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ABSTRACT

One explanation for insufficient use of primary care in the U.S. is a lack of trust between patients and providers – particularly along racial lines. We assess the role of racial concordance between patients and medical providers in driving use of preventive care and the implications for patient outcomes. We use unique data from the Military Health System, where we observe providers as patients so that we can identify their race, and where moves across bases change exposure to provider race. We consider patients with four chronic, deadly, but ultimately manageable illnesses, where the relationship with the provider may have the most direct and important impact on health. We find striking evidence that racial concordance leads to improved maintenance of preventive care – and ultimately lower patient mortality. Pooling across these diseases, we estimate that a one-standard deviation increase in the share of providers who are Black leads to a 15% relative decline in Black mortality among those with these manageable illnesses. Our results further suggest that between 55 and 69% of this mortality impact arises through improved medication use and adherence, with other aspects of the provider-patient relationship accounting for the residual.

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One of the major problems of the U.S. health care system is insufficient use of preventive care. Consider the case of diabetes, an incurable, progressive and ultimately fatal disease – but one that can readily managed with proper medication and lifestyle choices. Among the 37 million diabetics in the U.S. today, only 18-36% have their diabetes under control (Centers for Disease Control 2022). The consequences of this inadequate control for ultimate health outcomes are profound. Adherence to preventive care recommendations has been found to reduce mortality in diabetes patients by 24-42% (Chen et al. 2016; Kung et al. 2020).

Why is there such incomplete use of proper preventive care in the U.S.? A number of hypotheses have been explored, ranging from a lack of access to the primary care providers that deliver such care to "behavioral moral hazard" (overweighting short term cost sharing for preventive care and underweighting the long run consequences of getting such care, as in Baicker et al. (2015)). One hypothesis that has gotten particular attention is poor trust in the advice given by providers and poor communication between providers and patients. And a primary determinant of such trust and communication may be the racial concordance of the provider and their patient. This hypothesis is supported in particular by experimental evidence in Alsan (2019), who showed that Black patients randomized to Black physicians in Oakland, CA were more likely to receive preventive care.

In this paper, we use a quasi-experimental approach to explore the role of racial concordance in a very different setting, but one with a broad geographic scope and a large Black population: the U.S. military. The Military Health System (MHS) provides care for roughly 9.6 million active-duty military, retirees, and dependents with expenditures of nearly $56 billion/year (Congressional Research Service 2022). Patients either receive care from a nearby Military Treatment Facility (MTF)—at which over 70% of the chronic-care encounters are with active-duty providers—or from a nearby civilian medical facility that contracts with the MHS.

This system and its associated databases have several features that provide for an excellent setting to study concordance. First, facilitating the movers-based design that we employ, moves are abundant in the military context and plausibly unrelated to factors such as health or race. Second, the MHS provides complete claims data for all care used by MHS enrollees, allowing us to assess the impact of concordance on both preventive care and on

follow up health outcomes – including mortality –that may result from differential use of preventive care. Finally, while most claims databases lack demographic information on providers thereby impeding the ability to conduct concordance analyses, we are able to collect this information by taking advantage of the fact that active-duty providers are also MHS beneficiaries, to whose demographics we have access.

We use these data to carry out a quasi-experimental analysis of the role of racial concordance on the use of primary care, and the resulting implications for patient health and spending. In particular, we use a movers strategy that builds on Finklelstein et al. (2016). We restrict the analysis to patients who move across military bases to remove any impacts of moving itself on outcomes; we also measure the relative outcomes of Black and non-Black patients who move across bases with different shares of Black providers – which allows us to include base-specific effects for both the "sending" and "receiving" bases.

We focus on patients with four different chronic, deadly, but ultimately manageable illnesses, where the relationship with the provider may have the most direct and important impact on health: diabetes, hypertension (high blood pressure), hypercholesterolemia (high cholesterol), and clinical atherosclerotic cardiovascular disease (clogged arteries). Though potentially deadly, these conditions can be managed through a combination of relatively inexpensive medications, diagnostic monitoring and a range of lifestyle measures. While our data do not provide direct lifestyle indicators, we can observe preventive measures bearing on diagnostic and pharmaceutical protocols. Our primary measure of preventive maintenance is adherence to recommended medications. Where available, we further employ consensus-based guidelines promulgated by organizations including the National Committee for Quality Assurance (who develop the HEDIS guidelines) and the World Health Organization.

Our findings are striking: across each chronic condition, concordance has a sizeable effect on preventive care and health outcomes. For example, we find that a move-induced one-standard deviation increase in the share of physicians who are Black is associated with a roughly 4-day increase in metformin fill days and a roughly 5-8% increase (relative to the mean) in the annual receipt of Comprehensive Diabetes Care for Black relative to non-Black diabetes patients. In connection with these preventive-care increases, we likewise find a 33% relative

decline in mortality. We consistently find positive effects on preventive care and biomarkers for good health, and negative effects on mortality, for all of our samples. When pooling across the various chronic disease samples, we estimate that a move-induced one-standard deviation increase in the share of physicians who are Black leads to a roughly 15% decline in mortality (relative to the mean) for Black relative to non-Black patients.

We subject these striking findings to a broad battery of specification checks. We show that there are no corresponding changes in observables corresponding to these impacts. We show that the odds of moving and of receiving care on the base is not related in a systematic way to the existing racial composition of base physicians, and that our results only operate for those who live close to the base, for which concordance is measured. And we show dynamics that clearly illustrate no systematic pre-move evolution of our outcome measures.

We also offer a rich interpretation of our findings. We discuss the potential limitations in interpreting our ITT results and address them empirically. We discuss how to interpret our results relative to pre-existing gaps in mortality across races. And we demonstrate that preventive medication use and adherence is a key mechanism underlying the mortality benefits of provider-patient racial concordance. That is, in a decomposition analysis that combines our findings with estimates from the relevant medical literature on the link between preventive care and mortality, we determine that between 55 and 69% of the mortality effects we estimate can be attributed to preventive medication use, with the remainder likely being attributable to other forms of preventive care and lifestyle factors influenced by providers. This suggests that provider trust is essential in ensuring adherence with low cost but live saving medical treatments.

Our paper proceeds as follows. Part I provides a review of the relevant literatures on racial concordance and on preventive care and discusses possible theoretical mechanisms underlying the beneficial effects of concordance. In Part II, we describe more specifically the key institutional details of the MHS setting. Part III describes our data and empirical strategy. Part IV presents our results and specification checks, while Part V discusses the interpretation of the findings. Part VI concludes.

Part I: Theoretical Benefits of Racial Concordance and Literature Review

The literature on racial concordance between providers and patients has theorized various behavioral mechanisms for a positive concordance effect. To begin, some scholars have stressed informational deficiencies on the provider side that fall along racial lines. For example, Hoffman et al. (2016) shows that a large portion of white medical students and residents incorrectly believe there are certain biological differences between Black and white individuals that do not exist in reality, while also finding that white medical students and residents made less accurate treatment choices for hypothetical Black patients and underrated their pain.

Other scholars have addressed the role of racial patterns in provider-patient communication, finding evidence that provider-patient race match is associated with greater markers of patient participation in medical decisionmaking (Cooper-Patrick et al. 1999), with greater degrees of information disclosure by providers to patients (Gordon et al. 2006) and with longer visit durations (Cooper et al. 2003). Finally, others highlight the compromised trust that minority patients, particularly Black patients, have in the medical profession as a result of a history of abuse by the medical community (Alsan and Wanamaker 2018; Hostetter and Klein 2021).

Compounding the aggregate harms potentially associated with racial discordance via these channels is evidence that Black individuals are underrepresented among doctors and medical students, with roughly 5.5% of active medical doctor residents self-identifying as Black in 2019-2020 (AAMC 2020). While the Military Health System (MHS) appears to have a slightly higher rate of Black providers, that rate still greatly lags behind that of the population of Black MHS beneficiaries.[1]

These possible mechanisms have motivated scholars to explore whether racial concordance between providers and patients, in practice, has led to better health care delivery and outcomes. This empirical literature, however, has produced somewhat mixed results and has been limited in certain methodological respects (Meghani et al. 2009). Most such studies have focused on health care inputs with little investigations into ultimate health care

[1] In our chronic disease samples, on-base visits with active-duty providers occur between 6.2 and 9.1 percent of the time with Black providers.

outcomes.[2] Perhaps more critically, most such studies are merely associational and have failed to address the key selection challenges underlying any attempt to infer causal impacts of concordance—e.g., selection due to unobservable determinants of patient choice of providers (and/or provider choice of patients) and due to heterogeneity in facility/regional quality combined with residential segregation by race (Chandra and Skinner 2003; Bach et al. 2004).

Several recent studies have confronted this selection challenge in various ways. Most notable is the seminal study of Alsan et al. (2019), who randomized Black patients to white and Black physicians in Oakland, CA. They found that Black patients who were assigned to Black providers were more likely to receive preventive care, and that this is at least partly due to better communication and trust.

While an important and novel finding, this paper cannot be considered the last word on this crucial topic. While their randomized design is compelling, it suffers from the potential limitation that they don't observe the same doctors treating a meaningful share of white patients - so they can't rule out that their results are driven by underlying differences in physician quality. Moreover, their finding applies to a very disadvantaged population in a particular location. And the paper is unable to follow patients beyond the primary care setting, so that they cannot measure the impacts on health care outcomes.

Other studies have tried to address some of these shortcomings through quasi-experimental analyses. Using administrative hospitalization records from Florida, Greenwood et al., (2020) estimates physician fixed-effects specifications and finds that the mortality penalty for Black newborns is 39% lower under the care of Black pediatricians (during newborn stays) than white pediatricians. Hill et al. (2020) likewise uses administrative hospitalization records from Florida and instruments for racially-concordant hospitalizations with the lagged share of concordant matches at the focal hospital during the relevant time of day of the hospital admission, finding that physician-patient race-match reduces the likelihood of within-hospital mortality by 15% (in relative terms).

Neither of these strategies, however, can fully address the two-sided problem of selection of providers by patients, and within-facility assignment of providers to patients, which

[2] Some studies also explore the effects on patient satisfaction with medication encounters (Saha et al. 1999).

our movers-based strategy will directly address (and which we will test). Further, relative to the latter two studies' hospitalization focus, our investigation into concordance effects within the chronic-care setting will more likely implicate the trust and communications mechanisms underlying racial concordance's theorized effects. Finally, none of these studies consider jointly the impact of concordance on both use of preventive care and health outcomes. By combining all of these outcome domains, our analysis will be able to explore the mortality impacts of racial concordance while also decomposing such effects into the contribution of preventive care and other influences.

More broadly, our investigation benefits from certain features of our data and institutional setting that will allow us to avoid some common pitfalls generally encountered by health care researchers. First, we will ensure that all patients in our analytical sample have identical insurance coverage. Second, the completeness and continuity of the military's beneficiary files will allow us to avoid concerns over "missingness" of Black patients (Black men in particularly) that frequently encumber medical research (Svensson 1989; Green et al 1994; Shavers-Hornaday et al. 1997; Murphy et al. 2013).

Our analysis also contributes to a related literature exploring the effects of patient-provider concordance along other demographic dimensions, such as gender. For instance, scholars have found an association between gender match and feelings of rapport (Gross et al., 2008), provider recommendations to others (Rogo-Gupta et al., 2018), patient satisfaction and reduced mortality among heart-attack patients (Greenwood et al. 2018), and increased benefits receipt in connection with workers' compensation medical examinations (Cabral and Dillender 2022). In additional, our analysis contributes to an even broader literature within economics outside of the health care context exploring the effects of racial and gender concordance between different subjects, such as teacher/pupil (Dee 2004; Dee, 2005; Ehrenburg et al. 1995; Bettinger and Long 2005; Fairlie et al., 2014; Lusher et al., 2018), mortgage-applicant/loan office (Frame et al. 2022), and others. Riise et al. (2022) bridge the health care and education literature and present evidence of an impact of having a female general practitioner on young girls' decisions to pursue STEM in high school and college. Finally, our analysis contributes to a literature in health economics and medicine on the determinants of preventive care (Kenkel

1994; Hsieh and Lin 1997; Deb 2002; Parente et al. 2005; Carrieri and Bilger 2013; Chen et al. 2013; McMorrow et al. 2014).

Part II: The MHS Setting[3]

The Military Health System (MHS) is the primary insurer for all active-duty military, their dependents, and many military retirees through the TriCare program. Of the nearly 9.6 million patients covered by TriCare, 20% are actively serving. The remainder are a combination of dependents of active-duty, military retirees[4] and dependents of retirees. TriCare is not involved in health care delivery in combat zones and operates separately from the Department of Veterans Affairs' Veterans Health Administration (Schoenfeld *et al.* 2017). For TriCare enrollees, care can be delivered in one of two ways: either directly at Military Treatment Facilities (MTFs) on military bases (direct care), or purchased from private providers (purchased care). MTFs themselves are a combination of inpatient facilities and outpatient clinics.

For those receiving care at MTFs, the care is delivered by a mix of providers including active-duty military providers, federal civilian employee providers, and providers hired using contract mechanisms who work full time at the MTF. In the outpatient encounters of focus in our analysis of chronic-care conditions, roughly 70% of the encounters are associated with treatment by active-duty providers. Providers are primarily salaried with payment not explicitly tied to either quantity or quality of care delivered.[5]

Alternatively, TriCare enrollees can receive "purchased care" outside the MTF. This care is delivered by a network run by a contracting insurer—United HealthCare, in the case of our sample period—where patients go to private providers within the insurer's contracted network. In principle, enrollees who live within the "catchment area" of an MTF are supposed to go to the MTF for care. This area was defined as 40 miles originally, though the military has shifted to

[3] This section, as well as the data description, closely follows Frakes et al. (forthcoming).
[4] Retirees is a term not synonymous with veterans, as various conditions—including twenty-plus years of service—are required to attain retirement status.
[5] Military providers are on a military pay scale, while civilian providers are on the federal General Schedule (GS); both pay systems include base pay, and local cost of living adjustments. Contractors are flexibly hired to meet local needs. The contracting process solicits competitive bids to provide a service for an annual salary—once the contract is awarded the MTF commander does not have authority to alter its terms or to provide additional awards (DoD, 2019).

time-based boundaries. Our data show clearly that a mileage boundary rule was not rigorously enforced during our sample period. Those who live closer to an MTF are much more likely to go there, but with a more gradual fall off rather than a strong distance discontinuity.

Across-base moves are frequent in our chronic-condition samples. For instance, roughly 38 percent of the diabetes sample is associated with patients who move across bases over the period during which we observe their diabetes care. As more fully summarized in Frakes et al. (forthcoming), across-base moves by the active-duty and their dependents (ADD) are driven by Department of Defense personnel management strategies and staffing needs. Importantly, active-duty assignments "will not be influenced by….[the] health of a Service member's family member" (DoD 2015). Several papers have demonstrated that the timing and frequency of relocations is not subject to soldier preferences, including Lyle (2006), Lleras-Muney (2010), Carter and Skimmyhorn (2017), and Carter and Wozniak (2018). While the choice of location may be influenced by Soldier preferences, these choices are constrained by the timing of the move, and the sponsor's rank and occupation. Accordingly, we follow Lleras-Muney (2010) in controlling for time, rank, and occupation in our control set.

Part III: Data and Empirical Strategy

Data

Our data come from the Military Health System Data Repository (MDR), a comprehensive set of health records collected at MTFs and from civilian providers reimbursed under TriCare. The data are comprised of several separate repositories, including administrative eligibility files and separate sets of claims data for direct and purchased care, both inpatient and outpatient. While TriCare does not pay claims for direct care, the data are structured as if they were claims and include detailed information such as physician identifiers and diagnosis and procedure codes. The sample period covers fiscal years 2003 through 2013.

Critically, these data have a somewhat uncommon feature in including information on the race of both the provider and the patient.[6] We have access to information on the race of

[6]Only the race of sponsors are recorded in the MDR beneficiary files, in which event our concordance analysis is technically exploring concordance between the race of providers and the race of the patient's sponsor (e.g,.

the provider in our direct-care records by virtue of the fact that active-duty providers are themselves beneficiaries of the Military Health System and are thus represented in the beneficiary eligibility files (Frakes et al. 2021). This means that we characterize the heterogeneity in the share of prevailing Black providers across bases by the share of active-duty providers treating at MTFs, leading to some potential concerns that we address below.

