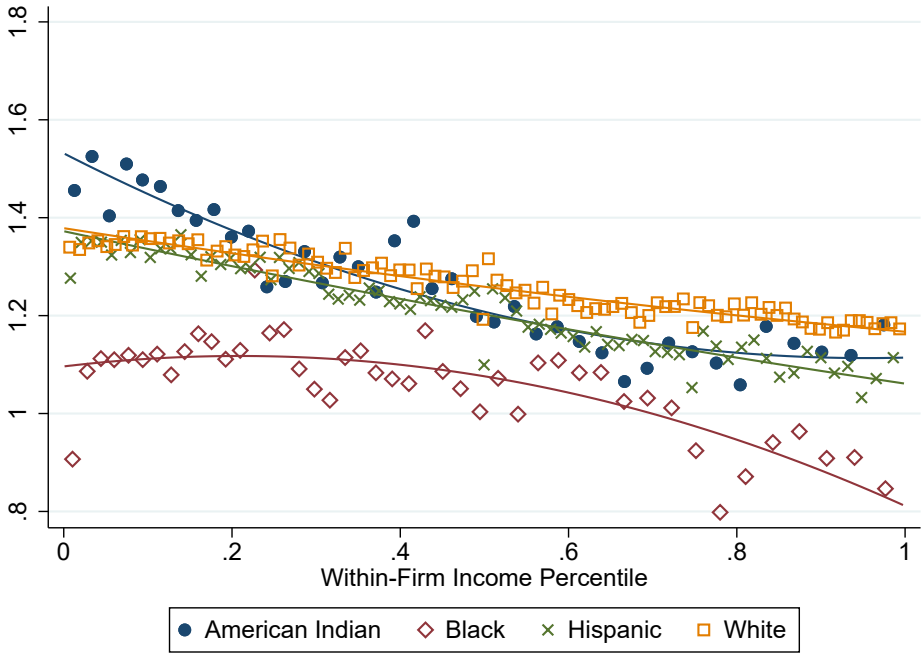
We begin by documenting the conditional relationships between within-firm relative rank and health status. In estimating this relationship, we improve on the previous literature in several ways. First, we observe workers at about 50,000 firms with different earnings distributions, in addition to the relative positions of each worker within each firm over time. In contrast, the Whitehall studies focused on civil servants' employment grade, or rank within British civil servant classes, 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.[11] We are able to exploit the between-firm variation in rank while holding income constant in a set geographic area where such income controls are meaningful.[12] Combining both within and across-firm variation allows us to compare workers who earn the same salary but have different relative earnings ranks within their firms.

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 for total earnings levels (from all jobs). The conditional means are constructed using the semiparametric partial linear regression approach developed in Cattaneo et al. (2023). For every demographic

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

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

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 for total earnings levels. The conditional means are constructed using the semiparametric partial linear regression approach developed in Cattaneo et al. (2023).

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 0.24 (16.3%) fewer chronic health conditions than workers of the same age in the bottom percentile of the distribution.

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 we

re-estimate Figure 1 including controls for as many of these factors as is feasible with our data. Specifically, we condition on total earnings, the full set of interactions between each year of age and each zip code of residence, gender, year, fixed effects for each health insurance policy, employer size, and job tenure.[13] 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 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. Insurance plan effects may also absorb some differences in demand-side factors like risk preferences or latent health status differences that influence plan choices.

Figure 2 shows that the relationship between poor health and employment rank not only remains strongly negative after conditioning on all of these factors, the average strength of the conditional relationship increases with the inclusion of the large set of controls. To understand the role of these other factors in the relationship, we decompose the changes in health disparities that can be explained by the control variables. Since the relationships in Figures 1 and 2 are nonlinear there are many different ways of comparing slopes. In Table 2 we report differences in chronic disease rates between the 90th and 10th percentiles of employment rank by race.[14]

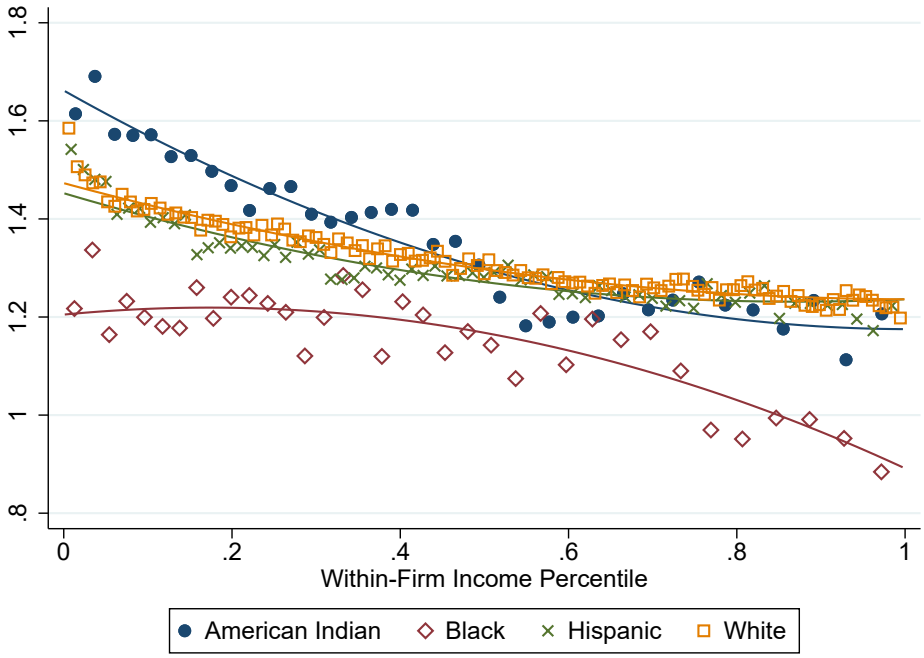
Conditional on age and earnings, White workers at the 10th percentile of employment rank have on average 0.169 more diagnosed chronic conditions than workers at the 90th percentile, or about 13.4% of the overall mean chronic condition prevalence. This baseline gap is almost twice as large for American Indians, 0.334, consistent with the much more negative relationship visible in Figure 1. Among Black and Hispanic workers, the baseline gaps are 0.228 and 0.249, respectively, each more than one-third larger than the gap for White workers.

In rows two through five, we quantify the average marginal impact of adding additional sets of control variables to the model on the 90-10 percentile gap. These measures are

[13] 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).

[14] More precisely, to reduce the impact of noise in the percentile-race-specific cells, we report the 90-10 gap in the fitted quadratic functions shown in Figure 2.

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 total earnings levels, 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).

Table 2: Impacts of Control Variables on the 90-10 Percentile Gap in Chronic Conditions

	American Indian	Black	Hispanic	White
Baseline 90-10 Gap (Figure 1):	0.334	0.228	0.249	0.169
Location	-0.047	0.032	0.001	-0.006
Social and Demographic Controls	0.002	0.014	0.022	0.019
Health Insurance Policy FE	0.071	0.015	-0.072	0.021
Firm/Job Characteristics	0.029	-0.039	-0.028	-0.013
Fully Conditional 90-10 Gap (Figure 2):	0.389	0.251	0.173	0.189

Notes: This table reports differences in chronic disease rates between the 90th and 10th percentiles of employment rank by race. We include zip codes, gender, and year as our social demographic controls. We use policy group ID fixed effects as the policy ID control. Firm features contain the average firm size and average tenures within a firm. We calculate all the possible permutations of orders by adding the three categories and report the average changes.

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.[15] Controlling for location through zip code effects has disparate impacts on the chronic condition gap across races. Removing the impact of location reduces the gap by 0.047 for American Indians, but increases it by 0.032 for Black workers. On average, controlling for age, gender, and year tends to increase the health-rank gradient modestly. Adding fixed effects for each health insurance policy has large and disparate impacts across races. These controls increase the health-rank gradient for American Indian workers by 0.071 (21% of the baseline gap), but narrow it substantially, by 0.072, for Hispanic workers.[16] Controlling for firm size and tenure tends to decrease the health-rank gradient by 0.013 to 0.039 for all groups except American Indians, for whom the gap increases by 0.029.

On aggregate, rather than explaining away the negative relationship between employment rank and chronic conditions, the controls tend to increase the relationship modestly

[15] As a consequence, the sum of the average marginal effects may differ slightly from the difference between the baseline and conditional gaps.

[16] A potential reason why the impact of controlling for insurance policy effects may differ for American Indians is because of the unique, and separate, healthcare system provided to many American Indians in the US. Since 1955, American Indians and Alaskan Natives have received care at separate federally-run hospitals and other health care facilities operated by the Indian Health Services, which only provide care for eligible members and have zero out-of-pocket costs, regardless of income, for care provided at these facilities. As a consequence, the nature of selection into formal health insurance policies may be different for American Indians.

for the average worker. The gap increases by between 10%-16% for Black, White and American Indian workers, but decreases by over 30% for Hispanic workers, primarily because of the influence of differences in health insurance policy assignment for Hispanics.

4.1 The Relative Importance of Workplace Rank

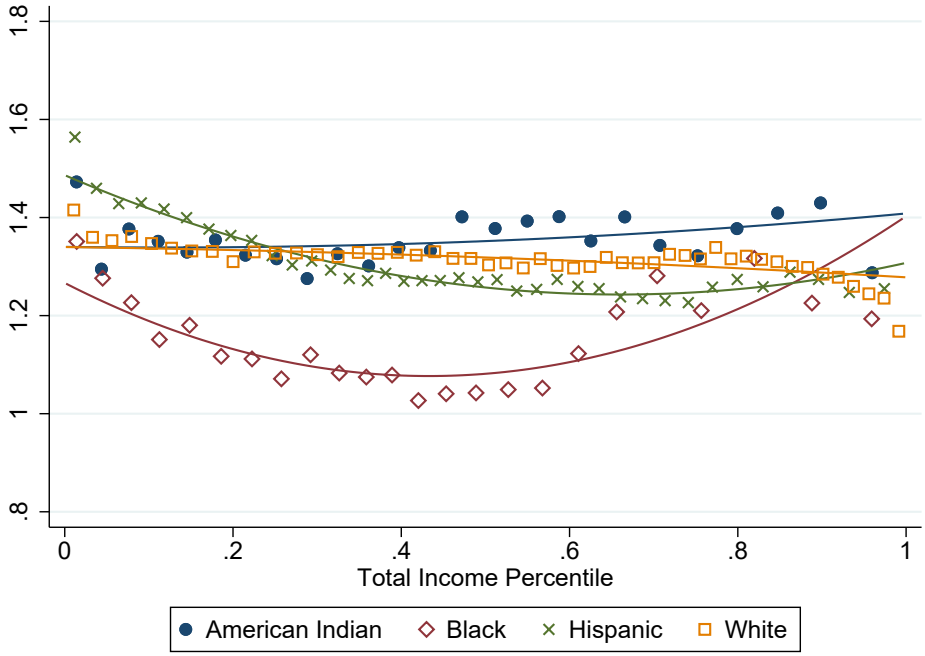
There is extensive evidence of large disparities in health outcomes associated with socioeconomic factors like earnings and education. For example, Chetty et al. (2016) estimates that the gap in average life expectancy between people in the top and bottom 1% of household earnings in the US is 14.6 years. 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 also stronger than the association between earnings and health status.

To begin, in Figure 3 we show the same semiparametric relationship between chronic condition prevalence and total earnings, controlling for the same controls as in Figure 2. In this figure, the horizontal axis is the percentile rank of each worker's total earnings in the overall earnings distribution in Utah. To parallel the design of Figure 2, here we control for within-firm percentile rank effects to isolate the pure earnings component that operates outside of the impact of within-firm ranks.

The figure shows a weaker and less consistently downward-sloping relationship between earnings levels and chronic condition prevalence than was observed in Figure 2. Quantitatively, the relationship is weaker for all four racial groups shown. We find almost zero conditional association between income and health status for American Indian workers, and evidence of a modest u-shaped relationship for Black workers. And although there is a substantial literature on the paradoxically small relationship between socioeconomic status and health status among Hispanic individuals, we find that the conditional rela-

Figure 3: 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 total earnings in the overall earnings distribution in Utah. Each dot is the race-specific conditional mean chronic condition count in a given bin of total earnings levels, 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).

tionship between income and health status, after removing the impact of workplace rank, is stronger for Hispanic workers than for all three other demographic groups.

