The Effect of Provider Diversity on Racial Health Disparities: Evidence from the Military

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Abstract:

We assess the relationship between the racial diversity of medical providers and racial health disparities in the use of preventive care and in 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 in a plausibly exogenous fashion. We consider patients with four chronic, deadly, but manageable illnesses, where the relationship with the provider may have the most direct impact on health. We find striking evidence that provider racial diversity leads to reduced disparities in maintenance of preventive care and mortality.

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It is well established in the health economics and medicine literatures that, along many dimensions of health care delivery, rampant disparities exist across patients of different races. For instance, evidence has shown that the rate by which Black Americans between ages 18 and 49 die from diabetes is nearly double that for white Americans of the same ages (Cunningham et al. 2017). The relevant literatures have also shown that the racial composition of the physician workforce is not representative of the underlying American population. While Black Americans constitute roughly 13% of the population, Black physicians in the U.S. account for only 5% of the physician workforce (AAMC 2020). What is much less known to researchers is the extent to which these two sets of facts are causally related to one another. That is, to what extent will an increase in the diversity of the medical provider workforce reduce racial health disparities?

A growing literature has investigated one aspect of this question, which is how the concordance between the races of individual physicians and their patients impacts patient outcomes – most notably the randomized assignment evidence of Alsan et al. (2019). But this literature does not fully speak to the policy question of relevance to current debates over affirmative action and related matters: how will a systematic increase in the share of the medical provider workforce that is Black change the racial health gap?

In particular, there are two reasons why a change in the racial composition of the workforce might have a different effect than that implied by studies exploring individual physician-patient racial concordance. First, there may not be a one-to-one correspondence between more racial diversity of providers and more concordant visits by race. In reality, patients are not randomly assigned to providers but instead choose them. How a changing racial profile of providers will impact that choice is unknown. For example, a higher share of providers who are Black may not deliver measured individual concordance gains if Black patients are less likely than average to see these providers – or may deliver larger gains if Black patients are more likely than average to see them.

Second, there could be important spillovers from increased workforce diversity. For instance, white providers may learn from their newly expanded Black-provider peers on how to better treat the Black patient population, including how to build trust and communication with Black patients. Moreover, such peer exposure may also debias white providers (Cohen et al. 2002). Spillovers may even occur at the patient level—e.g., Black chronic disease patients being treated by white providers may nonetheless gain greater trust in the medical system as a whole when witnessing or hearing about greater Black provider involvement in treating chronic conditions in the community. Spillover and community-level effects of these natures are not captured by the extensive set of studies that have singularly focused on the outcomes associated with racially concordant patient-provider matches during individual encounters.

If one could directly study the effects of an increase in the Black provider share on the differential outcomes between Black and non-Black patients, one could capture the net effect of all those various forces and mechanisms missing from the narrower racial-concordance studies. Surprisingly, this direct policy parameter of interest has been the subject of much less scholarship, perhaps attributable to the difficulty in finding exogenous sources of variation in the racial composition of prevailing provider communities. In this paper, we aim to fill this important gap in the literature by presenting estimates from specifications that precisely aim to explore the link between increases in the availability of Black providers in the nearby medical community and differential health gains for the Black patient population.

To derive such a design, we turn to a setting with broad geographic scope and a large Black population: the U.S. military. The Military Health System (MHS) provides care for 9.6 million active-duty military, retirees, and dependents with expenditures of nearly $56 billion/year (Congressional Research Service 2022). The MHS and its associated databases have several features that provide for an excellent setting to study the effects of provider diversity on racial health disparities. First, unlike much of the related racial-concordance literature, the MHS provides complete claims data for all care used by MHS enrollees, allowing us to assess the impact of medical provider diversity on care patterns and on downstream health outcomes, including mortality. Second, while most claims databases lack demographic information on providers thereby impeding the ability to study provider diversity in the first place, we collect this information by taking advantage of the fact that active-duty providers and providers on military-retiree status are also MHS beneficiaries, to whose demographics we have access.

We use these data to carry out a quasi-experimental analysis of the impacts of increased medical-provider diversity, focusing specifically on the role of Black providers. To derive variation in availability of Black providers, we use a movers strategy that builds on Finkelstein et al. (2016). In particular, we draw on patient moves across military bases combined with heterogeneity across bases in the share of Black providers. To shed light on the determinants of racial health disparities, we explore the effects of such moves for Black relative to non-Black patients, which further allows us to include base-specific effects for both the "sending" and "receiving" bases.

A final limitation of the more specific racial-concordance literature is that its individual-encounter-focused framework fails to adequately capture the care that is delivered in dynamic contexts such as chronic-disease management. In such settings, care is coordinated over a period of time, during the course of which there may be a series of encounters with multiple providers (of potentially multiple races). The literature's failure to consider this context is particularly significant given that provider-patient trust and communication—the conceptual target of many concordance studies—is likely to be an especially meaningful determinant of outcomes in chronic-disease settings. In this paper, we focus on patients with four different chronic 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

Our findings are striking. The results of our movers-based strategy consistently suggest that increases in the diversity of the provider workforce lead to beneficial effects for Black relative to non-Black patients—i.e., positive relative effects in preventive care utilization and biomarkers for good health and negative relative effects for self-reported pain and mortality. For example, we find that a one-standard deviation move-induced increase in the share of Black military providers is associated with an increase of roughly 3 fill days of preventive medications for Black relative to non-Black patients, an amount that is roughly 4% relative to the mean fill day amount. Further, we estimate that a move-induced one-standard deviation increase in the share of providers who are Black leads to a roughly 18% decline in mortality (relative to the mean) for Black relative to non-Black patients.

We note that the identifying assumption inherent in our movers design is that the health outcomes of Black patients relative to non-Black patients—and patients experiencing Black-provider-increasing moves relative to Black-provider-decreasing moves—would have trended in parallel over time but for such moves actually transpiring. The above conclusion is supported by the fact that we find no evidence of differential health trends of this nature in the pre-move period. Further supportive of this assumption, we show that the odds of moving are not related in a systematic way to the existing racial composition of base providers. We also demonstrate that our results are stronger for those who live close to the base, for which the Black-provider-share measure is relevant. Finally, we consider a range of exercises aimed at addressing concerns that our key source of variation—move-induced changes in provider race—is instead capturing move-induced changes in other base characteristics.

We also offer a rich interpretation of our findings. We discuss how to interpret our results relative to pre-existing gaps in mortality across races. We show that provider diversity effects are stronger in settings in which patients have more decision-making capacity. We find in particular that provider diversity reduces disparities more strongly in chronic care than in emergency care settings; for medication adherence rather than initial medication fills; and for planned versus unplanned cesarean section births. This suggests that our findings may stem from trust and communication mechanisms as distinct from alternative provider-driven explanations such as implicit or explicit bias. We also provide evidence that suggests that half or more of the effects that we estimate may be coming from spillovers onto other providers, highlighting the limitations of focusing solely on patient concordance.

Finally, we demonstrate that preventive medication use and adherence is a key mechanism underlying the mortality benefits of enhanced provider diversity. 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 roughly 31% of the mortality effects we estimate can be attributed to preventive medication use.

Our paper proceeds as follows. Part I provides a review of the relevant literatures on racial concordance and racial diversity gains in the workplace. 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, while Part V runs through a series of specification checks. Part VI discusses the interpretation of the findings. Part VI concludes.

Part I: Effects of Provider Diversity on Racial Health Disparities: Literature Review

Broader Diversity Studies

Much academic and policy commentary has insinuated that racial health disparities would be reduced if the physician workforce were to diversify along racial lines. However, the empirical evidence set forth to support that claim is generally limited to specific pieces of this overall puzzle. The most frequent piece of evidence invoked to support this claim is research that has shown beneficial effects in individual medical encounters in which there is racial concordance between providers and patients (IOM 2004; Sullivan 2004). Beyond the failure of such studies to capture more system-wide effects of diversifying the provider workforce—as discussed in the Introduction—such concordance studies are also limited by various econometric challenges as we summarize in the next sub-Part below. Our review of the relevant medical and health economics literature produced no studies that take a quasi-experimental approach to explore in a more direct and comprehensive sense how racial health disparities in a community may be reduced when that community experiences a plausibly exogenous increase in the availability of racially underrepresented medical providers.

Outside of the health care context, there is a large literature that explores the relationship between racially / ethnically diverse workplace teams or broader communities and the associated productivity of such teams or communities (Herring 2009; Marx et al. 2021). Specific inquiries include questions such as whether ethnically diverse teams of scientists produce more highly cited articles (AlShebli et al. 2018; Freeman and Huang 2014) and whether industry-specific changes in diversity indices are associated with prevailing wages (Sparber 2009).

Ultimately, while some of this broader literature has taken steps to address endogeneity, reviews of the relevant literature have concluded that robust causal evidence of the link between diverse workplaces and productivity remains elusive, in part due to a dearth of studies employing observational data with objective and quantifiable output metrics and research designs that produce quasi-random variation in workplace diversity (Chilton et al. 2022; Herring 2009).[1]

Racial Concordance Studies

The literature on racial concordance between providers and patients has theorized several mechanisms for a positive concordance effect, particularly with respect to the experience of Black patients. First, non-Black providers may have knowledge-based deficiencies in treating Black patients (Hoffman et al. 2016). Second, concordance may facilitate better communication that aids decision-making (Cooper-Patrick et al. 1999, Gordon et al. 2006) and ameliorates the compromised trust that Black patients have in the medical profession as a result of a history of

[1] Shift share instruments are commonly employed to study industry-specific diversity effects (Sparber 2009). Marx et al. (2021) randomizes team composition in their analysis.

abuse by the medical community (Alsan and Wanamaker 2018). Third, concordance may ameliorate harms stemming from implicit or explicit bias on the part of providers.

The empirical literature exploring the actual effects of racial concordance has produced mixed results and has been limited in certain methodological respects (Meghani et al. 2009). Perhaps most critically, most such studies have failed to address the key selection challenges underlying this inquiry—e.g., selection due to unobservable determinants of patient choice of providers and/or provider choice of patients. A notable exception includes the seminal study of Alsan et al. (2019), which 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. While setting forth a compelling randomized design, this paper cannot rule out that their findings are driven by underlying differences in physician quality given that it does not observe the same doctors treating a meaningful share of white patients.[2] Moreover, their finding applies to a very disadvantaged population in a particular location and they do not measure the impacts on patient outcomes.

Other studies have taken quasi-experimental approaches, including two that use administrative hospitalization records from Florida. Greenwood et al., (2020) employs physician fixed-effects specifications and finds that the mortality penalty for Black newborns is 39% lower under the care of Black (relative to white) pediatricians during newborn stays. Hill et al. (2020) instruments for racially-concordant hospitalizations with the lagged share of concordant matches at the focal hospital during the relevant time of day, finding that physician-patient race-match reduces the likelihood of within-hospital mortality by 15% (in relative terms). Neither of these quasi-experimental strategies, however, can fully address the two-sided problem of selection of providers by patients and within-facility assignment of providers to patients. And, neither considers jointly the impacts on both use of preventive care and health outcomes.