In the MDR beneficiary files, race is self-identified and coded into the following classifications: White, Asian or Pacific Islander, Black, American Indian or Alaskan Native, and Other. Our primary empirical analysis will follow a coarser racial classification and will focus on the binary distinction between Black and non-Black individuals.[7] It is with respect to Black patients where one would expect concordance/discordance to be a meaningful determinant of preventive care and outcomes, particularly in light of the special history of mistreatment of Black patients in the U.S. that has likely damaged this trust (Alsan and Wanamaker 2018).

Clinical Samples and Outcome Measures

As noted in the Introduction, we focus on patients with four chronic, deadly, but ultimately manageable illnesses, where the relationship with the provider may have the most direct and important impact on health. The first is diabetes, a progressive blood sugar imbalance that, if untreated, can lead to multiple organ failure, amputation, or death. The second is hypertension (high blood pressure), which significantly raises the risk of life-threatening cardiac events. The third is hypercholesterolemia, a disease characterized by elevated levels of cholesterol in the blood that leads to increased risk of heart disease and cardiovascular issues. The fourth is clinical atherosclerotic cardiovascular disease, a disease category that broadly entails a build-up of fats, cholesterol and other substances in the arteries and that includes acute coronary syndrome, peripheral arterial disease, and events such as myocardial infarction and stroke.[8]

A key preventive measure for all of these diseases is the regular and proper use of very low cost, generic medications. We discuss these medications below for each separate chronic

spouse, parent, etc.). The results presented below are statistically indistinguishable from that estimated when we restrict our sample to plan sponsors only.

[7] As discussed below, we also consider an approach that compares Black and white patients and drops patients of other races.

[8] See Lee et al. (2010), Lai and Hou (2013), Besseling et al. (2016), Chou et al. (2016), and Rodriguez et al. (2019).

condition. In the case of each condition, we select the relevant medications while drawing from an appropriate set of consensus guidelines promulgated by medical researchers and expert physician organizations. Despite these recommendations, adherence to recommended medications among chronic disease patients is distressingly low, as documented by Burnier and Egan (2019), Sharma et al. (2014), World Health Organization (2003) and others. This is true in our sample as well, as demonstrated by the annual fill-day averages set forth in Table 1 and in Table A1 of the Online Appendix. For instance, when pooling over the four chronic conditions, we find that patients on average only fill roughly 76 days of the relevant medications annually. A natural mechanism through which provider trust, and therefore racial concordance, may operate is through improved adherence to medications of this very nature.

For each condition, we begin by building a chronic-disease sample organized at the patient-year level, though our primary changes specification will ultimately collapse this data to the individual patient level. We define the person-year sample associated with each of these illnesses by drawing on our claims data and tracking our MHS patients after they have been identified in either the direct- or purchased-care records as having a diagnosis of chronic illness.

We begin with diabetes.[9] To explore adherence to recommended medication among this sample, we observe annual fill-days of metformin, an anti-diabetic drug used to control high blood sugar. Metformin is the most common diabetes medication included among clinical practice guidelines that we reviewed and was recommended by the American Diabetes Association throughout the course of our sample period (ADA 2020). While diabetes experts recommend metformin use daily, research has shown that not all patients even take metformin in the first place and those that do adhere at rates as low as 36% (Christofides 2019).

In addition, for the diabetes sample, we are able to supplement our analysis with a HEDIS-recommended measure[10] of broader preventive care. We follow Frakes et al. (2021) and identify compliance with "Comprehensive Diabetes Care" (CDC) by coding for receipt of all of the following during the focal year: (1) hemoglobin A1c (HbA1c) testing, (2) retinal eye exam,

[9] Following HEDIS, we limit this sample to those 18 and over.

[10] HEDIS measures are well used inputs by various organizations in evaluating the performance of health plans and providers. For instance, they are key inputs into the Star Ratings system employed by the Centers for Medicare and Medicaid Services

and (3) medical attention for nephropathy (the latter of which is met by receiving a microalbumin exam, receiving ACE/ARB therapy, or receiving treatment for nephropathy in the last measurement year).

HEDIS also recommends tracking whether patients with diabetes control their blood pressure, maintaining systolic levels of less than 140 and diastolic levels of less 90. Accordingly, we code for blood pressure control. However, we only possess vital statistics data of this nature for the 2009-and-beyond sample years and for those patients treated at MTFs.[11]

We likewise monitor a preventive drug adherence and biomarker measure for patients with hypertension. The World Health Organization (WHO) recommends that patients with uncontrolled blood-pressure levels take one of the following: thiazide and thiazide-like agents, angiotensin-converting enzyme inhibitors (ACEs)/angiotensin-receptor blockers (ARBs), or long-acting dihydropyridine calcium channel blockers (CCBs). As our adherence measure, we calculate the number of fill days annually for these medications. With respect to biomarkers, we follow HEDIS and use the same blood pressure indicators used for the diabetes sample.[12]

For hypercholesteremia, we track the fill days associated with use of the following cholesterol-lowering medication categories identified by Briesacher et al. (2008): antilipemic agents, bile acid sequestrants and statins. In the case of the atherosclerotic cardiovascular disease sample, we follow HEDIS and code for the receipt of statin therapy over the focal year (for males between 21 and 75 and females between 40 and 75).

Finally, across all four clinical samples, we flag whether or not the relevant patient died during the sample period.[13] Further, we estimate concordance specifications on pooled samples across these conditions. While a pooled investigation allows for greater statistical power, separate investigations are nonetheless helpful to the extent one is interested in

[11] Unfortunately, our vital statistics data do not collect information on HbA1c levels, despite our ability to track the incidence of HbA1c testing.

[12] Following HEDIS, we limit our investigation into hypertension to those 18 and over.

[13] Unfortunately, our data do not include cause-of-death information, limiting our ability to specifically track disease-related mortality. Despite the age bands for the HEDIS-driven adherence measure (statins use) for the atherosclerotic cardiovascular disease sample, our mortality analysis tracks patients with atherosclerotic cardiovascular disease over all ages, since HEDIS does not specify such age limitations when tracking mortality outcomes for atherosclerotic cardiovascular disease. The mortality results are nearly identical if we likewise limit the cardiovascular-disease mortality analysis to the ages that HEDIS specifies when tracking statin use for this condition.

observing how trust and communication channels operate in specific disease settings. This is particularly true in the case of diabetes, where the medical literature has placed even more emphasis on the importance of the provider-patient relationship and where the degree of routine disease management and monitoring is most extensive.[14]

In all cases, we focus on MHS beneficiaries enrolled in TRICARE Prime plans. All active-duty personnel are required to enroll in TRICARE Prime. While facing other alternatives, nearly 90% of the non-active duty likewise choose to enroll in TRICARE Prime. We further limit the sample to those that are not Medicare eligible, so that our entire sample is non-elderly.

Necessary for our analysis is a measure reflecting the share of providers on each base that are Black. A key issue arises, however, in defining *which* physicians are associated with "treating" these chronically ill patients. We take an empirical approach to this matching. Consider, for instance, the diabetes sample. We begin by identifying those providers that have outpatient office visits where diabetes is identified in the relevant diagnosis fields. To determine the base's Black provider share, we then calculate the share of diabetes visits that are seen by Black diabetes providers. In our primary approach, we form these base-specific Black-provider shares while observing visit data over the full sample period. In the Online Appendix, we present a full set of results taking an alternative approach whereby we calculate this share while focusing only on the first two sample years' worth of data.[15]

[14] See, generally, Christofides 2019, Brundisini et al. 2015 and the ADA (2022) for a discussion of the importance of provider-patient relationships in encouraging diabetes management and for a discussion of the extent of management involved. To offer a comparative view of the extent of self-management steps associated with the care of hypertension and high cholesterol, see Grundy et al. (2019) and Unger et al. (2020). This differentiation is further reflected in our own data collection which offers more preventive-care measures to observe in the case of our diabetes sample.

[15] This alternative approach has the benefit of accounting for certain endogeneity concerns when weighting race shares by visits—i.e., the possibility that concordance effects alter where patients go for care. We show using other tests below that this is unlikely to be a concern – and given within-sample moves, this alternative approach presents a noisier measure in only drawing on a limited time frame. This discussion is related to another challenge: the possibility of provider moves across bases, which, in turn, may create within-base variation over time in provider-race shares. While this may induce noise in our patient-movers strategy, we note that a significant majority of the base-year variation in the share of visits with Black providers can be explained by across base variation (with an R-squared of roughly 0.72 when regressing the base-year Black-provider visit share on a set of base fixed effects). In unreported regressions, we show that our key concordance findings are not meaningfully different when we just focus on those bases that have below-median degrees of within-base variation in provider-race shares. On a final related note, given the predominant role of across base variation here, we also elect to focus on patient moves rather than identifying the effects of concordance using provider moves. Even when

We also limit our sample of patients to those that move during the 2003-2013 sample period. This will ensure that the movers-based strategy discussed below only uses movers (across-base) to construct the relevant control groups and thus account for the possible effects of moving itself on the relevant health care outcomes. This approach thus differs from the moving based strategy in Finkelstein et al. (2016) which had included non-movers to help identify the place effects inherent in its moving design. As highlighted in Frakes et al. (forthcoming), a moving-based strategy that excludes non-movers has the advantage of not needing to assume place effects—in our case, base effects—that are comparable between movers and non-movers. To facilitate the most straightforward before-after quasi-experimental investigation, we focus on those who move a single time during the sample period.[16] Below, we present a number of tests to confirm the assumption that these moves are exogenous to the differential in the health outcomes across race that are of interest in this investigation.

As noted, MHS patients receive care on and off the base from active-duty and non-active-duty providers even though our movers-based strategy will be capturing variation only in the Black share of active-duty providers (since we do not measure race for other providers). This raises two concerns. First, some of the providers on the base are not active-duty; roughly 30% of the MTF-clinic visits are with civilian providers with respect to which race is unknown. If a base's share of Black active-duty providers is related to its share of civilian employees treating at the MTF, and the civilian share matters independently for patient health, this could bias our results.

To address this hypothetical concern, we control in our movers specifications for the move-induced change in the prevailing base's share of docs with missing race codes (civilian employee docs) along with the interaction of that measure with patient race. Moreover, as demonstrated by the forest plot depicted in Figure A1 of the Online Appendix, we find that

provider race shares vary within bases over time, that variation is gradual. By focusing on patient moves, we attain greater power by taking advantage of larger, more discrete sources of variation.

[16] To use the diabetes sample as an example, conditional on those who move across bases during the sample, 60% of those are individuals who move just once. Nonetheless, as discussed below, we also consider an alternative approach in which we include all movers and—for those that move more than once—explore the impacts of their final move.

changes in this missing-race measure have no differential impact on health outcomes for Black relative to non-Black patients.

The second concern is that the sample who chooses to receive care on the base is selected in a way that might bias our concordance estimate. Accordingly, we undertake an intent to treat approach, assessing the outcomes of patients that go both on and off base for care, among those who live near (within ten miles of) the base. Patients in this range are very likely to use the base for care (66% on-base utilization) while those who live outside the range only use the base for care 29% of the time. We later use those who live farther from the base as a falsification test.

The resulting sample is described in Table 1 and, more completely, in Table A1 of the Online Appendix. We have 74,689 patients with diabetes, 239,911 patients with hypertension, 332,016 patients with hypercholesterolemia and 48,682 patients with atherosclerotic cardiovascular disease; between 22% and 31% of these samples consistently live close to the base. Between 17% and 24% of the patients in each sample are Black. As demonstrated by Online Appendix Figures A2 – A5, we also document substantial variation across bases in the share of clinical visits for the respective clinical samples that are associated with Black providers. In Figure 1, we demonstrate the same for the pooled chronic-disease sample, with substantial base Black-provider share variation in the range from zero to 20%.

Empirical Strategy

Our motivating empirical strategy is captured by the following interaction specification:

$$Y = \alpha + \beta B + \delta PB + \tau B*PB + \epsilon \quad (1)$$

where B indicates a Black Patient, PB a Black provider and Y the outcome of interest. This structure will allow us to account for fixed differences in the relevant outcome between Black and non-Black Patients and between Black and non-Black providers, with the coefficient of the interaction term capturing the Black patient-provider concordance effect of interest.

Consistent with the implicit strategy of Alsan et al. (2019), we are choosing to model Black patient-provider concordance as our object of interest, rather than modeling a symmetrical racial concordance effect—i.e., rather than specifying an indicator for any provider-patient racial concordance (either Black/Black or non-Black/non-Black). As stated

above, we focus our concordance inquiry on Black patients given that the theorized concordance mechanisms—e.g., trust and communication—are motivated by the present and historical experiences of the Black patient population.[17]

The key challenge in estimating this motivating specification relates to concerns over selection on behalf of both patients and providers. For instance, Black patients may choose better or worse doctors relative to white patients, and Black doctors may be of higher or lower quality relative to white doctors. This can lead to an unknown bias from simple cross-sectional regressions of patient outcomes on concordance between race of patient and provider.

Ideally, this is addressed by random assignment of both Black and white patients to both Black and white physicians. As noted above, Alsan (2019) does do this, although only for Black patients. We can approximate this random assignment using quasi-random variation in both patient moves and the share of Black physicians across bases, following Finkelstein et al. (2016) and Frakes et al. (forthcoming). We present a range of specification checks to confirm the quasi-randomness of this approach.

Focusing on the pooled sample of chronic-diseases patients, Figure 2 shows the distribution of changes in the prevailing Black physician share that are induced by the across-base moves experienced by the relevant patients (we show corresponding disease-specific variants of this figure in Figures A6-A9 of the Online Appendix). There is a wide distribution, with a standard deviation of 6.3 percentage points, and significant support in the range from -20 to 20 percentage point changes. This allows us meaningful variation with which to estimate our impacts.

In particular, consider a chronically ill patient *i* moving across areas served by MTFs with different racial compositions of physicians who serve chronically ill patients. Let PB_j be the percentage of physicians treating a given chronic illness at base *j* who are Black. Let B_i be an

[17] There are not enough degrees of freedom to separately include variables capturing patient race and provider race, along with separate variables for each of Black patient-provider concordance and non-Black patient-provider concordance, leaving the researcher to either choose a Black-specific concordance approach or a symmetrical race concordance approach. Our concern with the latter is that it will tend to attenuate concordance effects—and attenuate the object of interest—if the concordance channels are indeed unique to the Black experience.

indicator for whether the patient is Black. Then for a given outcome Y_{ij}, we estimate regressions of the form: [18]

$$\Delta Y_{ij} = \alpha + \beta B_i + \delta \Delta PB_j + \tau B_i * \Delta PB_j + \Omega \Delta PX_j + \gamma B_i * \Delta PX_j + \pi M_i + ¥ B_i * M_i + \mu_j + \epsilon_{ij} \quad (2)$$

In this specification, β captures general differential changes of the impact of moving by race, while δ captures any general differences associated with moving from a base with a high versus a low share of Black physicians. The coefficient τ captures the concordance effect: the impact on Black patients, relative to non-Black patients, of moving to a base with more Black physicians, relative to one with fewer.[19]

It is important to note that, unlike Alsan et al. (2019), by including non-Black patients and by estimating the interaction specification inherent in equation (1) and its movers-based implementation in equation (2), we are able to separate a racial concordance effect from a Black-provider effect. However, this approach comes at a cost of understating the absolute magnitude of Black concordance impacts if there are also non-Black concordance effects—i.e., the estimated interaction coefficient of interest technically captures the degree to which a Black patient / Black-provider concordance effect exceeds that of a non-Black patient / non-Black provider concordance effect.

The set of variables M in specification (2) further accounts for certain baseline characteristics of the focal patients as of the first year they enter the sample, including fixed

[18] In the mortality specifications, ΔY captures the incidence of mortality over the sample. This incidence still signifies the change in the moved-induced incidence of mortality because the movers-based sample does not die pre-move. Note that the mortality results presented below are virtually identical with the inclusion or exclusion of fixed effects capturing the year of the move, appeasing censoring concerns with this measure—i.e., concerns that there is a lesser opportunity for mortality incidence in later sample years (an irrelevance that is perhaps not surprising as we find near identical distributions of move years between Blacks and non-Blacks).