Of course, this is not to suggest 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 workplace rank and earnings are highly correlated. Specifically, in our analysis sample 53.7% of the variance in workers' percentile ranks in the overall earnings distribution is explained by their within-firm workplace ranks. Figure 3 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.

To more clearly visualize the differences in the conditional relationships between health status and workplace rank versus health status and earnings, in Figure 4 we pool the four demographic groups together and show the combined relationships. The conditional relationship between health status and workplace rank is clearly stronger than the relationship

between health status and earnings for most of the distribution, with the exception of the extreme upper tail of the earnings distribution.

Figure 4: 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) or their total 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 total 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. For both graphs, the conditional means are constructed using the semiparametric partial linear regression approach developed in Cattaneo et al. (2023).

To further help disentangle the relative importance of each component, we estimate the same semiparametric relationship without controlling for within-firm rank, and find that the 90-10 gap in chronic disease prevalence by total earnings is 0.283. Controlling for within-firm rank reduces this gap to 0.065. This suggests that, by this measure of health outcomes, 77% of the conditional relationship between earnings and health outcomes 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 58%, from 0.303 to 0.127, when within-firm rank is added to the model.

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, in addition to interactions between race indicators and socioeconomic variables including quarterly earnings in levels, within-firm earnings percentile rank, and within-firm inequality measured using the Gini coefficient. Our interest is in understanding the relative importance of these socioeconomic factors in explaining the racial gap in chronic disease prevalence, conditional on included controls.

Specifically, we estimate the following regression model:

$$y_{it} = \sum_r \beta_{1r} c_{it} + \sum_r \beta_{2r} c_{it}^2 + \gamma x_{it} + \theta_r + \theta_{az} + \theta_p + \varepsilon_{it} \tag{1}$$

where y_{it} is the count of chronic conditions for worker i in quarter t, c_{it} is a vector that includes worker i's percentile rank in the overall state earnings distribution, their within-firm earnings percentile rank, and the firm-level Gini. β_{1r} is the corresponding vector of race-specific coefficients, which we model as fixed. Similarly, β_{2r} contains the coefficients on the squared terms in c_{it}. x_{it} is a vector of observed worker and firm characteristics including gender, firm size, and years of tenure. θ_r, θ_{az}, and θ_p are fixed race effects, age-by-zip code effects, and health insurance policy ID effects, respectively.

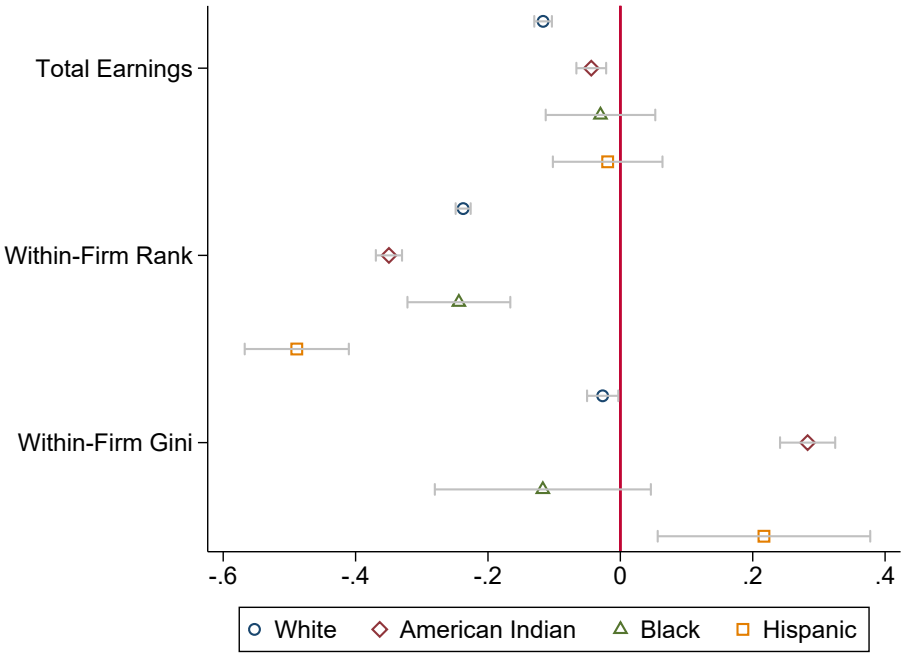
Figure 5 shows the estimated average marginal effects of each element of c_{it} on y_{it}, by race. The average marginal effect of c_{it} in Equation 1 is equal to $\beta_{1r} + 2 * \beta_{2r} c_{it}$.[17] The coefficients on earnings percentiles are scaled so that they represent the marginal effect of moving between the lowest and highest percentiles of the corresponding distribution. As the figure shows, for the average White worker in the sample, moving from the top to the bottom of the earning distribution, while controlling for the impacts of within-firm percentile rank, age-by-location effects, health insurance effects, firm Gini coefficients, and x_{it}, is predicted to increase the number of chronic conditions by 0.117, or 11.2% of the overall mean count of chronic conditions among all workers in the sample. Among minority workers, the estimated effects are smaller, 0.019 for American Indians and statistically insignificant for Black and Hispanic workers.[18]

The small and insignificant relationship between income levels and chronic conditions for Hispanic workers is consistent with the large literature on the "Hispanic paradox,"

[17] Note that for this model specification the marginal effect at the means is equal to the average marginal effect.

[18] The full set of coefficients and standard errors are reported in Appendix Table 1.

Figure 5: Strength of Relationships between Work Outcomes and Health Status, by Race



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

which finds that health outcomes for Hispanics are frequently comparable, if not better, than those of non-Hispanic White individuals despite differences in income and other socioeconomic factors. This literature also finds that health status is less sensitive to socioeconomic factors such as income and education for Hispanics. The weak relationship for Black workers is in part explained by the u-shape of the relationship between earnings and health status that appears to be most pronounced for Black workers in Figure 3.

For every race and ethnic group, the average marginal effect of a change in within-firm percentile ranks is larger than the effect of a change in salary levels. On average, across all races, the average marginal effect of within-firm rank is a remarkable 2.2 times larger than the average marginal effect of earnings. Among White workers, moving from the top to the bottom of the within-firm earnings distribution, conditional on earnings levels and other controls, is predicted to increase the number of chronic conditions by 0.24, or 22.7% of the overall sample mean. Estimates are similar for Black workers, 0.24. However, the effects are meaningfully larger for American Indians and Hispanic workers, 0.35 and 0.49, respectively.

To provide further perspective on the relative magnitude of the within-firm rank coef-

ficients, we also show the relationships between workplace earnings inequality and health status, although this is not a central focus of the current paper. The average marginal effect of an increase in the firm-level Gini coefficient from 0 (perfect equality) to 1 (perfect inequality) is slightly negative for White workers, but significantly positive for American Indians and Hispanic workers, with coefficients of 0.28 and 0.22, respectively. The coefficient is the most negative for Black workers, though it is statistically insignificant.

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 3 we show that the negative relationship between workplace rank and health status holds for every one of the chronic medical conditions we study. We consider all 18 chronic conditions for which the ACG software constructs disease indicators using medical claims, diagnoses, and prescription drug purchases. In column 1 we report 90-10 disease prevalence gap my chronic condition in levels. To compare the relative size of the gap, 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 every disease, there is substantial heterogeneity in the ratio. One-third of the conditions have ratios exceeding 1.25, while 5 have ratios below 1.10.

The patterns of difference in disease rates by workplace rank are even more striking when visualized. In Figure 6 we show the relationships between disease rates and rank for a sample of diseases using the same method as in Figure 2, controlling for total earnings levels, 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 each of the 18 chronic diseases that we use in our aggregate measure of health status.

These results help corroborate the breadth of the effects of workplace social position

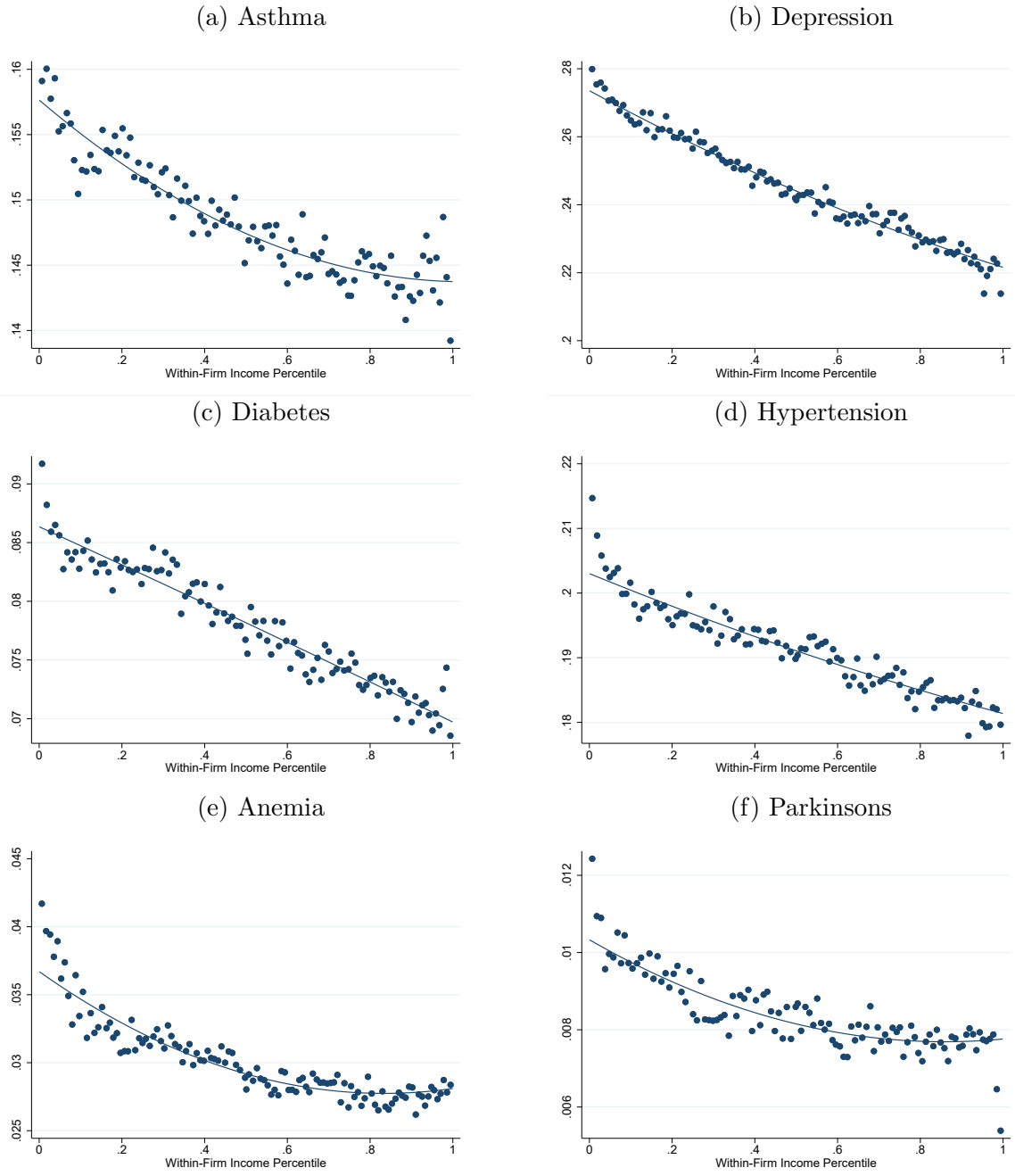
Table 3: Within-Firm 90-10 Percentile Gap by Chronic Condition

	$p_{10} - p_{90}$	p_{10}/p_{90}
Congestive Heart Failure	0.001	1.341
Bipolar	0.004	1.305
Schizophrenia	0.001	1.273
Parkinsons	0.002	1.267
Chronic Obstructive Pulmonary Disease	0.005	1.266
Seizures	0.009	1.259
Anemia	0.007	1.248
Ischemic Heart Disease	0.004	1.189
Diabetes	0.013	1.186
Depression	0.042	1.184
Osteoporosis	0.002	1.141
Lower Back Pain	0.027	1.127
Arthritis	0.001	1.109
Hypertension	0.017	1.095
Glaucoma	0.002	1.089
Asthma	0.011	1.077
Hyperthyroidism	0.006	1.067
Macular Degeneration	0.000	1.037

Notes: For each chronic condition, we conduct the semiparametric partial linear regression approach developed by Cattaneo et al. (2023), similar to Figure 2, where we control for total income percentiles, age × zip code fixed effects, gender, year, plan ID fixed effects, average firm size, and average tenure within a firm. We obtain the 10th and 90th percentiles from the fitted polynomials and calculate their differences and ratios.

on health status. But what mechanisms are potentially consistent with such broad-based effects on chronic disease rates? One potential explanation that is consistent with this evidence is chronic stress. The idea that chronic psychic distress can predispose people to many forms of illness has been discussed for centuries, but evidence in recent decades has more exhaustively connected stress to immunology, neurology, and endocrinology, and has shown that chronic stress causes clinically important levels of immunosuppression. The immunosuppresive and imflammatory responses to chronic stress can therefore be a risk factor for a very broad set of medical conditions. The medical literature has connected chronic stress to each on of the chronic conditions in Table 3. 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.