Perhaps most importantly, the racial concordance studies to date have limited their focus to the effects of provider-patient racial concordance during isolated medical encounters. As

[2] Even if Alsan et al. (2019) had included a meaningful number of white patients in the experiment, there would still be fundamental challenges in uncovering the desired structural parameters. Though this point has not sufficiently been appreciated by the racial concordance literature, if one draws on data from individual encounters and wants to separate a Black-patient effect, a Black-provider effect, and a Black-patient-provider concordance effect, one also needs to assume that there are no non-Black-patient / non-Black-provider concordance effects.

discussed in the Introduction, this limits the ability of such studies to capture the kinds of broader system-level effects—e.g., worker-to-worker spillovers—that form the conceptual foundations of the broader diversity-focused studies surveyed above. By exploring how patients respond to shocks in the availability of surrounding Black providers, our empirical strategy will allow us to capture such system / community-level effects.

Our investigation will further benefit from features of our data that will allow us to avoid some common pitfalls generally encountered by researchers in the concordance and related literatures. First, we will ensure that our sample has identical insurance coverage. Second, the completeness and continuity of the MHS's beneficiary files will allow us to avoid concerns over "missingness" of Black patients that frequently encumber medical research (Murphy et al. 2013).

Other Studies

Our analysis also contributes to a related literature exploring the effects of patient-provider concordance along other demographic dimensions, such as gender,[3] in addition to related literatures in non-health-care contexts exploring the effects of racial and gender concordance specifically[4] and the effects of workforce diversity more broadly along non-racial demographic lines.[5] Finally, our analysis contributes to other strands of literature that explore various aspects of the doctor-patient relationship, such as those exploring the determinants of preventive care[6] and those exploring the implications of trust and communications between providers and patients by observing the consequences of disruptions in those relationships.[7]

Part II: The MHS Setting

The Military Health System (MHS) is the primary insurer for all active-duty military, military retirees, and their dependents through the TriCare program.[8] TriCare is not involved in

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[3] See, for instance, Greenwood et al. (2018), and Cabral and Dillender 2022.
[4] Contexts explored include teacher/pupil (Dee, 2005; Bettinger and Long 2005; Fairlie et al., 2014), mortgagor/loan officer (Frame et al. 2022), and physician/student (Riise et al. 2022).
[5] For instance, a number of studies have explored the impacts of gender diversity on corporate boards (Ahern and Dittmar 2012; Matsa et al. 2013).
[6] See, for instance, Carrieri and Bilger (2013); and McMorrow et al. (2014).
[7] Such studies include Sabety et al. (2021); Sabety (2022); Staiger (2022); and Schwab (2023). Moreover, our analysis is related to those that draw on physician mobility (Molitor 2018).
[8] "Retiree" status requires meeting certain eligibility criteria, including twenty-plus years of service. Moreover, "retiree" in this context is not a term synonymous with departing the workforce. The average age at which individuals go on retirement status in the military is 43.

health care delivery in combat zones and operates separately from the Veterans Health Administration (Schoenfeld *et al.* 2017). For TriCare enrollees, care can be delivered either directly at Military Treatment Facilities (MTFs) on military bases (direct care) or purchased from private providers (purchased care) who are part of a network run by a contracting insurer. A patient's use of direct versus purchased care is primarily a function of residential proximity to the base, where the Military expects those living farther from the base to use the purchased care network (Frakes et al. 2023a). For those receiving care at MTFs, the care is delivered by a mix of active-duty providers, former active-duty providers on "retirement status," federal civilian employee providers, and independent contracting civilian providers. In the outpatient encounters of focus in our chronic-care analysis, roughly 70% of the MTF encounters are associated with treatment by active-duty or former active-duty providers (collectively, "military providers").

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 care. To improve power, our specifications will draw on moves of both active-duty and retirees, along with dependents of each. As discussed below, our analysis will draw on these moves to effectuate what is akin to a triple-differences specification. We note that for the active-duty portion of our sample, the timing and location of moves are predominantly driven by military staffing needs rather than solider preferences (Lyle 2006; Lleras-Muney 2010; Carter and Skimmyhorn 2017; and Carter and Wozniak 2018). However, even with respect to active-duty members, we do not view moves in and of themselves as random and tangential to patient health, as Department of Defense guidelines allow for moves to be impacted in some instances by specialized medical treatment (DoD 2015). Nonetheless, we emphasize that our analysis does not require that the timing and location of moves be purely random in the first instance. Below, we discuss, and explicitly test, the identification assumption that is inherent in our movers-driven triple-differences approach.[9]

[9] Though we again do not view the active-duty (and dependents) sample as one marked by fully exogenous moves, we present a set of results from our key specifications in Table A15 of the Online Appendix that are limited to the active-duty and active-duty-dependents sample. Doing so, we get more mixed results for preventive-care adherence but retain our primary findings of a significant reduction in mortality and self-disclosed pain for Black-relative to non-Black patients in connection with Black-provider-increasing moves.

Part III: Data and Empirical Strategy

Data

Our data come from the Military Health System Data Repository (MDR), which includes eligibility files and separate sets of inpatient/outpatient claims data for both direct and purchased care (Defense Health Agency 2003-2013). The sample period covers Fiscal Years 2003 through 2013. The key feature of the MDR that makes the present analysis possible is that these data have a somewhat uncommon feature in including information on the race of *both* the provider and the patient, at least with respect to the direct care records. We have such information by virtue of the fact that active-duty and retired military providers are themselves MHS beneficiaries and by the fortuitous fact that the MDR uses the same person identifier coding system across the claims and eligibility repositories (Frakes et al. 2021). Using self-identified race fields, our primary empirical analysis focuses on the binary distinction between Black and non-Black individuals, as we discuss further below.[10]

Clinical Samples and Outcome Measures

While the breadth of the MDR database presents us the opportunity to construct rich metrics of health care quality and patient outcomes, it also presents a challenge: deciding upon clinical contexts to explore. To navigate and discipline this choice, we aim to identify clinical settings with the following characteristics: (1) frequent enough occurrence to construct robust sample sizes, (2) the subject of consensus medical guidelines from which we can construct our outcome measures and samples, (3) amenable to a movers design in the sense that we can track outcomes within individuals over a period of time, [11] and (4) affords the ability to study the effect

[10] As discussed below, we also consider an approach that compares Black and white patients and drops patients of other races. In our base specification, we do not differentiate Hispanic individuals. In the Appendix, we show results for alternative treatments of patients of Hispanic origin: first, dropping patients of Hispanic origin; and second, classifying racial groups into Black, non-Hispanic individuals and all others. Both results are very similar to our base findings.

[11] An example of a clinical context in which a movers strategy is not feasible is childhood or adolescent vaccination, where individuals are not expected to routinely comply over time in a way we could track pre and post Black-provider-increasing moves. Theoretically, we could track parents who have multiple children, one child that we could follow over a full compliance period pre-move and one child that we could follow over a full compliance period post-move, but the resulting sample of families is too small (less than 1,000) leaving imprecise findings.

of provider diversity on preventive care and health outcomes without being confounded by the effect of provider diversity on the underlying diagnosis itself (i.e., entry into the sample).

Applying these criteria, we elect to focus on chronic disease care. In the remainder of Part III, we discuss the role of the first and third criteria in this setting, and, in the Online Appendix, we discuss the guidelines from which we draw to construct our relevant chronic-disease samples and measures. Regarding the fourth criteria, in Part IV below, we find no diagnosis effect among our chronic-disease sample. Beyond these points, the chronic-disease context is also nicely suited to our investigation into the effects of provider diversity given that the provider-patient relationship is a central component to chronic-disease management. Nonetheless, as discussed at various points below, our analysis will consider certain additional clinical contexts, including one in which the patient's decisionmaking role is more constrained: emergency-department care. Ultimately, our analysis will demonstrate that the effect of provider diversity is not universal but depends on the nature of the provider-patient relationship.

Our chronic-care analysis focuses on patients with four chronic, deadly, but manageable illnesses: diabetes, hypertension, hyper-cholesterolemia, and clinical athero-sclerotic cardiovascular disease. For each such sample, we construct a range of preventive-care and health-outcome measures. The primary preventive-care measure that we construct is the annual fill days associated with low cost, generic medications recommended for the focal disease (e.g., statins for cardiovascular disease). The primary health outcome that we focus on is the incidence of mortality during the sample period.[12] For the diabetes and hypertension sub-samples, we take advantage of a set of vital statistics records available for direct care encounters from 2009-2013 and track systolic blood pressure levels. Finally, further using the vital statistics data, we code across all chronic diseases for the incidence of self-reported pain on a 1-10 scale as a proxy for self-reported well-being (Chireh and D'Arcy 2018). We discuss these measures and sample-construction matters in greater detail in the Online Appendix.

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[12] The medication fill-days measure includes certain additional sample restrictions mandated by the guidelines from which we draw—e.g., we follow the WHO in only monitoring hypertension-medication adherence for those patients with uncontrolled hypertension. In our mortality analyses, lacking instruction to do so from the guidelines, we do not impose these medication-specific sample restrictions to our chronic-disease samples. However, in the Online Appendix, we demonstrate that our mortality findings are identical when we estimate the mortality results on the same sub-sample on which we estimate the medication specifications.

Our primary analysis pools across all four chronic-disease samples. Of course, the samples are not fully independent: 19.6% of the patients in the sample have two of the relevant conditions, 5.9% have three and 0.7% have all four. To address this non-independence, our primary specifications include fixed effects for each possible disease combination. In the Online Appendix, we show a set of our key results separately across the relevant chronic conditions.

As discussed below, our analysis will capture variation in provider race by drawing on patient moves across bases. To facilitate the most straightforward quasi-experimental investigation, we focus on those who move a single time during the sample period (Finkelstein et al. 2016). We limit this analysis to movers—i.e., we exclude non-movers—to ensure that our differencing strategy accounts for the possible effects of moving itself on the relevant health care outcomes. While this approach is consistent with Finkelstein et al.'s (2016) secondary event-study estimation strategy, it differs from their primary strategy, which had included non-movers to help identify place effects. As highlighted in Frakes et al. (2023a), a moving-based strategy that excludes non-movers has the advantage of not needing to assume place effects that are comparable between movers and non-movers.