[19] This specification has a number of advantages over a two-way fixed effects analog to this approach that would structure the data at the patient-year level and would capture move-induced changes in the prevailing Black-provider share of the base by including this share as the key regressor and including time and person fixed effects. To begin, specification (2) is designed to center time in generalized terms around moving events and then averages over all such events, thereby avoiding a range of concerns associated with two-way fixed effects designs within staggered-treatment settings. For instance, a two-way fixed effects approach to this interaction specification would pose concerns over early-move individuals serving as controls for later-move individuals and would place greater weight on mid-sample moves (Goodman-Bacon 2018). Moreover, another desirable feature of a changes approach to dose-response specifications like equation (2) is that it naturally weights moving events based on the sample distribution of observed doses—i.e., observed changes in Black-provider shares—allowing it to better approximate the average causal response relative to a two-way fixed effects approach, which tends to place greater weight on estimated causal responses near the mean dose (Callaway et al. 2021).

effects capturing their pay-grade level, age, active-duty status, and sex, along with the following levels indicative of their first-year / baseline health status: Charlson Comorbidity Index, RVUs and inpatient days.[20] Through variables captured in ΔPX, we account for the impact of other changes in base characteristics associated with a move, besides the change in the share of Black providers, so that we can more specifically identify the Black provider share change. In particular, we include the change in the share of physicians who are male, the change in the mean physician age, and the change in the share of active-duty providers among providers on the base, along with the change in the share of residents who are male, the change in the mean resident age, and the change in the share of residents who are Black. For both ΔPX and M, we further allow for differential impacts by race, so that we are not identifying our coefficient of interest based on racial differences in these variables.

Finally, and importantly, we also show results with and without the inclusion of fixed effects for the pre- and post-move base assignments, μ. Since patients from a number of different bases move to any given base—and vice versa—we are able to include such effects and still identify the general effect of a moved-induced change in the prevailing Black-provider share. The ability to account flexibly for regional effects provides a powerful tool to address classic sources of selection in attempting to identify the effects of racial concordance—i.e., concerns that Black patients and Black providers may tend to reside and work, respectively, in regions that apply different health care practices to all patients within the region, Black and non-Black alike.

Part IV: Results

Diabetes Results

To provide a demonstration of the analysis with respect to one of our specific disease categories, we present the results of equation (2) for the diabetes sample in Table 2. In all cases, we normalize the ΔPB_j variable (the change-in-Black-provider-share measure) by its

[20] For an overview of RVUs, see https://www.cms.gov/apps/physician-fee-schedule/overview.aspx. Measured on a common scale across services, RVUs reflect the resources used in providing the relevant service, including resources associated with physician work effort, non-physician practice expenses and medical liability expenses. The Charslon score is a weighted index of 17 comorbidity conditions (Deyo et al. 1992).

standard deviation, such that our results can be interpreted as the move-induced change in the outcome variable associated with a 1-standard deviation move-induced increase in the Black-provider share, interacted with the incidence of a Black patient. We then report the estimated coefficient of the interaction, τ, the racial concordance effect of interest.

We show in Column 1, Panel A1 that our coefficient of interest is 5.96, implying that if a Black patient with diabetes moves across bases and the move results in a 1-standard deviation increase in the prevailing Black-diabetes-provider share, they will experience a nearly 6 fill-day increase (a roughly 16% increase relative to the mean) in their rate of adherence with metformin relative to a non-Black patient experiencing the same move. As demonstrated by Panel A2, we likewise find a corresponding 3 percentage-point increase (roughly 8% relative to the mean) for Black relative to non-Black patients in the rate of compliance with Comprehensive Diabetes Care (CDC). As demonstrated by Column 2, these results are generally robust to the inclusion of fixed effects for the pre- and post-move base assignments, though the point estimate does fall slightly in each case.

In Panel B, we explore corresponding analyses for the case of blood-pressure control among patients with diabetes. The point estimates align with that the metformin and CDC preventive-care results in suggesting a positive concordance effect—i.e., an increase in the Black-provider share induced by a move is associated with an increase in blood pressure control for Black relative to non-Black patients. However, this analysis is estimated on a smaller sample given that the MDR vital stats data is available only starting in 2009 and only for those that receive on-base care, leaving us with relatively imprecise results.

These preventive-care and biomarker findings have potential implications for mortality, which we can test directly. The estimates in Panel C across Columns 1 and 2 suggest that mortality falls by 0.4 percentage points for a Black patient relative to a non-Black patient in connection with a move that is characterized by a one-standard-deviation increase in the prevailing Black-provider share of the base. This estimate is hardly changed with the pre- and post-move base fixed effects included.

In Tables A2 and A3 of the Online Appendix, we present results from certain robustness checks and additional analyses on the diabetes sample, including a presentation of the main

Black-provider share coefficients (omitted for brevity purposes from Table 2) and a demonstration of the robustness to alternative constructions of the Black-provider share variable.

All Diseases

In Tables A4-A6 of the Online Appendix, we likewise conduct similar analyses for each of the separate samples for the remaining chronic-conditions, including hypertension, hypercholesterolemia and atherosclerotic cardiovascular disease. While there is some variability in the degree of precision across these exercises, the pattern of point estimates depicted in the diabetes context—positive effects on preventive care and blood pressure control and negative effects on mortality—are replicated across these other chronic samples.

The advantage of our detailed data is that we are indeed able to separately analyze patients with different chronic illnesses, allowing us to target the specific indicators of high-quality care that apply to each disease. But we can increase power by pooling these samples. Of course, the samples are not fully independent: of those with any of these four chronic illnesses routinely living close to the base, 73.9% have only 1, 19.6% have 2, 5.9% have three and 0.7% have all four. To address this non-independence, we estimate a changes specification analogous to equation (2) that pools over all four chronic-condition samples and that includes fixed effects for each possible disease combination—e.g., diabetes only, hypertension only, atherosclerotic cardiovascular disease only, diabetes and hypertension together, and so on. Even though base Black-provider shares are tailored to each disease, the pooled specification will specify a single variable capturing the change in the provider-race share induced by the move. For those patients with multiple conditions, this variable effectively averages over the relevant individual's Black-provider-share measures across the different conditions.

We are able to draw from all four samples in estimating pooled mortality specifications and medication fill-days specifications—the latter being our primary preventive-care measure. As an alternative, we also estimate a pooled specification drawing from all four samples that replaces the medication fill day measure for diabetes with the Comprehensive Diabetes Care measure and standardizes each sample's relevant adherence measure to use the Z-score for as the dependent variable. In the case of our one biomarker measure—blood pressure control—

we are only able to estimate a pooled specification on a more limited sample, as this measure is only collected for the hypertension and diabetes samples.

Table 3 shows the results of estimating equation (2) on the pooled chronic-disease sample. Panel A1 shows that we observe a roughly 3 fill-day increase for the relevant preventive medication for Black relative to non-Black patients in connection with a move-induced increase of 1-standard-deviation in the base Black-provider share. Alternatively, we can compute a preventive-care-Z-score that replaces the preventive care measure for diabetes with the CDC comprehensive preventive care measure; as we show in Panel A2, we estimate a Z-score increase of between 0.02 and 0.03 for Black relative to non-Black patients in connection with a move-induced increase of 1 standard deviation in the base Black-provider share. Likewise, when pooling over the diabetes and hypertension samples, we find positive concordance effects (but only marginally significant) on blood pressure control.

In Panel C, we document substantial mortality effects, with point estimates suggesting a 0.2 percentage point reduction in mortality—roughly 15% relative to the mean mortality rate—for Black relative to non-Black patients in connection with a 1-standard deviation move-induced increase in the Black-provider share. These pooled results confirm the positive impact of concordance on patient health outcomes among chronically ill patients. Table A7 shows the robustness of these pooled sample results to alternative specifications.[21]

Specification Checks

We subject these striking findings to a battery of specification checks to illustrate their robustness. We summarize those tests here.

Observable Balance: As a first step, we confirm balance in observables across our basic specification. In particular, we estimate a series of specifications analogous to equation (2) that do not include M on the right-hand-side but that instead sequentially set the characteristics included in M—e.g., patient age at the beginning of the sample—as the dependent variable. In Table 4, we report the concordance coefficient, τ, associated with these balance regressions.

[21] Further, in Table A8 of the Online Appendix, we demonstrate that our analysis is robust—though with slightly weaker precision—to an alternative approach where we focus only on Black and white patients and drop the remaining non-Black racial categories from our control group. Further, in Table A9, we demonstrate that the results are robust to an alternative approach in which we include all movers and—for those that move more than once—explore the impacts of their final move.

As demonstrated by the reported coefficients, across each such characteristic, we find that moves leading to large-relative-to-small changes in the Black-provider share of the base—for Black relative to non-Black patients—are not related to the relevant baseline patient characteristic.

Further, recall that when estimating the interaction between Black patients and the move-induced change in the prevailing share of Black providers, we are controlling for corresponding interactions between Black patients and a range of move-induced changes in other demographic measures associated with the prevailing providers and the fellow base residents—e.g., the prevailing mean ages of providers, the prevailing share of Black beneficiaries on the base, etc. In Figure A1 of the Online Appendix, we also expand on the mortality findings depicted in Panel C of Table 3 to show a forest plot that presents estimated coefficients of these other interaction terms. These other estimates are generally closer to 0 and are all statistically indistinguishable from 0, relative to provider race.

Endogeneity of Moving or On-Base Care: As discussed earlier, a wide variety of researchers document and rely on the fact that active-duty moves are exogenous. Nevertheless, it is important to revisit that conclusion in our context – particularly given that our sample includes a number of retirees for whom such exogeneity arguments are considerably weaker.

To start, we illustrate that our results are generally robust for the non-retiree sample where the move exogeneity argument is strongest. Appendix Table A10 shows the impact on our key indicators in a sample that pools across chronic conditions and that focuses on those that are active-duty or active-duty-dependents (showing results with sending and receiving base fixed effects). The point estimate for the mortality results is nearly identical at -0.0019. Though this result is less precise given the notably smaller sample size when excluding retirees, this estimate is still marginally significant. The point estimate for the preventive-care Z-score analysis (which includes Comprehensive Diabetes Care for the diabetes sample) is also marginally significant though slightly smaller than the main results that include retirees (0.028 versus 0.018). Though still positive point estimates, the estimates for the medication fill days and the blood-pressure-control analysis (the latter of which was already based on a smaller sample as it related to only two conditions) is not significantly different from zero.

We then more directly test the endogeneity of movement in our sample. To do so, we consider a broader pooled-disease sample that does not exclude non-movers and then estimate the following specification:

$$Move_i = \alpha + \beta B_i + \delta PB_j + \tau B_i * PB_j + \Omega PX_j + \gamma B_i * PX_j + \pi M_i + ¥B_i * M_i + \epsilon_{ij} \quad (3)$$

where, i indicates a chronic disease patient and j indicates their base of residence at the beginning of the sample. The rest of the variables are as defined in specification (2). But now, we do not track how these measures change pre- and post-move. Rather, these measures are all determined as of the beginning of the sample. In other words, this specification regresses whether a patient moves on an indicator for the patient being Black, on a measure capturing the share of Black providers on their initial base and the interaction between those measures, all while controlling for a range of other initial-base demographic characteristics and patient characteristics (and how they interact with an indicator for Black patients). Doing so, we estimate a coefficient of τ of 0.008, with a standard error of 0.023 (with a baseline moving probability of 0.41). That is, we find no evidence that Black patients are more or less likely to move away from a high-Black-provider-share base relative to non-Black patients, easing moving endogeneity concerns.

Even if moves across bases are not endogenous, one might be concerned that there are differential race-specific decisions to seek care on versus off-base within our ITT sample. To test this, we estimate specification (2), but use as the dependent variable the move-induced change in patient i's share of outpatient care that is on-base (weighting outpatient visits by Relative Value Units). Doing so, we estimate a coefficient of τ of 0.002, with a standard error of 0.003 (with a baseline on-base share of 0.47). Accordingly, when patients experience a move that increases the prevailing share of Black providers, we find no evidence that Black patients—relative to non-Black patients—respond by changing their on-base versus off-base choice.[22]

Distance Falsification: In Table 5, we show a falsification test for our results, tracking the set of pooled-disease results presented in Table 3 but now estimating the relevant interaction

[22] Similarly, when patients experience a move that increases the prevailing share of Black providers, we find no evidence that Black patients—relative to non-Black patients—change their residential distance from the base. This is perhaps not surprising given the on-base-share findings given that on-base-care selection is predominantly a function of distance from the base.

specifications on the sample of patients who do not consistently live within 10 miles of the base (i.e., all those excluded from the previous tables). Across the various outcome measures, we consistently estimate concordance coefficients, τ, in this falsification sample that are closer to 0 in magnitude than our main findings and that are statistically insignificant. This confirms our ITT interpretation of preventive care and mortality effects as arising from changes in base-specific Black provider shares.

Dynamics: We can also go beyond binary before-after comparisons and instead track the year-by-year evolution of our outcome levels around the timing of moves. Estimating an event-study counterpart to the above changes analysis requires a simple modification to our approach underlying specification (2).

Recall that the Δ operator called for a calculation of the post-pre-move change in the relevant measure for individual i. We still want to use this operator Δ on the right-hand-side of our event-study specification as it is a critical part of characterizing the nature of the move—e.g., , ΔPB still signifies the dose response of interest (the change in the share of Black providers associated with the focal move). However, since our goal is to now track how the level of our outcome measure, Y, evolves in event time around these moves, we need to define a series of new operators, Δ^e, for the outcome variable, one for each event-time period. We consider event periods, e, within a window marked by 3 years pre- and post-move. Δ^e then measures the differences in levels for the outcome variable Y between the period of time signified by e—e.g., three years prior to the move—and a reference period. Following convention, we set the period prior to the move as the reference period. Accordingly, for each e within this event-time window, we estimate separate versions of the following analog to specification (2):[23]

$$\Delta^e Y_{ij} = \alpha + \beta B_i + \delta \Delta PB_j + \tau B_i * \Delta PB_j + \Omega \Delta PX_j + \gamma B_i * \Delta PX_j + \pi M_i + ¥ B_i * M_i + \mu_j + \epsilon_{ij} \quad (4)$$

By plotting the estimated concordance coefficient, τ, across these different specifications for each event-time bin, e, we are able to dynamically track the differential in the outcome of interest between Black and non-Black patients in connection with a 1-standard deviation move-induced increase in the Black-provider share.

[23] Furthermore, we focus on a balanced sample of individuals that we can observe over the entire event window.

Unfortunately, we cannot conduct this exercise on our mortality analysis, since we are focusing on individuals that we, in fact, observe move. And thus, we cannot observe differences in mortality outcomes in the years *leading up* to the move. We can track these year-by-year dynamics in our non-mortality outcomes, however. In Figure 3, we present results of this exercise using our two alternative prevention adherence measures (medication fill days in Panel A and the Z-score of the alternative adherence measure in Panel B), showing results from the specification that includes sending and receiving base fixed effects.

Encouragingly, we do not document evidence of an increase in the prevention adherence measures—for Black relative to non-Black patients—prior to this move. Consistent with a causal racial concordance effect, we begin to see a positive divergence in these measures upon the onset of the Black-provider-increasing move.[24]

Part V: Interpretation

While the specification checks help to demonstrate the causal nature of our concordance findings, there are a number of important interpretative issues that must be addressed as well.

Do More Black Doctors Imply More Visits to Black Doctors by All Patients?

Implicit in our analysis is the assumption that an increase in the density of Black doctors on a base leads to more visits to those doctors by both non-Black and Black patients. For example, suppose that as the share of Black physicians rises, this only increases the odds of visits for Black patients and not white patients. In this case, we could not separate a concordance effect from a physician quality effect – we could be seeing better outcomes for Black patients because Black doctors are just better, and Black patients are seeing them more. This would invalidate one of the key advantages that we offer relative to the design of Alsan et al. (2019).

To address this, we estimate a "first stage" specification which examines how changes in Black provider density impacts the odds that Black versus non-Black patients see more Black

[24] In Figure A10 of the Online Appendix, we replicate this dynamic analysis for the sample of active-duty patients and active-duty dependents.

providers. The reason that it is not a true first stage is that we can only observe the race of the attending physician for those patients who receive their care on the base (as this is the sample where provider-race is available). As a result, we cannot run true IV models, but rather use this analysis to rule out the concern expressed above. In particular, on this more limited sample, we estimate specification (2) but use as the dependent variable the post- to pre-move change in the incidence of seeing a Black active-duty provider.

For the purposes of this specification, we do not normalize the Black-provider-share measure by its standard deviation, so that the results can be interpreted as the increased probability of visiting with a Black provider in connection with a move from a base with no Black providers to all Black providers. This interpretation will facilitate an assessment of whether the relationship between Black-provider availability and Black-provider visit incidence is close to one-for-one.

The results of this analysis are shown in Table 6. Focusing on the specification with based fixed effects, we draw two important conclusions. First of all, there is indeed an essentially one-to-one relationship between having more Black providers and being more likely to see a Black provider. Second of all, that relationship is not highly differentiated by race – it is slightly stronger for Blacks, but the differential is modest. This suggests that having more Black doctors causes both races to see Black doctors on essentially a one-for one basis.