Figure 6: Diagnosis Rates by Condition vs Within-Firm Income Percentile
Full Set of Controls

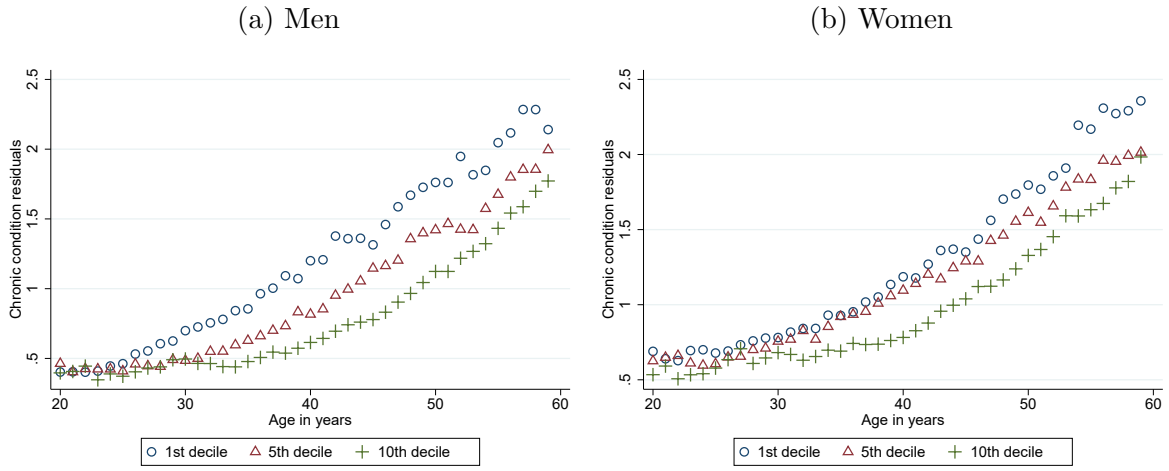


Notes: This figure shows the joint distribution of chronic condition prevalence and workers' percentile earnings rank within their primary employer. Each dot is the conditional mean chronic condition rate in a given bin of within-firm ranks, controlling for total earnings levels, age × zip code fixed 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).

5 Firm Rank and Health Production over the Life-cycle

Since all health conditions we consider are related to the gradient in workplace rank, a critical question is uncovering when this relationship appears. 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 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 human capital, including health status, affects workplace rank via job assignment, even after conditioning on earnings.

Figure 7: Evolution of Chronic conditions by Age across firm ranks



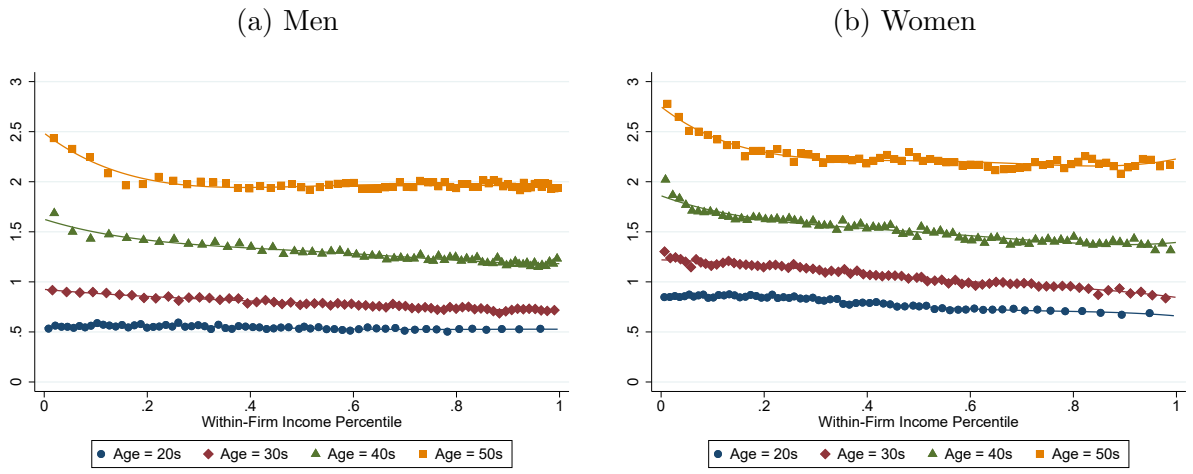
Notes: The figures show the life-cycle health production patterns of workers at different deciles of the workplace rank distribution for men and women, separately. The residuals are constructed by regressing the number of chronic conditions on zip code fixed effects, gender, year, plan ID fixed effects, average firm size, and average tenure within a firm.

Figure 7 shows the life-cycle health production patterns of workers at different deciles of the workplace rank distribution.[19] For male workers in Figure 7a, the average number of chronic conditions is approximately the same for workers in their 20s at different deciles of

[19]The figures control for x_{it}, average firm tenure and log firm size, and $\theta_s, \theta_t, \theta_z, \theta_p$, base salary percentile, year, zipcode, and policy ID fixed effects, respectively.

workplace rank. The disparity in health status grows sharply during the 30s, particularly at the lower end of the workplace rank distribution, between the 1st and 5th deciles. The gap continues expanding among workers in their 40s, including a widening gap at the upper end of the distribution, between the 5th and 10th deciles. Finally, among workers in their 50s the pre-existing health disparities remain relatively consistent. An alternative way of expressing the disparities in the lifecycle evolution of health is that it takes about 13 additional years for a man to reach one expected chronic condition if they work in the top decile of workplace rank, relative to a man with the same earnings level, who lives in the same zip code, but is in the bottom decile of workplace rank. This 13 year gap is comparable in magnitude to the 14.6 year life expectancy gap across earnings quantiles documented by Chetty et al. (2016).

Figure 8: Number of Chronic Conditions vs Within-Firm Income Percentile By Age in Decades, By Gender



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

For women (Figure 7b), there is some weak evidence of differences in health status by workplace rank among young workers in their 20s. Female workers with more chronic conditions are more likely to end up in jobs with lower workplace rank, potentially consistent with either gender differences in labor supply behavior (eg. part-time work rates) as a function of health, or gender differences in labor demand behavior by worker health (eg. health-based discrimination in job assignment).[20]

[20]We investigate patterns of labor force participation rates by sex, age, and health status in Appendix Figure 3. We find that among young workers in their 20s or 30s, the responsiveness of labor force

In Figure 8 we show the semiparametric conditional means across the distribution of workplace rank for each decade of age. Again we find roughly zero relationship between within-firm rank and health status among young men in their 20s. The slope of the relationship between workplace rank and health status increases through exposure to workplace rank during workers' careers. Among young women in their 20s (Figure 8b), there is again some weak evidence of a relationship between chronic conditions and within-firm rank.

In Figure 8, the ratio of average chronic conditions for men at the 10th percentile of workplace rank to those at the 90th percentile increases from 0.045 among workers in their 20s, to 0.164 in the 30s, 0.290 in the 40s, and then declines to 0.150 in the 50s. For women, the same ratios are 0.194, 0.296, 0.328, and 0.233. In Appendix Figure 6 we show similar figures separately for each race and gender combination. Qualitatively, these figures show that for White, Black, and Hispanic men there is nearly zero relationship between workplace rank and health status in the 20s, but for all female groups and for male American Indians there is some weak evidence of a relationship among young workers.

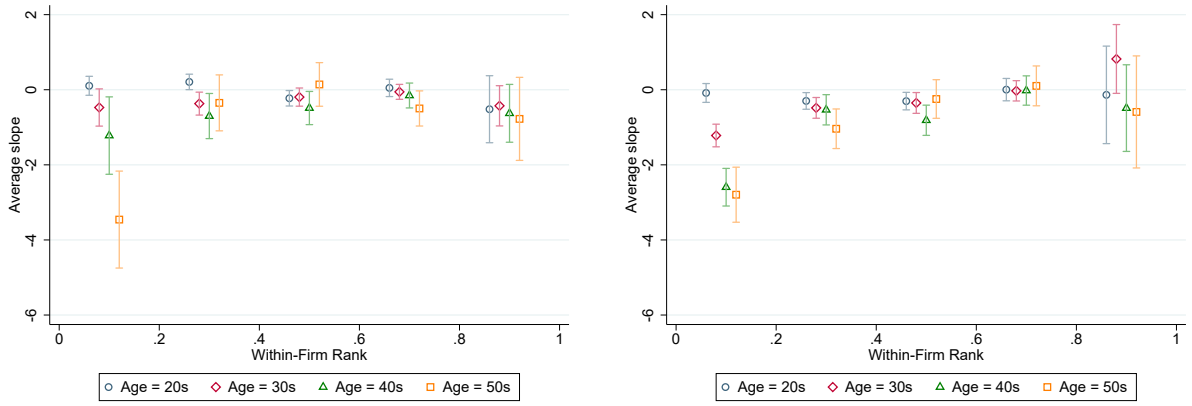
To more precisely visualize the slopes of these relationships, in Figure 9 we show the average marginal effect of workplace rank on health by gender, race, and quintile of the workplace rank distribution.

participation to health status is stronger for men than it is for women. At older ages, the patterns by sex are very similar, with men tending to have roughly uniformly higher labor force participation rates at all health status levels.

Figure 9: 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.

6 Model of Exposure to Workplace Social Status

In this section we consider a model with a similar specification as our descriptive model, but we recast it as a model of exposure over workers' careers to different relative workplace rank treatments. We use this model to characterize the conditional effects on health status of exposure to treatment for a given duration, using an approach similar to Chetty and Hendren (2018). We then describe the research design we use to estimate these effects.

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 over time as people 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 the magnitude of health status deterioration. We begin by considering the model:

$$y_{it} = \eta_r p_{it} + \beta_r wp_{it} + \theta_{z(i,t),c(i),r(i)} + \psi_{j(i,t)} + \theta_{p(i,t)} + \theta_t + \theta_{g(i)} + \varepsilon_{it} \tag{2}$$

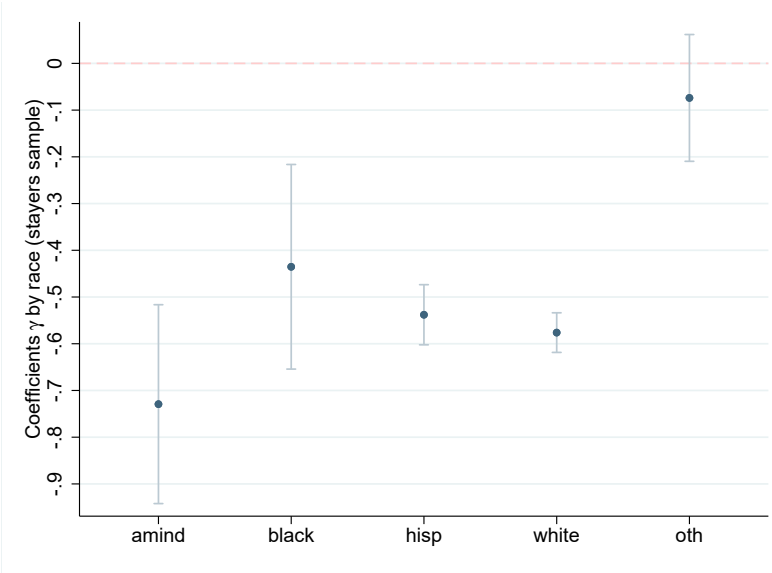
where p_{it} is worker i's percentile in the distribution of statewide earnings in quarter t, wp_{it} is the worker's earnings percentile within their firm, $\theta_{z(i,t),c(i),r(i)}$ is a fixed zip code by birth cohort by race effect, $\psi_{j(i,t)}$ is a fixed employer effect, where the subscript $j(i,t)$ indexes firm j at which worker i is employed in quarter t. $\theta_{p(i,t)}$ is a fixed health insurance plan contract effect. Within an insurance contract, all aspects of the insurance plan, including networks, coverage rules, insurer identity, and cost-sharing rules are identical. θ_t and $\theta_{g(i)}$ are quarter-year and sex effects, respectively.