At a chronic-disease-base-specific level, we calculate our key regressor: the share of Black providers among those with known race on the base. We determine base Black-provider shares while weighting providers by their encounter counts, in which case we technically are forming the share of visits with Black providers. This allows us to address concerns that certain providers only spend a portion of their practice on the relevant chronic conditions.[13]

Our movers strategy captures variation only in the Black share of military providers—i.e., providers with known race. Of course, many encounters in our data are with providers of unknown race—e.g., off-base encounters. Rather than conditioning our sample on patients who receive care on-base with military providers, which would be potentially endogenous, we undertake an intent-to-treat approach and assess the outcomes of patients that go both on and off

[13] Our primary approach characterizes each base by a time-invariant provider-race share. While within-base variation over time in provider race shares 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, as we demonstrate in Table A3 of the Online Appendix. In Table A5 (Panel H) of the Online Appendix, we show that our key findings presented below in Table 2 are nearly identical when we exclude bases with the most within-base variability in Black-provider shares.

base for care. However, we do limit this primary analysis to those patients predicted to go on-base by focusing on those who live near (within ten miles of) the base.[14] Patients in this range use the base for their chronic disease care 66% of the time 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. Below, we identify—and thereafter address—certain residual concerns stemming from our inability to observe the race of non-military providers.

Demonstrating the implications of these choices, we note that there are 411,726 patients with diabetes in our MHS sample, 74,689 of which move once during the sample, and 19,655 of which consistently live within 10 miles of the base and are thus included in our primary specifications. The corresponding numbers for (i) the hypertension sample are 1,194,339; 239,910; and 72,216, (ii) the hypercholesterolemia sample are 1,581,623; 332,016; and 93,727 and (iii) the atherosclerotic cardiovascular disease sample are 324,763; 53,665 and 12,152.

The resulting pooled sample is described in Table 1. Panel A presents summary statistics at the pooled individual-disease-category level. Roughly 24% of the patients are Black and 56% are male. The average age of each patient at the beginning of the sample is 40. Roughly 1% of the chronic disease patients die over the sample period. In Panel B, we present summary statistics for certain variables at the pooled person-year-disease level—i.e., before collapsing to the individual level and calculating move-induced changes in the key variables. Foremost among these measures is the share of military providers on the focal base that are Black, which averages 8.2%. In Figure A1 of the Online Appendix, we demonstrate substantial variation in the range from zero to 20% in this Black-provider share measure across bases.

Empirical Strategy

The key question with which we engage in this paper is whether and to what extent racial health disparities are reduced following an increase in the racial diversity of the medical provider workforce. We will explore this question with a particular focus on the Black patient experience. That is, given the historical treatment of Black individuals by the American health care system and given the associated evidence of compromised trust among Black patients (Alsan and Wannamaker 2019), our analysis will ask the following specific question: to what extent will Black

[14] Zipcode data is from U.S. Dept. of Commerce, Bureau of the Census (2010).

Table 1. Summary Statistics

Variable	Full Sample Mean	Full Sample Standard Deviation	Black Patients Mean	Black Patients Standard Deviation	Non-Black Patients Mean	Non-Black Patients Standard Deviation
Panel A: Analytical Sample. Unit of Observation = Person / Disease Category						
Move-Induced Change in Black Military Provider Share	0.003	0.067	0.004	0.072	0.003	0.065
Incidence of Mortality (At All Over Sample)	0.010	0.100	0.010	0.101	0.010	0.100
Patient Age	39.98	11.15	39.88	10.49	40.06	11.35
Active-Duty Status	0.419	0.493	0.445	0.497	0.410	0.492
Male Patient	0.564	0.498	0.560	0.496	0.567	0.496
Black Patient	0.245	0.429	-	-	-	-
Incidence of Junior Enlisted Pay Grade	0.120	0.324	0.115	0.319	0.120	0.325
Incidence of Senior Enlisted Pay Grade	0.651	0.476	0.763	0.425	0.614	0.487
Incidence of Junior Officer Pay Grade	0.057	0.232	0.033	0.178	0.065	0.246
Incidence of Senior Officer Pay Grade	0.145	0.352	0.065	0.246	0.171	0.377
Panel B: Person-Year Chronic-Disease Sample (Prior to Creating Move-Induced Changes)						
Annual Medication Fill Days	76.024	134.228	72.428	129.284	76.984	135.502
Systolic Level	130.699	11.763	132.354	11.900	130.089	11.652
Self-Reported Pain (1-10)	1.731	1.947	1.863	2.032	1.682	1.912
Black Military Provider Share	0.082	0.066	0.095	0.070	0.078	0.065
Military Provider Age	41.567	3.044	41.907	2.983	41.465	3.054
Male Military Provider Share	0.578	0.116	0.573	0.112	0.580	0.118
Share of Black Beneficiaries on Base	0.193	0.075	0.222	0.083	0.184	0.070
Age of Beneficiaries on Base	29.926	3.543	30.019	3.614	29.899	3.522
Share of Male Beneficiaries on Base	0.559	0.036	0.556	0.035	0.559	0.036

Notes: data are from the Military Health System Data Repository 2003-2013. Descriptive statistics from Panel A are taken from a person-disease-category sample of chronic-disease patients (N = 197748). Patient characteristics (e.g., race, age, sex, pay-grade, active-duty status) are of the patient's first year in the sample. 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. Both cases focus on patients that live within 10 miles of the base (N=1,886,984). We assign each patient-year cell a base (N=116) according to the MTF catchment/service area (designated by the MHS) associated with the closest MTF clinic to the patient's residence at that time. Distance to the base is signified by the distance between the zip-code centroid of the patient's residence and the zip-code centroid of the closest MTF clinic.

patients incur health care gains relative to non-Black patients when they experience an increase in the surrounding share of medical providers that are Black?

Simply comparing the outcomes across regions with different shares of Black providers would be problematic for various reasons. For instance, such a comparison would be subject to concerns over provider selection of bases based on underlying patient health and concerns over regional variations in health care quality combined with residential segregation by race (Chandra and Skinner 2003). Necessary to study the impacts of provider diversity on racial health disparities is quasi-random variation in the racial composition of surrounding providers. Subject to certain assumptions discussed in greater depth below, we can capture such variation using a combination of patient moves and heterogeneity in the share of Black providers across bases, following Finkelstein et al. (2016) and Frakes et al. (2023a).

In particular, consider a patient *i* moving across areas served by MTFs with different racial compositions of chronic-disease providers. Let PB_j be the percentage of providers treating a given chronic illness at base *j* who are Black. We then estimate regressions of the form:[15]

$$\Delta Y_{ij} = \alpha + \beta B_i + \delta \Delta PB_j + \tau B_i * \Delta PB_j + \Omega \Delta PX_j + \gamma B_i * \Delta PX_j + \pi M_i + ¥ B_i * M_i + \mu_j + \epsilon_{ij} \quad (1)$$

where β captures differential impacts of moving by patient race and δ captures any general differences associated with moving from a base with a low versus a high share of Black providers. The coefficient τ captures the parameter of interest and represents the impact on Black patients, relative to non-Black patients, of moving to a base with more Black providers, relative to one with fewer.[16] Though our focus will be on the τ coefficient as it speaks directly to

[15] In the mortality specifications, ΔY captures the incidence of mortality over the sample. The mortality results presented below are virtually identical with the inclusion or exclusion of fixed effects capturing the year of the move, appeasing truncation concerns with this measure.
[16] Specification (1) is designed to center time in generalized terms around moving 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 analog to this 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 (1) is that it 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).

the racial disparities implications of provider diversity, we will also discuss the associated absolute effects of Black-provider increasing moves for Black ($\delta + \tau$) and non-Black patients (δ).

Certain baseline patient characteristics—as of their first sample year—are captured in M. These include a set of fixed effects for pay-grade level, age, active-duty status, and sex, along with the following continuous measures of the patient's baseline health status: Charlson Comorbidity Index, total Relative Value Units (RVUs) and total inpatient days.[17] Through variables captured in ΔPX, we account for the impact of move-induced changes in other base demographic characteristics, including: (i) share of providers who are male, (ii) mean provider age, (iii) share of non-military providers on the base (iv) share of beneficiaries who are male, (v) mean beneficiary age, and (vi) the share of beneficiaries who are Black. For both ΔPX and M, we allow for differential impacts by race. Provider demographic controls are important to account for correlations in provider demographic patterns and for the possibility that Black patients' degree of trust in, and communication with, providers may also vary by other provider demographics. The same is true for base resident demographics, particularly with respect to the share of Black beneficiaries on the base, whereby a control for the latter is important to rule out a general Black-base effect.

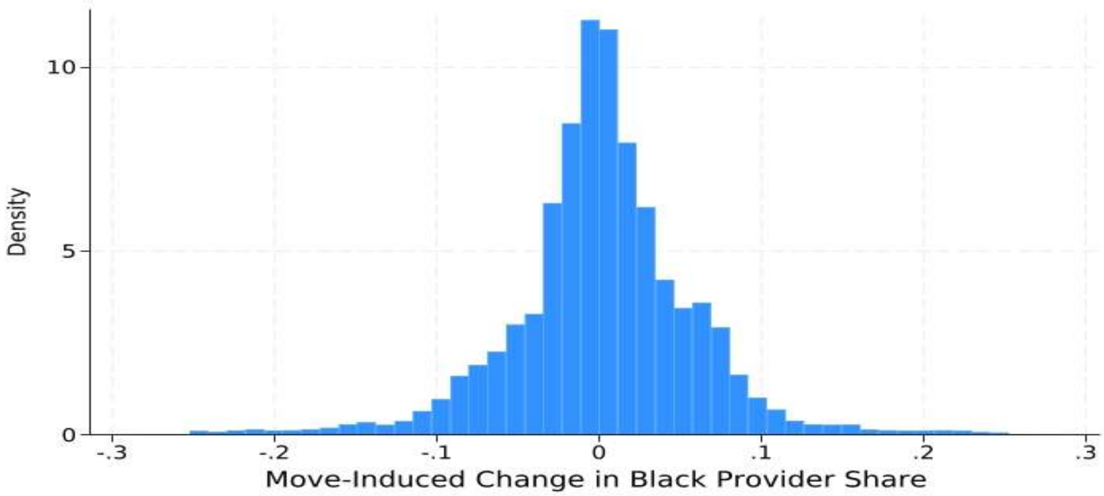
Figure 1 shows the distribution of the move-induced changes in the prevailing Black provider share of the base experienced by the relevant patients in the sample. There is a wide distribution with significant support in the range from -20 to 20 percentage point changes.

Finally, we show results with and without fixed effects for pre- and post-move bases, μ. While the move-induced change in the Black-provider share is collinear with this full set of sending and receiving base effects, the interaction of that move-induced change in provider race with patient race is not, thereby facilitating identification of τ with the inclusion of such effects. This provides an advantage over the specifications estimated in Finkelstein et al. (2016) which included a single set of place fixed effects (i.e., not separately for sending and receiving locations) in their primary specification and no place fixed effects in their event-study specifications. In our context, the ability to account flexibly for regional effects provides a

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

powerful tool to address classic sources of selection in attempting to identify the connection between racial diversity of the provider workforce and racial disparities among the patient population—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.

Figure 1. 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 (N=197748).