Note that it is also possible that the increases in the share of prevailing Black providers that drive our reduced-form approach may have system-wide spillover effects that happen to differentially benefit Black patients. For instance, perhaps non-Black providers become more educated about treating Black patients when they are surrounded by more Black providers. We cannot rule out that some of our findings may be arising through a spillover mechanism of this or a related nature. To be sure, such a mechanism can still be seen as emanating from the concordance/discordance inquiry motivating this paper—after all, as discussed in Part I, negative discordance effects may stem from provider-knowledge deficiencies.

Gap-Closing

We now consider the degree to which increasing availability of Black providers may decrease prevailing racial gaps in chronic-disease mortality rates. We use the diabetes sample for demonstration purposes.

Among MHS beneficiaries between 20 and 65, Black beneficiaries are roughly 38% more likely to have diabetes and die over the sample period than non-Black beneficiaries (relative to mean mortality rates). This disparity appears, however, to be driven by disparities in diabetes disease prevalence between Black and non-Black MHS beneficiaries. Conditional on having diabetes, as demonstrated by Table 1, there is no meaningful racial mortality gap. This paramount role of disease-prevalence disparity in explaining overall mortality disparities is generally consistent with the results from the diabetes literature, though (1) the overall diabetes racial mortality gap is smaller in the MHS context (e.g., Cunningham et al. 2017 find an overall diabetes mortality rate that is nearly double that for Black Americans) and (2) various studies have found diabetes mortality gaps across race that exist—albeit to a smaller extent than the overall gap—when conditioning on a sample of patients with diabetes (Gu et al. 1998).

In our context, our estimates therefore imply that, conditional on prevalence, there are significantly better outcomes for Black patients with Black doctors than for white patients. Applying that to the total gap in mortality in our data using the mortality results from Table 4 – i.e., a 0.4 percentage-point reduction in Black relative to non-Black mortality rates in the diabetes sample—we estimate that a one-standard deviation increase in the Black-provider share leads to a reduction in the overall mortality gap from 38 to 21%. That is, our results suggest that improved concordance has a large enough effect to significantly offset the higher mortality gap arising from baseline prevalence differences.

Generalizability

Our analysis poses several generalizability questions. First, one might be concerned that our focus on movers is not representative of all MHS beneficiaries. Indeed, we note that the movers in our chronic-disease sample tend to be slightly younger and healthier at baseline than never movers. In Table A11 of the Online Appendix, we show this using the diabetes sample for demonstration purposes. For instance, movers in the diabetes sample are on average four

years young than never movers and 0.2 percentage points less likely to die over the sample period. However, if anything, that perhaps suggests that our results (while local to movers) may be an underestimate of the full benefits of racial concordance.

Second and more fundamentally, our analysis is specific to the Military Health System, with beneficiaries that may not be representative of the full population. It is important to note that even if our results are specific to the MHS, this is an important and under-studied (from a health systems perspective) population, containing over 3% of all black residents of the United States. Moreover, while many concordance studies focus on limited geographical areas, our analysis—albeit focused on the MHS—covers expansive geographical ground. Moreover, concerns that the MHS population may be in general healthier than the overall population are mitigated by conditioning our analysis on those with chronic diseases.

Mortality-Effects Decomposition Analysis

We have explored the impacts of concordance on both preventive care and mortality – but these outcomes are of course conceptually linked. To further interpret our findings, we explore how much of the documented mortality effect likely arises through improved preventive care. Necessary for this decomposition are certain parameters from the associated medical literature—e.g., the effects of preventive care on mortality. For tractability purposes, we focus our demonstration on the diabetes sample and draw from the relevant diabetes literature for the necessary parameters.[25]

We begin by decomposing the overall racial-concordance mortality effect into the effect of metformin adherence and a residual:

Overall Percentage-Point Mortality Effect (From Table 2 = -0.4) =

[*Percentage-point Increase in Metformin Fill-Days* (From Table 2 = 4.2) X

Percentage-point Impact on Mortality of 1 Fill-Day Increase in Metformin Use (Average Estimate from Literature = -0.056)] + *Residual* (5)

Solving for the residual and scaling it by the -0.4 overall mortality result, we find that roughly 41.2% of the mortality effect of racial concordance can be explained by something other than the increased metformin adherence. Correspondingly, we find that 58.8% of

[25] The diabetes studies drawn from include Roussel et al. (2010), Aguilar et al. (2011), Ekström et al. (2012), Hung et al. (2015), Cederholm and Gudbjörnsdottir (2012), and Hu et al. (2020).

concordance's mortality effect can be explained by increased metformin fill-days. Some of the 41.2% residual can be explained by effects of racial concordance not otherwise explored in our analysis due to data limitations—e.g., improved exercise, improved dietary behaviors, etc. (not otherwise arising from improved metformin use). Of course, these are just rough estimates as we assume that the average impact of improved preventive care from the literature applies on the margin to our concordance effects.

Some of that 41.2% residual may also be explained by adherence to Comprehensive Diabetes Care (CDC), which we can and do measure. When we conduct a similar decomposition that independently focuses on CDC as the preventive care measure of interest, we find that 21% of the mortality effect arises through the effect of racial concordance on CDC adherence.[26] However, given the possibility that CDC adherence and metformin adherence may be behaviorally linked (a relationship unexplored in the literature, to our knowledge), we cannot claim that the 21% CDC contribution to the racial concordance mortality effect is independent from the 58.8% metformin contribution.

In the Online Appendix, we conduct a similar exercise in the case of the remaining chronic disease samples. We find that between 55 and 69% of the respective mortality effects across these other samples can be explained by the respective medication fill-day effect. Those results correspond closely with the 58.8% finding from the diabetes mortality decomposition. Taken together, our results suggest that a primary mechanism for improved patient-provider trust is through medication adherence and that this can explain the majority of the concordance mortality effect that we document.

Part VI: Conclusions

There is a broad consensus that preventive care is underused for the most common, and easily treatable, chronic illnesses in the U.S. Indeed, results such as Chandra et al. (2010)

[26] *Overall Percentage-Point Mortality Effect* (From Table 2 = -0.4) = [*Percentage-point Increase in Comprehensive Diabetes Care* (From Table 2 = 1.7) X *Percentage-point Impact on Mortality of Percentage-Point Increase in Comprehensive Diabetes Care* (Average Estimate from Literature = -0.05)] + *Residual*. Solving here for the residual as a share of the overall mortality effect suggests that 79% is due to the residual, with a corresponding 21% of the mortality effect being due to increased CDC adherence. The diabetes preventive-care-mortality studies drawn from include Chen et al. (2016), Lai and Hou (2013), Lee et al. (2010), and Kung et al. (2020).

suggest that more preventive care may provide a rare "win-win" situation where total costs fall and health care outcomes improve. But a variety of ongoing barriers limit the availability of such win-win opportunities in the U.S. context. One such barrier is mistrust and other factors that limit the effectiveness of care delivered to Black patients by white doctors, relative to their Black counterparts.

In this paper, we provide compelling economic evidence of the consequences of such racial discordance. Using unique data on a very large geographically dispersed sample of individuals in the military, we are able to use the fact that physicians are themselves patients to identify concordance between patient and provider. And using the large number of plausibly exogenous moves in our sample, we show that when Black patients are more likely to be treated by a Black physician, they both use more preventive care, and have better health outcomes, then when they are more likely to be treated by a physician of another race, relative to non-Black patients in a similar circumstance. Our findings are robust to both a variety of different diseases, and several different objective measures of preventive care quality – and we build on previous literature by following the implications of these care differences downstream to show sizeable effects on patient mortality. Our results are also robust to a battery of specification checks, and we suggest that the majority of the benefits of concordance may arise through improved medication adherence.

This set of findings has important implications not only for the military population and their dependents, which account for 3% of all Blacks nationwide, but also for the more general population. They suggest that investments in increased patient concordance could have a meaningful impact on the large racial mortality disparity in the U.S. They also raise the interesting question of what other types of concordance can matter most for health care communication and trust, and further work would usefully explore these avenues as well. Finally, from a policy perspective, these findings highlight the importance of efforts to increase the diversity of the physician workforce and are particularly timely in light of the upcoming Supreme Court decisions in *Students for Fair Admissions Inc. v. University of North Carolina* and Students for Fair Admissions Inc. v. President & Fellows of Harvard College, which will have significant bearing on the future of affirmative action in medical school admissions.

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Table 1. Summary Statistics

	Full Sample		Black Patients		Non-Black Patients	
Variable	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Panel A: Diabetes Sample						
Medication Fill Days	36.438	(100.064)	38.768	(99.254)	35.790	(100.279)
Incidence of Comprehensive Diabetes Care	0.362	(0.481)	0.391	(0.488)	0.354	(0.478)
Incidence of Blood Pressure Control	0.838	(0.368)	0.788	(0.409)	0.853	(0.353)
Incidence of Mortality	0.012	(0.108)	0.011	(0.105)	0.012	(0.109)
Black Physician Share of Base	0.091	(0.069)	0.104	(0.067)	0.088	(0.069)
Black Patient	0.218	(0.413)	-		-	
Panel B: Pooled, Chronic Disease Sample						
Medication Fill Days	76.5128	(134.091)	67.268	(124.772)	78.611	(136.031)
Incidence of Mortality	0.012	(0.108)	0.011	(0.103)	0.012	(0.109)
Black Physician Share of Base	0.084	(0.061)	0.096	(0.058)	0.082	(0.061)
Black Patient	0.201	(0.400)	-		-	

Notes: data are from the Military Health System Data Repository 2003-2013. Descriptive statistics from Panel A are taken from a person-year sample of diabetes patients prior to collapsing down to the person-level sample estimated in the changes specification set forth in specification (2). Descriptive statistics from Panel B are taken from a person-year-disease-category sample of patients with four chronic diseases prior to collapsing down to the person-disease-category level estimated in the changes specification set forth in specification (2).

Table 2. Diabetes Sample: Relationship between Racial Concordance and Preventive Care Utilization and Outcomes

Panel A. Adherence Analysis		
Panel A1. Dependent Variable = Change (Post-Move minus Pre-Move) in Metformin Fill Days.		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	5.961***	4.223**
	(1.879)	(1.892)
N	19549	19549
Panel A2. Dependent Variable = Change (Post-Move minus Pre-Move) in Mean Incidence of Comprehensive Diabetes Care		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	0.029***	0.017**
	(0.009)	(0.008)
N	19549	19549
Panel B. Biomarker Analysis		
Dependent Variable = Change (Post-Move minus Pre-Move) in Mean Incidence of Blood Pressure Control (Systolic < 140 and Diastolic < 90)		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	0.007	0.008
	(0.010)	(0.011)
N	8466	8466
Panel C. Mortality Analysis		
Dependent Variable = Incidence of Mortality Over Sample		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	-0.004**	-0.004**
	(0.002)	(0.002)
N	19549	19549
Including Fixed Effects for Pre- and Post-Move Bases?	NO	YES

Notes: The unit of observation is a given diabetes patient. The sample includes adult MHS beneficiaries who consistently live within 10 miles of the base and who have moved once over the sample period. Coefficients of the Black-patient indicator and the move-induced change (normalized by standard deviation) of the base Black-provider share are omitted for brevity. All specifications include moved-induced changes in the following characteristics of the prevailing base (and their interactions with the Black-patient indicator): mean active-duty-provider age, mean incidence of male active-duty providers, mean incidence of on-base encounters with civilian providers, mean incidence of Black residents, mean age of residents, and mean incidence of male residents. All specifications likewise include fixed effects capturing different levels (as of the initial sample period) for the following patient characteristics (and their interactions with the Black-patient indicator): patient age, rank, active-duty status, sex, Charlson comorbidity score and RVUs. Base Black-provider shares capture the share of the base's diabetes visits with known doctor race that are overseen by Black providers. ***Significant at the 1 percent level; ** Significant at the 5 percent level; *Significant at the 10 percent level.

Table 3. Pooled Disease Sample: Relationship between Racial Concordance and Preventive Care Utilization and Outcomes

Panel A. Adherence Analysis		
Panel A1. Dependent Variable = Change (Post-Move minus Pre-Move) in Medication Fill Days. All Samples Pooled.		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	2.989***	2.670***
	(0.982)	(0.992)
N	130705	130705
Panel A2. Dependent Variable = Change (Post-Move minus Pre-Move) in Alternative Preventive Adherence Measure (Z-score for Sample-Specific Measure). All Samples Pooled.		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	0.031***	0.028***
	(0.010)	(0.010)
N	130705	130705
Panel B. Biomarker Analysis		
Dependent Variable = Change (Post-Move minus Pre-Move) in Incidence of Blood Pressure Control. Diabetes and Hypertension Samples Pooled.		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	0.013*	0.012*
	(0.007)	(0.007)
N	43306	43306
Panel C. Mortality Analysis		
Dependent Variable = Incidence of Mortality Over Sample. All Samples Pooled.		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	-0.0018***	-0.0019***
	(0.0007)	(0.0007)
N	197508	197508
Including Fixed Effects for Pre- and Post-Move Bases?	NO	YES

Notes: This sample pools patients across all four disease categories. The unit of observation is a patient-by-disease-category cell. Standard errors are clustered at the patient level. The sample includes adult MHS beneficiaries who consistently live within 10 miles of the base and who have moved once over the sample period. Specifications include fixed effects capturing the focal patient's incidence of each possible combination of the four disease categories, along with fixed effects capturing the disease category associated with the unit of observation. Coefficients of the Black-patient indicator and the move-induced change (normalized by standard deviation) of the base Black-provider share are omitted for brevity. All specifications include moved-induced changes in the following characteristics of the prevailing base (and their interactions with the Black-patient indicator): mean active-duty-provider age, mean incidence of male active-duty providers, mean incidence of on-base encounters with civilian providers, mean incidence of Black residents, mean age of residents, and mean incidence of male residents. Specifications also include fixed effects capturing different levels (as of the initial sample period) for the following patient characteristics (and their interactions with the Black-patient indicator): patient age, rank, active-duty status, sex, Charlson comorbidity score and RVUs. Base Black-provider shares capture the share of the base's visits—for the focal disease category—with known doctor race that are overseen by Black providers. ***Significant at the 1 percent level; ** Significant at the 5 percent level; *Significant at the 10 percent level.

Table 4. Covariate Balance. Relationship between 1-Standard-Deviation Move-Induced Increase in Black-Provider Share and Differential in Indicated Patient Characteristic between Black and Non-Black Patients

	(1)	(2)
Age (Mean in Pooled Sample = 42.94)	-0.218	-0.102
	(0.233)	(0.079)
Pay-Grade (1-7 Scale) (Mean in Pooled Sample = 3.38)	0.000	-0.005
	(0.017)	(0.008)
Active-Duty Status (Mean in Pooled Sample = 0.33)	0.002	0.003
	(0.007)	(0.005)
Male (Mean in Pooled Sample = 0.56)	-0.009	-0.002
	(0.009)	(0.004)
Annual RVU (Mean in Pooled Sample = 51.38)	-2.199	0.251
	(1.620)	(0.548)
Charlson Index (Mean in Pooled Sample = 0.47)	-0.002	-0.003
	(0.016)	(0.006)
Inpatient Days (Mean in Pooled Sample = 0.78)	-0.069	-0.005
	(0.079)	(0.004)
Sample	Diabetes	Pooled Sample

Notes: Each row captures a different specification whose dependent variable is the initial-sample-period level of the indicated measure. The unit of observation in Column 1 is a diabetes patient. The unit of observation in Column 2 is a patient-by-disease-category cell. Standard errors in Column 2 are clustered at the patient level. All samples includes adult MHS beneficiaries who consistently live within 10 miles of the base over the sample period and who have moved once over the sample period. Specifications in Column 2 include fixed effects capturing the focal patient's incidence of each possible combination of the four disease categories, along with fixed effects capturing the disease category associated with the unit of observation. All specifications likewise include moved-induced changes in the following characteristics of the prevailing base (and their interactions with the Black-patient indicator): mean active-duty provider age, mean incidence of male active-duty providers, mean incidence of on-base encounters with civilian providers, mean incidence of Black residents, mean age of residents, and mean incidence of male residents. The reported results are the estimated coefficient of the interaction between the Black-patient indicator and the move-induced change (normalized by standard deviation) in the base Black-provider share (which captures the share of the base's visits—for the focal disease category—with known doctor race that are overseen by Black providers). Specifications also include the constitutive terms of this interaction. ***Significant at the 1 percent level; ** Significant at the 5 percent level; *Significant at the 10 percent level.