We estimate Equation 2 on the set of workers who are job stayers by restricting the sample to those 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 10 presents estimates of $\widehat{\beta}_r$ from Equation 2. The estimated coefficients imply that among job stayers, American Indian workers at the bottom percentile of the wp distribution have 0.72 more chronic conditions than those at the top of

the wp distribution. For Black, Hispanic, and White workers the corresponding gaps are 0.43, 0.53, and 0.58, respectively.

Figure 10: Estimates of $\widehat{\beta}_r$ from Equation 2



Notes: Estimates are based on the sample of job stayers at firms with at least 50 total workers for whom $wp_{it}^{max} - wp_{it}^{min} < 0.15$ during the three-year panel spanning 2010-2012. Other races include Asian and Pacific Islanders, which account for less than 0.5% of the sample.

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:

$$y_i = \alpha_m + \beta_m \overline{y}_{rwp} + \varepsilon_i \tag{3}$$

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_i of moving at age m to a job such that the outcomes of job stayers predict that worker i would have one additional chronic condition due to exposure to wp_{it}. ε_i 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 ε_i 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

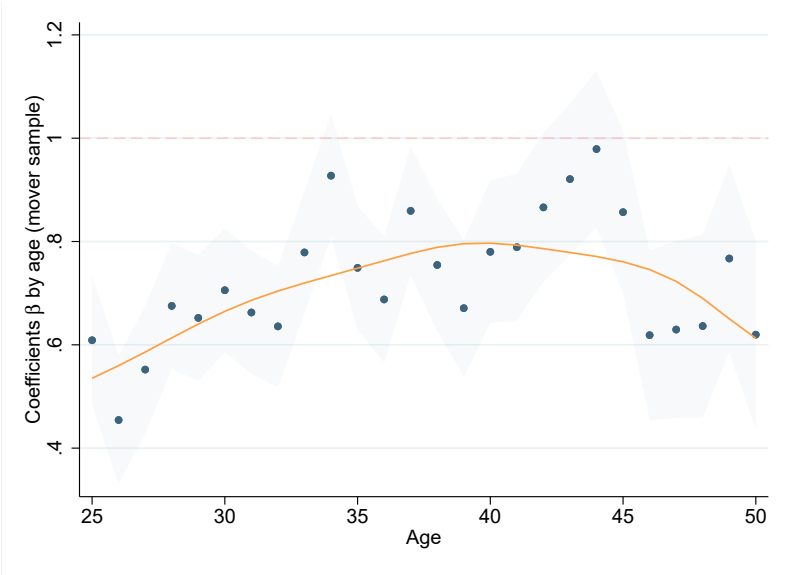
with workplace rank wp. We define the effect of a single year of exposure at age m as $\gamma_m \equiv \beta_m - \beta_{m+1}$.

Of course, job assignment is unlikely to be random, which we discuss below. However, we first present estimates of β_m and γ_m. These estimates are informative of several important features of the data generating process. 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, the more likely scenario is $0 < \beta_m < 1$, in which case β_m tells us how much of the difference in health status associated with wp among job stayers is imparted on jobs movers.

Estimates of $\widehat{\beta_m}$ from Equation 3 are shown in Figure 11. We find that $\widehat{\beta_m}$ is around 0.6 for workers in their late 20s, increases to a peak of 0.8 for workers around age 40, and then declines back to between 0.6 - 0.7 for workers in their late 40s. Because $\widehat{\beta_m}$ is consistently below 1 at all ages, this suggests that there are some impacts of nonrandom selection that cause the patterns of job assignment of movers to differ from that of stayers. However, $\widehat{\beta_m}$ is also much greater that 0, indicating that there are substantial treatment effects of exposure to workplace rank, and that movers experience a treatment effect that is around 60%-80% of the health differences by workplace rank of stayers.

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

Figure 11: Estimated β_m from Equation 3



Notes: The dots in the figure represent the estimates of $\widehat{\beta_m}$ from Equation 3. The shaded area is the 95% conference interval, and the solid line is the locally estimated scatterplot smoothing fit of the estimates.

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

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

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

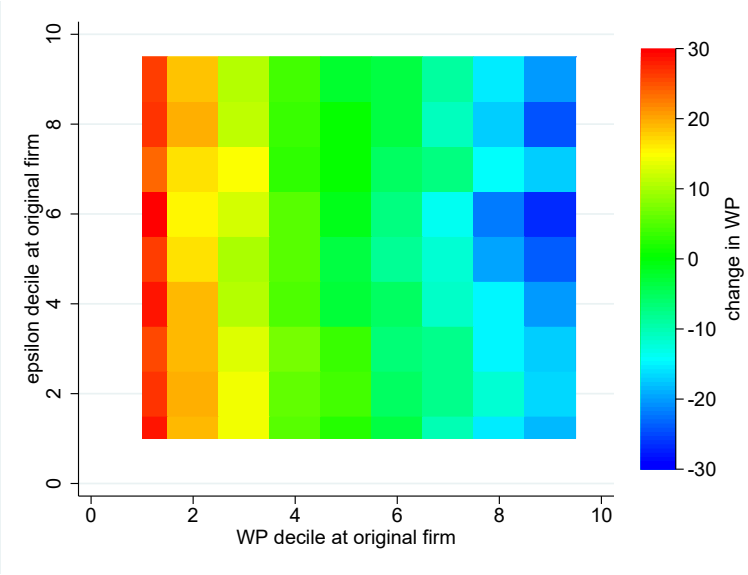
What do we know about this selection term? First, suppose it is reasonable to assume that $\frac{cov(\epsilon_i, \overline{y}_{rwp})}{var(\overline{y}_{rwp})}$ is the same on average for workers who have nearly the same age. That is, even though there is non-random sorting in the labor market that may 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 3 to estimate $\widehat{\gamma_m} = \widehat{\beta_m} - \widehat{\beta_{m+1}} = \beta_m + \frac{cov(\epsilon_i, \overline{y}_{rwp})}{var(\overline{y}_{rwp})} - \beta_{m+1} - \frac{cov(\epsilon_i, \overline{y}_{rwp})}{var(\overline{y}_{rwp})} = \beta_m - \beta_{m+1} = \gamma_m$. This assumption therefore implies that the slope of the age-based function in Figure 11 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, $\widehat{\varepsilon_i}$, and the change in wp_{it} associated with the job move, conditional on the origin job wp_{it-1}.

Figure 12: Change in Workplace Rank for Job Movers
by $\widehat{\varepsilon_i}$ 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 $\widehat{\varepsilon_i}$.

Figure 12 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 $\widehat{\varepsilon_i}$, and each cell is color-coded to depict 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 0.3, dark blue cells indicate downward moves of -0.3, and green 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 $\widehat{\varepsilon_i}$ conditional on the origin job 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.

If sicker workers were disproportionately less likely to move into high ranked jobs, for example, 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, with the possible exception of workers who begin at jobs in the top decile of workplace rank. In the top right corner,

when highly ranked sicker workers move jobs they tend to move around 0-5 percentage points further down the workplace rank distribution than highly ranked healthy workers who change jobs. Overall, the full distribution provides diagnostic evidence suggestive that the selection component of the observational patterns of job mobility are likely to have small impacts on $\widehat{\beta}_m$.

Table 4: A Test of Selection on Future Workplace Rank

	(1)	(2)
Prior Rank * American Indian	-0.269	-0.227
	(0.250)	(0.249)
Prior Rank * Black	-0.006	0.017
	(0.193)	(0.193)
Prior Rank * Hispanic	-0.200***	-0.211***
	(0.041)	(0.040)
Prior Rank * White	-0.291***	-0.308***
	(0.028)	(0.028)
Prior Rank * Other Races	-0.211*	-0.210*
	(0.120)	(0.120)
Future Rank * American Indian	0.007**	
	(0.003)	
Future Rank * Black	0.005***	
	(0.002)	
Future Rank * Hispanic	-0.001***	
	(0.000)	
Future Rank * White	-0.001***	
	(0.000)	
Future Rank * Other Races	-0.001	
	(0.001)	
Observations	311,981	311,981

Notes: The table shows the estimates from Equation 4, where we control for race-specific within-firm ranks and total earnings, and also for race × cohort × zipcode, gender, year, policy ID, and firm fixed effects. In Column 1, we additionally control for the race-specific post-move future ranks.

A second diagnostic test to quantify the impact of potential selection on the estimate of $\widehat{\beta_m}$ is to restrict the job mover sample to only the pre-move period and estimate:

$$y_{i\tau-} = \eta_r p_{i\tau-} + \beta_r^- w p_{i\tau-} + \beta_r^+ w p_{i\tau} + \theta_{z(i,\tau-),c(i),r(i)} + \psi_{j(i,\tau-)} + \theta_{p(i,\tau-)} + \theta_{\tau-} + \theta_{g(i)} + \varepsilon_{i\tau-} \quad (4)$$

where τ is the period in which the worker changes jobs, and $\tau-$ refers to all quarters

prior to the move. β_r^- is the coefficient on workplace rank prior to changing jobs, and β_r^+ measures the relationship between future workplace rank (measured in the first full quarter of work post-move) and pre-move health, $y_{i\tau-}$. Since this model is estimated using only pre-move data it is not possible for a worker's future exposure to impact their current health unless there is selection that is not accounted for by the model covariates, so $\widehat{\beta_r^+}$ is a direct measure of the selection effect.

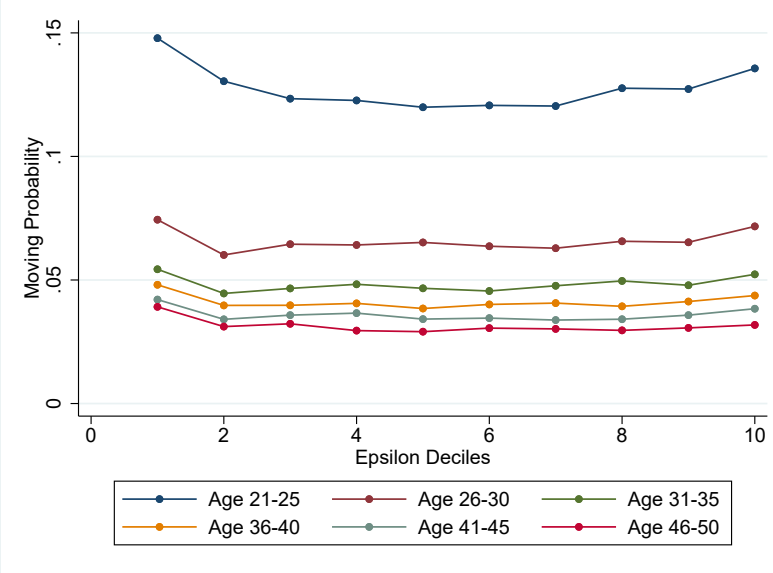
Table 4 reports estimates from Equation 4 in Column 1. All of the estimated coefficients β_r^+ on future rank are very small in magnitude. The largest coefficient in absolute value is 0.007, which is less than 1% of the of the corresponding β_m from Figure 11, and 2.6% of the corresponding within-sample β_r^-. This suggests that $wp_{i\tau}$ is uncorrelated with $\varepsilon_{i\tau-}$. Additionally, Column 2 shows that within the same sample the estimates of β_r^- remain very similar when future workplace rank $wp_{i\tau}$ is excluded from the model. This suggests that the dimensions upon which workers sort when changing jobs are not related to the error term.

A final selection diagnostic is motivated by the discussion above about the potential for nonrandom sorting being caused by job-lock, which describes the scenario in which sicker workers are less likely to change jobs. This can occur if sicker workers depend more strongly on employer-provided health insurance, or for other reasons, including if employers discriminate against sicker workers in hiring decisions, reducing employment opportunities for these workers.