Part IV: Results

In Table 2, we show the estimated interaction coefficient, τ, from equation (1), capturing the relationship between the racial diversity of chronic-disease providers and racial disparities among chronic-disease patients, classifying racial groups as Black and non-Black. To facilitate interpretation, we normalize the ΔPB_j variable by its standard deviation. Panel A shows that we observe a roughly 3 fill-day increase for the disease-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. In Panel B, we document substantial mortality effects, with point estimates suggesting a corresponding 0.2 percentage point reduction in mortality—roughly 18% relative to the mean mortality rate—for Black relative to non-Black patients in connection with moves of the same nature. In broad terms, the results suggest that racial diversity among providers is connected with racial disparities among patients.

Drawing from our more limited set of vital statistics records (direct-care only from 2009-2013), Table 3 presents results on certain additional outcomes. When pooling over the diabetes and hypertension samples, we find that increases in the prevailing share of Black providers is beneficially—i.e., negatively—associated with systolic levels for Black relative to non-Black patients (in the Online Appendix, we show corresponding beneficial increases in the incidence of blood pressure control as defined by systolic < 140 and diastolic < 90). Further, we find that Black relative to non-Black patients experience a reduction in self-reported pain (when pooling over all samples) in connection with an increase in Black provider availability.

Table 2. Relationship between Increased Racial Diversity of Providers and Changes in Racial Health Disparities, for Preventive Care and Mortality

Panel A. Adherence Analysis		
Dependent Variable = Change (Post-Move minus Pre-Move) in Medication Fill Days.		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	2.992***	2.676***
	(1.007)	(0.939)
N	130952	130952
Panel B. Mortality Analysis		
Dependent Variable = Incidence of Mortality Over Sample.		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	-0.0018***	-0.0019***
	(0.0007)	(0.0007)
N	197748	197748
Pre- and Post-Move Base Fixed Effects?	NO	YES

Notes: The unit of observation is a patient-by-disease-category cell. Standard errors are clustered at the sending-by-receiving level. The sample includes adult MHS patients 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 of the base Black-provider share are omitted for brevity (but see Table A2 of the Online Appendix). Specifications include the additional controls included in Specification (1). Following the relevant guidelines, as discussed further in the Online Appendix, the preventive-care analysis imposes further sample restrictions—e.g., only following medication adherence of hypertension patients who are not in control of their blood pressure. Nonetheless, as demonstrated in the Online Appendix, the mortality results are virtually identical when imposing those sample medication-specific sample restrictions. ***Significant at the 1 percent level; ** Significant at the 5 percent level; *Significant at the 10 percent level.

These results are not only statistically significant, but also meaningfully large. Consider the mortality findings. The mean Black military-provider share is 8.2 percent and the mean Black MHS beneficiary share is 24 percent. Our mortality results imply that an increase in Black providers at a level that would make the Black military provider share representative of that of the beneficiary population would lead to a roughly 41% decline in mortality for Black relative to non-Black patients (relative to the mean mortality rate).

We can also assess the percentage of the prior racial gap that is closed in connection with the increase in diversity. We use the diabetes sample for demonstration purposes and consider gaps in mortality rates. Among MHS beneficiaries between 20 and 65, Black relative to non-Black beneficiaries are roughly 38% more likely to have diabetes and die over the sample (relative to mean mortality rates). This finding is consistent with the literature, though 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). Much of this disparity in the MHS appears to be driven by disparities in diabetes disease prevalence between Black and non-Black MHS beneficiaries. However, as demonstrated by Table A1, there is also a notable racial mortality gap conditional on having diabetes (0.011 for Black patients versus 0.009 for non-Black patients), a finding consistent with the literature (Gu et al. 1998).

Table 3. Relationship between Increased Racial Diversity of Providers and Changes in Racial Health Disparities, for Biomarkers and Other Outcomes

Panel A. Blood Pressure Analysis (Diabetes and Hypertension Samples Pooled)		
Dependent Variable = Change (Post-Move minus Pre-Move) in Systolic Level		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	-0.326**	-0.287*
	(0.164)	(0.156)
N	43456	43456
Panel B. Self-Reported Pain (All Samples Pooled)		
Dependent Variable = Change (Post-Move minus Pre-Move) in Self-Reported Pain (0-10)		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	-0.036	-0.055**
	(0.024)	(0.025)
N	87075	87075
Pre- and Post-Move Base Fixed Effects?	NO	YES

Notes: These results are from specifications that follow those estimated in Table 2, except now focusing on the indicated dependent variables and on the indicated samples. These data are from the more limited set of vital statistics records in the MDR, which are for on-base records only and for 2009-2013 only. ***Significant at the 1 percent level; ** Significant at the 5 percent level; *Significant at the 10 percent level.

Once again, we consider a change in the Black Provider share from its baseline level (8.2%) to the mean rate of Black MHS beneficiaries (24%). We estimate that such an increase would reduce the overall Black mortality gap from 38% to 21%. That is, our results suggest that making the medical provider workforce racially representative of the underlying population has a large enough effect to significantly offset the higher mortality gap arising from both baseline prevalence differences and baseline differences in mortality conditional on having diabetes.

Part V: Specification Checks

Interpretation of these basic results as causal relies on a set of identification assumptions. In this section, we run through various tests and checks on these assumptions.

Covariate Balance and Pre-Move Diagnostics

The key identifying assumption underlying our triple-differences movers specification is that the health outcomes of Black patients relative to non-Black patients, who experience Black-provider-increasing moves relative to Black-provider-decreasing moves, would have trended in parallel over time but for such moves actually transpiring. A first step in showing that this assumption is valid is illustrating covariate balance within our movers design.

In particular, we estimate a series of specifications analogous to equation (1) that sequentially set the patient characteristics in M as the dependent variable (excluding all such characteristics as controls). In Table 4, we report the interaction coefficient, τ, associated with these balance regressions. As demonstrated, 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 characteristics. Nearly all coefficients are very close to zero with confidence intervals tightly bound around zero.

To provide an omnibus test of covariate balance, we predict a mortality rate for each patient based on these characteristics and then use this prediction as the dependent variable (again excluding characteristics as controls). While we estimate a τ coefficient of -0.0019 using actual mortality, we estimate a τ coefficient of only -0.00007 using predicted mortality. With a 95% confidence interval for this latter estimate spanning from -0.00029 to 0.00015, our actual mortality result falls far outside of the confidence range for predicted mortality.

For further reassurance, we extend this balance exercise to a dynamic context, at least in the pre-move period. That is, with respect to time-varying measures reflective of patients' health status—e.g., overall outpatient RVUs—we would also expect to observe no differential trends in such measures for Black relative to non-Black patients in the period leading up to a Black-

Table 4. Covariate Balance. Relationship between 1-Standard-Deviation Move-Induced Increase in Black-Provider Share and Differential in Black-Versus-non-Black Patient Characteristics

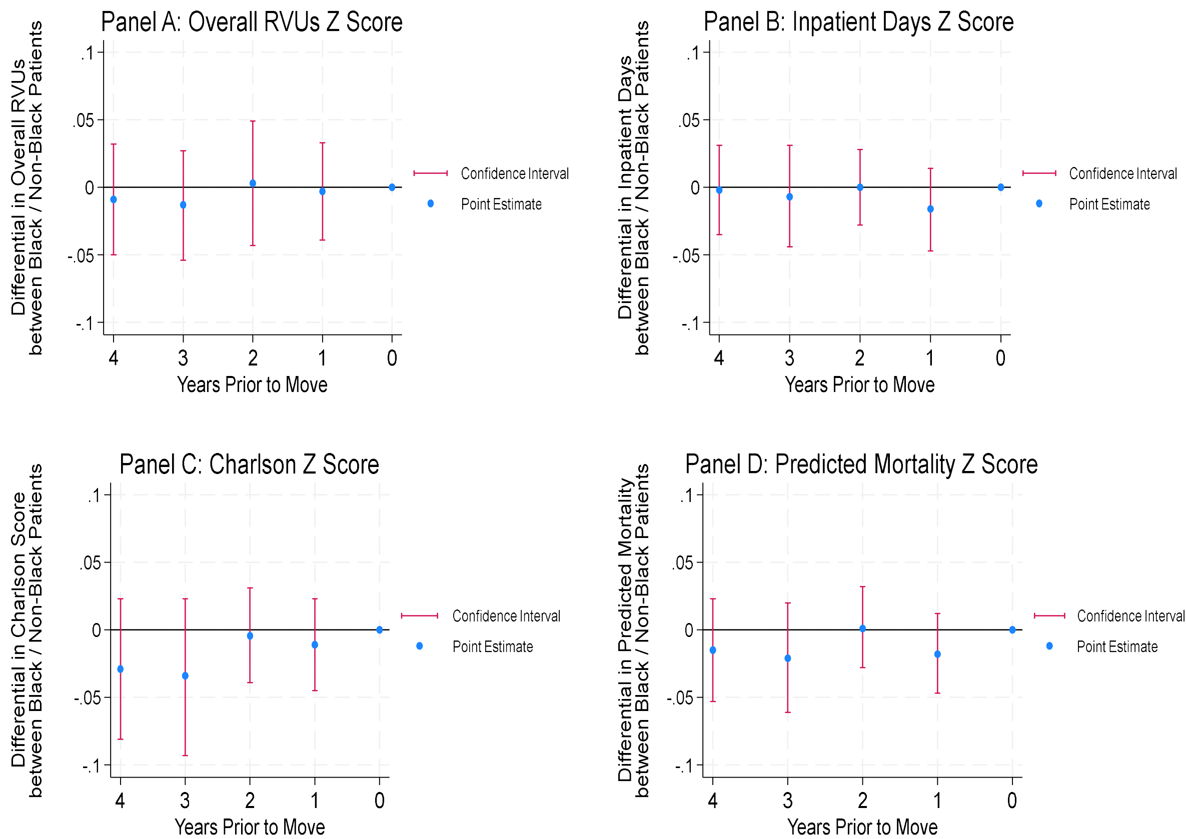
Age (Mean in Pooled Sample = 39.98)	-0.035	-0.126
	(0.169)	(0.091)
Active-Duty Status (Mean in Pooled Sample = 0.41.92)	-0.003	0.002
	(0.006)	(0.005)
Male (Mean in Pooled Sample = 0.56)	-0.000	0.001
	(0.004)	(0.004)
Annual RVU (Mean in Pooled Sample = 51.38)	0.011	0.050
	(0.505)	(0.476)
Charlson Index (Mean in Pooled Sample = 0.47)	-0.006	-0.007
	(0.007)	(0.007)
Inpatient Days (Mean in Pooled Sample = 0.78)	-0.033	-0.033
	(0.031)	(0.031)
Incidence of Junior Enlisted Pay Grade (Mean in Pooled Sample = 0.09)	0.005	0.004
	(0.003)	(0.003)
Incidence of Senior Enlisted Pay Grade (Mean in Pooled Sample = 0.68)	0.007	0.010*
	(0.007)	(0.005)
Incidence of Junior Officer Pay Grade (Mean in Pooled Sample = 0.05)	-0.001	-0.001
	(0.002)	(0.002)
Incidence of Senior Officer Pay Grade (Mean in Pooled Sample = 0.15)	-0.010	-0.012
	(0.008)	(0.005)
Predicted Incidence of Mortality (Based on Observable Patient Characteristics; Mean in Pooled Sample = 0.010)	-0.00001	-0.00007
	(0.00014)	(0.00011)
Pre-Move Medication Fill Days (Mean in Pooled Sample = 54.3)	-0.240	-0.357
	(0.996)	(0.909)
N	197748	197748
Pre- and Post-Move Base Fixed Effects?	NO	YES

Notes: The specifications are identical to that estimate in Table 2 except that all baseline patient health characteristics are excluded. Instead, those characteristics are included as dependent variables in separate specifications estimated across each row. Covariate balance is conducted on the sample with the broadest eligibility criteria among those specifications estimated in Tables 2 and 3, though the results from this exercise generalize to focusing on the limited samples estimated in some specifications in Tables 2 and 3 based on the eligibility criteria for the relevant clinical context. ***Significant at the 1 percent level; ** Significant at the 5 percent level; *Significant at the 10 percent level.

provider increasing move, for those who move to bases with different shares of Black providers.[18] Encouragingly, as we show in Figure 2, we observe no such pre-trends.

[18] To explore such dynamics, we estimate a simple modification to the specifications underlying Table 4. The right-hand-side of the specifications in this dynamic approach remains the same as in Table 4—after all, we still need to characterize a move by the resulting move-induced change in the Black-provider share. However, to achieve the desired dynamic tracing of the relevant patient characteristics, we now estimate a series of regressions and define a set of new operators, Δ_e, to serve as the dependent variable in each regression. For each event-time period, e—which

Figure 2. Relationship between Differential in Z-Scores of Patient Characteristics between Black and non-Black Patients in Years Leading up to Move Associated with 1-Standard Deviation Increase in Black Provider Share



Notes: each point estimate depicted is from a different regression, where the dependent variable represents the difference in levels for the indicated outcome variable at the indicated event time relative to the reference period (the year of the move in the case of this balance exercise). The right-hand side of each regression follows that of specification (1) except excluding baseline patient characteristics (rather, those characteristics represent the outcomes studied across the different panels). The figures present estimates of the τ interaction coefficients. 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 and who consistently live within 10 miles from the base (N = 36491).

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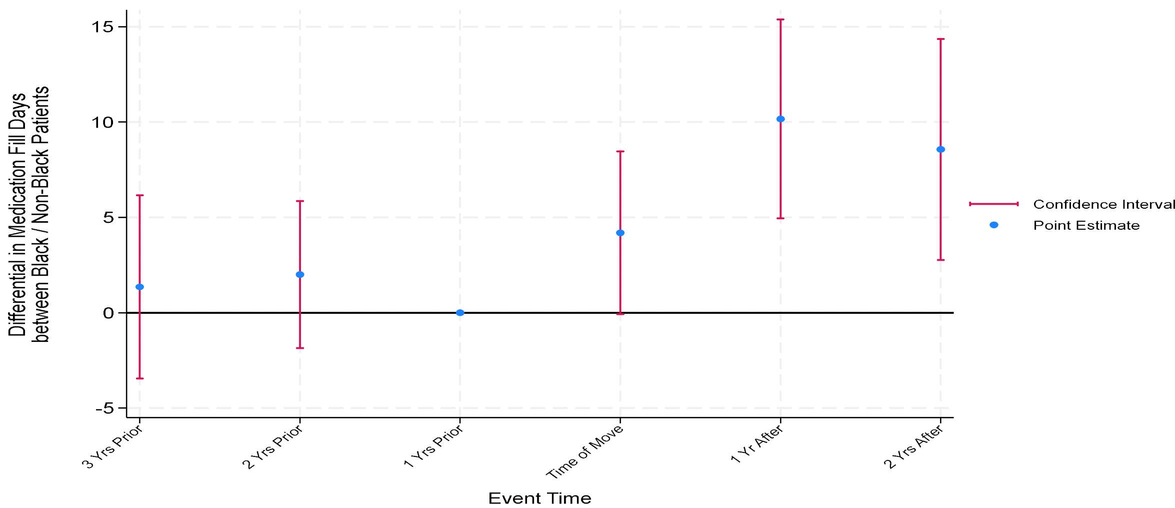
span from four years pre-move to the year of the move—Δ_e measures the differences in levels for the relevant characteristic between event time, e, and a reference period, which we set to the year of the move. By plotting the estimated τ coefficients across these specifications, we can dynamically track the differential in the measure of interest between Black and non-Black patients in connection with a 1-standard deviation move-induced increase in the Black-provider share. Furthermore, we focus on a balanced sample of individuals that we can observe over the entire event window.

In a related exercise probing the parallel-trends assumption, we show dynamics in our key preventive-care measure in the time surrounding a Black-provider-increasing move.[19] Encouragingly, as presented in Figure 3, we do not document evidence of an increase in medication fill days—for Black relative to non-Black patients—prior to this move. Consistent with a causal effect, we begin to see a positive divergence upon the onset of the Black-provider-increasing move. We also see an effect that grows substantially after one year of exposure – which is consistent with a dosage response to more exposure to same-race medical providers.[20]

Figure 3. Event-Study Results. Relationship between Increased Racial Diversity of Providers and Changes in Racial Health Disparities in Prevention Adherence Measures in Years Leading up to and Following Patient Move



Notes: each point estimate depicted is from a different regression, where the dependent variable represents the difference in levels for the medication fill-day measure at the indicated event time relative to the reference period (the year prior to the move). The right-hand side of each regression follows that of specification (1) and the figure presents estimates of the τ interaction coefficients. Accordingly, this figure traces how medication fill days—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 and who consistently live within 10 miles of the base (N=13239).

[19] We cannot track pre-move dynamics for the mortality analysis since we are focusing on individuals that, in fact, move before dying.

[20] The event-study analysis for medication fill days follows the same dynamic methodology underlying Figure 2 except using the year prior to the move as the reference category and showing both pre- and post-move dynamics (in each case following convention in triple-difference settings), again focusing on a balanced sample that is observed over the entire event window. Further, in both dynamic exercises in Figures 2 and 3, we find that the incidence of being included in the respective balanced sample—for Black relative to non-Black patients—is not associated with a Black-provider-increasing move, easing selection concerns.

Endogeneity of Moving Generally

In general terms, identification in our study requires that the difference along the spectrum of move-induced changes in provider race reflects prevailing provider-race itself rather than unobservable factors, such as unobservable differences among patients that happen to be experiencing different types of moves along this spectrum. One possible threat to this move-type comparability assumption arises if the incidence of moving itself—for Black relative to non-Black patients—is a function of the prevailing race of providers, a scenario which might create compositional changes along this spectrum. While Table 4 and Figure 2 already test our identifying assumption based on observables, we attempt to shed light on selection on unobservables by testing this more specific moving-incidence concern. Specifically, we consider a sample that includes non-movers and estimate:

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

where j indicates the patient's base at the beginning of the sample. The rest of the variables are as defined in specification (1) except that, now, instead of tracking how these measures change pre- and post-move, these measures are all determined as of the beginning of the sample. 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, encouragingly, we find no evidence that Black relative to non-Black patients are more or less likely to move away from a high-Black-provider-share base.[21]

Distinguishing Base Characteristics from Provider Diversity

As just stated, a concern with our movers strategy is that move-induced changes in provider race may be capturing something other than provider race. In this sub-section, we specifically address the possibility that such changes may be reflecting move-induced changes in other base-level characteristics that happen to be correlated with move-induced changes in the prevailing share of Black providers on the base. In particular, we demonstrate the robustness of

[21] Relatedly, we also find that, among movers, Black patients are no more or less likely to experience Black-provider increasing moves. That is, we regress the incidence of a Black patient on the move-induced increase in the Black-provider share, along with the other controls in equation (1) (except those representing the interaction terms between such controls and the Black-patient-indicator). Doing so we estimate a coefficient on the move-induced change in the Black provider share of -0.002 (relative to a mean Black beneficiary incidence rate of 0.20), with a confidence interval spanning from -0.011 to 0.006.

our findings to a broad range of base-level controls, while also discussing what our findings surrounding these controls imply about our ability to infer a provider-race interpretation.

To begin, we note that specification (1) includes a set of move-induced changes in other base-level demographics—e.g., the share of military providers on the base that are male. These controls capture base characteristics that one would predict are most likely to pose identification threats given the possibility of demographic intersectionalities among base providers and beneficiaries. Of course, any such correlation between base-level provider race and other base-level demographics alone is not a threat if move-induced changes in other base-level demographics would not be expected to impact racial health disparities specifically. And while existing scholarship leads us to predict a link between racial health disparities and provider race, we are not aware of scholarship that would likewise lead us to predict a link between racial health disparities and community-level sex and age patterns.

In Figure A3 (and surrounding text) of the Online Appendix, we shed light on this inquiry and demonstrate that the only base-level demographic that is consistently associated with an improvement for Black relative to non-Black health outcomes is provider race. The fact that our findings persist after including other base demographic controls and that such controls are not themselves associated with improvements in preventive care and health outcomes lends confidence to the conclusion that are our findings reflect a link between provider racial diversity and racial health disparities.

To provide even more confidence that our analysis is not picking up move-induced changes in other base-level characteristics, we consider another exercise in Table A5 (Panel I) of the Online Appendix in which we demonstrate the virtual insensitivity of our results to a range of other characteristics of the regions surrounding the corresponding bases. Such characteristics include measures reflective of the local (i) economic environment (e.g., median household income), (ii) socio-economic environment (e.g., educational attainment), (iii) geography (e.g., population density), (iv) resource availability and treatment intensity of the overall civilian and military medical community (e.g., total hospital beds, physicians and inpatient days per capita) and (v) racial composition of the surrounding community (beyond just MHS beneficiaries).[22] Further demonstrating the tangential nature of our quasi-experimental source of variation, in

[22] As discussed in the Online Appendix, data on these other characteristics is obtained at the county-level from Health Resources & Services Administration (2016).

Tables A12 and A13 of the Online Appendix, we demonstrate that move-induced changes in provider race are essentially uncorrelated with move-induced changes in these various measures, with the exception of only modest correlation in the case of the community racial composition measure. In Table A14, we further demonstrate that move-induced changes in these other surrounding economic characteristics are themselves not determinative of move-induced changes in our key outcome measures. Collectively, these findings continue to reinforce that the estimated impacts of move-induced changes in provider race, in fact, reflect changes in prevailing provider race.

In a related exercise, we calculate each base's average rates for our key outcomes from Table 2. We then calculate the move-induced change in the prevailing base's relevant outcome (e.g., medication-fill-days) and then include this new move-induced base-related measure and its interaction with the Black patient indicator in specification (1). Essentially, this allows us to estimate the effects of a move-induced change in provider race while ensuring that the results we are picking up are not merely a reflection of a move-induced change in the prevailing base's practice style that happens to be correlated with the move-induced change in provider race and that happens to differentially impact Black and non-Black patients. In other words, with these controls, we are separating the present provider-diversity inquiry of interest from the practice-style inquiry of interest in Finkelstein et al. (2016). As demonstrated by Table A5 (Panel J) of the Online Appendix, our results from Table 2 are virtually unchanged with these controls.

In a final exercise in this vein, we demonstrate in Table A16 of the Online Appendix that our findings are not capturing provider-racial-diversity effects in general but are specific to those providers who actually treat the chronic diseases of relevance. The fact that our provider race effects are specific to the relevant clinical context provides a rationality check that reinforces that our results are speaking to provider diversity rather than to spurious base characteristics.

Endogenous Diagnosis

Our movers-based strategy is conditioning on those with the underlying chronic-disease diagnoses and then asking how the change in Black provider availability impacts the outcomes under investigation. Nonetheless, if Black provider availability also affects—differentially by patient race—the incidence of diagnosis of the underlying conditions, one may be concerned that the underlying sample is itself selected in a way that biases our desired investigation. Mitigating this concern, in Appendix Table A4 (Panel D), we estimate an analog to equation (1) that uses a sample of all adults and find a near-zero relationship between the move-induced change in the Black provider share and the differential in the likelihood of receiving a relevant chronic-disease diagnosis between Black and non-Black patients.[23]

Distance Falsification

In Tables A17 and A18 of the Online Appendix, we conduct a falsification check in which we test for weaker concordance effects among a sample of patients who do not live consistently within 10 miles of the base. Such patients are less likely to receive care on base (as noted above) and thus less likely to be sensitive to our quasi-experimental source of variation—i.e., the share of Black military providers. Specifically, we stack the live-close and live-far samples and then estimate a triple-interaction changes specification, interacting the right-hand-side of specification (1) with an indicator for the live-close sample. Doing so, bolstering a causal interpretation of the above findings, we demonstrate smaller provider-racial-diversity effects on Black relative to non-Black patients among those not consistently living close to the base.

Addressing Missing Provider Race

While our data are unusual in providing information on provider demographics, they again do not do so with respect to non-military providers. This may create a concern within our intent-to-treat (ITT) approach depending on the relationship between the race of military and non-military providers. In particular, if the move-induced change in the Black military provider share is negatively correlated with the move-induced change in the share of Black non-military providers nearby (the latter of which will affect some patients in our ITT sample), this may hinder our ability to interpret the impacts of this shock as stemming from an increase in the availability of Black providers generally and thus hinder our ability to draw inferences about the impacts of increased provider diversity. We take three approaches to address this concern.

[23] In the Online Appendix, we conduct certain additional exercises to further appease such concerns—e.g., (i) showing robustness to controls for the pre-move level of the base Black provider share, which may have impacted the initial sample entry (i.e., diagnosis) differentially for Black relative to non-Black patients and (ii) estimating a specification where we wait two years after observing patients in the sample to begin tracking outcomes to address concerns that we don't observe diagnoses that occurred immediately prior to our sample.

First, we can directly assess its scope by collecting data from external sources on the race of civilian providers in the regions associated with each base and then observing the relationship between move-induced changes in the prevailing Black military provider share and the Black civilian provider share. While we do not have data on the race of civilian providers actually treating chronic disease patients in these regions, we can proxy for this information by turning to county-level data from the 2000 Census on the race of physicians, surgeons, physician assistants and registered nurses (U.S. Dept. of Commerce, Bureau of the Census 2000). Doing so—and alleviating the above-stated concern—we find that a move that results in a change in the Black military provider share from 0% to 100% is associated with only a 0.4 percentage point increase in the share of nearby Black civilian providers.

Second, we control in equation (1) already for the share of on-base encounters with non-military providers and its interaction with patient race. This allows us to explore the impacts of a move-induced change in Black military providers—our treatment of interest—while partialing out the general effect of non-military providers. Encouragingly, our results are virtually insensitive to these controls, as we demonstrate in Panel M of Table A5 of the Online Appendix.

The third approach addresses missing provider race through an imputation exercise. Now, we make our object of interest the overall provider race share itself, rather than the military provider race share specifically. For each base, we calculate the share of all providers treating on-base that are Black, weighting the Black military provider share and the Black on-base civilian provider share (using the Census data discussed above). The weights are the share of visits at MTFs with military providers and civilian providers, which we directly observe. We then form ΔPB_j using this new measure, while doing the same for the other provider demographic controls. We also form an alternative overall Black-provider share based on all care delivered within 10 miles of the base, whether at MTFs or off-base. In each case, as demonstrated by Table A19 in the Online Appendix, we estimate coefficients of the interaction between patient race and the move-induced change in the Black provider share that are nearly identical to—if not slightly larger in absolute magnitude—than that estimated in Table 2 above. One might even expect that these results are attenuated towards 0 given classical measurement error in civilian provider race.

Additional Specification Checks

In Tables A4-A11 of the Online Appendix, we demonstrate robustness of the key findings from Part IV to: (i) the estimation of the mortality specifications on the more restrictive sample on which the medication analysis is estimated, (ii) the estimation of the individual chronic-disease samples separately (e.g., diabetes, hypertension, etc.),[24] (iii) the use of non-medication preventive-care measures for the diabetes observations, (iv) alternative constructions of the racial groups of interest (e.g., comparing Black and white only, focusing on the interaction between Black non-Hispanic patients and providers, etc.), (v) alternative constructions of the Black-provider share variable (and specifications that drop bases with the highest such shares and the most variability in shares over time), (vi) the inclusion of patients who move more than once, (vii) specifications that drop pay-grade controls, [25] (viii) specifications that drop patients with gaps in their Tricare coverage, (ix) alternative approaches to clustering the standard errors,[26] (x) specifications that exclude controls for move-induced changes in other base-level demographics, and (xi) the estimation of Oster (2019) adjusted treatment effects.

Part V: Interpretation and Discussion

While the specification checks help to demonstrate the causal nature of our physician-diversity findings, there are a number of important interpretative issues that we discuss.

Determinants of Provider-Patient Racial Matches and Associated Interpretation Implications

To the extent commentators wish to use the findings of the concordance literature to speak to the overall effects of diversifying the provider workforce—as many have done—a challenge arises in that it is not clear how many more concordant matches will even result from increasing the share of Black providers. One of the advantages of our movers strategy is that it obviates the need to scale the individual-encounter concordance findings by such an estimate. Rather, our analysis immediately delivers on the relationship of interest between racial health disparities and provider diversity. Nevertheless, to help understand the relationship with the concordance literature and to help validate that a concordance mechanism may underly at least

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[24] We note that the magnitude of our findings is strongest among patients with diabetes.

[25] Retiree pay-grades reflect their status as of the last active-duty assignment; given a potential concern over this being an imperfect proxy for their pay levels after the military, we estimate specifications without these controls.

[26] In our primary approach, we cluster the standard errors at the level of treatment—i.e., sending-by-receiving base combinations.

part of our findings, we estimate a version of specification (1) that uses as the dependent variable the post- to pre-move change in the incidence of seeing a Black military provider for chronic-disease care at some point in that pre- or post-move period.

As demonstrated in Table A21 of the Online Appendix, we find a strong, close to one-to-one relationship between having more Black providers and being more likely to see a Black provider. Moreover, that relationship is not highly differentiated by race – it is slightly stronger for Black patients, but the differential is modest and, in several specifications, not significantly different from zero. Overall, the proportional increase in the number of Black-patient / Black-provider concordant matches will track the increase in the Black provider share itself.

Moreover, the fact that Black and non-Black patients both increase their visits with Black providers to a nearly similar degree rules out one possible mechanism underlying our findings. Imagine that only Black patients visit with the expanded Black-provider set. If Black patients experience relative health gains following this development, this may actually be attributed to the possibility that Black providers simply deliver better outcomes for all of their patients—Black and non-Black alike—and Black patients differentially benefit from such gains given the assumption just stated. The findings from Table A21 are inconsistent with such a scenario.

Exploration of Spillover / System-Level Effects

While we have just ruled out that the estimated τ coefficients are a reflection of main provider-race effects that are disproportionately felt by Black patients, this does not mean that the only remaining explanation is the one-to-one provider-patient racial concordance effect that has been the target of much of the literature to date. Other possible powerful mechanisms underlying the estimated τ coefficients include the various community-level effects / spillovers identified in the Introduction—e.g., (i) non-Black providers being debiased by their Black peers, (ii) non-Black providers learning from their Black peers how to communicate with, gain the trust of, or generally treat their Black patients, or (iii) Black chronic-disease patients gaining generalized trust in the medical system when witnessing more Black chronic-disease providers in the community (even if not their providers). We now consider two additional exercises designed to shed empirical light on the existence and extent of such spillovers/system-level effects.

The first such exercise is an interpretative one. Recall that we find a 0.19 percentage-point reduction in mortality (18% relative to the mean) for Black relative to non-Black chronic-disease patients following a move-induced 7 percentage-point increase in the Black provider share. Viewed in terms of an increase from no Black providers to 100 percent Black providers, these results suggest a 2.7 percentage-point reduction in mortality—around 270% relative to the mean—for Black relative to non-Black patients. If we assume there are no spillover effects from increasing the share of Black providers, then one can view this latter interpretation as the provider-patient racial concordance effect. After all, moving from 0 to 100% Black providers captures the effect of seeing a Black provider for sure, which, when interacted with the Black patient indicator, gives the classic concordance effect (Greenwood et al. 2020).

Is a 270% relative mortality effect realistic under a patient-provider concordance story alone? It is notably greater than the 39-58% relative mortality effect from Greenwood et al. (2020), which focused on racial concordance between newborns and pediatricians assigned during the newborn hospitalization. We acknowledge there are several reasons why one might expect a larger concordance effect in the present chronic-disease context relative to the acute childbirth context. After all, a 100% Black-provider share in our chronic-disease mortality inquiry entails patients visiting with Black providers across all of their chronic-disease-care visits over the post-move years collectively. There may simply be a greater scope for provider-patient contact over this time relative to an acute hospitalization during a single childbirth. Moreover, as already discussed, the chronic-disease context affords the potential for an additional set of mechanisms to operate relative to an acute health care context—mainly, it facilitates a stronger role for trust and communication mechanisms. Nonetheless, while one might thus expect a larger effect than the 39-58% estimate from Greenwood et al., one might not expect that these reasons alone would lead to a concordance effect that is 4.6-6.9 times as large. This magnitude, however, becomes more plausible if some of the impact here arises through the theorized systemic / spillover effects.

We next attempt to explore these spillover / system-level effects directly. Since the essence of these theorized effects is that Black patients will still gain with an increase in Black providers when they nonetheless are seen by non-Black providers, we can explore this story by estimating specification (1) on a sub-sample of patients that always see non-Black providers for their chronic-disease care—i.e., both before and after the move.[27] As demonstrated by Table

[27] This exercise, however, can only be performed on those patients that see military doctors for their chronic-disease care given the above provider-race data-availability limitations. If the

A20 of the Online Appendix, we find estimated τ coefficients from this approach that are roughly the same size as the full sample τ coefficients. These findings suggest a potentially meaningful role for spillover mechanisms and thereby suggest that the present literature's predominant focus on the gains from provider-patient racial concordance in individual encounters only captures part of the gains that may ensue from increasing provider diversity.

Provider vs. Patient Drivers

Some of the mechanisms that we have theorized involve a heavier patient component to the patient-provider relationship—e.g., trust- and communication-related mechanisms. Others have involved a stronger provider component to the patient-provider relationship—e.g., provider knowledge of how to treat patients or provider bias in treating patients. These divisions are, of course, not perfect – e.g. trust can be built by providers. But they provide a useful way of dichotomizing the channels through which provider-diversity impacts may operate. In this section, we consider exercises designed to differentiate among these explanations.[28]

Our main approach here is to differentiate aspects of medical treatment where patient trust is likely to be particularly strong or weak relative to the more provider-driven mechanisms. To do so, we turn to clinical contexts where patients have a relatively stronger role to play in the clinical decision-making process. Consider, for example, medication use along the extensive (initial prescription) and intensive (adherence) margins. While both provider and patient play a role in deciding to initially fill the prescription, the refill decision is driven almost fully by the patient; therefore, if patient trust is the mediating mechanism, we would expect the provider diversity effects to be relatively strong for refills. We explore this in Table 5.

Panel A restates our primary results, which uses a medication-fills-day measure that combines the intensive and extensive margins. In Panels B and C, we split our results into its intensive and extensive components. To explore the intensive margin, we limit the sample to

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primary focus of this paper were to estimate one-to-one provider-patient racial concordance effects and if we were to explore that question via our movers strategy, one can view this exercise as estimating our reduced form on a sub-group of patients that are akin to "never-takers."

[28] We note upfront that this trust/bias/knowledge discussion does not present as an alternative to the concordance-versus-spillovers mechanism discussion; rather it represents another dimension of the mechanism discussion. For instance, patient trust may represent a key component of any observed concordance effects, along with any observed spillover effects.

those patients that have at least one prescription filled in both the pre- and post-move period and then explore the impact of move-induced increases in Black providers—for Black relative to non-Black patients—on the number of fill days. We estimate an increase in fill days with this intensive-margin specification that is over double that of the primary results.

Table 5. Medication Intensive and Extensive Margin Analysis

Panel A: Intensive and Extensive Margins Collectively (Primary Results from Table 2)		
Dependent Variable = Change (Post-Move minus Pre-Move) in Medication Fill Days		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	2.992***	2.676***
	(1.007)	(0.939)
N	130952	130952
Panel B: Intensive Margin		
Dependent Variable = Change (Post-Move minus Pre-Move) in Medication Fill Days Among those Chronic Disease Patients with Some Fills in Both the Pre- and Post-Move Period (Mean Medication Fills Among This Sample = 218.29)		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	6.936***	6.305***
	(2.599)	(2.572)
N	58186	58186
Panel C: Extensive Margin		
Dependent Variable = Change (Post-Move minus Pre-Move) in Incidence of Some Medication Fill Days Among all Chronic Disease Patients (Mean Incidence of Some Medication Fill Days = 0.29)		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	0.006	0.005
	(0.004)	(0.004)
N	130952	130952
Dependent Variable = Change (Post-Move minus Pre-Move) in Incidence of Some Medication Fill Days Among Chronic Disease Patients with no Medication Fills in the Pre-Move Period (Mean Incidence of Some Medication Fill Days = 0.29)		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	0.003	0.003
	(0.004)	(0.003)
N	92064	92064
Including Fixed Effects for Pre- and Post-Move Bases?	NO	YES

Notes: The specifications and samples estimated are identical to that estimated in Table 2 except that the samples and dependent variables are as indicated. ***Significant at the 1 percent level; ** Significant at the 5 percent level; *Significant at the 10 percent level.

Exploring the extensive margin in Panel C, we estimate a near-zero relationship between the move-induced change in the incidence of any fills for Black relative to non-Black patients and the move-induced change in the Black provider share. To better isolate the physician component to this extensive-margin analysis, we also estimate a specification that focuses on the initial fill itself rather than also capturing information from withdrawals from utilization in the post-move period. Accordingly, we focus on patients with no medication fills in the pre-move-period and explore their response to a move-induced change in the Black provider share on the likelihood of filling a prescription in the post-move period. Doing so, we continue to find a near-0 impact on medication fills. Moreover, specifying each of these intensive and extensive outcomes as Z-scores to facilitate comparisons across Panels B and C, we find that the estimated τ coefficient for the intensive-margin analysis (0.047) is larger than the upper end of the 95% confidence interval (0.035) of the corresponding extensive-margin coefficient.

In a second approach to distinguish the trust channel, we consider a different setting than chronic care: the emergency room. In emergent situations, providers are likely to have a notably greater role in determining the treatments applied, whereas patient trust will matter relatively less, particularly for acute emergencies. We therefore extend our analysis to consider a sample of patients with multiple ER visits, one before and after an across-base move, and again estimate a movers-based strategy. There is no natural analog to the preventive-care measures in the chronic-care context, so we look at measures indicative of the level of care applied during the ER visit itself, as captured by various metrics: (1) Relative Value Units (RVU), (2) number of procedures and (3) number of diagnostic tests. In addition, we follow the long-term outcomes for the patient and observe the incidence of mortality, though the baseline rate for this is low.

The results are shown in Table 6. In all cases, we find no relationship between a move that increases the prevailing share of Black ER providers in the community and the relative value of these measures between Black and non-Black patients. The relevant interaction coefficients are tightly bound around zero. For instance, relative to the mean RVU level, the confidence interval for the estimated τ coefficient spans from -1.6% to 0.5%. In other words, the relative preventive care and mortality effect sizes from the chronic disease results fall far outside of the relative effect-size confidence interval for the ER analysis. We apply various robustness checks to this analysis, including focusing on more emergent ER visits (see Column 2, where we condition on ER visits with diagnosis codes with large weekend shares, suggesting non-discretionary visits), and the results hold. The lack of provider diversity effects on the delivery of ER care, when they are so prevalent for chronic care, further bolsters the suggestion that patient trust is a meaningful determinant of our provider diversity findings.

Table 6. Relationship between Increased Racial Diversity of Providers and Changes in Racial Health Disparities, for Emergency Room Care

Panel A: Dependent Variable = Change (Post-Move minus Pre-Move) in Average RVU Total Per ER Admission (Mean RVU Per ER Admission = 1.865)		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	-0.007	-0.010
	(0.009)	(0.010)
N	470677	129929
Panel B: Dependent Variable = Change (Post-Move minus Pre-Move) in Average Number of Diagnostic Procedures Per ER Admission (Mean Number of Diagnostic Procedures Per ER Admission = 0.63)		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	-0.008	0.008
	(0.010)	(0.016)
N	502084	151863
Panel C: Dependent Variable = Incidence of Mortality in Post-Move Period (Mean Mortality Incidence over Sample = 0.0096) (Reported Coefficients Multiplied by 1,000)		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	0.366	0.155
	(0.466)	(0.432)
N	502269	151916
Sample Restricted to Diagnosis Codes with Weekend Shares >= 0.24	NO	YES

Notes: this table presents results from a sample of patients that move across bases once during the sample period and that visit the emergency room in each of the pre- and post-move periods. The specifications estimated are otherwise structured similarly to that estimated in Table 2. The move-induced change in the Black provider share (based on providers who see patients in the emergency room) is normalized such that coefficients capture the effect of a 1-standard deviation change in the Black provider share (1-standard deviation represents 0.11). ***Significant at the 1 percent level; ** Significant at the 5 percent level; *Significant at the 10 percent level.

In a third, related approach to probing a trust-based mechanism behind provider-diversity effects, we again turn to a clinical context outside of the chronic-care setting and consider the distinction between planned and unplanned cesarean deliveries during childbirth.[29] Planned cesareans result from more detailed consultation between physicians and patients than do unplanned cesareans. And once again, we find that in the context where the patient plays a larger role, the effects of provider diversity on racial disparities are much larger. As demonstrated by Table 7, employing an analogous movers strategy in the childbirth context, we

[29] This analysis also draws on data from Frakes et al. (2023b).

find that provider diversity is associated with a notable increase in planned cesarean rates for Black relative to non-Black patients with no association with unplanned cesarean rates. Moreover, the τ coefficient for the planned cesarean rates specification is greater than the upper end of the confidence interval for the unplanned cesarean specification.[30]

Table 7. Relationship between Increased Racial Diversity of Providers and Changes in Racial Health Disparities for Cesarean Section Delivery Incidence, Scheduled and Unscheduled

Panel A: Dependent Variable = Change (Post-Move minus Pre-Move) in Incidence of Scheduled Cesarean Delivery (Mean Incidence of Scheduled Cesarean Delivery = 0.190)		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	0.012* (0.007)	0.012* (0.007)
N	37304	37304
Panel B: Dependent Variable = Change (Post-Move minus Pre-Move) in Incidence of Unscheduled Cesarean Delivery (Mean Incidence of Unscheduled Cesarean Delivery = 0.061)		
Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	-0.004 (0.006)	-0.003 (0.006)
N	37304	37304
Pre- and Post-Move Base Fixed Effects?	NO	YES

Notes: this sample includes mothers who have delivered more than once and moved across bases between the moves, where both the pre- and post-move base has a base hospital. The specification is otherwise structured similarly to that estimated in Table 2, except that the specifications also include the following unavoidable delivery risk factors (following Frakes et al. 2023a): breech presentation, multiple deliveries, cord prolapse, placenta previa and placenta abruption. The move-induced change in the Black provider share (based on providers who deliver children on the base) is normalized such that coefficients capture the effect of a 1-standard deviation change in the Black provider share (1-standard deviation represents 0.10).

While our findings suggest a stronger role for provider diversity in situations where patients have a more determinative decision-making role—and thus support a trust / communication mechanism—the possible mechanisms involved are not mutually exclusive and there may remain some role for provider-driven mechanisms. In another exercise, we attempt to shed further light on such mechanisms. For these purposes, we consider variation across bases in an important determinant of provider knowledge and potential bias: the share of patients that are Black. White provider knowledge of Black patent needs may be stronger in an environment with

[30] The fact that we do not find that provider racial diversity leads to reduced health disparities across the board—but only when theorized based on trust-based provider-patient mechanisms—reinforces that our movers design is not likely picking up something just about move-induced changes in omitted base characteristics but rather reflects something about move-induced changes in provider race.

more Black patients. And past research suggests that exposure to different races significantly reduces racial bias (Zebrowitz et al. 2008). Accordingly, any effects stemming from increasing the racial diversity of the provider workforce that operate via provider-based mechanisms may be crowded out in environments with many Black patients. That is, if provider-based mechanisms explain our estimated treatment effects, one might expect them to operate more strongly when there are fewer Black patients around. It is unclear whether a provider-racial-diversity effect based on a trust-based mechanism would operate differentially in these environments.

To assess provider diversity effects in low Black-patient populations, we estimate specification (1) on a sub-sample of chronic-disease patients whose pre- and post-move bases have below-median shares of Black MHS beneficiaries. Likewise, to assess provider diversity effects in high Black-patient populations, we do the same for patients whose pre- and post-move bases have above-median shares of Black MHS beneficiaries. As shown in Appendix Table A23, the point estimates are quite similar above and below median. This suggests that provider-driven mechanisms may not be strong drivers of our results in Table 2—in favor of a more patient-driven interpretation, such as trust- and communication. However, we acknowledge that this last exercise—which focuses on patient-level spillovers—does not rule out the kind of provider-to-provider spillovers bearing on provider knowledge and bias that we theorize above may possibly result from increased provider diversity.

Leveling Up or Down

There are two stories collectively captured by the interaction coefficient, τ: (1) the degree to which Black patients gain ground over non-Black patients and (2) the degree to which non-Black patients lose ground relative to Black patients, in each case when the Black provider share increases. Estimating the main effects of Black-provider-increasing moves from equation (1), δ, can help illuminate these respective contributions. That is, the net effect of Black-provider increasing moves on Black patients can be estimated by $\delta + \tau$ and the net effect on non-Black patients is captured by δ. We present estimates of δ in Table A2 of the Online Appendix.

We note certain caveats with this exercise. First, the identifying assumptions associated with the estimation of δ are slightly different than with τ. Second, and more importantly, we are unable to estimate δ while also simultaneously including sending and receiving base effects due to perfect collinearity concerns. We nonetheless account partially for place effects by estimating specifications that include fixed effects for sending bases. In the case of the mortality analysis, our findings in Table A2 suggest a leveling-up story in which Black patients experience gains in connection with Black-provider increasing moves, while non-Black patients do not lose ground. In the case of the preventive medications analysis, we estimate a δ coefficient of -1.0 (statistically indistinguishable from zero), in comparison with the estimated τ coefficient of 2.6, suggestive of a story of absolute positive gains for Black patients though possibly with some degree of non-Black patients losing ground.

Generalizability

Despite certain representativeness advantages of our analysis over many of the related racial concordance studies—e.g., due to our broader geographic coverage—our analysis nonetheless poses certain generalizability caveats of its own. 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 than never movers, as we demonstrate in Table A23 of the Online Appendix. However, if anything, that perhaps suggests that our results (while local to movers) may be an underestimate of the full benefits of provider diversity on reduced racial health disparities.

Second and more fundamentally, our analysis is specific to the MHS, with beneficiaries that may not be representative of the full population—for instance, they may be healthier (Frakes and Gruber 2019) and more trustful of institutions at baseline. Nonetheless, both of these points might suggest that our results are again a lower bound. Concerns that the MHS population may be in general healthier than the overall population are also mitigated by conditioning our analysis on those with chronic diseases. Ultimately, even if our results are specific to the MHS, this is an important and under-studied population.

Further, we acknowledge that our primary analysis is focused on just a segment of the MHS population—those with chronic conditions—and may not generalize to other patient populations. In fact, one of the key points of the preceding analysis probing trust-based mechanisms is that our chronic-care results are not universal. Nonetheless, in a final empirical exercise, we show that our preventive-care findings among chronic-disease patients generalizes—albeit at a smaller magnitude—to a preventive care setting among a sample of patients not limited by disease status: cancer screening. In particular, we explore Pap-testing to screen for cervical cancer and mammogram screening for breast cancer. We estimate specification (1) on a sample of women eligible for cervical cancer screening pooled with a sample of women eligible for mammogram screening,[31] where the dependent variable captures the move-induced change in the average annual incidence of compliance with the relevant screening guideline (we set forth such guidelines in the Online Appendix). We present our findings in Table 8.

Our results suggest that a 1-standard-deviation move-induced increase in the Black-provider share is associated with 0.006 to 0.009 increase in the incidence of screening-guideline compliance for Black relative to non-Black patients, representing an effect that is roughly 0.8 to 1.3% relative to the mean concordance rate.

Table 8. Relationship between Increased Racial Diversity of Providers and Changes in Racial Health Disparities in Cancer Screening Rates (Pooling Over Cervical Cancer and Breast Cancer Samples)

Black Patient X 1-Standard Deviation Change in Base Black-Provider Share	0.009**	0.006*
	(0.004)	(0.003)
N	143701	143701
Including Fixed Effects for Pre- and Post-Move Bases?	NO	YES
Mean Cancer-Screening Rate	0.69	

Notes: the results presented are from a sample of women between the ages of 20 and 65 who move across bases over the sample (cervical cancer sample) pooled with a sample of women between the ages of 50 and 65 who likewise move across bases over the sample (breast cancer sample). The results presented are the estimated τ coefficients from specification (1) on this pooled sample. The regression includes a dummy indicating the relevant sub-sample. Standard errors are clustered at the sending-by-receiving base level. The move-induced change in the Black provider share is normalized such that coefficients capture the effect of a 1-standard deviation change in the Black provider share (1-standard deviation represents 0.12). ***Significant at the 1 percent level; ** Significant at the 5 percent level; *Significant at the 10 percent level.

On a final note regarding generalizability, there may be additional reasons to believe that increasing the racial diversity of providers more broadly may have even larger effects on racial health disparities than we find in the MHS context considering that we have explored this question within a population that is guaranteed access to care. In non-MHS settings, the access

[31] The Black-provider share for the cervical cancer observations is based on providers who perform annual gynecological exams. For the breast cancer observations, this measure is based on providers who perform annual physical exams.

effects of increasing provider diversity may be notable given evidence that Black providers are more likely to locate and practice in communities with disproportionate Black populations (Cohen et al. 2002).

Mortality-Effects Decomposition Analysis

We have explored the impacts of provider diversity on both preventive care and mortality, but these outcomes are conceptually linked. To further interpret our findings, we explore how much of the documented mortality effect likely arises through improved preventive care. To begin this decomposition, we identify in the Online Appendix a set of studies for each chronic condition from which we determine the percentage-point impact on mortality arising from a 1-fill-day increase in the relevant preventive medication. We then average these results over the four chronic conditions, weighting these findings by the relevant disease's share of the pooled sample from our analysis. Doing so, our review of the medical literature suggests that a 1-fill-day annual increase in preventive medication is associated with a 0.021 percentage-point reduction in the incidence of mortality among the chronic diseases we study.

We then decompose the overall mortality effect from Table 2 into the effect of preventive medication adherence and a residual:

Overall Percentage-Point Mortality Effect (From Table 2 = -0.19) =
[*Increase in Medication Fill Days* (From Table 2 = 2.67) X
Percentage-point Impact on Mortality of 1 Fill-Day Increase in Metformin Use (Average Estimate from Literature = -0.021)] + *Residual* (5)

Solving for the residual and scaling it by the -0.19 overall mortality result, we find that roughly 68.9% of the mortality effect on Black relative to non-Black patients of increasing provider diversity can be explained by something other than the increased medication adherence. Correspondingly, we find that 31.1% of the mortality effect can be explained by increased medication fill-days. The residual may be due to effects of provider diversity not otherwise explored in our analysis due to data limitations—e.g., improved exercise, improved diet, etc. (not otherwise arising from improved medication use).[32] Of course, these are just rough estimates as

[32] We note that for the purposes of tractability in this back-of-the-envelope decomposition exercise, we are effectively assuming a linearity in the relationship between medication fill-days

we assume that the average impact of improved preventive care from the literature applies on the margin to our provider diversity effects.

Part VI: Conclusions

In this paper, we provide compelling economic evidence of the consequences of compromised trust in the medical system and in medical providers among Black patients and the gains that may ensue from increasing the racial diversity of the provider workforce. Using unique data on a very large geographically dispersed sample of individuals in the military, we are able to use the fact that military medical providers are themselves patients to identify the racial composition of a set of providers whose practices and patient outcomes we can observe. And drawing on a combination of across-base moves and heterogeneity in Black provider shares across bases, we show that when Black patients are exposed to a greater availability of Black providers, they both use more preventive care and have better health outcomes relative to non-Black patients in a similar circumstance. Our findings build on previous literature by capturing mechanisms of provider diversity that go beyond just the likelihood that there will be more racially concordant matches during individual encounters.

Along with a series of specification tests to statistically probe our identification assumptions, the richness of our data allows us to explore further the mechanisms that can explain our findings. We show that much of the effect appears to arise from spillovers to non-black providers. We also show that there is strong evidence for a trust mechanism, since we see stronger effects of provider diversity in initial prescriptions than in refills; stronger effects for chronic than emergency care; and stronger effects for planned than emergency cesarean sections.

These findings are particularly timely in light of the recent 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 significantly limit the ability of medical schools to use race as a consideration in medical school admissions and thereby hamstring affirmative action programs as a tool to increase the diversity of the physician workforce. Drawing on state-level bans on race-based admissions that took place prior to these Supreme Court decisions, recent scholarship has validated concerns that they may lead to

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and mortality. While there may be non-linearities in this relationship in reality, our review of the medical literature does not provide specific guidance in this regard.

reduced Black representation in law schools and medical schools (Ly et al. 2022; Brooks et al. 2023). Such findings, together with those set forth in this paper, suggest that these judicial developments may expand the gaps between Black and non-Black patient outcomes. In this light, in her dissent in the above-stated decisions, Justice Jackson referenced the racial concordance findings of the Greenwood et al. (2020) article summarized above in arguing that the diversification of medical school graduates is not "a trendy slogan. It saves lives."

Judicial consideration of affirmative action is unlikely to have ended with the *Students for Fair Admissions* opinion, with the Supreme Court being expected to consider next the legality of affirmative action programs in the workplace, as distinct from college admissions. As such, debates over both the existence of the "diversity rationale" for affirmative action as an empirical matter, and its propriety as a justification for affirmative action as a legal matter, are likely to continue. At the same time, the empirical foundations of the diversity rationale continue to be limited by a lack of studies with suitable data and research designs. Though with generalizability caveats, our analysis contributes to this ongoing conversation.

Data Availability Statement

All code required to replicate the results in this paper is publicly available at Zenodo: https://doi.org/10.5281/zenodo.15288849. Our primary data source is confidential (restricted health data) and cannot be made publicly available pursuant to the terms of the Data Use Agreement between the authors and the Military Health System (MHS). Instructions on how to request these data from the MHS are provided in the above Zenodo link. Certain additional secondary data files that are publicly available are nonetheless subject to dissemination restrictions and are thus not provided in this replication package. Instructions on where to find these secondary data sources are also included in the above Zenodo link.

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