Table 5. Falsification Mortality Analysis (Pooled Sample). Relationship between Racial Concordance and Preventive Care Utilization and Outcomes among those Not Consistently Living within 10 Miles of Base

Panel A. Adherence Analysis		
Panel A1. Dependent Variable = Change (Post-Move minus Pre-Move) in Medication Fill Days. All Samples Pooled.		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	-0.283	-0.048
	(0.564)	(0.570)
N	335482	335482
Panel A2. Dependent Variable = Change (Post-Move minus Pre-Move) in Alternative Preventive Adherence Measure (Z-score for Sample-Specific Measure). All Samples Pooled.		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	-0.000	0.003
	(0.006)	(0.006)
N	335482	335482
Panel B. Biomarker Analysis		
Dependent Variable = Change (Post-Move minus Pre-Move) in Incidence of Blood Pressure Control. Diabetes and Hypertension Samples Pooled.		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	-0.005	-0.004
	(0.008)	(0.008)
N	41250	41250
Panel C. Mortality Analysis		
Dependent Variable = Incidence of Mortality Over Sample. All Samples Pooled.		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	-0.0008	-0.0002
	(0.0007)	(0.0006)
N	498744	498744
Including Fixed Effects for Pre- and Post-Move Bases?	NO	YES

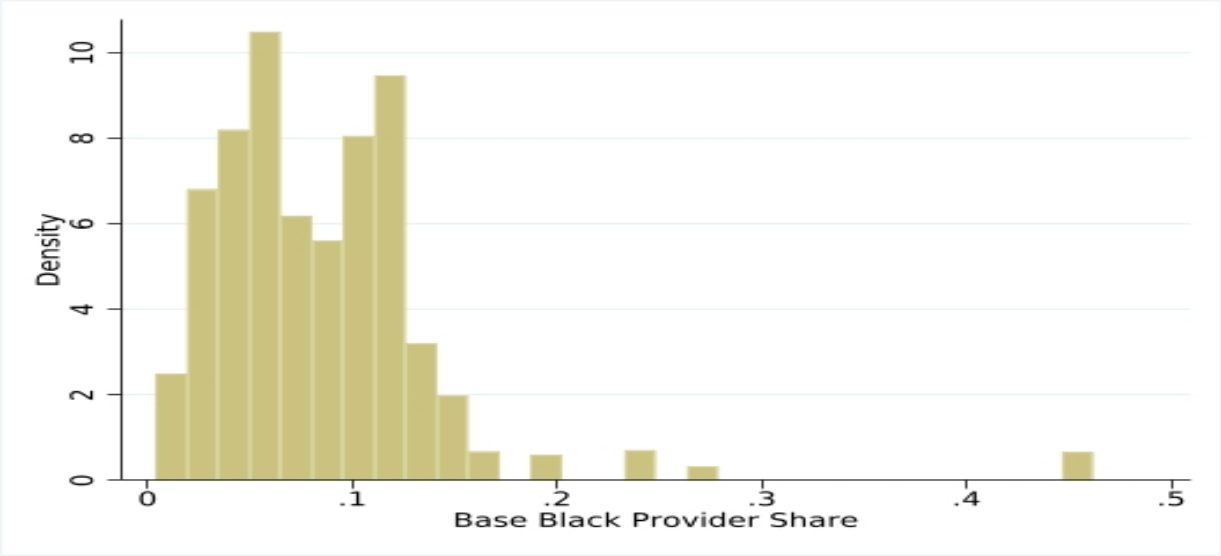
Notes: The specifications and samples estimated are identical to that estimated in Table 3 except that the sample in this table includes those adult MHS beneficiaries that do not consistently live within 10 miles of the base. ***Significant at the 1 percent level; ** Significant at the 5 percent level; *Significant at the 10 percent level.

Table 6. "First Stage" Analysis: Effect of Move-Induced Change in Black-Provider Share on Move-Induced Change in Annual Incidence of Patient Seeing Any Black Active-Duty Provider (Among Patients Seeing Active-Duty Providers)

Change in Base Black-Provider Share	0.663***	0.624***	1.132***	0.972***
	(0.035)	(0.043)	(0.117)	(0.123)
Change in Base Black-Provider Share X Black Patient	-	0.110	-	0.148*
		(0.078)		(0.078)
F-test	348.49	169.67	93.49	37.30
Pre- and Post-Move Base Fixed Effects?	NO	NO	YES	YES

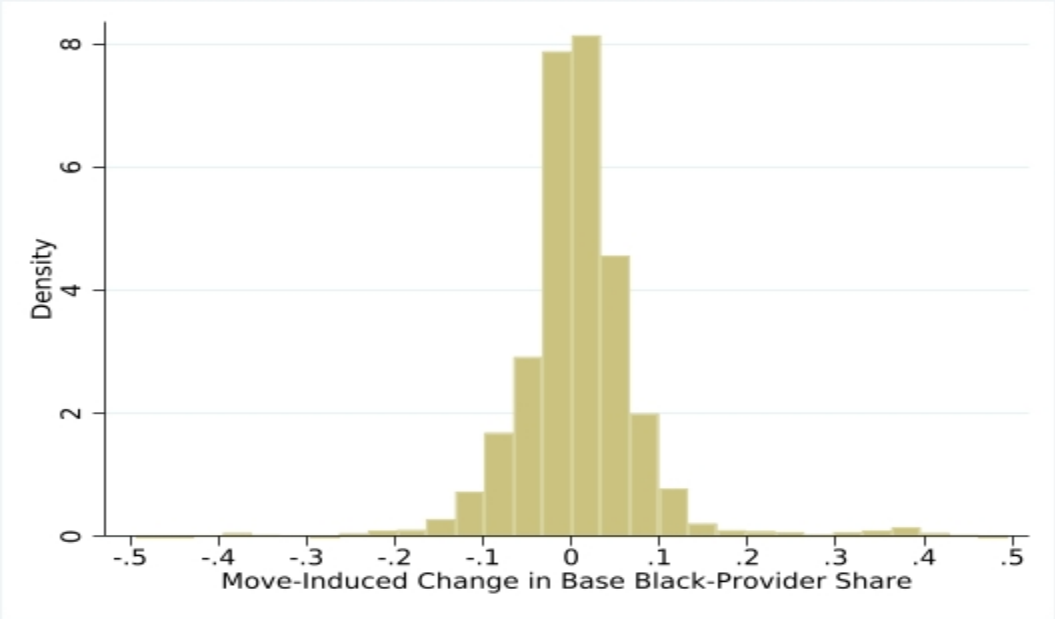
Notes: Each specification includes the same controls included in Table 3. The samples explored are identical to that estimated in Table 3 except that this "first-stage" analysis is estimated on the sample of patients that has seen an active-duty provider in both the pre- and post-move period. The dependent variable is calculated by (1) determining the average annual likelihood in the pre-move period of seeing a Black provider for the disease category associated with the unit of observation, (2) doing the same for the post-move period and (3) taking the difference between the two to determine the move-induced change in the likelihood of being treated by a Black active-duty provider. ***Significant at the 1 percent level; ** Significant at the 5 percent level; *Significant at the 10 percent level.

Figure 1. Across-Base Distribution of Shares of Pooled Chronic-Disease Visits with Black Providers



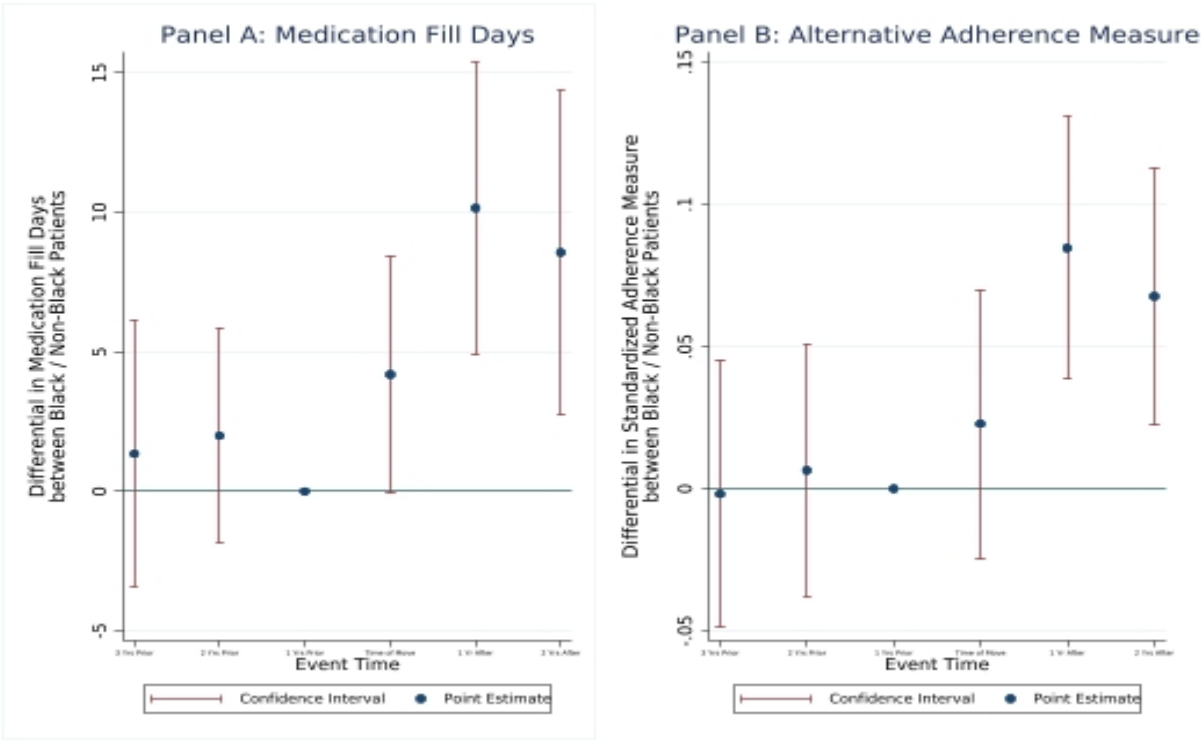
Notes: this figure presents a histogram of base-specific proportions of chronic-disease visits with Black providers, among visits with providers of known race.

Figure 2. Distribution of Move-Induced Changes in Black-Provider Shares



Notes: this figure presents a histogram of move-induced changes in prevailing shares of Black providers across a sample of chronic disease patients who move over the sample period.

Figure 3. Event-Study Results. Relationship between Racial Concordance and Prevention Adherence Measures in Years Leading up to and Following Patient Move (Pooled Sample)



Notes: these figures present estimates of the racial concordance interaction coefficient from specification (4)—i.e., the estimated coefficient of the interaction between the Black-patient indicator and the move-induced change in the prevailing Black-provider share. Each point estimate (and reported 95% confidence interval) depicted in each figure is from a different regression, where the dependent variable in each regression represents the difference in levels for the indicated outcome variable at the indicated event time relative to the reference period (1-year prior to the move). Panel A reports results for medication fill days and Panel B for the standardized prevention measure (Z-score). Accordingly, these figures trace how the indicated outcome measure—for Black relative to non-Black patients—evolves in the time leading up to and following a move event, where a move is characterized by a 1-standard deviation increase in the prevailing Black-provider share. Specifications include sending and receiving base fixed effects. We explore a balanced sample and include only those patients we can observe over the entire event window.

ONLINE APPENDIX

TO

Racial Concordance and the Quality of Medical Care: Evidence from the Military

Michael Frakes, Duke Law and NBER

Jonathan Gruber, MIT and NBER

Additional Descriptive Statistics

Table A1. Summary Statistics

	Full Sample		Black Patients		Non-Black Patients	
Variable	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Panel A: Diabetes Sample						
Medication Fill Days	36.438	(100.064)	38.768	(99.254)	35.790	(100.279)
Incidence of Comprehensive Diabetes Care	0.362	(0.481)	0.391	(0.488)	0.354	(0.478)
Incidence of Blood Pressure Control	0.838	(0.368)	0.788	(0.409)	0.853	(0.353)
Incidence of Mortality	0.012	(0.108)	0.011	(0.105)	0.012	(0.109)
Black Physician Share of Base (Whole Sample)	0.091	(0.069)	0.104	(0.067)	0.088	(0.069)
Black Physician Share of Base (Initial Sample)	0.078	(0.057)	0.088	(0.059)	0.074	(0.056)
Black Patient	0.218	(0.413)	-		-	
Panel B: Hypertension Sample						
Number of Fill Days of Hypertension Medications	175.783	(188.862)	181.416	(185.974)	173.421	(190.011)
Incidence of Blood Pressure Control	0.725	(0.446)	0.688	(0.463)	0.738	(0.439)
Incidence of Mortality	0.013	(0.112)	0.011	(0.105)	0.013	(0.114)
Black Physician Share of Base (Whole Sample)	0.085	(0.058)	0.097	(0.055)	0.081	(0.058)
Black Physician Share of Base (Initial Sample)	0.075	(0.047)	0.084	(0.046)	0.073	(0.047)
Black Patient	0.240	(0.427)	-		-	
Panel C: Hypercholesterolemia Sample						
Number of Fill Days of Hypercholesterolemia Medications	71.511	(128.473)	58.899	(113.610)	74.140	(131.207)

Incidence of Mortality	0.005	(0.069)	0.005	(0.069)	0.005	(0.070)
Black Physician Share of Base (Whole Sample)	0.086	(0.060)	0.097	(0.057)	0.084	(0.060)
Black Physician Share of Base (Initial Sample)	0.069	(0.045)	0.076	(0.044)	0.067	(0.045)
Black Patient	0.172	(0.377)	-	-	-	-
<u>Panel D: Cardiovascular Disease Sample</u>						
Number of Fill Days of Statins	125.911	(150.218)	89.657	(131.261)	133.531	(152.816)
Incidence of Mortality	0.022	(0.146)	0.019	(0.135)	0.023	(0.149)
Black Physician Share of Base (Whole Sample)	0.062	(0.063)	0.064	(0.054)	0.061	(0.065)
Black Physician Share of Base (Initial Sample)	0.052	(0.051)	0.054	(0.050)	0.051	(0.052)
Black Patient	0.174	(0.379)	-	-	-	-

Notes: data are from the Military Health System Data Repository 2003-2013. Descriptive statistics in each panel are taken from a person-year sample of patients with the indicated chronic disease/condition prior to collapsing down to the person-level sample estimated in the changes specification set forth in specification (2).

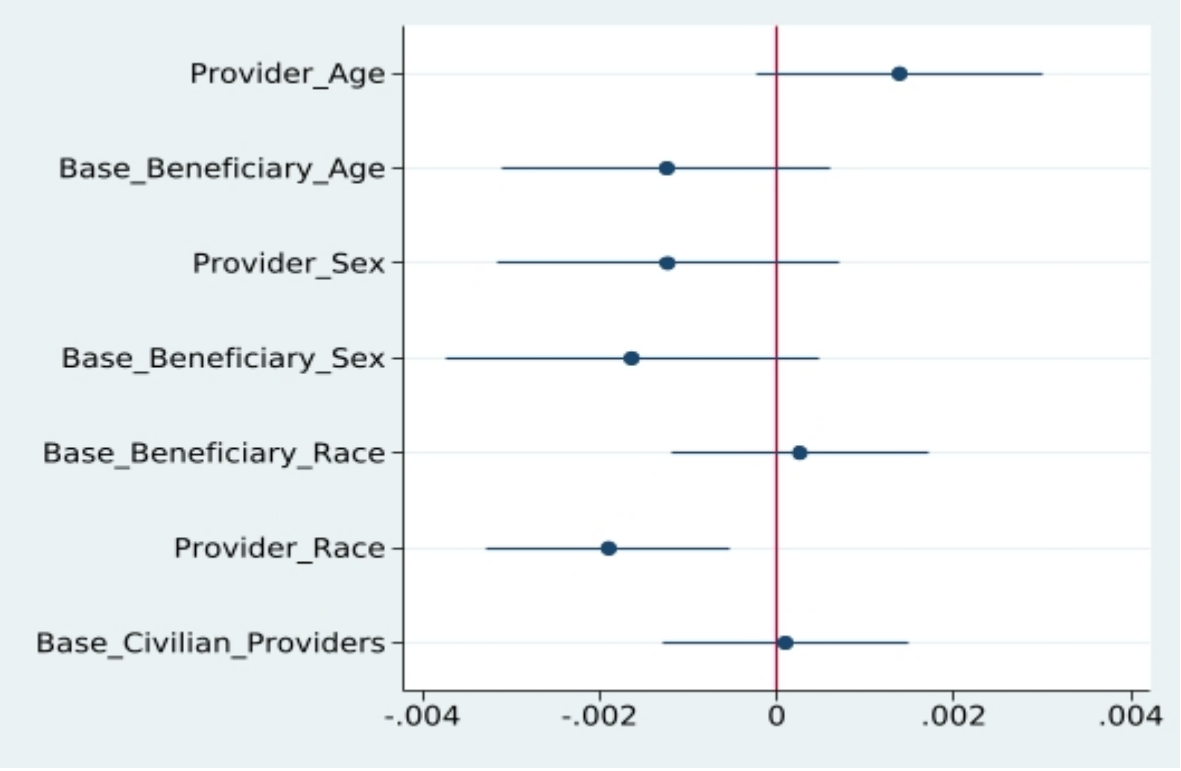
Estimated Effects of Move-Induced Changes in Other Base-Specific Demographic Measures

In Table 3 of the text, we only reported the coefficients of the key concordance variable of interest—i.e., the coefficient of the interaction term in equation (2) between the incidence of a Black patient and the change in the Black-provider share induced by the move (scaled by its standard deviation). However, the underlying specification included a number of other variables. Included in such controls were measures of move-induced changes in other factors, along with the interactions of such changes with the incidence of a Black patient. Those factors include: the mean age of the prevailing providers, the mean incidence of Male providers among the prevailing providers, the mean age of the prevailing base beneficiary population, the mean incidence of a Black beneficiary in the prevailing base population, the mean incidence of a Male beneficiary in the prevailing base population, and the mean incidence of missing-provider-race codes among MTF encounters on the base (mainly, due to the incidence of encounters with civilian contractors/employees on the base). These other variables—and their interactions with beneficiary race—are mainly included as nuisance controls—i.e., in our attempt to isolate the influence of provider race, it is important to control for provider sex and age. In the forest plot below, we expand on the results presented in Panel C, Column 1 of Table 3 and plot the estimated coefficients of the interactions between patient race and these various nuisance controls. We also include the coefficient of the main interaction term, capturing our concordance effect of interest—labelled as Provider Race in the figure.

Encouragingly, a move-induced change in the degree to which provider race is unavailable in base-encounter records—because of the presence of civilian providers on the base—has no differential impact on outcomes between Black and non-Black patients. Since the goal of the paper is to draw inferences when observing concordance in demographic measures, it would be concerning to the extent we saw impacts in situations where there is a change in the availability of demographic information.

It would be less concerning if we were to observe a differential effect on Black versus non-Black patients on move-induced changes in the other demographic measures depicted in this Figure A1. That is, it is important that we include them as nuisance controls, but not necessarily important for our analysis that these other factors have zero influence on differential outcomes for Black and non-Black patients. They are included as nuisance controls, after all, based on a conceptual concern that Black and non-Black patients may respond differently to prevailing changes in such measures. None of the other interaction terms are significantly different from zero and none of the other terms have point estimates as large as the racial concordance measure of interest. However, several of them have point estimates that likewise suggest differential declines in mortality for Black relative to non-Black patients—e.g., as more males are present (both in terms of beneficiaries and providers).

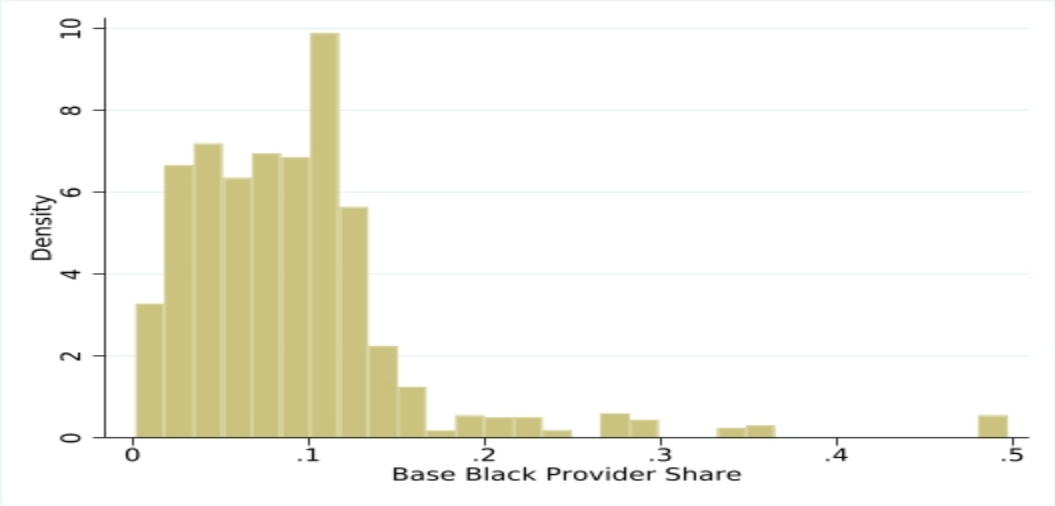
Figure A1. Expanded Results from Table 4, Panel C, Column 1. Estimated Coefficients of Interaction between Black Patient and the Move-Induced Change (Normalized by Standard Deviation) of the Indicated Measure



Notes: this figures plots additional estimated coefficients from the specification reported in Panel C, Column 1 of Table 3 of the text. More specifically, it plots the coefficients of the interaction term between patient race (Black) and the move-induced change in the indicated base-specific measure. These measures capture move-induced changes in other provider demographics and in demographics of the base population, along with move-induced changes in the percentage of on-base encounters with providers of unknown race (because those encounters on the base are with civilian providers). Provider_Race captures the coefficient of interest already reported in Table 3. 95% confidence intervals are reported in the horizontal blue bar. Point estimates are indicated by the blue dot.

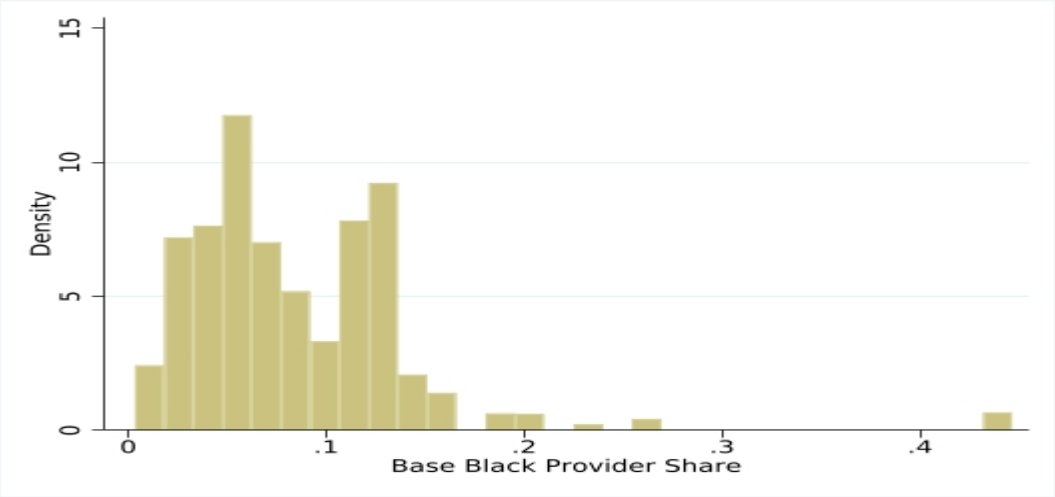
Disease-Specific Black-Provider Share Distributions Across Bases

Figure A2. Across-Base Distribution of Shares of Diabetes Visits with Black Providers



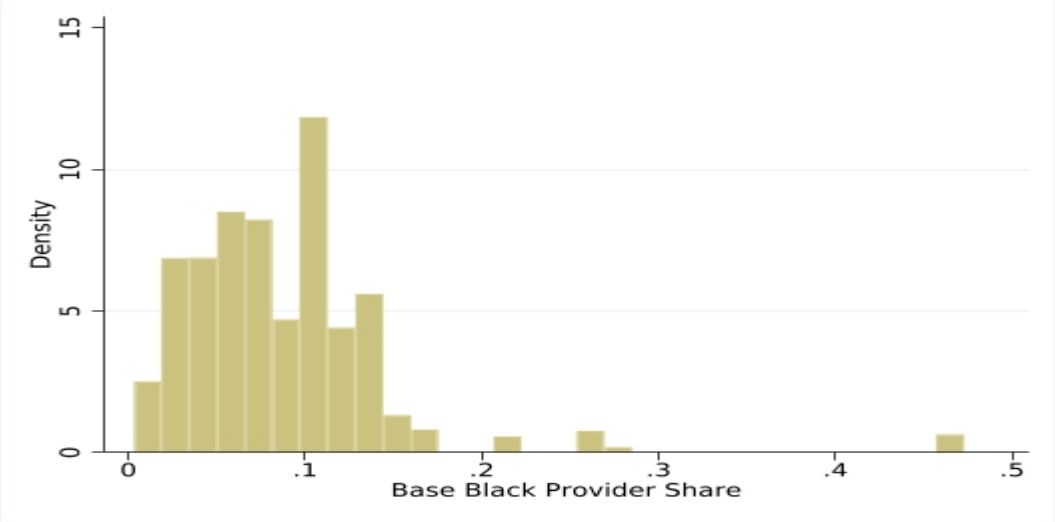
Notes: this figure presents a histogram of base-specific proportions of diabetes visits with Black providers, among visits with providers of known race.

Figure A3. Across-Base Distribution of Shares of Hypertension Visits with Black Providers



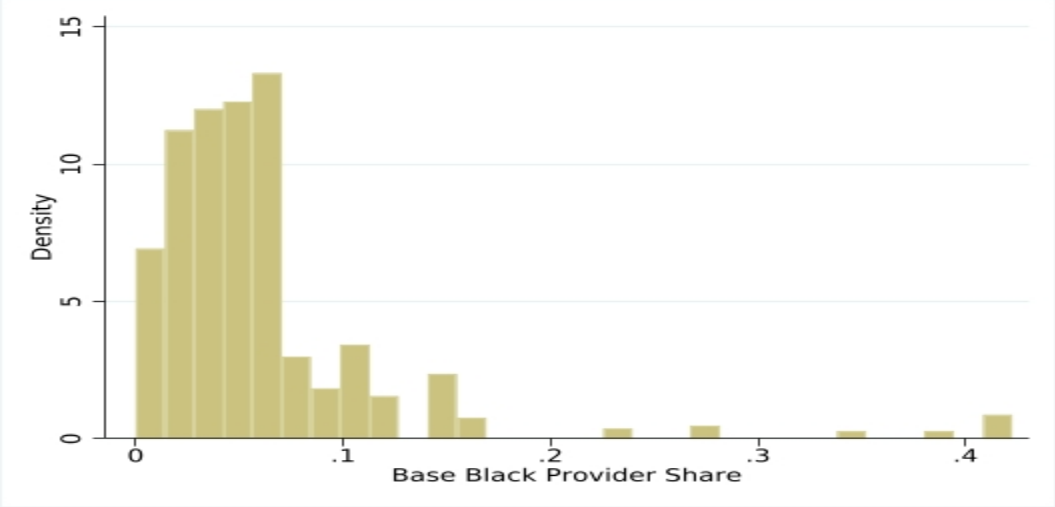
Notes: this figure presents a histogram of base-specific proportions of hypertension visits with Black providers, among visits with providers of known race.

Figure A4. Across-Base Distribution of Shares of High Cholesterol Visits with Black Providers



Notes: this figure presents a histogram of base-specific proportions of hypercholesterolemia visits with Black providers, among visits with providers of known race.

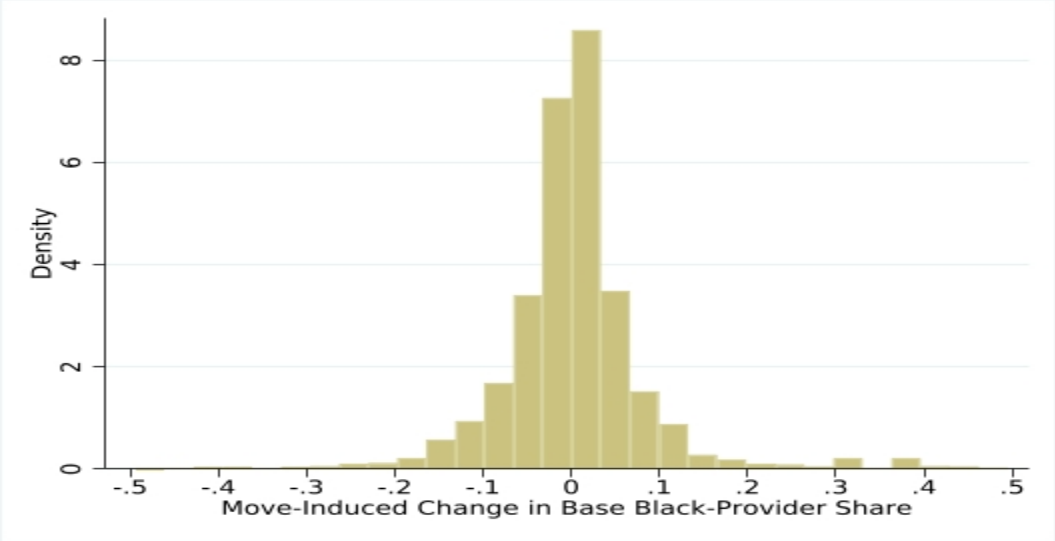
Figure A5. Across-Base Distribution of Shares of Atherosclerotic Cardiovascular Disease Visits with Black Providers



Notes: this figure presents a histogram of base-specific proportions of atherosclerotic cardiovascular-disease visits with Black providers, among visits with providers of known race.

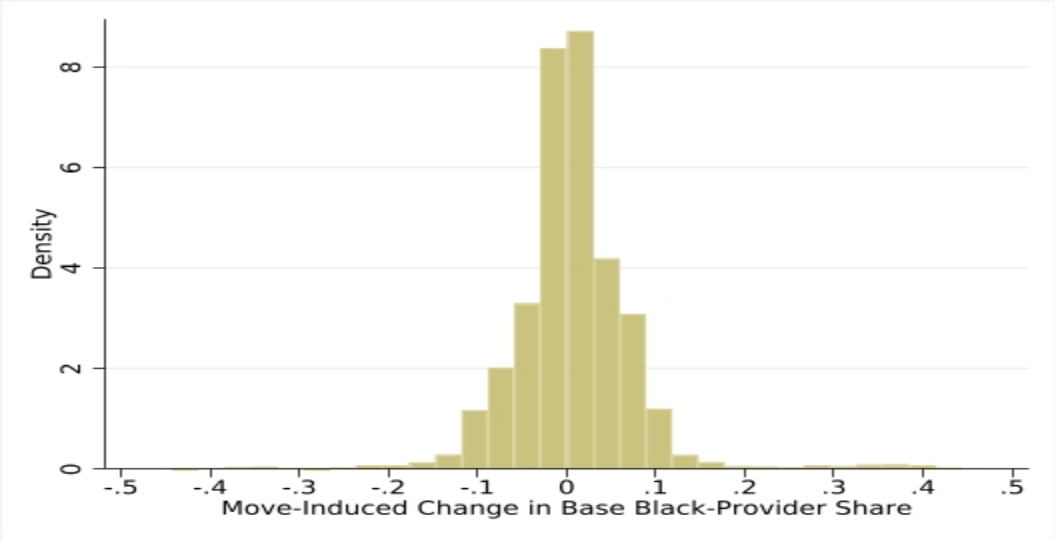
Distribution of Move-Induced Changes in Black Provider Shares, Sample Specific

Figure A6. Distribution of Changes in Black Physician Share Induced by Patient Move, Diabetes Sample



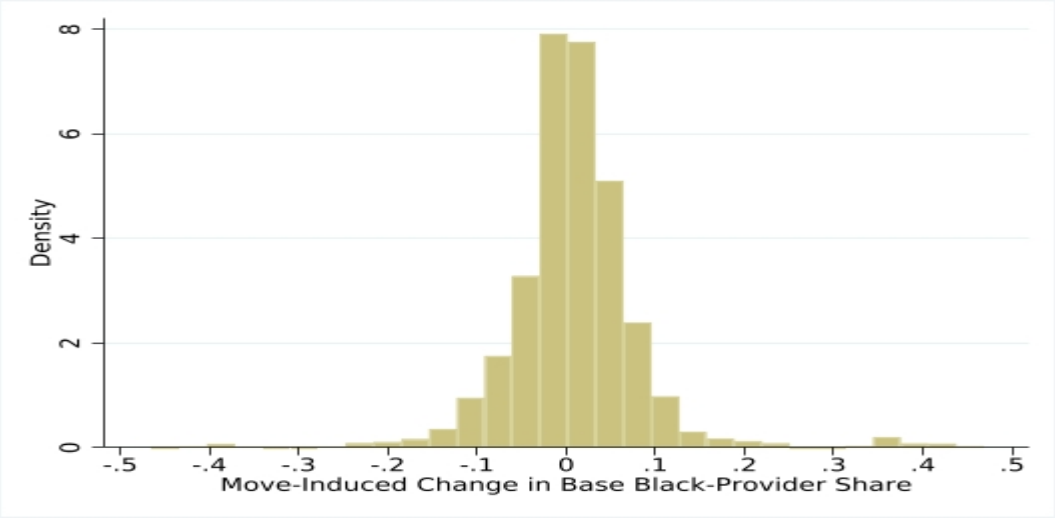
Notes: this figure presents a histogram of move-induced changes in prevailing shares of Black providers across a sample of diabetes patients who move over the sample period.

Figure A7. Distribution of Changes in Black Physician Share Induced by Patient Move, Hypertension Sample



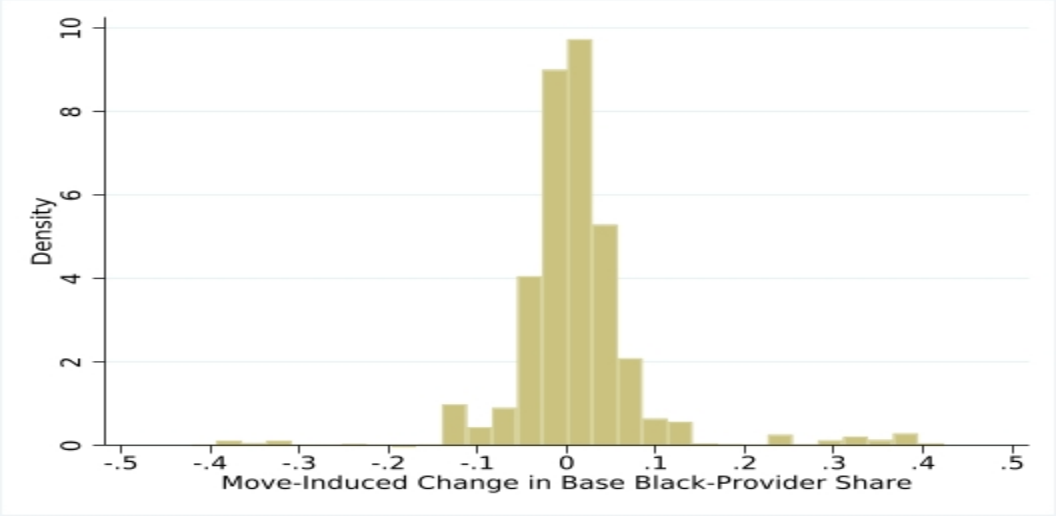
Notes: this figure presents a histogram of move-induced changes in prevailing shares of Black providers across a sample of hypertension patients who move over the sample period.

Figure A8. Distribution of Changes in Black Physician Share Induced by Patient Move, Hypercholesterolemia Sample



Notes: this figure presents a histogram of move-induced changes in prevailing shares of Black providers across a sample of high cholesterol patients who move over the sample period.

Figure A9. Distribution of Changes in Black Physician Share Induced by Patient Move, Atherosclerotic Cardiovascular Disease Sample



Notes: this figure presents a histogram of move-induced changes in prevailing shares of Black providers across a sample of atherosclerotic cardiovascular disease patients who move over the sample period.

Diabetes Analysis: Robustness

In Table 2 of the text, we focus on presenting estimated coefficients of the key interaction term from specification 2, τ. In the follow table, we expand on the results presented in Column 1 of Table 2 but also show the estimated coefficient of the ΔPB_j variable, which is suggestive of the impact of a Black-provider-increasing move for non-Black patients.

Table A2. Diabetes Sample: Relationship between Racial Concordance and Preventive Care Utilization and Outcomes, Including Main Provider-Share Coefficients

	Adherence Analysis. Dependent Variable = Change in Metformin Fill Days	Adherence Analysis. Dependent Variable = Change in Mean Incidence of CDC	Biomarker Analysis. Dependent Variable = Change in Mean Incidence of Blood Pressure Control	Mortality Analysis. Dependent Variable = Incidence of Mortality over Sample
1-Standard Deviation Change in Base Black-Provider Share	-0.685	-0.014***	-0.003	0.000
	(1.058)	(0.005)	(0.006)	(0.001)
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	5.961***	0.029***	0.007	-0.004**
	(1.879)	(0.009)	(0.010)	(0.002)
N	19549	19549	19549	19549

Notes: this table replicates the specifications estimated in Column 1 of Table 2 of the text. It includes the estimated coefficient of the move-induced change in the base black-provider share (omitted from Table 2 for brevity purposes). ***Significant at the 1 percent level; ** Significant at the 5 percent level; *Significant at the 10 percent level.

Next, we demonstrate the robustness of the diabetes findings to an alternative volume weighting of the Black-provider share. In Table 2 of the text, we use diabetes visit data from all years to form measure of the average share of diabetes visits seen by Black providers on the focal base. We replicate Table 2's findings in Columns 1 and 2 of Table A3. In Columns 3 and 4, we now use only data from the first two sample years to form the relevant share of base-specific diabetes visits seen by Black providers.

Table A3. Diabetes Sample: Relationship between Racial Concordance and Preventive Care Utilization and Outcomes, with Alternative Black-Provider Share Calculations

Panel A. Adherence Analysis				
Panel A1. Dependent Variable = Change (Post-Move minus Pre-Move) in Metformin Fill Days				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	5.961***	4.223**	3.268*	1.456
	(1.879)	(1.892)	(1.963)	(1.989)
N	19549	19549	19549	19549
Panel A2. Dependent Variable = Change (Post-Move minus Pre-Move) in Mean Incidence of Comprehensive Diabetes Care				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	0.029***	0.017**	0.025***	0.015*
	(0.009)	(0.008)	(0.009)	(0.009)
N	19549	19549	19549	19549
Panel B. Biomarker Analysis				
Dependent Variable = Change (Post-Move minus Pre-Move) in Mean Incidence of Blood Pressure Control (Systolic < 140 and Diastolic < 90)				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	0.007	0.008	0.005	0.007
	(0.010)	(0.011)	(0.010)	(0.010)
N	8466	8466	8466	8466
Panel C. Mortality Analysis				
Dependent Variable = Incidence of Mortality Over Sample				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	-0.004**	-0.004**	-0.003	-0.004*
	(0.002)	(0.002)	(0.002)	(0.002)
N	19549	19549	19549	19549
Including Fixed Effects for Pre- and Post-Move Bases?	NO	YES	NO	YES
Characterization of Base Black-Doc Share	Volume-Weighted Black-Doc Share Over Whole Sample Period		Volume-Weighted Black Doc Share at Beginning of Sample Period	

Notes: Columns 1 and 2 of this table replicate Table 2 of the text. Columns 3 and 4, in turn, replicate Columns 1 and 2 except that we now form the Black-provider share of each base by taking the share of diabetes visits on the relevant base in the first two sample years that are with Black providers (out of visits with doctors of known race). ***Significant at the 1 percent level; ** Significant at the 5 percent level; *Significant at the 10 percent level.

Hypertension sample results, separately

To comprehensively show the main hypertension results and certain robustness analysis in one table, we replicate Table A3 from the diabetes discussion but now focusing on the sample of patients with hypertension. The results are presented in Table A4. The prevention measure used for the hypertension analysis is the number of fill days for hypertension medications, focusing (following the WHO) on a sample of patients with uncontrolled blood-pressure (and thereby also focusing on the 2009-plus data considering the unavailability of vital statistics records prior to that point).

Starting with our prevention measure, our point estimates across the different specifications consistently demonstrate a positive relationship between the differential in the fill days of hypertension medications for Black relative to non-Black patients in connection with moves that increase the prevailing Black-providing share; however, the results are somewhat noisy in light of the small sample over which they are estimated—i.e., those with uncontrolled diabetes, which we can only track in the 2009-plus period anyway. Below and in Table 3 of the text, we pool this adherence analysis with that of the other disease samples to achieve greater power.

In Panel B of Table A4, we find evidence of beneficial concordance effects on the incidence of maintaining blood pressure control (systolic less than 140 and diastolic less than 90). In unreported regressions, we demonstrate that this is largely driven by a decrease in systolic levels.

Our point estimates imply a large negative effect on mortality, although these results are only marginally significant in Columns 3 and 4 where we determine provider-race shares across bases based on visit-volume weighting at the beginning of the sample period.

Table A4. Hypertension Sample: Relationship between Racial Concordance and Preventive Care Utilization and Outcomes

Panel A. Adherence Analysis				
Dependent Variable = Change (Post-Move minus Pre-Move) in Fill Days of Hypertension Medications (among Patients with Uncontrolled Hypertension)				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	2.735	3.208	5.551	7.435
	(5.196)	(5.323)	(4.896)	(5.077)
N	6994	6994	6994	6994
Panel B. Biomarker Analysis				
Dependent Variable = Change (Post-Move minus Pre-Move) in Incidence of Blood Pressure Control				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	0.015**	0.014**	0.013**	0.012*
	(0.007)	(0.007)	(0.006)	(0.006)
N	34810	34810	34810	34810
Panel C. Mortality Analysis				
Dependent Variable = Incidence of Mortality Over Sample				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	-0.0010	-0.0012	-0.0018*	-0.0020*
	(0.0011)	(0.0011)	(0.0011)	(0.0011)
N	71981	71981	71981	71981
Including Fixed Effects for Pre- and Post-Move Bases?	NO	YES	NO	YES
Characterization of Base Black-Doc Share	Volume-Weighted Black-Doc Share Over Whole Sample Period		Volume-Weighted Black Doc Share at Beginning of Sample Period	

Notes: This table replicates Table A3 but using the hypertension sample. The medication fill-days sample is estimate on a limited sample of patients not in control of their blood pressure (data of which is available only in the 2009-plus period and only for on-base records). Panel B is likewise estimated only in the 2009-plus period and only for on-base records. ***Significant at the 1 percent level; ** Significant at the 5 percent level; *Significant at the 10 percent level.

Atherosclerotic Cardiovascular Disease Sample Analysis

Table A5 shows the analysis for the atherosclerotic cardiac disease sample. Here our prevention measure is once again input based, the number of fill days of statins. Across three of the four specifications, we find point estimates suggesting a positive effect of concordance on statin fill days, though none of these findings are significant. We also see large reductions in mortality in connection with racial concordance, although this effect is only significant when basing provider race shares on early-sample data. Again, pooling this sample with the other chronic-disease samples will allow us to estimate these general relationships with greater precision.

Table A5. Atherosclerotic Cardiovascular Disease Sample: Relationship between Racial Concordance and Preventive Care Utilization and Outcomes

Panel A. Adherence Analysis				
Dependent Variable = Change (Post-Move minus Pre-Move) in Fill Days of Statins				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	1.320	-0.271	1.608	2.348
	(2.885)	(3.007)	(2.408)	(2.502)
N	10496	10496	10496	10620
Panel B. Mortality Analysis				
Dependent Variable = Incidence of Mortality Over Sample				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	-0.0040	-0.0045	-0.0068**	-0.0066**
	(0.0037)	(0.0038)	(0.0030)	(0.0031)
N	12090	12090	12090	12090
Including Fixed Effects for Pre- and Post-Move Bases?	NO	YES	NO	YES
Characterization of Base Black-Doc Share	Volume-Weighted Black-Doc Share Over Whole Sample Period		Volume-Weighted Black Doc Share at Beginning of Sample Period	

Notes: This table replicates Table A3 but using the atherosclerotic cardiovascular disease sample. The medication fill-days sample follows HEDIS recommendations for statin use and does not include women under the age of 40. We include the full adult sample in the mortality specifications (though the results are robust to including the same age restrictions in the mortality analysis). ***Significant at the 1 percent level; ** Significant at the 5 percent level; *Significant at the 10 percent level.

Hypercholesterolemia Sample Analysis

Table A6 shows the analysis for the hypercholesterolemia sample. Our prevention adherence analysis in this sample is consistent with the above prevention analysis in suggesting a positive effect of racial concordance. Further consistent with above, we find that the incidence of mortality falls for Black relative to non-Black patients in connection with a move-induced increase in the Black provider share (though not significant when forming provider shares using visit-volume weights based on whole-sample-period data).

Table A6. Hypercholesterolemia Sample: Relationship between Racial Concordance and Preventive Care Utilization and Outcomes

Panel A. Adherence Analysis				
Dependent Variable = Change (Post-Move minus Pre-Move) in Fill Days of Hypercholesterolemia Medications				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	2.465**	2.400**	1.186	1.493*
	(0.993)	(0.998)	(0.786)	(0.796)
N	93,486	93,486	93,486	93,486
Panel B. Mortality Analysis				
Dependent Variable = Incidence of Mortality Over Sample				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	-0.0013	-0.0013	-0.0015**	-0.0015**
	(0.0008)	(0.0008)	(0.0007)	(0.007)
N	93,486	93,486	93,486	93,486
Including Fixed Effects for Pre- and Post-Move Bases?	NO	YES	NO	YES
Characterization of Base Black-Doc Share	Volume-Weighted Black-Doc Share Over Whole Sample Period		Volume-Weighted Black Doc Share at Beginning of Sample Period	

Notes: This table replicates Table A3 but using the hypercholesterolemia sample. ***Significant at the 1 percent level; ** Significant at the 5 percent level; *Significant at the 10 percent level.

Pooled Sample Robustness

Table A7. Pooled Disease Sample: Relationship between Racial Concordance and Preventive Care Utilization and Outcomes, with Alternative Provider-Base-Share Measure

Panel A. Adherence Analysis				
Panel A1. Dependent Variable = Change (Post-Move minus Pre-Move) in Medication Fill Days. All Samples Pooled.				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	2.989***	2.670***	2.109	2.283***
	(0.982)	(0.992)	(0.798)	(0.814)
N	130705	130705	130705	130705
Panel A2. Dependent Variable = Change (Post-Move minus Pre-Move) in Preventive Adherence Measure (Z-score for Sample-Specific Measure). All Samples Pooled.				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	0.031***	0.028***	0.021***	0.022***
	(0.010)	(0.010)	(0.008)	(0.008)
N	130705	130705	130705	130705
Panel B. Biomarker Analysis				
Dependent Variable = Change (Post-Move minus Pre-Move) in Incidence of Blood Pressure Control. Diabetes and Hypertension Samples Pooled.				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	0.013*	0.012*	0.011*	0.011
	(0.007)	(0.007)	(0.007)	(0.007)
N	43306	43306	43306	43306
Panel C. Mortality Analysis				
Dependent Variable = Incidence of Mortality Over Sample. All Samples Pooled.				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	-0.0018***	-0.0019***	-0.0020***	-0.0021***
	(0.0007)	(0.0007)	(0.0007)	(0.0007)
N	197508	197508	197508	197508
Including Fixed Effects for Pre- and Post-Move Bases?	NO	YES	NO	YES
Characterization of Base Black-Doc Share	Volume-Weighted Black-Doc Share Over Whole Sample Period		Volume-Weighted Black Doc Share at Beginning of Sample Period	

Notes: Columns 1 and 2 of this table replicate Table 3 of the text. Columns 3 and 4, in turn, replicate Columns 1 and 2 except that we now form the Black-provider share of each base by taking the share of chronic-disease visits on the relevant base in the first two sample years that are with Black providers (out of visits with doctors of known race). ***Significant at the 1 percent level; ** Significant at the 5 percent level; *Significant at the 10 percent level.

Table A8. Pooled Disease Sample Analysis, Excluding All Races Other Than Black and White Patients

Panel A. Adherence Analysis				
Panel A1. Dependent Variable = Change (Post-Move minus Pre-Move) in Medication Fill Days. All Samples Pooled.				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	2.803***	2.590***	1.895**	1.895**
	(0.997)	(1.010)	(0.808)	(0.808)
N	115502	115502	115502	115502
Panel A2. Dependent Variable = Change (Post-Move minus Pre-Move) in Preventive Adherence Measure (Z-score for Sample-Specific Measure). All Samples Pooled.				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	0.027***	0.025***	0.016***	0.019***
	(0.010)	(0.010)	(0.008)	(0.008)
N	115502	115502	115502	115502
Panel B. Biomarker Analysis				
Dependent Variable = Change (Post-Move minus Pre-Move) in Incidence of Blood Pressure Control. Diabetes and Hypertension Samples Pooled.				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	0.011	0.010	0.010	0.009
	(0.007)	(0.007)	(0.007)	(0.007)
N	37761	37761	37761	37761
Panel C. Mortality Analysis				
Dependent Variable = Incidence of Mortality Over Sample. All Samples Pooled.				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	-0.0020***	-0.0020***	-0.0021***	-0.0021***
	(0.0007)	(0.0007)	(0.0007)	(0.0007)
N	172685	172685	172685	172685
Including Fixed Effects for Pre- and Post-Move Bases?	NO	YES	NO	YES
Characterization of Base Black-Doc Share	Volume-Weighted Black-Doc Share Over Whole Sample Period		Volume-Weighted Black Doc Share at Beginning of Sample Period	

Notes: this table replicates that of Table A7, except that it excludes all patients with self-identified race other than Black or white. ***Significant at the 1 percent level; ** Significant at the 5 percent level; *Significant at the 10 percent level.

Multiple Movers Analysis

Our primary approach in the text focuses on a sample that moves one time over the sample period. In this section of the Appendix, we expand the sample to further include those that move more than once. For the multiple movers, we focus on just one of their moves—i.e., their final move. For multi-movers, we choose their final move as we cannot consider the effects of prior-to-final moves on mortality (after all, ostensibly, one would never experience a mortality event during a pre-final-move period). We preference the single move approach as it provides us with a longer collective observation window for our subjects—e.g., an average of 6.7 years of observation versus an average of 4.4 years of per-patient observation if we included all movers and focused on the last move for multi-movers. In any event, we present results from this broader movers sample in Table A9.

Table A9. Pooled Disease Sample Analysis, including the Last Move For Movers who Move More Than Once

Panel A. Adherence Analysis				
Panel A1. Dependent Variable = Change (Post-Move minus Pre-Move) in Medication Fill Days. All Samples Pooled.				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	2.690***	2.391***	1.864***	1.939***
	(0.748)	(0.754)	(0.594)	(0.605)
N	212068	212068	212068	212068
Panel A2. Dependent Variable = Change (Post-Move minus Pre-Move) in Preventive Adherence Measure (Z-score for Sample-Specific Measure). All Samples Pooled.				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	0.027***	0.023***	0.016***	0.016***
	(0.008)	(0.008)	(0.006)	(0.006)
N	212068	212068	212068	212068
Panel B. Biomarker Analysis				
Dependent Variable = Change (Post-Move minus Pre-Move) in Incidence of Blood Pressure Control. Diabetes and Hypertension Samples Pooled.				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	0.009	0.009	0.010*	0.010*
	(0.006)	(0.006)	(0.006)	(0.006)
N	55011	55011	55011	55011
Panel C. Mortality Analysis				
Dependent Variable = Incidence of Mortality Over Sample. All Samples Pooled.				
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	-0.0015***	-0.0015***	-0.0015***	-0.0015***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
N	332382	332382	332382	332382
Including Fixed Effects for Pre- and Post-Move Bases?	NO	YES	NO	YES
Characterization of Base Black-Doc Share	Volume-Weighted Black-Doc Share Over Whole Sample Period		Volume-Weighted Black Doc Share at Beginning of Sample Period	

Notes: this table replicates Table A7 except that it expands the sample to include patients who move more than one during the sample period. For those that move more than once, these specifications include data for their final move—i.e., the pre-move period represents the time following their penultimate move and their final move. ***Significant at the 1 percent level; ** Significant at the 5 percent level; *Significant at the 10 percent level.

Pooled Sample Analysis, Focusing Only on Active-Duty and Active-Duty Dependents

We now estimate specifications where we focus only on a sample of active-duty and active-duty dependents (ADD). While this sample is notably smaller and thus generates less precise estimates, it represents a sample with an even stronger case for exogenous moves. To better ensure exogeneity in moves, as discussed in the text, we follow Lleras-Muney (2010) in controlling for time, rank, and occupation when focusing on this ADD sample.

We also show a version of the event study graph depicted in Figure 3 for both versions of the prevention adherence measure. In Figure 3 of the text, we focused on a balanced approach and followed only those patients we could observe over the entire event window (3 years pre and post move). Given the significant reduction in sample size when dropping retirees, we focus on a slightly narrower event window in these counterparts to Figure 3 so that our balance condition does not cut our sample size too extensively (though, the pattern we present is not very sensitive to the precise event-size window).

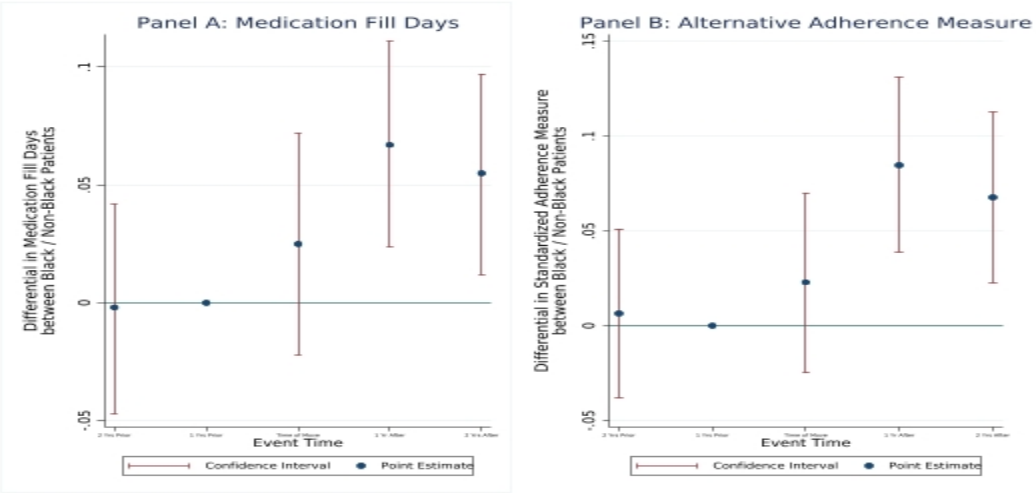
Overall, the results suggest similar results when focus on this more selected ADD sample. Our overall mortality results are nearly identical. The point estimates of our biomarker analysis are smaller and indistinguishable from zero, though still positive. The biomarker analysis was already estimated on a limited sample (2009 and beyond, on-base only) and this ADD restriction only cut that sample further, leaving relatively imprecise results. Between the tabular before-after-move results depicted in Table A10 and the event study results depicted in Figure A10, we also find evidence of an increase in preventive care for Black relative to non-Black patients in connection with a move-induced increase in the Black-provider share, which is in turn suggestive of a beneficial effect of racial concordance between providers and patients.

Table A10. Pooled Disease Sample: Relationship between Racial Concordance and Preventive Care Utilization and Outcomes, Active Duty Sample

	Prevention Adherence Analysis: Medication Fill-Days	Prevention Adherence Analysis: Standardized Adherence Measure	Biomarker Analysis: Incidence of Blood Pressure Control	Outcomes Analysis: Incidence of Mortality
Black Patient X Change in Base Black-Provider Share	0.923	0.018*	0.005	-0.0019*
	(1.107)	(0.011)	(0.008)	(0.0010)
N	65388	65388	27050	98364

Notes: This table replicates the specifications underlying Column 2 of Table 3, except that it excludes retirees and dependents of retirees and includes fixed effects for occupation groups of the active-duty sponsor along with fixed effects for the move year following Lleras-Muney (2010). ***Significant at the 1 percent level; ** Significant at the 5 percent level; *Significant at the 10 percent level.

Figure A10. Event-Study Results. Excluding Retirees



Notes: This figure replicates Figure 3, except that it excludes retirees and dependents of retirees and includes fixed effects for occupation groups of the active-duty sponsor along with fixed effects for the move year following Lleras-Muney (2010). Specifications are estimated on a balanced sample of chronic disease patients whom we can observe over full event window. ***Significant at the 1 percent level; ** Significant at the 5 percent level; *Significant at the 10 percent level.

LATE Analysis: Comparing Movers and Non-Movers

TABLE A11: COMPARING MOVERS AND NON-MOVERS IN DIABETES SAMPLE

	Full Sample	
	Movers	Non-Movers
Medication Fill Days	36.677 (101.503)	39.880 (105.976)
Incidence of "Comprehensive Diabetes Care"	0.362 (0.480)	0.413 (0.492)
Incidence of mortality over sample	0.012 (0.108)	0.014 (0.118)
Incidence of blood pressure control (systolic < 140 & diastolic < 90)	0.838 (0.368)	0.802 (0.399)
Age	46.667 (11.905)	50.634 (10.977)
Charlson Index	0.974 (1.142)	1.068 (1.194)
Annual Relative Value Units	47.614 (58.809)	48.073 (59.896)
Annual Inpatient Days	0.658 (5.104)	0.663 (5.594)

Notes: the reported statistics are based on a person-year sample of patients with diabetes.

Diabetes Sample: Additional Mortality-Effect-Decomposition Analysis

We also explore how much the effect of racial concordance on blood pressure control contributes to concordance's effect on mortality. At the outset, we note that our review of the literature evidenced no relationship between metformin adherence and blood pressure control. In this case, whatever role in the mortality-effect decomposition we attribute to improved blood pressure control is independent of the role attributed to improved metformin use. To decompose the mortality effect into the effect of improved blood pressure control and a residual, consider the following:

Overall Percentage-Point Mortality Effect (From Table 2 = -0.4) =

[*Percentage-point Increase in Blood Pressure Control* (Point estimate from Table 2 = 0.8) X

Percentage-point Impact on Mortality of Percentage-Point Increase in Blood Pressure Control (From literature = -0.16)] +

Residual

The residual of this equation as a share of the overall mortality effect amounts to 68%, with 32% of the mortality effect being explained by an increase in blood pressure control stemming from racial concordance (0.8 times -0.16 divided by -0.4).

While, as above, improved metformin use does not account for any portion of this 32% blood-pressure contribution, improved Comprehensive Diabetes Care (CDC) does account for some. Our review of the diabetes literature, after all, suggests a relationship between CDC adherence and blood pressure control.

To determine how much of the mortality effect derives from (i) the effect of improved CDC on blood pressure control and (ii) from the resulting effect of improved blood pressure control on mortality (again drawing from the diabetes literature), consider the following:

Overall Percentage-Point Mortality Effect (From Table 2 = -0.4) =

[*Percentage-point Increase in Comprehensive Diabetes Care* (From Table 2 = 1.7) X

Percentage-point Impact on Blood Pressure Control of Percentage-Point Increase in Comprehensive Diabetes Care (Average from literature = 0.22) X

Percentage-point Impact on Mortality of Percentage-Point Increase in Blood Pressure Control (Average from literature = -0.16)] + *Residual*

Solving for the residual as a share of the overall mortality effect, we find that roughly 85% of the overall concordance mortality effect can be explained by something other than a CDC-induced increase in blood pressure control. In turn, this suggests that 15% of the overall mortality result can be explained by this channel. In other words, most of the Comprehensive-Diabetes-Care explanation for concordance's mortality effect—the 21% derived in the text—operates through an intervening effect on blood pressure control.

References

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Hypertension Disease Mortality Decomposition Analyses

Let's begin with a decomposition of the racial-concordance mortality effect for the hypertension sample into the effect arising from improved preventive care and from other sources. Intuitively, we can do so by decomposing the overall mortality effect into the effect of

concordance on the relevant medication fill days times the effect of hypertension medication fill days on mortality plus a residual:

Overall Percentage-Point Mortality Effect (From Table A4 = -0.1) =

[Fill-Day Increase in Hypertension Medication (From Table A4 = 2.735) X

Percentage-point Impact on Mortality of Fill-Day Increase in Hypertension Medications (Average Estimate from Literature = -0.02)] +

Residual

Solving for the residual and scaling it by the -0.1 overall mortality result, we find that roughly 45% of the mortality effect of racial concordance can be explained by something other the increased fill-days of hypertension medications and that 55% can in turn be explained by the preventive-care increase.

Next, we determine how much of the mortality effect derives from (i) the effect of improved preventive-care on blood pressure control and (ii) from the resulting effect of improved blood pressure control on mortality (drawing from the hypertension literature):

Overall Percentage-Point Mortality Effect (From Table A4 = -0.1) =

[*Percentage-point Increase in Hypertension Medication Fill-Days* (From Table A4 = 2.735) X

Percentage-point Impact on Blood Pressure Control of Fill-Day Increase in Hypertension Medication (Average from literature = 0.22) X

Percentage-point Impact on Mortality of Percentage-Point Increase in Blood Pressure Control (Average from literature = -0.05)] + *Residual*

Again solving for the residual as a share of the overall mortality effect, we find that roughly 70% of the overall concordance mortality effect can be explained by something other than a preventive-care-induced increase in blood pressure control. In turn, this suggests that 30% of the overall mortality result can be explained by this channel. In other words, much of the preventive-care explanation for concordance's mortality effect—the 55% derived above—operates through an intervening effect on blood pressure control.

Finally, we determine how much of the hypertension mortality effect derives from the improved blood-pressure control channel but not arising through improved preventive care. To

determine this, we first need to decompose the concordance effect on blood pressure control into the role played by improved preventive care and a residual:

Overall Percentage-Point Blood Pressure Effect (From Table A4 = 1.5) =

[Fill-Day Increase in Hypertension Medications *(*From Table A4 = 2.735) X

Impact of Hypertension Medication Fill Day on Blood Pressure Control (Average from literature = 0.22)] +

Residual

Solving for the residual as a share of this overall blood-pressure effect, we find that roughly 60% of the effect of concordance on blood-pressure effect cannot be explained by the concordance effect on hypertension medication fill-days. Now, to determine how much of the mortality effect in turn arises from a blood-pressure-control effect not arising through improved preventive care, we derive:

Overall Percentage-Point Mortality Effect (From Table A4 = -0.1) =

[*Percentage-point Increase in Blood Pressure Control* (Point estimate from Table A4 = 1.5) X

Portion of Blood Pressure Control Effect Not Due to Improved CDC (just estimated = 0.60) X

Percentage-point Impact on Mortality of Percentage-Point Increase in Blood Pressure Control (From

literature = -0.05)] + *Residual*

The multiplicative terms on the right hand side give us our object of interest: -0.045 (1.5 X 0.60 X -0.05). This amount is roughly 45% of the overall mortality effect of concordance. In other words, this decomposition suggests that 45% of the mortality effect can arise through an improved blood pressure control effect that does not arise due to higher hypertension medication fill-days.

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Hypercholesterolemia Mortality Decomposition Analyses

We now decompose the racial-concordance mortality effect for the high cholesterol sample (the point estimate from Table A6) into the effect arising from improved preventive care—high cholesterol medication fill days—and from other sources:

Overall Percentage-Point Mortality Effect (From Table A6 = -0.13) =

[Hypercholesterolemia Medication Fill-Days *Increase* (From Table A6 = 2.465) X

Percentage-point Impact on Mortality of Fill-Day Increase in Hypercholesterolemia Medication (Average

Estimate from Literature = -0.032)] +

Residual

Solving for the residual as a share of the overall concordance effect of mortality in this high cholesterol sample, we find that roughly 39% of the mortality effect of racial concordance can be explained by something other the increased use of high cholesterol medication fill days. In which case, roughly 61% can be explained by the fill-day effect.

References (for fill-day-to-mortality relationship)

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Atherosclerotic Cardiovascular Disease Mortality Decomposition Analyses

Finally, we decompose the racial-concordance mortality effect for the atherosclerotic cardiovascular disease sample (point estimate from Table A5) into the effect arising from improved preventive care—statin fill days—and from other sources:

Overall Percentage-Point Mortality Effect (From Table A5 = -0.4) =

[Statin Fill-Days *Increase* (From Table A5 = 1.320) X

Percentage-point Impact on Mortality of Fill-Day Increase in Statins (Average Estimate from Literature = -0.21)] +

Residual

Solving for the residual as a share of the overall concordance effect of mortality in this atherosclerotic cardiovascular disease sample, we find that roughly 31% of the mortality effect of racial concordance can be explained by something other the increased use of statins and that roughly 69% can be explained by statin fill-days increase.

References (for fill-day-to-mortality relationship)

1. Rodriguez F, Maron DJ, Knowles JW, Virani SS, Lin S, Heidenreich PA. Association of Statin Adherence With Mortality in Patients With Atherosclerotic Cardiovascular Disease. JAMA Cardiol. 2019;4(3):206–213. doi:10.1001/jamacardio.2018.4936

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