In Figure 13 we show direct evidence that such a pattern of nonrandom job mobility appears to be weak among workers in Utah. Within each 5-year age band, the probability of changing jobs is very similar in every decile of the distribution of $\widehat{\varepsilon_i}$. Of course, there are large differences in job mobility rates across age groups, particularly among young workers, but there is very weak correlation between $\widehat{\varepsilon_i}$ and job mobility rates within age groups. There is some slight evidence that the healthiest workers in the first decile of $\widehat{\varepsilon_i}$ are about 1-2 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 $\widehat{\varepsilon_i}$.[21] This evidence also helps corroborate the assumption discussed above that within narrow age bands between ages 26-50 there appears to be very minimal evidence that job mobility is correlated with the error term ε_i, supporting the identification assumption for $\widehat{\gamma_m}$.

[21] 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.

Figure 13: Job Mobility Rates by Age and Health Status



Notes: This figure shows the probability of moving across decile of the distribution of health measured by $\widehat{\varepsilon_i}$. Within each 5-year age band, the probability of changing jobs is very similar.

6.3 IV Estimates using Labor Market Network Structure

The analyses in the preceding section show that when workers move between jobs there is very little evidence that changes in relative workplace rank, conditional on income and other controls, are correlated with unmodeled idiosyncratic differences in health status. However, there remains important evidence in the literature that the initial assignment of workers to jobs of different ranks may be correlated with factors like childhood health and height (Case and Paxson, 2011). That is, the starting point prior to job moves may be endogenous. Our aim in this section is to return to the model in Equation 2 and use the information contained in the network structure of the labor market data to construct instruments for workplace rank that are independent of the health status residuals for individual workers. Because we have already estimated the relative exposure effects for stayers and movers, an estimate of either of the absolute effects that corrects for the potential endogeneity of initial rank identifies the effects for both groups. Here we focus on the sample of job movers.

The objective in constructing instruments is to find variation that is informative about wp_{it} at the origin job, but conditionally uncorrelated with unmodeled differences in individual health status contained in ε_i, which may include factors like health in childhood. There are two key features of Equation 2 and the data generating process that motivate

our construction of the instruments. The first feature is that Equation 2 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_i. The inclusion of $\psi_{j(i,t)}$ in the model makes ε_i 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.

The potential source of endogeneity that we wish to overcome is that some aspect of the process by which workers are initially assigned to jobs may be influenced by health status in a way that creates a correlation between wp_{it} and ε_i. One example is if childhood health status affects productivity and leads sicker workers to be overrepresented in jobs with lower wp_{it}, even after conditioning on earnings levels, age, geography, and the other included controls.

We begin by breaking wp into two terms: $wp_{it} = wp^O_{it} + wp^D_{it}$ where wp^O_{it} refers to the origin job (O) prior to moving, and D refers to the destination job. We construct an instrument for wp^O_{it} using information about other workers who moved to the same destination firm D, but who originated at different origin firms than worker i. Using this prior job mobility history of worker i's coworkers at the destination firm, we construct network-based instruments by first estimating:

$$wp^O_{-it} = \nu^D_{j(-i,t)} + \alpha_{j(-i,t)}\, wp^D_{-it} + u_{-it} \tag{5}$$

where $-i$ indicates that the set of workers excludes the focal worker i. wp^O_{-it} and wp^D_{-it} are the workplace ranks of coworkers $-i$ at firm D and at their respective origin firms, $\nu^D_{j(-i,t)}$ is a firm-specific intercept for the destination firm D, and $\alpha_{j(-i,t)}$ is a firm-specific coefficient that captures the average relationship between origin and destination ranks of workers who exit origin firm $j(-i,t)$ and move to firm D. In general, the $\alpha_{j(-i,t)}$ differs from zero because workers departing some origin firms are systematically upwardly (or downwardly) mobile in workplace rank. We then use this model to predict $\widehat{wp^O_{-it}}$ for a worker who arrives at destination firm D at the median wp rank: $\widehat{wp^O_{-it}} = \widehat{\nu^D_{j(-i,t)}} + \widehat{\alpha_{j(-i,t)}} * 0.5$. We estimate 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 7 are substantial.

We use $\widehat{wp^O_{-it}}$ as an instrument for wp^O_{it}. This instrument is valid because worker i and their coworkers $-i$ who moved to the same destination firm share similarities in their labor market opportunities, so $\widehat{wp^O_{-it}}$ is strongly informative of worker i's origin workplace rank. The smallest first stage F-statistic of the excluded instrument among the five race

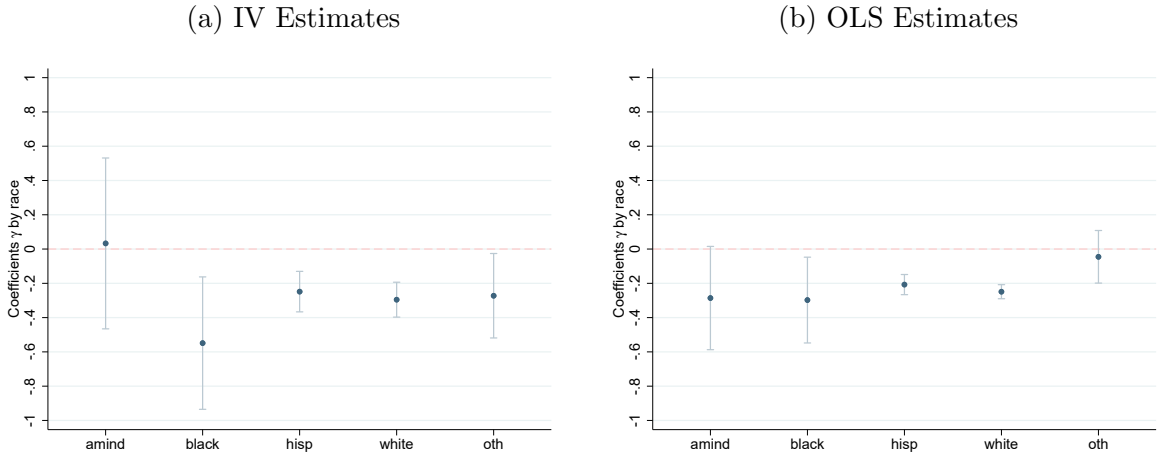
groups is 602. However, since ε_{it} is mean zero in every firm, the idiosyncratic differences in the unexplained residual component of health status of worker i's future coworkers prior to their job moves should be unrelated to worker i's residual health status prior to their move. In other words, worker i's health prior to moving is not affected by the past health of their future coworkers', suggesting that the instrument is excludable from the model.

We estimate Equation 6 by 2SLS using $\widehat{wp^{O}_{-it}}$ as race-specific instruments for wp^{O}_{it}:

$$y_{it} = \eta_r p_{it} + \beta_r wp^{O}_{it} + \beta_r wp^{D}_{it} + \theta_{z(i,t),c(i),r(i)} + \psi_{j(i,t)} + \theta_{p(i,t)} + \theta_t + \theta_{g(i)} + \varepsilon_{it} \tag{6}$$

Figure 14 shows IV estimates from this model (14a) along with OLS estimates from the IV sample (14b).

Figure 14: IV and OLS Estimates of β_r in Job Movers Sample



Notes: Figure (a) shows the IV estimates for each race from Equation (6) using the mover sample. Figure (b) plots the OLS estimates of the same model. The bands are 95% confidence intervals.

We find that in the job movers sample there is a significant negative effect of workplace rank on health status for every racial and ethnic group except American Indians, where the estimate is imprecise and has a large confidence interval. Black workers have the strongest negative effect: workers at the bottom of the workplace rank distribution have on average 0.55 more chronic health conditions than comparable black workers at the top of the distribution. Hispanic, White, and Asian workers all have very similar patterns of effects. For these groups, moving from the top to the bottom of workplace rank is associated with 0.25 to 0.29 additional chronic health conditions, or about 24-28% of the population mean. With the exception of American Indians, all of the IV estimates are

slightly larger than the corresponding OLS estimate for the same demographic group. This suggests that the influence of the unmodeled health component on workplace rank is relatively small, and that it tends to attenuate estimates slightly, leading OLS results to understate the effects of workplace rank on health status.

7 Conclusion

This paper provides the first American evidence of the relationship between a workers' relative status within their workplace and health disparities. 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 three quarters of the conditional income-health gradient is explained by relative rank *within* the workplace, and only one-quarter 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 every one 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 slightly larger than the descriptive fixed effects estimates. After showing that job mobility up and down the workplace rank are not themselves 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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URL: *https://www.bmj.com/lookup/doi/10.1136/bmj.323.7327.1491*

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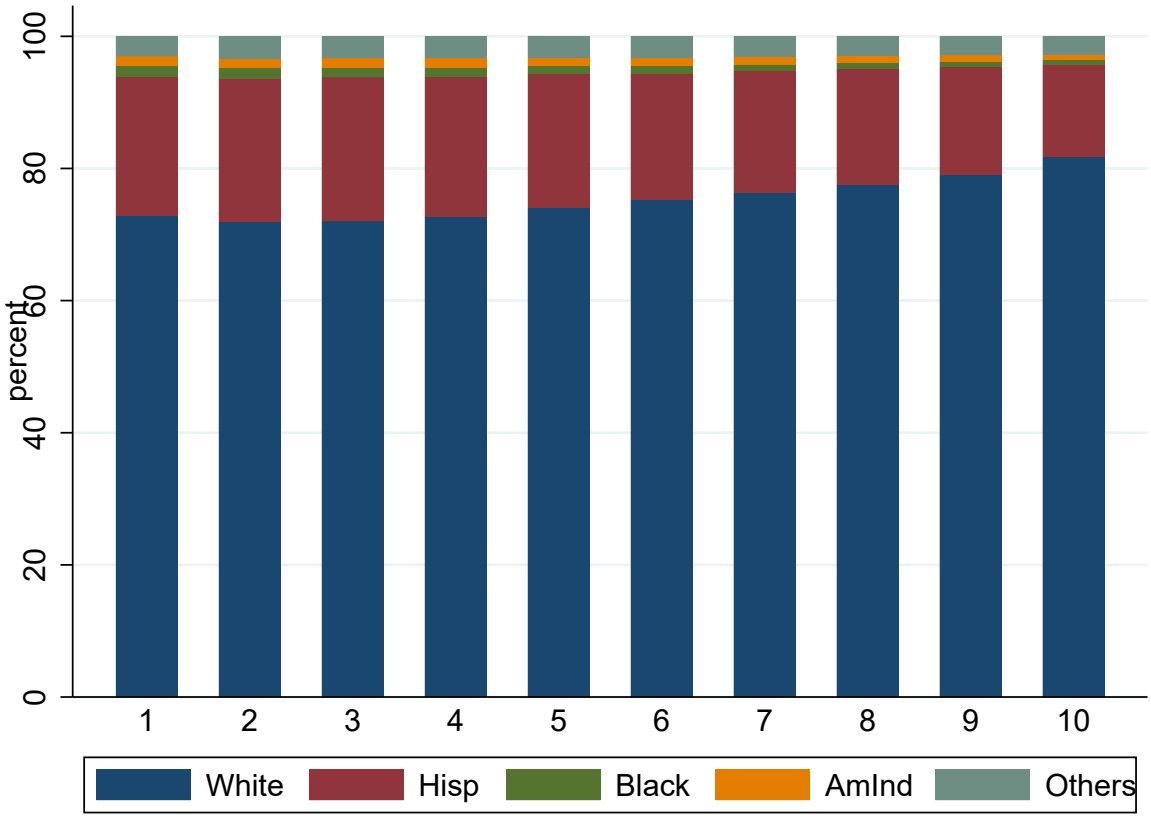
Appendix A Defining Firm IDs

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.

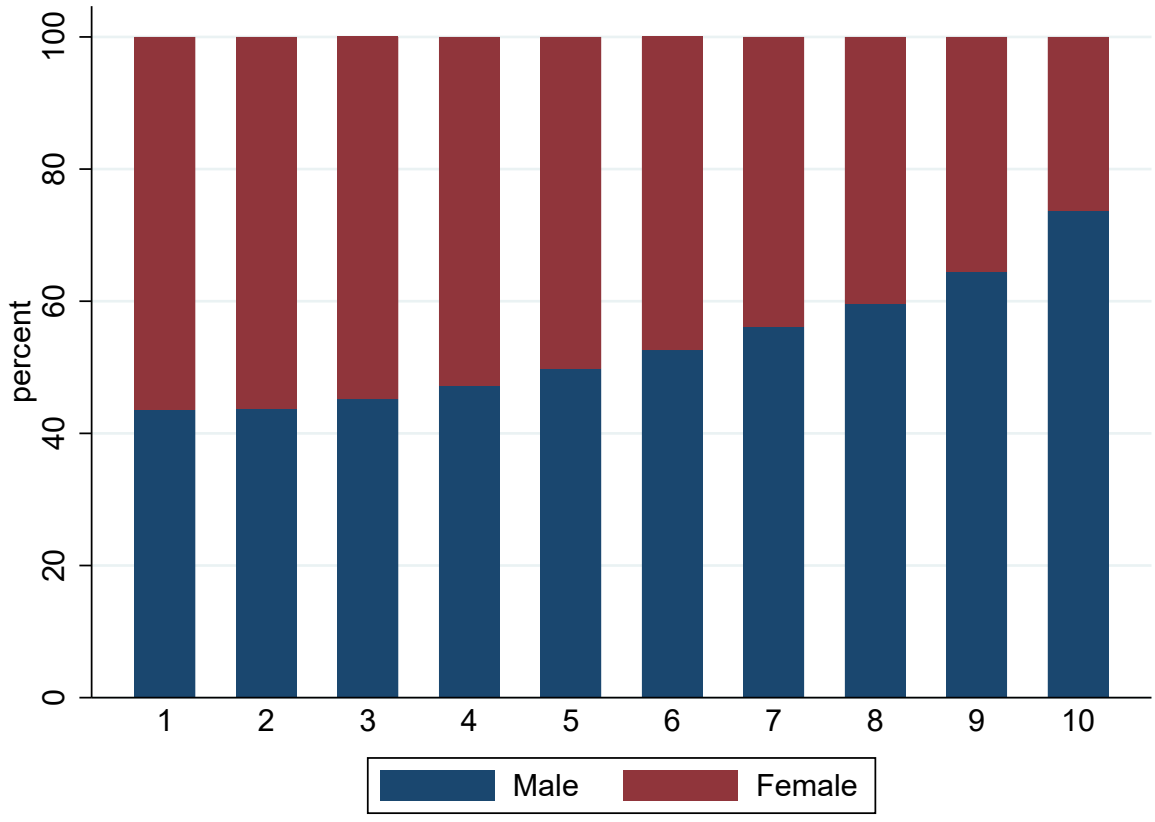
For large firms, the data contains large blocks of workers with the same sequence of quarterly firm average pay. Given the precision with which average pay is recorded, the probability of this happening by statistical chance is infintesimal. Using similar logic, we compute the statistical probabilities 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.

Appendix B Additional Tables and Figures

Appendix Figure 1: Racial Composition by Within-Firm Rank Decile



Appendix Figure 2: Gender Composition by Within-Firm Rank Decile

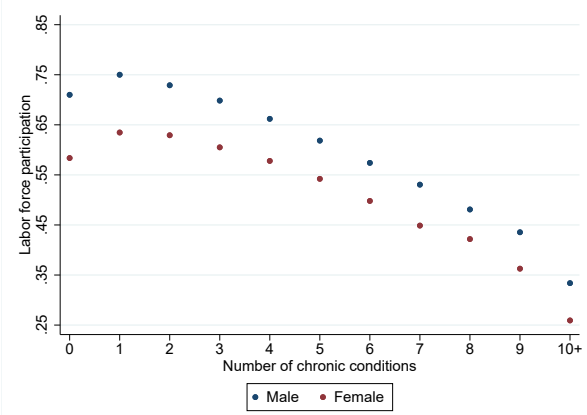


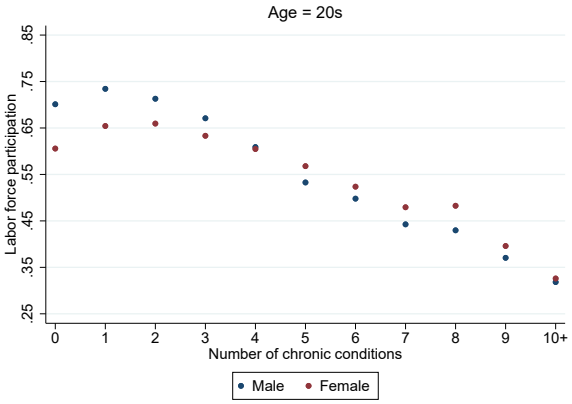
Appendix Table 1: Strength of Relationships between Work Outcomes and Health Status, by Race

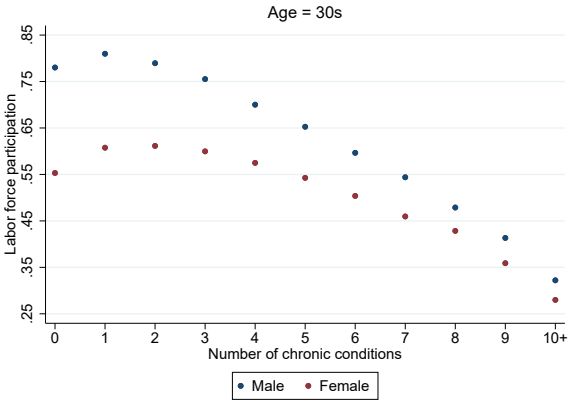
	American Indians	Black	Hispanic	White
Total Earnings	-0.019	-0.030	-0.044	-0.117
	(0.042)	(0.042)	(0.011)	(0.007)
Within-Firm Rank	-0.489	-0.244	-0.350	-0.237
	(0.040)	(0.040)	(0.010)	(0.006)
Within-Firm Gini	0.217	-0.117	0.283	-0.027
	(0.082)	(0.083)	(0.021)	(0.012)

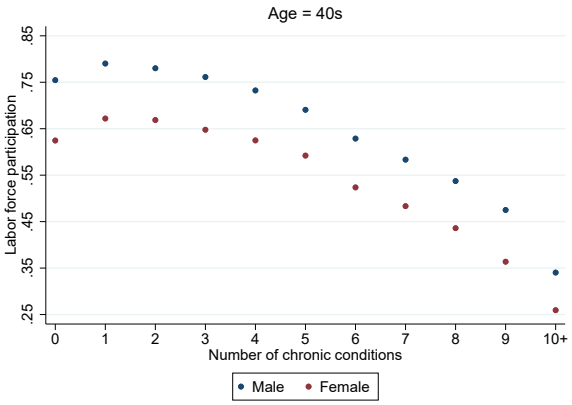
Notes: This table reports estimates from Equation 1, as shown in Figure 5.

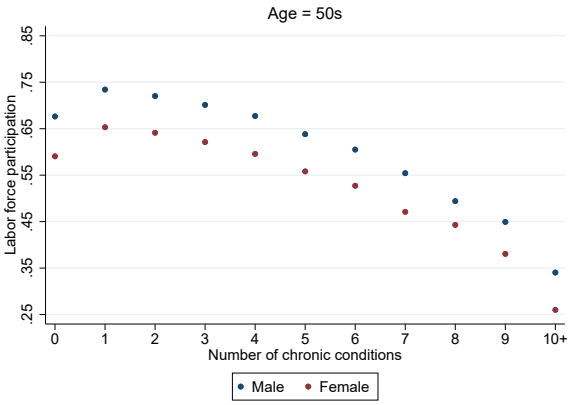
Appendix Figure 3: Chronic conditions and labor supply





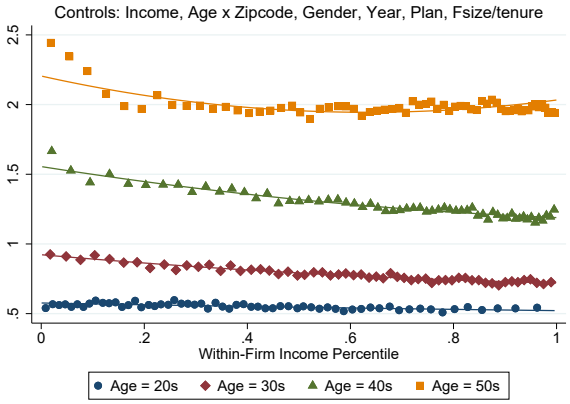




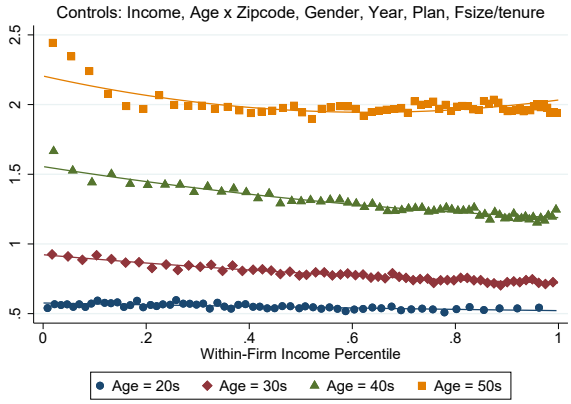


Appendix Figure 4: Number of Chronic Conditions vs Within-Firm Income Percentile By Age in Decades, By Race-Gender

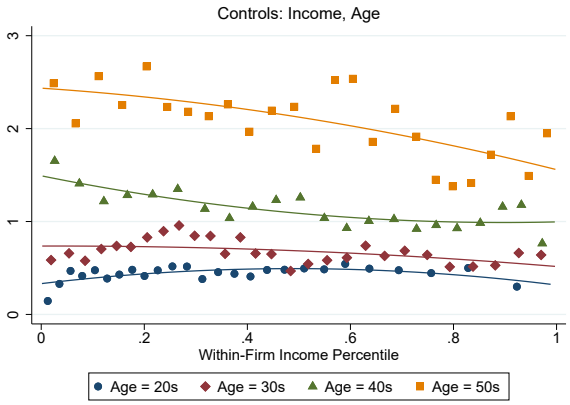
(a) White Men



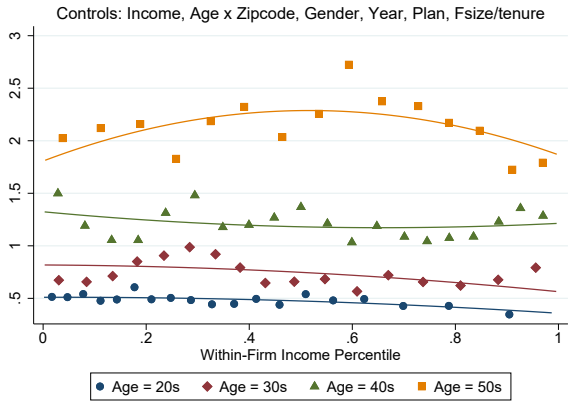
(b) White Men



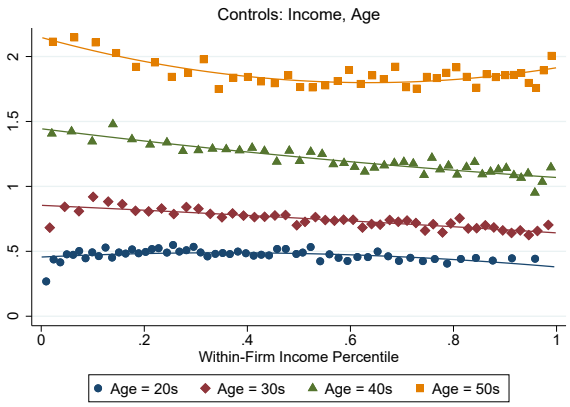
(c) Black Men



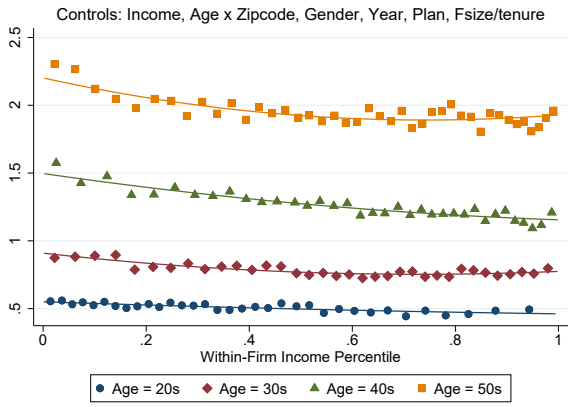
(d) Black Men



(e) Hispanic Men



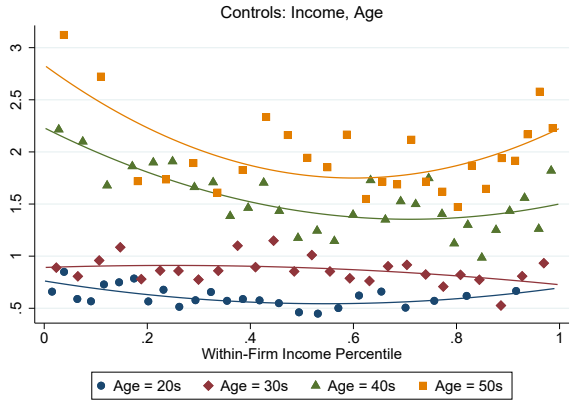
(f) Hispanic Men



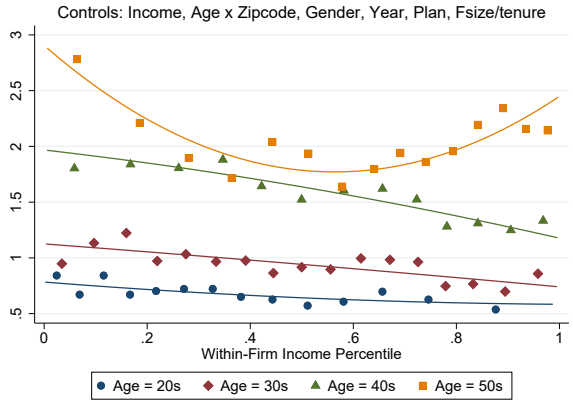
Notes:

Appendix Figure 5: Number of Chronic Conditions vs Within-Firm Income Percentile By Age in Decades, By Race-Gender, Continued

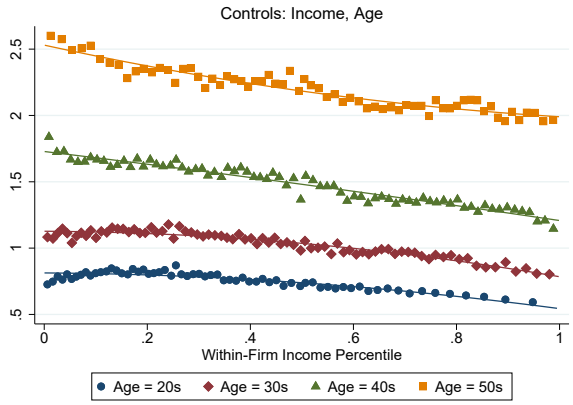
(a) American Indian Men



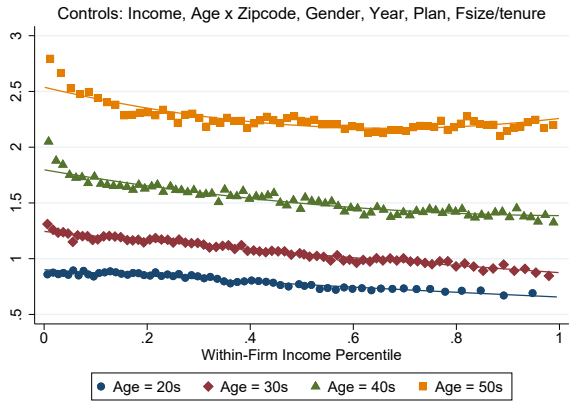
(b) American Indian Men



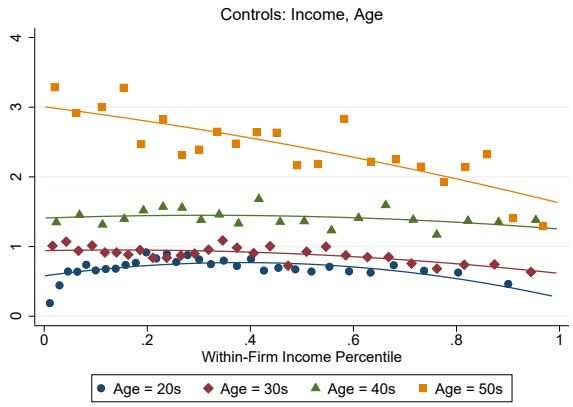
(c) White Women



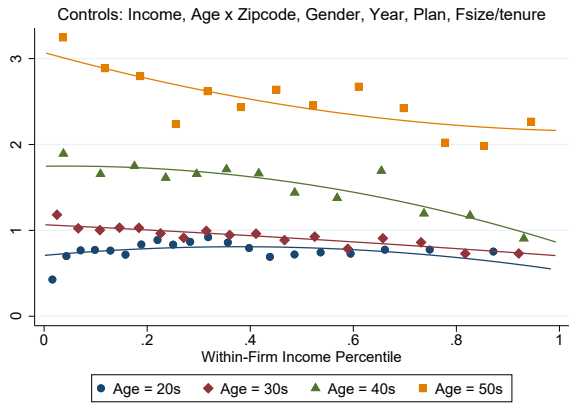
(d) White Women



(e) Black Women



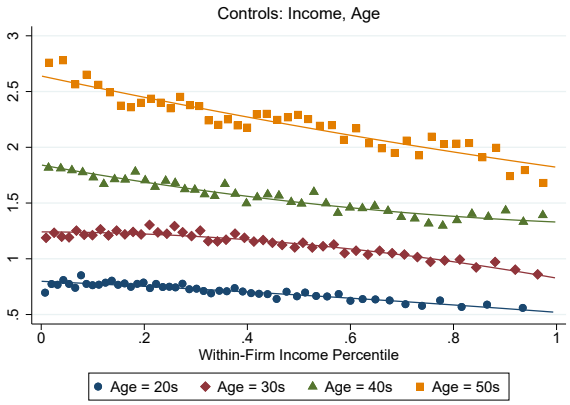
(f) Black Women



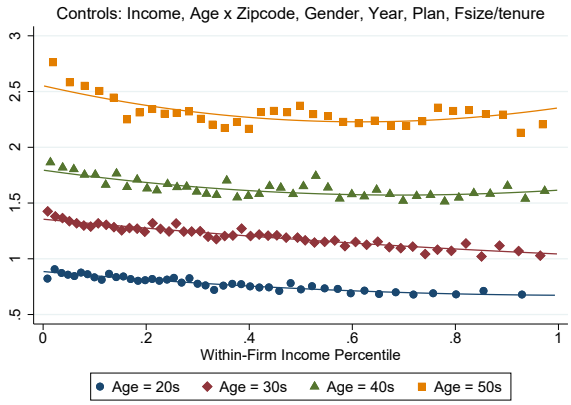
Notes:

Appendix Figure 6: Number of Chronic Conditions vs Within-Firm Income Percentile By Age in Decades, By Race-Gender, Continued

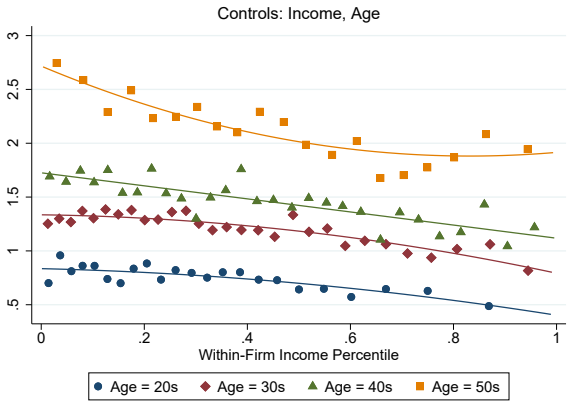
(a) Hispanic Women



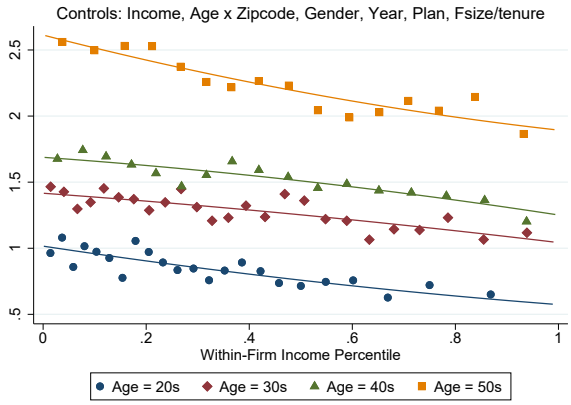
(b) Hispanic Women



(c) American Indian Women



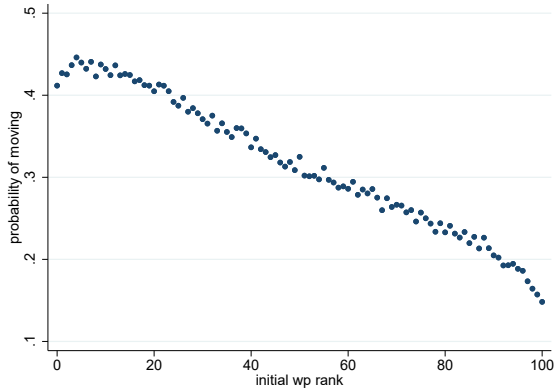
(d) American Indian Women



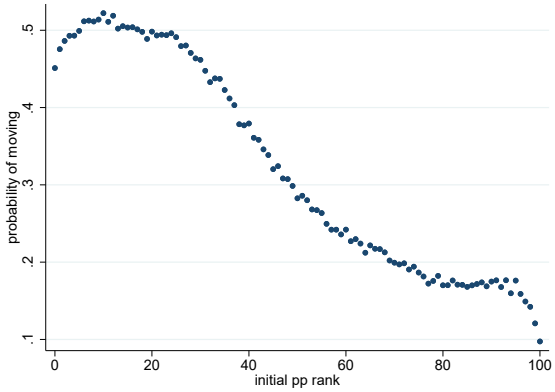
Notes:

Appendix Figure 7: Annual Probability of Exiting Job
by Origin Job Relative Rank and Earnings Percentile

(a) Relative Rank, wp



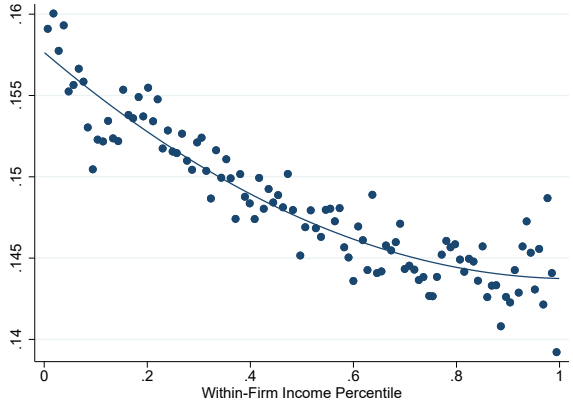
(b) Earnings Percentile, p



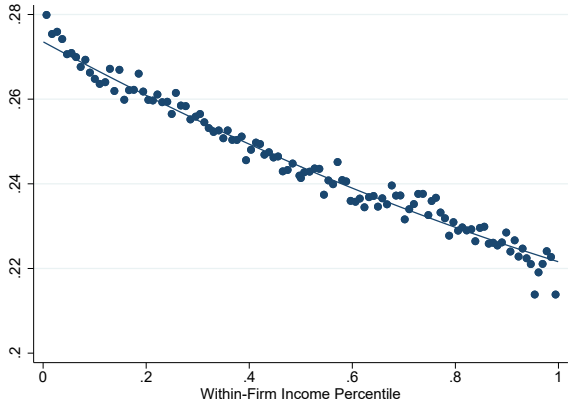
Notes:

Appendix Figure 8: Diagnosis Rates by Condition vs Within-Firm Income Percentile
Full Set of Controls

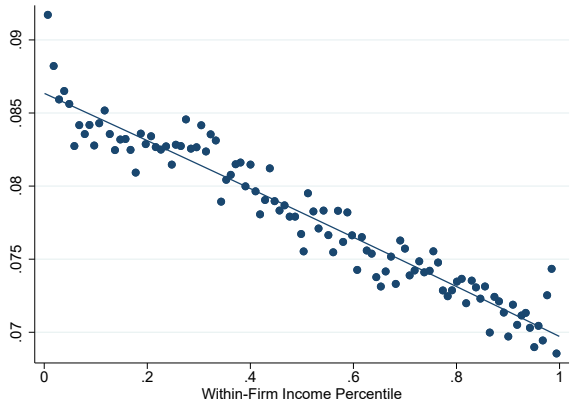
(a) Asthma



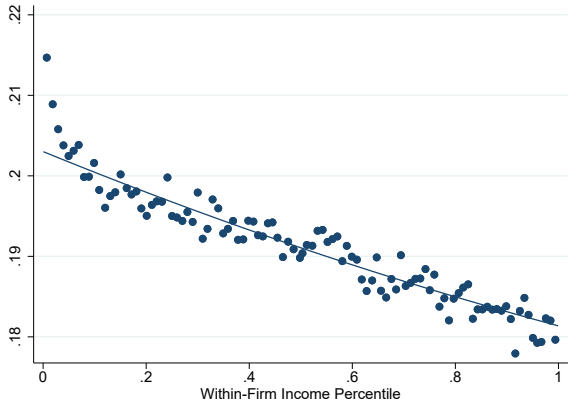
(b) Depression



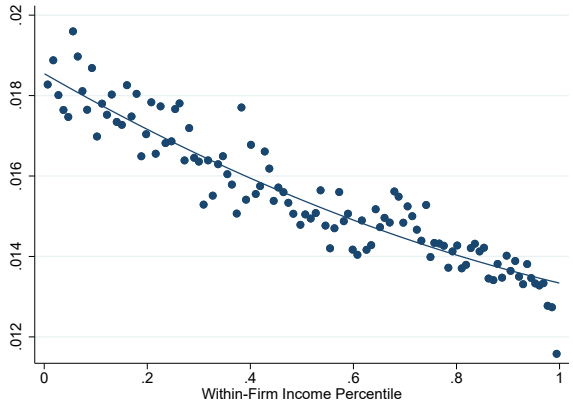
(c) Diabetes



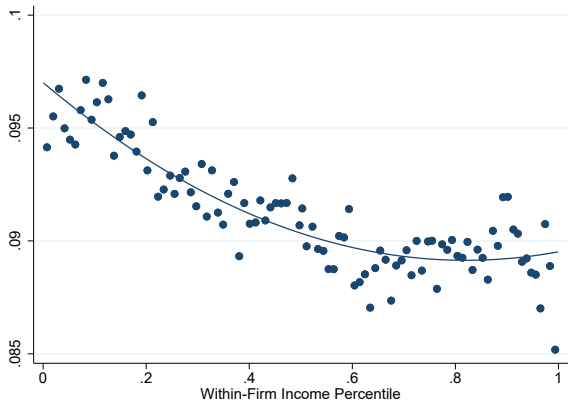
(d) Hypertension



(e) Bipolar



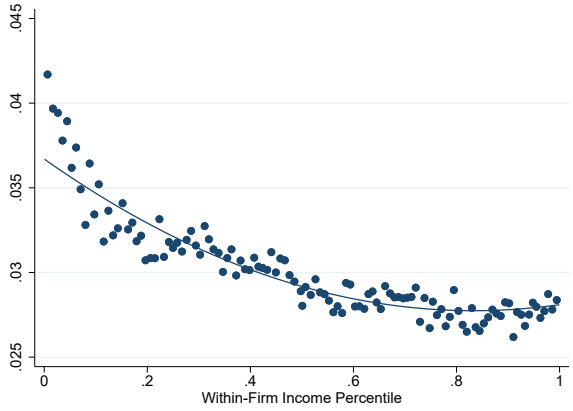
(f) hyperthyroidism



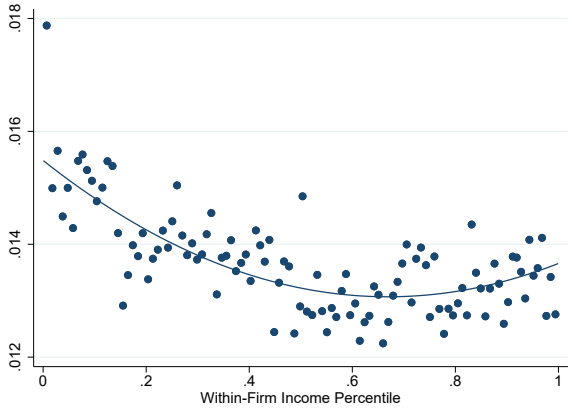
Notes:

Appendix Figure 9: Diagnosis Rates by Condition vs Within-Firm Income Percentile
Full Set of Controls

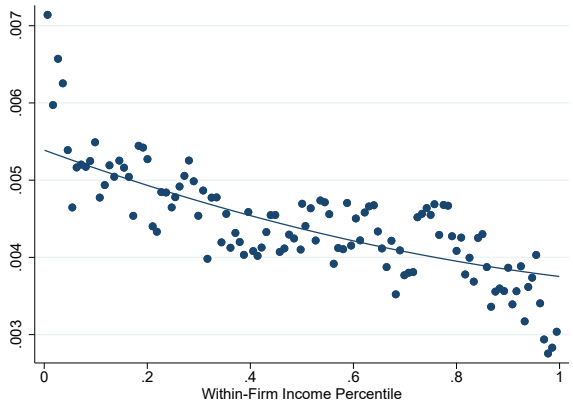
(a) Anemia



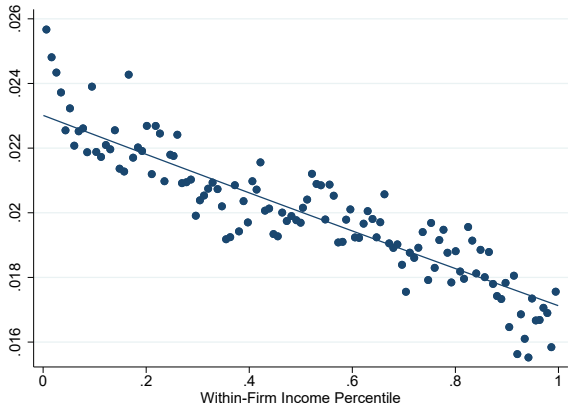
(b) Arthritis



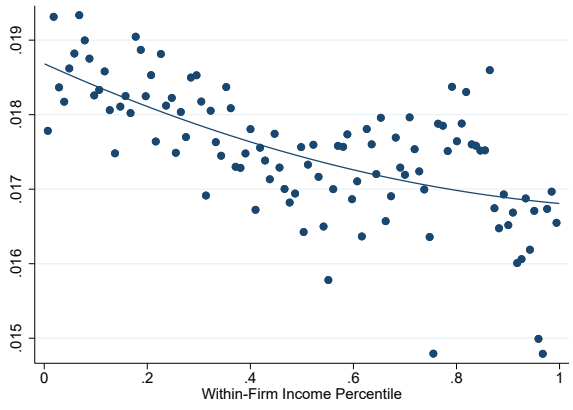
(c) Congestive Heart Failure



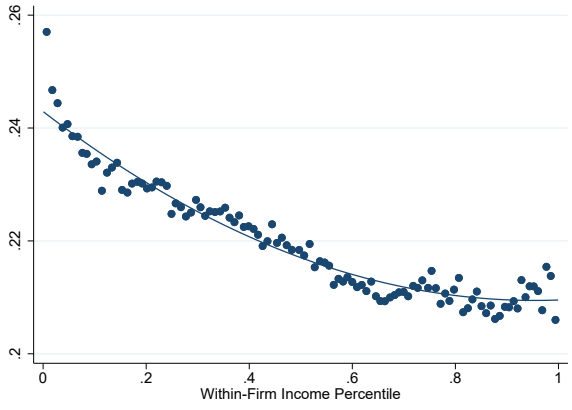
(d) COPD



(e) Glaucoma



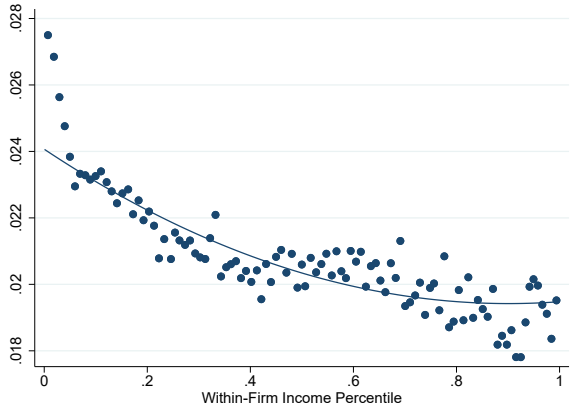
(f) Lower Back Pain



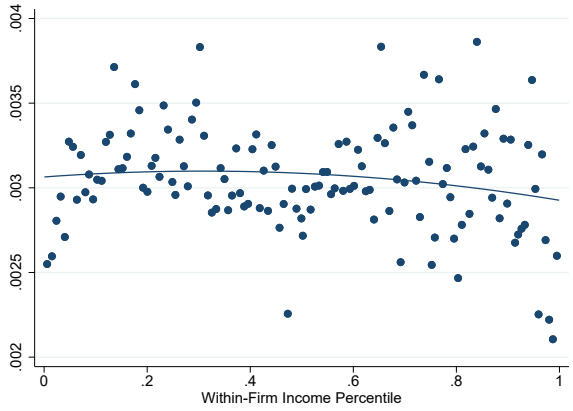
Notes:

Appendix Figure 10: Diagnosis Rates by Condition vs Within-Firm Income Percentile
Full Set of Controls

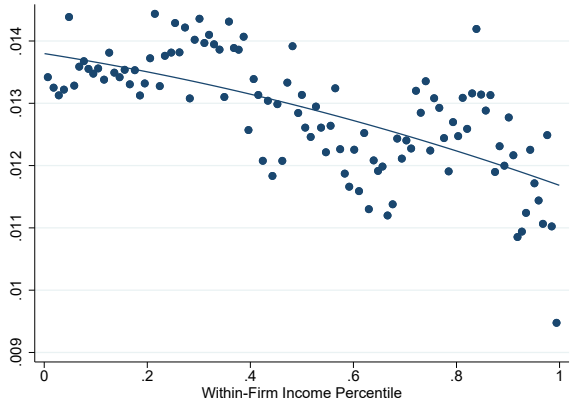
(a) Ischemic Heart Disease



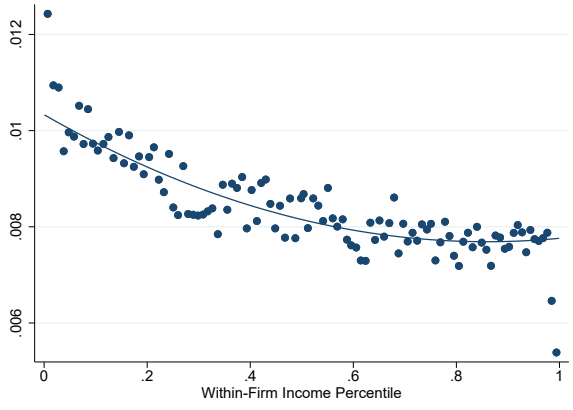
(b) Macular Degeneration



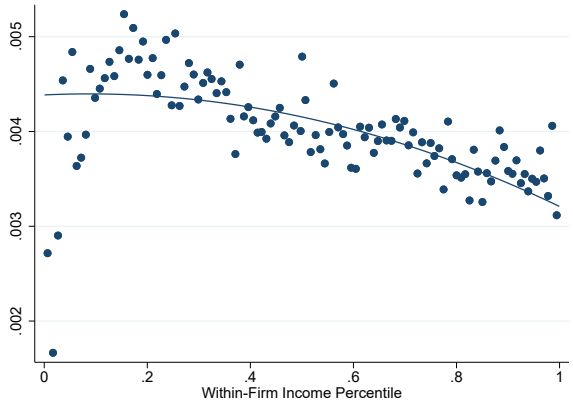
(c) Osteoporosis



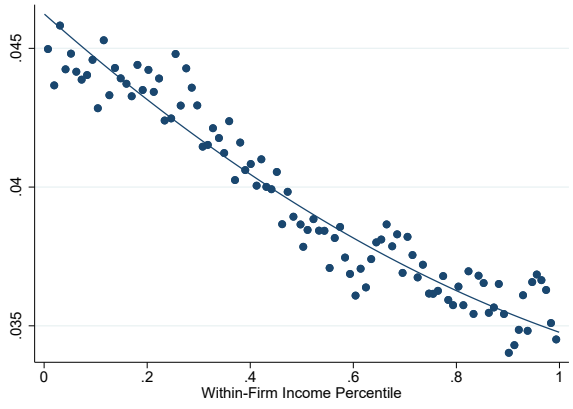
(d) Parkinsons



(e) Schizophrenia



(f) Seizures



Notes: