A Doctor Like Me:
Physician-Patient Race-Match and Patient Outcomes

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Abstract. This paper assesses the impacts of physician-patient race-match on patient mortality. We draw on administrative data from Florida, linking hospital encounters from mid-2011 through 2014 to information from the Florida Physician Workforce Survey. We focus on a subset of hospital patients who are conditionally randomly assigned to physicians. We find that physician-patient race-match reduces the likelihood of within-hospital mortality by 0.14 percentage points, a substantial 13% reduction relative to the overall mortality rate. Results are primarily driven by gains experienced by black patients when matched to black physicians.

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1. Introduction

Despite substantial convergence during the 20th century, there remain notable disparities in health outcomes between blacks and whites in the United States. As of 2011, black life expectancy was approximately four years shorter than white life expectancy (Boustan and Margo, 2014). At the same time, African Americans are dramatically underrepresented in medicine: only four percent of physicians are black.[1] Both phenomena are extremely complex with numerous potential explanations, but their potential link has been highlighted by policymakers and researchers for decades. For instance, in 1985 the U.S. Department of Health and Human Services published *The Report of the Secretary's Task Force on Black and Minority Health*, which argued that racially driven disparities in health outcomes should be a national priority while noting that "most minorities receive health care from providers who do not share their own ethnic/cultural background". The report went on to assert that efforts should be made to improve minority representation in the health profession. This paper aims to address the possibility that increased minority representation in medicine may play a role in reducing disparities by allowing for more frequent matching of minority patients with minority physicians. Specifically, we ask: in a hospital setting, does doctor-patient race-match impact patient mortality?

We address this question by drawing on encounter-level data from Florida hospitals and pairing these data with the Florida Physician Workforce Survey, which identifies the race of the universe of physicians in Florida. Our primary outcome is patient mortality during their hospital stay. To ensure plausibly exogenous assignment of patients to attending physicians, we focus on

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[1] This figure was drawn from the Association of American Medical Colleges' "Diversity in the Physician Workforce" report from 2014. It can be retrieved here: https://www.aamc.org/data/workforce/reports/439214/workforcediversity.html .

uninsured patients admitted through the emergency department.[2,3] The vast majority of uninsured patients are admitted through emergency departments, so our sample of uninsured patients is representative of that population. These patients' hospitalizations are almost certainly unscheduled, so whoever is on call is likely to become their attending physician. Our main specifications include a rich set of physician, hospital, patient ZIP code, and diagnosis fixed effects to rule out lingering selection concerns. We perform additional analyses using an instrumental variables approach to further ensure that our estimates can be interpreted as causal.

We find that doctor-patient race-match leads to a reduction in mortality while in the hospital. In our estimation sample, 1.1% of hospitalizations end in the patient's death. A race-match leads to a 0.14 percentage point decline in the likelihood of mortality. Relative to the baseline, this represents a 13% reduction. When we restrict the sample to conditions that are more likely to lead to death, the effect is twice as large.

How does physician-patient race-match impact patient mortality in our setting? The existing research (discussed in more detail in the next section) highlights one potential mechanism: race-match improves communication. One reason this might occur is differential trust in the medical system across race groups. Survey evidence from Boulware et al. (2016) suggests that black respondents trust in their physicians less than white respondents. Alsan and Wanamaker (2018) document that the revelation of the Tuskegee Syphilis Study decreased black men's trust in the medical system, which in turn led to fewer interactions with physicians and higher mortality rates.

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[2] As noted later, we further restrict the data to black and white patients, and black and white physicians. We omit Hispanics from our analysis as assignment of patients to co-ethnic physicians may be nonrandom if the patient primarily speaks Spanish. Indeed, while we present conditional balance tests later in the paper to support the notion that black patients are equally likely to see a black or white physician, the same tests failed for Hispanic patients, who were substantially more likely to see a Hispanic physician.

[3] This is also an independently relevant subpopulation; uninsured patients represent a vulnerable group that faces many challenges accessing healthcare. Furthermore, mortality, the cleanest indicator of a patient's outcome, is rarely observed in outpatient settings. This makes our inpatient setting particularly useful.

Simeonova (2013) documents lower survival rates for blacks than whites in a subset of patients diagnosed with chronic heart failure, and finds that this gap is explained by patients' adherence to recommended clinical therapy rather than socio-economic status or quality of care. This patient- rather than provider-level factor is consistent with the type of differential engagement with the medical system that could arise from lower levels of trust or communication. These findings, to some extent, echo the findings from the medical literature; black patients – particularly when interacting with white physicians – engage less during their visits and are provided with less information. Importantly, this is no longer true when a patient interacts with a race-matched physician (Cooper et al., 2003; Gordon et al., 2006). It may be then that race-match improves trust, which in turn improves communication and outcomes.

A second potential mechanism is racial bias in treatment. A small number of studies test for discrimination as an explanation for the differential treatment provided to black patients relative to white patients. Green et al. (2007) administer an Implicit Association Test to emergency department and internal medicine residents to quantify implicit bias. They then present the residents with a medical vignette in which symptoms are common, but the photo and implied race of the patient are randomized. Residents with more implicit bias against black patients (according to the test) are less likely to suggest the appropriate treatment for black patients, and more likely to do so for white patients. Anwar and Fang (2012) reach the opposite conclusion in a study drawing on administrative data from emergency departments. Conditional on receiving a diagnostic test in the emergency department, they demonstrate that patients who return within 72 hours of discharge receive substandard treatment upon their return; this occurring more often among minority patients would suggest prejudicial treatment. They find no evidence of this phenomenon in a comparison of black and white patients. Thus, from the limited existing work, evidence on implicit bias in health care is mixed.

It is difficult to pin down the mechanisms that drive our results, but the evidence points away from bias and towards enhanced patient-physician communication as the more likely channel. First, we observe that black patients fare better than white patients under the care of black physicians, but there is no patient race effect for white physicians. If our results were driven by discrimination, we would expect to find that black patients fare worse than white patients under *white* physicians and no race effect for *black* physicians. We interpret this as consistent with the hypothesis that race-match between black patients and black physicians improves communication, so access to black physicians differentially benefits black patients. Second, the effect is also only evident for race-matches with attending physicians, the physician with whom patients most often communicate, and not surgeons, who have less verbal interactions with patients. And, third, under the discrimination hypothesis, one might expect evidence of lower effort towards diagnosing and treating race-mismatched patients. We do not see evidence of this sort. Instead, when we regress proxies for medical treatment on race-match, we find that race-match does not systematically predict the level of care a patient receives.

In addition to the health economics and medical literatures that we discuss in more detail in the next section, this study contributes to the broader economics literature on the impacts of race-match in principal-agent settings. The large and growing “teacher like me” literature documents educational gains from teacher-student race-match in a K-12 setting (Dee, 2004; Dee, 2005), in undergraduate education (Lusher et al., 2015; Fairlie et al., 2014), and in graduate education (Birdsall et al., 2016). Grissom and Keiser (2011) find that job satisfaction and retention are improved among teachers with same-race principals. Fisman et al. (2017) find that having a culturally proximate loan officer increases credit access and reduces collateral requirements. Two recent papers find race-match effects in judicial decisions. Shayo and Zussman (2011) analyze decisions in Israeli small claims courts where assignment to an Arab or Jewish judge is essentially random and show that assignment to an in-group judge increases the likelihood that a claim is accepted. Depew et al. (2017), on the other hand, show that juvenile defendants in Louisiana receive *longer* sentences from same-race judges. Given the considerably different settings, we hesitate to draw direct links between the mechanisms driving race-match effect in our paper and in the papers cited here. Nonetheless, with the exception of the Depew et al. paper, race-match has been shown to generate positive outcomes across a wide range of settings that have traditionally featured racial disparities between minority and majority groups; it is worth noting that cultural proximity and ability to communicate effectively could conceivably be an explanation driving a number of these findings – including ours.

2. Relation to Existing Work on Differential Treatment by Race and Physician-Patient Race-Match

This study contributes to a broad literature spanning public health, medicine, and health economics, which explores differences in medical experiences by patient race and the degree to which such differences are impacted by having a race-matched or -mismatched physician. We provide a brief overview of two strands of that literature here. First, some work has documented clear differences in the treatment received by black and white patients. Second, a body of work has documented that patients with race-matched physicians have better communication with their physician and higher satisfaction in their encounters. Our paper aims to contribute to these literatures by asking whether race-match in turn has an impact on patients' medical outcomes – specifically, mortality.

Beginning with work documenting stark differences in the treatments received by black and white patients: a very recent study finds that black patients are significantly less likely than white patients to be prescribed opioids for "non-definitive" pain conditions like back pain, while no gap in prescription is observed for "definitive" conditions like kidney stones (Singhal et al., 2016). Todd et al. (2000) report a similar finding; in a study of 217 emergency department patients with long-bone fractures, black patients are significantly less likely to be given analgesics than white patients, even after controlling for a variety of potential confounders. These findings are consistent with a hypothetical choice experiment conducted by Hoffman et al. (2016). Their study demonstrates that a sizable share of white medical students and residents hold false beliefs about biological differences between black and white patients. Moreover, medical students and residents underestimate the pain of hypothetical black patients relative to hypothetical white patients, and make less accurate treatment recommendations for hypothetical black patients. [4]

In some cases, treatment differences have been found to be an across-hospital rather than within-hospital phenomenon. A number of papers have argued that black and white patients receive different treatments after a heart attack, but Barnato et al. (2005) show that these differences are largely eliminated after including hospital fixed effects. This latter finding suggests that previous results are likely an artifact of differential place-of-care rather than differential treatment within a hospital. Chandra and Skinner (2003) and Bach et al. (2004) also highlight that different types of hospitals and doctors treat black and white patients. Therefore, the fact that we can include hospital and physician fixed effects in our analyses makes our data especially well-suited to answering the question at hand. These fixed effects allow us to rectify across-hospital and across-physician biases.

[4] Some race-based stereotypes have even appeared in modern medical textbooks. Pearson recently apologized for a nursing textbook that included the statements "Blacks often report higher pain intensity than other cultures" and "Hispanics may believe that pain is a form of punishment and that suffering must be endured if they are to enter heaven." https://www.insidehighered.com/news/2017/10/23/nursing-textbook-pulled-over-stereotypes.

While a body of work documents differences in treatments and outcomes between black and white patients, the empirical question of whether physician-patient race-match minimizes these differences – the focus of our paper – remains. Some existing research, largely in medicine, has documented that physician-patient race-match improves patient satisfaction and communication. Saha et al. (1999) draw on survey data and find that black patients with black physicians reported higher levels of satisfaction than black patients with non-black physicians. In a related study, Cooper-Patrick et al. (1999) conduct a survey of black and white physicians and patients, aiming to understand whether communication-style is a function of patient race, physician race, or physician-patient race-match. Their main outcome is an index of "participatory decision-making" (PDM); this captures the extent to which physicians involve patients in their decisions. While they uncover no statistical difference in the PDM scores of black and white physicians, black patients generally rate their physicians less favorably on the PDM index. More importantly for the sake of this paper, they find that patients with race-matched physicians rate their physicians more favorably on the PDM index than those in mismatched pairings.[5]

Gordon et al. (2006) analyze transcribed audiotapes of black and white lung cancer patients' doctor visits. Patients with racially mismatched physicians receive less information from their physician and participate less in the conversation. After controlling for patients' participation in the conversation, the gap in information is no longer significant, suggesting the gap in information is largely driven by patients' reduced engagement with a racially mismatched physician.[6] Cooper et al. (2003) similarly analyze transcribed audiotapes of patient-physician encounters and find that race-matched pairings feature longer visits, more positive patient affect, and higher patient satisfaction.

[5] Interestingly, there was no difference in gender match vs. mismatched patient-physician pairings. The same is true in our paper; gender match does not impact patient mortality.

[6] This point was further emphasized in follow-up work from Street et al. (2007).

They conclude that "increasing ethnic diversity among physicians may be the most direct strategy to improve health care experiences for members of ethnic minority groups." Related to Cooper et al.'s (2003) findings, it is worth noting that some research documents that improvements in patient affect have a direct effect on clinical outcomes (Kadom et al., 2017). This suggests an alternative causal channel through which race-match may impact outcomes in our study: race-match improves patient affect which in turn improves outcomes; this could be true even in the absence of any differences in treatment or adherence to treatment stemming from better communication.

Finally, Chen et al. (2001) address whether race-match in the provision of cardiac catheterization, a procedure used to evaluate heart functioning after a heart attack. Contrary to what might be expected based on some of the studies discussed above, they find that the use of cardiac catheterization is not more likely in race-matched than race-mismatched patients.

Our study contributes to these literatures in two major ways. First, it remains unclear whether physician-patient race-match affects patients' medical outcomes. Most studies measure perceived improvements in health care experiences. Our paper addresses this directly by measuring the impact on inpatient mortality.[7] Second, much of the work on physician-patient race-match discussed above measures outcomes for patients who select into physician-patient pairs, with no random or quasi-random assignment of patients to physicians. Accordingly, results are unlikely to reflect causal effects. By employing a research design that generates plausibly exogenous assignment of patients to physicians, our study provides some of the first causal evidence on the impacts of physician-patient race-match on medical outcomes.

Finally, in a working paper contemporaneous to ours, Alsan, Garrick and Graziani (2018) randomly assign black male patients to a white or black male physician at an outpatient clinic in

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[7] The Chen et al. (2001) study is one important exception in this regard, but notably – in assessing a broader wider range of patients – we find that race-match impacts patient outcomes, which runs counter to their findings.

Oakland, California. They find that race-match increases black males' demand for preventative services. Their study does not measure effects on clinical outcomes, but does suggest an impact on an intermediate health input, demand for healthcare. This finding corroborates our discovery of an effect on an important clinical outcome that is often hard to detect, patient mortality. Consistent with our findings, their study points towards better communication as a distinguishing factor between race-matched versus race-mismatched pairings. As their randomized control trial demonstrated (and similar to findings from Gordon et al. (2006)), during race-matched interactions patients talk more about their health problems and doctors take more thorough notes.

3. Data

Two sources of data are used in this study. The first is the Florida Hospital Discharge Data File from July 2011 through December 2014. The second is the Florida Physician Workforce Survey from 2006 and 2008 to 2016.

The Florida Hospital Discharge Data File contains encounter-level records on all hospitalizations in the state, except for those occurring within state-operated, federal, and Shiner’s facilities. These data report patients’ demographics, diagnoses, procedures, and National Practitioner Identification (NPI) numbers for patients’ attending physicians. Our focus in this paper is the impact of race-match between a patient and his or her attending physician. Note that we also observe the NPI number of a patient’s *operating* physician, which is available only for the smaller set of patients who have a procedure.

We restrict the sample to uninsured patients admitted to hospitals from an emergency department. Each year, approximately 1.5 million uninsured patients in the U.S. are hospitalized (HCUP, 2017). The vast majority of these are admitted through the emergency department. In our sample, 76% of uninsured hospital patients enter through the emergency department. We focus on these because, once within the hospital, these patients are generally assigned to an attending physician in a process that, for our purposes, is essentially random.[8] One exception, however, is women who are visiting the hospital for a labor and delivery related issue. Even if uninsured and admitted through the emergency department, these women may have established care with a health professional before their visit and this provider might become their attending physician. We therefore exclude all women whose hospitalization is for a labor and delivery related issue. We also exclude the newborns born during the hospitalization. Finally, to avoid measurement error, we drop observations in which there is a reported data entry error (e.g., incorrect patient race or physician NPI number) in the corresponding hospital-by-quarter file. This latter exclusion affects only 2% of observations.

Every two years, physicians in Florida must complete the Florida Physician Workforce Survey when renewing their licensure. The department that administers the survey merges data on respondents' demographic information to their responses. We obtain these demographic data, which identify physicians' NPI numbers.

We link the hospital and physician demographic data using the NPI numbers that are found in each source of data. This allows us to observe, for each hospital encounter, the race and ethnicity of the patient and the race and ethnicity of the patient's attending physician. To simplify the

[8] When we asked one physician about this process, she replied, "...gender/ethnicity is not a consideration in any hospital I have worked at for emergency department to inpatient service assignments (other than labor and delivery, which is a different monster). Usually there is a complicated matrix that assigns the patient based on their primary care provider (e.g., some old-fashioned primary care providers will actually follow their patients in the hospital) or primary care provider group (a traditional on-call system for groups of internal medicine/pediatric/family medicine physicians). For patients out of the system or without a primary care provider entirely, there is an algorithm for distributing these patients somewhat equally to minimize over-burdening any one service. It can be based on any number of factors. For instance, at [Hospital X], it was based on whether their medical record number was an even or an odd number and what their admitting complaint was. The Family Medicine inpatient service got odd numbered patients without primary care providers and a complaint of abdominal pain not otherwise specified, cellulitis, (...), and a few other things."

interpretation of the findings, we consider only non-Hispanic black and non-Hispanic white patients and physicians. Especially in Florida, Hispanic ethnicity will overlap with proficiency in the Spanish language. So that we do not conflate potential gains from racial concordance with language concordance, we omit all individuals with any mention of Hispanic ethnicity.[9] Other race groups are very small either on the patient or physician side; variation in race-match in those groups would be extremely limited. Thus, we only estimate – and speak to – the effect of black/white racial synchronicity. This also allows us to speak more directly to much of the existing literature discussed in a previous section, which has largely focused on black/white interactions between patients and physicians.

Descriptive statistics for the final sample are presented in Table 1. Over 150,000 hospitalizations are observed in the sample. Two-thirds of patients are assigned an attending physician who shares their race; one-third is assigned a racially mismatched physician. As expected given the underrepresentation of black physicians, Columns 2 and 3 confirm that black patients are much less likely to be matched to a doctor of the same race than white patients. A complete breakdown of the frequencies for each racial combination is shown in Appendix Table A1. We also provide a list of the ten most common primary diagnoses given to patients in our estimation sample in Appendix Table A2.

4. Empirical Specification

Our paper aims to assess the impact of race-match between a patient and his or her attending physician upon being admitted to the hospital.[10] A naïve version of an estimating equation might

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[9] The impact of physician-patient language congruence on health outcomes is, of course, an important issue on its own, and one that has received much attention from researchers. See, for example, Wilson et al. (2005) and Fernandez et al. (2011).

[10] Later in the paper, we discuss a test of race-match between a patient and his or her operating physician.

take a patient's outcome on the left-hand side and an indicator for whether the patient is in a race-matched pairing on the right-hand side, along with a vector of patient controls:

$$y_{idp} = \alpha + \beta_1 RaceMatch_{ip} + \beta_2 race_i + \beta_3 gender_i + \beta_4 age_i + \boldsymbol{\gamma}_d + \epsilon_{idp} \qquad (1)$$

The indices denote patient *i,* with diagnosis *d*, assigned to attending physician *p*. Diagnosis fixed effects are captured by $\boldsymbol{\gamma}_d$. The dummy variable $RaceMatch_{ip}$ indicates whether the patient and attending physician are of the same race. The coefficient β_1 identifies the impact of being in a race-matched pairing.

There are obvious problems with this specification that could bias estimates of β_1. Most importantly, existing work highlights that black and white patients are treated by different types of hospitals and physicians (Bach et al., 2004; Chandra and Skinner, 2003). These findings are largely – but not entirely – attributable to patients' proximity to high quality hospitals and the demographic make-up of their local physicians. If black patients disproportionately reside in areas with low quality hospitals, lower access to quality care more generally, and relatively large shares of black physicians, then β_1 will be biased towards finding an adverse effect of race-match. It is therefore important to adjust for patients' residential location and the quality of the hospital that they visit. We do so through patient residential ZIP code fixed effects and hospital fixed effects, respectively. Importantly, patient ZIP codes are not neatly nested inside particular hospitals. Patients who live in the same ZIP code often visit different hospitals based on the nature of their condition. Identification therefore stems from within-hospital comparisons of patients' outcomes as a function of race-match, controlling for where a patient resides. This is important. While our sample restrictions are designed to eliminate assignment to particular physicians inside the hospital, the above discussion highlights that assignment to a particular hospital is far from random.

Table 2 presents a series of balance tests documenting the extent to which the set of fixed effects described in the previous paragraph generate conditional random assignment of patients to race-matched physicians. We regress a dummy variable indicating that the patient's physician is black (rather than white) on a dummy variable indicating that the patient is black. Column 1 is the simplest specification and includes no fixed effects. This column corroborates the findings of other studies: without conditioning on hospital or locality, black patients are 9.05 percentage points more likely to see a black physician than white patients. This is true even for uninsured patients entering hospitals through emergency departments. As noted in the above cited papers, this relationship likely reflects larger shares of black physicians working in hospitals near where black patients live. In other words, assignment of patients to physicians is clearly not unconditionally random.

The inclusion of patient ZIP code and diagnosis fixed effects (Column 2) helps reduce the size of the coefficient, but there remains a statistically significant relationship between patient and physician race. Black patients from the same neighborhood and with the same diagnosis as white patients are still more likely to see a black doctor than a white doctor. Column 3 reports a specification that replaces the patient ZIP code and diagnosis fixed effects with hospital fixed effects. Within a given hospital, black patients are only 0.67 percentage points more likely than white patients to see a black physician. Finally, Column 4 includes the full suite of diagnosis, hospital, and patient ZIP code fixed effects. There is no longer a statistically significant relationship between patient and physician race. Black patients from the same neighborhood, with the same diagnosis, and in the same hospital as white patients are no more likely to be assigned to a black physician.

In short, we interpret Table 2, especially Column 4, as evidence that diagnosis, hospital and patient ZIP code fixed effects, in conjunction with our sample restrictions, largely eliminate concerns about non-random assignment of patients to physicians. Nonetheless, our main analysis takes this one step further and includes physician fixed effects. The impact of race-match is therefore identified not only from within-hospital comparisons, but from within-physician comparisons, further minimizing any concerns around selection of patients to physicians.[11]

Our primary estimating equation is given below:

$$y_{idphtz} = \beta_1 RaceMatch_{ip} + \beta_2 race_i + \beta_3 gender_i + \beta_4 age_i + \boldsymbol{\gamma}_d + \boldsymbol{\zeta}_p + \boldsymbol{\eta}_h + \boldsymbol{\theta}_z + \boldsymbol{\lambda}_t + \epsilon_{idphtz} \qquad (2)$$

where the indices denote patient *i*, with diagnosis *d*, assigned to attending physician *p*, in hospital *h*, in year-by-quarter *t*, who lives in ZIP code *z*. The primary outcome, *y*, is inpatient mortality. The explanatory variable of interest is *RaceMatch*, which assumes a value of one if patient *i* and physician *p* are of the same race, and zero otherwise. We control for patient characteristics (race, gender, age, diagnosis fixed effects, ZIP code fixed effects), as well as physician fixed effects (ζ_p), hospital fixed effects (η_h), and year-by-quarter fixed effects (λ_t). Given the preceding discussion, we interpret β_1 as the causal effect of race-match on the patient's probability of inpatient mortality.

In additional analyses, we adopt an instrumental variables approach to provide further evidence that we are identifying causal effects. The basic idea behind our instrument is that the set of physicians available to a new patient is constrained by the set of physicians inside the hospital at

[11] Note that physicians often work in multiple hospitals, so physician fixed effects do not fully absorb hospital fixed effects. Also note that we could not have included a specification in Table 2 that included physician fixed effects as the fixed effects would have fully explained the outcome variable.

the time the patient arrives.[12] Unfortunately, we do not observe the calendar date a patient arrives. We do observe the hour, weekday, year, and quarter of admission. We therefore calculate the demographic composition of physicians at the hour/weekday/year/quarter level of granularity and match the resulting composition averages to patients' race and time of arrival. Essentially, we calculate the share of physicians *typically* present during the "shift" that the patient arrives who are the same race as the patient.[13] For instance, if we observe in the data that attending physicians in Orlando Hospital from 8pm-9pm on Tuesdays during the first quarter of 2012 are 80% white, then for a white patient arriving at that hospital during the specified day/hour/quarter the instrument is equal to 80% and for black patients the instrument is equal to 20%.[14] As we will document in the next section, this instrument has a very strong first stage.

5. Results

5.1 Main Results

Estimates for the causal effect of physician-patient race-match on inpatient mortality are presented in Table 3. Treatment effects are reported for the full sample of patients in Panel A and for the subsample of patients with a diagnosis that led to death for at least one patient in the sample in Panel B. We cannot observe an effect on mortality for patients diagnosed with a condition that never leads to death. Therefore, the restriction in Panel B simply focuses our analysis on patients whose likelihood of death feasibly could be affected by the patient-physician race-match treatment.

[12] This is especially likely to be true in the subset of patients to which we have restricted our analysis. If our sample included patients arriving for elective procedures, the patient may schedule their arrival around their preferred physician's schedule. For uninsured patients arriving through the emergency department, this is unlikely to be true.

[13] This does assume some regularity to physicians' shifts within a hospital. Given the rise of hospitalists as inpatient attending physicians in recent decades, this is a reasonable assumption to make. If our primary focus were race-match between patients and operating physicians, this assumption would be more problematic, as operating physicians do not work regular "shifts" within a given hospital, and arrive only to conduct their surgery.

[14] The denominator for the percent is just the pool of black and white physicians in the hospital.

In Table 3, we move from a specification with only patient and physician controls in Column 1 to one with the full set of identifying fixed effects in Column 5. Beginning in Panel A, the negative estimate in Column 1 with the minimal set of controls indicates that, on average, race-matched patients enjoy lower mortality rates relative to other patients with the same demographic characteristics and diagnoses. This estimate, however, ignores the possibility that patients at some hospitals may be both more likely to be matched to physicians of the same race and have a greater or lesser propensity to die. For example, hospitals in the sample may have different staffing levels, employ different types of specialists, and have different levels of resources. Hospital fixed effects in Column 2 control for this confounding factor; the negative effect persists. We include physician fixed effects in Column 3. These control for any time-invariant physician characteristics that are correlated with both physician race and the probability of inpatient death. This might include physician experience or physician training, as well as any propensity for taking on difficult cases or working shifts that experience higher mortality rates. Controlling for the physician, we observe that patients with the same diagnoses matched to physicians of the same race are still 0.12 percentage points less likely to die. We include both hospital and physician fixed effects in Column 4 given that some physicians practice in more than one hospital. Notably, the parameter of interest is unchanged from Column 3.

Finally, we add patient ZIP code fixed effects in Column 5. Our richest specification tells us that after factoring out mean differences in mortality rates between races, when we compare a black patient to a white patient who lives in the same neighborhood, has the same diagnosis, shares the same age and gender, and who is treated in the same hospital, in the same quarter, by the same physician, then the patient whose race matches that of their physician is 0.14 percentage points less likely to die. Given the baseline likelihood of mortality in the sample of 1.1%, this represents a sizeable 13% reduction.

The inclusion of patients with non-fatal diagnoses may attenuate our estimates for the effect of patient-physician race-match on mortality. This is because we are essentially including "never takers" in the analyses. Panel B excludes patients whose primary diagnosis does not lead to death for any patient in our sample. As expected, the estimated treatment effects are larger within this targeted sample. Our main estimate in Column 5 of Panel B tells us that patients with potentially life-threatening conditions whose race matches that of their quasi-randomly assigned physician experience mortality rates that are 0.26 percentage points lower than other patients. The mortality rate amongst this subsample is roughly 1.7%, so this estimate represents a 15% reduction in mortality.

To help put the magnitude of this coefficient in context, we compare our treatment effect to another in the literature. Card et al. (2009) find that gaining access to Medicare reduces severely ill patients' likelihood of mortality by 20%. While Card et al. (2009) consider a different patient population, our estimates are somewhat close to their estimated treatment effect; loosely speaking, the magnitude of race-match's impact is comparable to that of gaining insurance.

5.2 Robustness Checks

To verify our results are not a byproduct of a type I error, we conduct a placebo test where we re-estimate our main specification five hundred times, each time randomly assigning every patient a "placebo race" (black or white) under the constraint that the share of "placebo black" patients in the sample matches the actual share of black patients in our sample. We then construct a "placebo race-match" dummy that is equal to one if a patient's "placebo race" matches the race of his or her attending physician. We depict the distribution of the five hundred "placebo race-match" coefficients in a histogram in Figure 1. Notably, the distribution of coefficients is symmetrical around zero, suggesting that there is no inherent bias towards finding a negative effect on patient mortality. More importantly, we find that our true estimated effect of race-match is in the tail of the distribution of placebo estimates. Only three of the five hundred placebo estimates (0.6%) are more negative than our true estimated effect.

We now turn to our two-stage least-squares approach. Our main empirical strategy relies on conditional random assignment of patients to physicians. This motivated our initial restriction to patients admitted to hospital through emergency departments; there is very little scope for selective assignment of patients to physicians in this environment, particularly for patients with the same diagnoses. Nonetheless, we check the robustness of this assumption in Table 4 using an instrumental variables approach. As explained in the methodology section, our instrument for race-match is the share of same-race physicians typically present in the relevant hospital at the hour, weekday, quarter, and year that the patient arrives. The 2SLS estimates in Table 4 mirror those from Table 3. Although the negative effect in Panel A is less precisely estimated, it is of a very similar magnitude, while the estimate in Panel B confirms a statistically significant reduction in mortality when there is patient-physician race congruence.

5.3 Exploring the Mechanism

As discussed in the introduction, there are a variety of mechanisms through which patient-physician race-match might affect health. We can broadly group these into a communication/trust channel and a discrimination/bias channel. Patients and physicians could experience enhanced communication in race-matches if race-matches generate better understanding or mutual trust.

Alternatively, physicians may have implicit biases and treat other-race patients differently than same-race patients. For example, they might make false assumptions about other-race patients' underlying conditions or exert less effort. Table 5 probes these possibilities by looking for effect heterogeneity across physicians' race. If we observe that among white physicians' caseloads, black patients have worse outcomes than white patients, this could be consistent with white physicians discriminating against black patients. If we observe that among black physicians' caseloads, black patients have better outcomes than white patients, this could be consistent with race-match inducing more trust and better communication. These mechanisms are not, of course, mutually exclusive, and it is possible that both are at play.

In Table 5, we estimate models similar to our main specifications, except we replace the race-match treatment indicator with a dummy variable for whether the patient is black, a dummy variable for whether the physician is black, and the interaction of the two dummies. This allows us to test whether race-match has a differential effect across white-white versus black-black pairings, for example. We first estimate the model without physician fixed effects to identify the effect of physician race (Column 1), and then estimate the model with physician fixed effects, omitting the "black physician" dummy (Column 2). While our preferred specification in the main analyses includes physician fixed effects, the ability to identify the average effect of physician race allows us to probe intra-treatment effect heterogeneity. Importantly, recall from Table 3 that our results are similar with or without physician fixed effects.

Across all columns of Table 5, we find that the estimated race-match effect is driven by black patients being treated by black physicians. In Column 1, the omitted category is a white patient treated by a white physician. The insignificant "black patient" coefficient, therefore, suggests that there is no difference in mortality between a black patient treated by a white physician relative to a white patient treated by a white physician. The insignificant "black physician" coefficient suggests that there is also no difference in mortality between a white patient treated by a black physician and a white patient treated by a white physician. We do, however, observe a clear reduction in mortality when both the patient and the physician are black (the "Black Pat × Black Phys" coefficient). This is notable because our previous results did not clarify whether race-match decreases mortality in absolute terms, or whether race-mismatch *increases* mortality. These coefficients point towards a race-match for black patients reducing mortality in an absolute sense, with little or no evidence of harm from mismatch among black or white patients.

The remaining columns of Table 5 show that the conclusion drawn from Column 1 is robust to physician fixed effects. In Column 2, a coefficient on "black physician" cannot be identified, but the coefficient on the interaction term is similar to that in Column 1. Columns 3 and 4 take an alternative approach by stratifying by the race of the physician. A similar pattern emerges. Within white physicians' caseloads, black patients (who are mismatched) do no worse than white patients (Column 3). Within black physicians' caseloads, however, mortality is significantly lower among black patients relative to white patients (Column 4). Race-match effects are confined to black-black pairings.

Implicit bias is typically considered to operate through whites discriminating against blacks. The absence of race-match effects for white physicians suggests impacts do not operate through the discrimination/bias channel. The reduction in the likelihood of mortality for black patients when treated by black physicians, however, is consistent with race-match improving patient-physician communication or trust, the other broad channel we consider. This interpretation is broadly consistent with findings of the literature described in Sections 1 and 2, wherein black patients communicate more with black physicians and this leads to more information being relayed. Black physicians might also allay any distrust black patients have of the medical system, which could impact black patients' engagement with their physicians and, in turn, their mortality rate (Alsan & Wanamaker, 2018).

To further shed light on the mechanism driving our results, we assess the impacts of other types of matches between patients and physicians. It may be that race-match reduces social distance, which in turn improves communication and patient outcomes. If this narrative fully captures the mechanism driving our results, we may expect that other types of matches – such as on gender – have a similar effect. Alternatively, given the literature on race and trust in the medical profession, it may be that the story is more nuanced. If race-match improves communication by increasing the trust patients have in their physicians, then – without any clear evidence on differences in trust by matches on gender – we might expect the race-match effect to be unique.

In Table 6, we test whether there are similar "match" effects from having a same-gender physician or from having a same-race surgeon. From Column 1, Panel A, we see that gender similarity with an attending physician has no statistically significant effect on patient mortality. Panel B reveals, if anything, a marginally significant *increase* in mortality from a gender match. The lack of a beneficial gender effect aligns with findings from Cooper-Patrick et al. (1999); they find that communication between a physician and a patient is improved in race-matched pairings, but not in gender-matched pairings. These results highlight that mortality improvements from race-match may not be driven by generic reductions in social distance, but by some factor that specifically overlaps with race – trust being a prominent candidate. Column B of Table 6 further demonstrates that the race-match effect is not dampened after controlling for a gender-match. This is important to note because black physicians are disproportionately female relative to the share of white physicians who are female.

Also in Table 6, we report the impact of having a race-matched surgeon. Only some patients have a surgeon in addition to an attending physician; therefore, the Column 3 sample is noticeably smaller. Contrary to our findings on the impacts of having a race-matched attending physician, hospital patients undergoing surgery are not affected by having a race-matched surgeon. This makes sense. Physician-patient communication is likely most important when the best course of action is still being determined. The discussion about what to do typically occurs between patients and attending physicians rather than between patients and their surgeons, at which point decisions about procedures have often already been made. This finding therefore provides additional suggestive, though not conclusive, evidence that improved communication may be the mechanism driving the observed race-match effects.[15]

Next, we note that that some conditions, such as broken legs, have clear and straightforward solutions that do not require especially effective communication between patients and physicians. If the mechanism driving our results is effective communication between patients and physicians (or some related mechanism), then we should observe stronger treatment effects when we restrict our attention to diagnoses that require more discretion and decision-making. We therefore focus on patients whose primary diagnosis is associated with a high degree of treatment variation. Specifically, we focus on those with diagnoses that have (1) high variance in length of stay, (2) high variance in number of procedures, or (3) high variance in total charges. “High variance” is defined as above-median variance.[16] In three separate regressions, we condition the sample on (1), (2), and (3) respectively. Results are reported in Appendix Table A3. Indeed, effect sizes are generally larger,

[15] Another natural test along these lines is looking at whether the effect is evident for patients who are admitted to the emergency department in a semi-conscious or unconscious state and are therefore unable to communicate with the physician; unfortunately, given the small number of patients with these conditions, we do not have enough statistical power to probe this.

[16] To have above-median variance in total charges, the diagnosis must have been on the upper end of the charge-variance spectrum in over 50% of hospitals.

especially for the latter two "high variance" conditions. For instance, when focusing on diagnoses with high variance in total charges, the impact of race-match is roughly *twice* as large as in the main model.

So far, we have established a robust effect of patient-physician race-match on mortality, as well as provided suggestive evidence that this effect operates through a broad communication or trust channel rather than a discrimination channel. This prompts consideration of the ways in which improved communication between patients and physicians might lower mortality. We explore this in Tables 7 and 8 by looking at whether patient-physician race matches prompt changes in intermediate health inputs that are affected by the physician.

Table 7 reports impacts on pharmacy charges, charges for therapy (physical, occupational and/or speech), and total hospital charges. We anticipate each of these measures to be correlated with the intensity of in-hospital treatment. We observe no clear evidence of increases in the provision of medication, therapy, or aggregate services when patients are matched to a physician of the same race. Some of the estimates are positive, but most are statistically insignificant and small.

The effects of patient-physician race-match on an additional set of intermediate health inputs are reported in Table 8. Here we consider length of stay, acquisition of a condition or injury after admission, and the number of decimal places in the patient's primary ICD-9 diagnosis code, which is a proxy for granularity of the physician's diagnosis or the physician's attention to detail (Balsa & McGuire, 2001).[17] The results in Table 8 provide no compelling evidence that race-match leads to any changes in these intermediate health inputs.

Taken as a whole, the findings in Tables 7 and 8 suggest that patient-physician race-match does not translate into patients receiving increased health services. We interpret this as broadly

[17] For example, ICD-9 code 780.0 is "Alteration of consciousness" while an ICD-9 code with an extra decimal place 780.01 is "Coma", a more detailed diagnosis.

consistent with our hypothesis that the mortality reductions from patient-physician race-match are a result of better communication and trust. If patients with race-match were provided with *more* care, this could be interpreted as consistent with the discrimination channel as it would suggest physicians under-treat other-race patients. This set of results is presented with the caveat that there are many other health inputs that are affected by physician-patient match which we do not observe. Our analysis is limited to inputs that are measurable and available in the data.

6. Conclusion

There is a robust medical literature indicating that patients prefer physicians of the same race. There is also evidence that patients *select* physicians of the same race when given the choice. Patients whose physician is of the same race experience better communication and report better satisfaction with their clinical experiences. Yet, it is not clear whether they enjoy better health outcomes as a result. It is also not clear whether, if patients were randomly assigned to physicians, any effects from racial concordance would persist. This study set out to address both of these gaps.

The causal effect of racial concordance in patient-physician relationships is important from a public health standpoint. There is currently a shortage of minority physicians in the U.S. This makes minority patients statistically less likely to be exposed to a health care provider of the same race. If exposure to a same-raced physician independently affects clinical outcomes, then medical school admission boards and hospital administrators should be aware of this finding. Minority patients will be statistically disadvantaged by the under-representation of minority doctors.

We measure the causal effect of patient-physician race-match on patients' clinical outcomes by focusing on a subset of patients in the hospital setting who are assigned to an attending physician in a process that is effectively random. This allows us to compare the mortality rates of race-matched versus race-mismatched pairs. The results are striking. Patients quasi-randomly assigned to same-race attending physicians are 13% less likely to die while in the hospital. Among those who are admitted to the hospital with relatively severe conditions, the survival benefit grows to 15%. This is almost entirely driven by improvements in black patient mortality when matched with black physicians, with very little evidence that black or white patients experience negative impacts of being assigned to a mismatched physician.

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Tables

Table 1. Descriptive Statistics

	Estimation Sample (N=153,264)	*Sample Stratified by Physician-Patient Race-Match:* Match=1	Match=0
Physician Characteristics:			
Black	22.6%	12.6%	41.6%
White	77.4%	87.4%	58.4%
Mean Age	47 years	47 years	46 years
Female	26.8%	25.7%	28.8%
Patient Characteristics:			
Black	28.4%	12.6%	58.4%
White	71.6%	87.4%	41.6%
Mean Age	43 years	44 years	42 years
Female	41.3%	41.6%	40.7%
Died	1.1%	1.1%	1.1%
Same Race as Physician	65.5%	100%	0%

Table 2. Test for Random Assignment

	Black Physician			
Black Patient	0.0905**	0.0211***	0.0067*	0.0045
	(0.0365)	(0.0069)	(0.0036)	(0.0030)
Observations	153,264	153,264	153,264	153,264
R-squared	0.0095	0.1533	0.2472	0.2684
Diagnosis FE		Y		Y
Hospital FE			Y	Y
Patient ZIP FE		Y		Y

Notes. Estimates are obtained from linear regression models using patient-physician encounter-level data with varying fixed effects. The table reports the additional probability that a black patient relative to a white patient is treated by a black physician.

Heteroskedastic-robust standard errors clustered by hospital in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 3. Main Results

	Died				
A. Full Sample (Dependent Variable Mean = 0.0108)					
Patient Same Race as Physician	-0.0017** (0.0007)	-0.0011* (0.0006)	-0.0012* (0.0006)	-0.0012* (0.0006)	-0.0014** (0.0007)
Observations	153,264	153,264	153,264	153,264	153,264
R-squared	0.0762	0.0795	0.1594	0.1604	0.1769
B. Excluding Patients with Zero-Mortality Diagnoses (Dependent Variable Mean = 0.0177)					
Patient Same Race as Physician	-0.0030** (0.0012)	-0.0020** (0.0010)	-0.0020* (0.0010)	-0.0021** (0.0011)	-0.0026** (0.0011)
Observations	92,345	92,345	92,345	92,345	92,345
R-squared	0.0881	0.0937	0.1886	0.1903	0.2098
Year×Quarter FE	Y	Y	Y	Y	Y
Patient Age	Y	Y	Y	Y	Y
Patient Gender	Y	Y	Y	Y	Y
Patient Race	Y	Y	Y	Y	Y
Diagnosis FE	Y	Y	Y	Y	Y
Physician Birth Year	Y	Y			
Physician Gender	Y	Y			
Physician Race	Y	Y			
Physician FE			Y	Y	Y
Hospital FE		Y		Y	Y
Patient ZIP FE					Y

Notes. Estimates are obtained from linear regression models using patient-physician encounter-level data with varying fixed effects. The table reports the change in the probability of within-hospital mortality when the physician and patient are of the same race. Panel A includes all patient-physician observations, while Panel B only includes patients with diagnoses that that have led to death for at least one patient in the study period.

Heteroskedastic-robust standard errors clustered by hospital in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 4. Robustness Check via 2SLS

	FIRST-STAGE: Patient Same Race as Physician	SECOND-STAGE: Died
A. Full Sample		
Instrument	0.9578***	
	(0.0699)	
	[F-stat: 257.57]	
Patient Same Race as Physician		-0.0018
		(0.0013)
Observations	153,264	153,264
R-squared	0.5750	0.1769
B. Excluding Patients with Zero-Mortality Diagnoses		
Instrument	0.9602***	
	(0.0877)	
	[F-stat: 202.28]	
Patient Same Race as Physician		-0.0044**
		(0.0021)
Observations	92,345	92,345
R-squared	0.5837	0.2098
Year×Quarter FE	Y	Y
Patient Age	Y	Y
Patient Gender	Y	Y
Patient Race	Y	Y
Diagnosis FE	Y	Y
Physician FE	Y	Y
Hospital FE	Y	Y
Patient ZIP FE	Y	Y

Notes. Estimates are obtained from linear regression models using patient-physician encounter-level data with the full set of fixed effects. The table reports the first- and second-stage results from an instrumental variable approach to estimating the change in the probability of within-hospital mortality when the physician and patient are of the same race. Column 1 shows the first-stage relationship between a patient having a same-race attending physician and the instrument, the share of same-race physicians typically present in the relevant hospital at the hour, weekday, quarter, and year that the patient arrives. Column 2 shows the second-stage effect of race-match on mortality using the instrument.

Heteroskedastic-robust standard errors clustered by hospital in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 5. Effects for Particular Race Combinations

	Died			
	All Physicians Included		Conditional on White Physician	Conditional on Black Physician
A. Full Sample				
Black Patient	-0.0006	-0.0003	0.0001	-0.0041***
	(0.0007)	(0.0006)	(0.0007)	(0.0012)
Black Physician	-0.0012			
	(0.0009)			
Black Pat×Black Phys	-0.0023*	-0.0027**		
	(0.0013)	(0.0013)		
Observations	153,264	153,264	118,379	34,080
R-squared	0.0974	0.1769	0.1902	0.1535
B. Excluding Patients with Zero-Mortality Diagnoses				
Black Patient	-0.0010	-0.0005	0.0002	-0.0070***
	(0.0012)	(0.0011)	(0.0012)	(0.0020)
Black Physician	-0.0020			
	(0.0015)			
Black Pat×Black Phys	-0.0047**	-0.0052**		
	(0.0021)	(0.0022)		
Observations	92,345	92,345	71,374	20,510
R-squared	0.1151	0.2098	0.2270	0.1866
Year×Quarter FE	Y	Y	Y	Y
Patient Age	Y	Y	Y	Y
Patient Gender	Y	Y	Y	Y
Diagnosis FE	Y	Y	Y	Y
Physician Birth Year	Y			
Physician Gender	Y			
Physician Race	Y			
Physician FE		Y	Y	Y
Hospital FE	Y	Y	Y	Y
Patient ZIP FE	Y	Y	Y	Y

Notes. Estimates are obtained from linear regression models using patient-physician encounter-level data with the full set of fixed effects. The table reports the change in the probability of within-hospital mortality when the physician and patient are of the same race. Column 1 shows the effect when the sample is restricted to white physicians. Column 2 shows the effects when the sample is restricted to black physicians.

Heteroskedastic-robust standard errors clustered by hospital in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 6. Other Dimensions of "Match"

	Died		
A. Full Sample			
Patient Same Race as Physician		-0.0014** (0.0007)	
Patient Same Gender as Physician	0.0008 (0.0005)	0.0008 (0.0005)	
Patient Same Race as Surgeon			-0.0004 (0.0026)
Observations	153,220	153,220	44,113
R-squared	0.1769	0.1770	0.3220
B. Excluding Patients with Zero-Mortality Diagnoses			
Patient Same Race as Physician		-0.0026** (0.0011)	
Patient Same Gender as Physician	0.0016* (0.0008)	0.0016* (0.0008)	
Patient Same Race as Surgeon			0.0015 (0.0052)
Observations	92,322	92,322	23,931
R-squared	0.2098	0.2099	0.3698
Full Set of Controls[a]	Y	Y	Y

[a] Full set of controls displayed in final column of Table 3; surgeon fixed effects replace (attending) physician fixed effects in the final column above.
Heteroskedastic-robust standard errors clustered by hospital in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7. Effects on Potential Mechanisms

	Any Pharmacy Charge	Ln(Amount Pharmacy Charge)	Any Therapy Charge	Ln(Amount Therapy Charge)	Ln(Total Charge)
Dependent Variable Mean:	0.9923	$6,530	0.1815	$1,526	$40,320
A. Full Sample					
Patient Same	-0.0000	0.0100	0.0037*	-0.0036	0.0058
Race as Physician	(0.0007)	(0.0085)	(0.0021)	(0.0145)	(0.0043)
Observations	153,264	152,061	153,264	26,519	153,264
R-squared	0.2195	0.4843	0.3631	0.4469	0.4892
B. Excluding Patients with Zero-Mortality Diagnoses					
Patient Same	0.0002	0.0196**	0.0053*	-0.0041	0.0075
Race as Physician	(0.0004)	(0.0096)	(0.0030)	(0.0241)	(0.0052)
Observations	92,345	92,077	92,345	15,588	92,345
R-squared	0.2083	0.4378	0.3244	0.4787	0.4693
Year×Quarter FE	Y	Y	Y	Y	Y
Patient Age	Y	Y	Y	Y	Y
Patient Gender	Y	Y	Y	Y	Y
Patient Race	Y	Y	Y	Y	Y
Diagnosis FE	Y	Y	Y	Y	Y
Physician FE	Y	Y	Y	Y	Y
Hospital FE	Y	Y	Y	Y	Y
Patient ZIP FE	Y	Y	Y	Y	Y

Notes. Estimates are obtained from linear regression models using patient-physician encounter-level data with the full set of fixed effects. The table reports the change in a set of intermediate health inputs when the physician and patient are of the same race. The dependent variable mean is calculated based on the full sample.

Heteroskedastic-robust standard errors clustered by hospital in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 8. Effects on Other Potential Mechanisms

	Stayed Overnight	Ln(Nights in Hospital)	Acquired Condition Post-Admission	Acquired Injury Post-Admission	ICD-9 Code Decimal-Place Count
Dependent Variable Mean:	0.9482	4 nights (if >0)	0.1301	0.0230	1.5
A. Full Sample					
Patient Same Race as Physician	0.0009 (0.0015)	0.0096* (0.0054)	0.0006 (0.0026)	0.0007 (0.0012)	-0.0018 (0.0014)
Observations	153,264	145,117	153,264	153,264	153,264
R-squared	0.0974	0.2580	0.1449	0.0852	0.8598
B. Excluding Patients with Zero-Mortality Diagnoses					
Patient Same Race as Physician	-0.0008 (0.0019)	0.0072 (0.0064)	0.0017 (0.0032)	-0.0001 (0.0015)	-0.0034** (0.0015)
Observations	92,345	87,700	92,345	92,345	92,345
R-squared	0.1061	0.2708	0.1567	0.0967	0.8767
Year×Quarter FE	Y	Y	Y	Y	Y
Patient Age	Y	Y	Y	Y	Y
Patient Gender	Y	Y	Y	Y	Y
Patient Race	Y	Y	Y	Y	Y
Diagnosis FE	Y	Y	Y	Y	Y
Physician FE	Y	Y	Y	Y	Y
Hospital FE	Y	Y	Y	Y	Y
Patient ZIP FE	Y	Y	Y	Y	Y

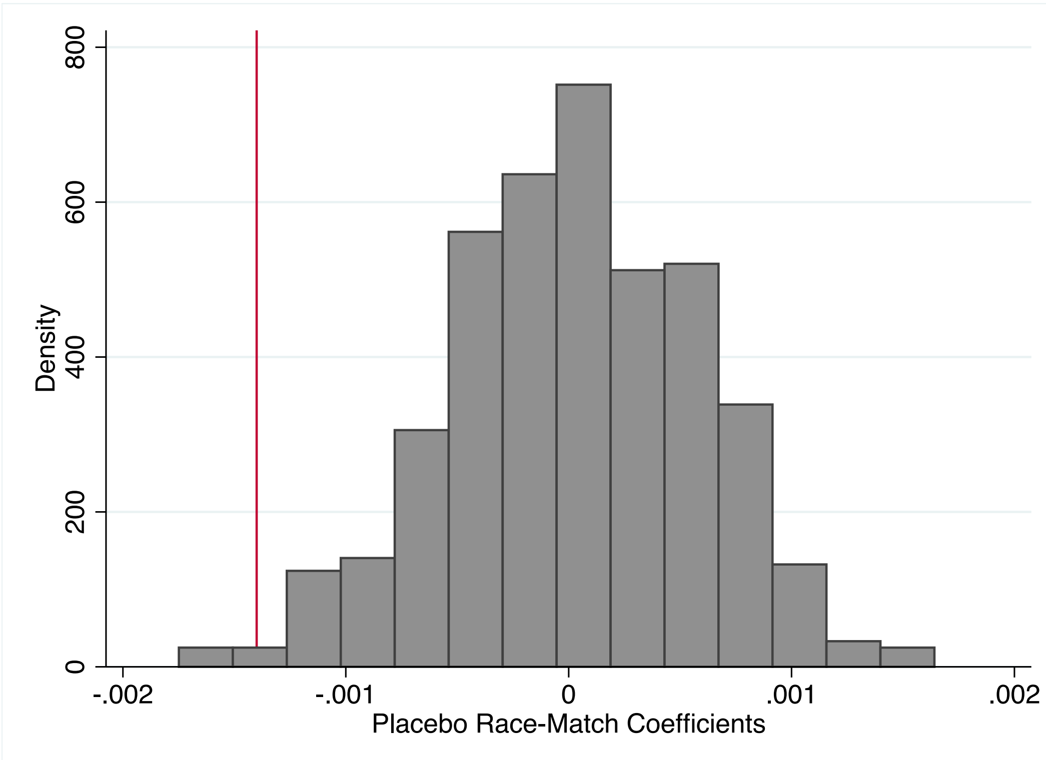
Notes. Estimates are obtained from linear regression models using patient-physician encounter-level data with the full set of fixed effects. The table reports the change in a set of intermediate health inputs when the physician and patient are of the same race.

Heteroskedastic-robust standard errors clustered by hospital in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Figures

Figure 1. Distribution of 500 Placebo Race-Match Estimates Relative to Main Estimate



Notes. The gray bars are a histogram depicting the distribution of five hundred "placebo race-match" coefficients, which result from randomly assigning patient race, reconstructing the "race-match" indicator, and re-estimating our main specification five hundred times. The vertical red line represents our main estimate (Table 3, Panel A, Column 5) for the sake of comparison.

APPENDIX: Additional tables

Table A1. Frequencies of Race Combinations

Patient:	*Physician:* Black	*Physician:* White
Black	12,651 (8.3%)	30,858 (20.1%)
White	21,982 (14.3%)	87,773 (57.3%)

Table A2. Ten Most Frequent Primary Diagnoses

	Share of Sample
A. Full Sample	
Other chest pain (786.59)	2.46%
Acute pancreatitis (577.0)	2.34%
Pneumonia, organism unspecified (486)	1.89%
Cellulitis and abscess of leg, except foot (682.6)	1.56%
Unspecified septicemia (038.9)	1.55%
Obstructive chronic bronchitis with (acute) exacerbation (491.21)	1.39%
Depressive disorder, not elsewhere classified (311)	1.39%
Alcohol withdrawal (291.81)	1.34%
Diabetes with ketoacidosis, type I [juvenile type], uncontrolled (250.13)	1.31%
Acute appendicitis without mention of peritonitis (540.9)	1.26%
	(N=153,264)
B. Excluding Patients with Zero-Mortality Diagnoses	
Other chest pain (786.59)	4.05%
Acute pancreatitis (577.0)	3.86%
Pneumonia, organism unspecified (486)	3.11%
Cellulitis and abscess of leg, except foot (682.6)	2.58%
Unspecified septicemia (038.9)	2.54%
Obstructive chronic bronchitis with (acute) exacerbation (491.21)	2.30%
Alcohol withdrawal (291.81)	2.21%
Diabetes with ketoacidosis, type I [juvenile type], uncontrolled (250.13)	2.17%
Cellulitis and abscess of upper arm and forearm (682.3)	2.06%
Acute kidney failure, unspecified (584.9)	1.90%
	(N=92,345)

Notes. ICD-9 diagnosis code in parentheses.

Table A3. Effects Among Conditions with High Treatment Variability

	Died		
	Among Conditions with High Variance in Length of Stay	Among Conditions with High Variance in Number of Procedures	Among Conditions with High Variance in Total Charges in Most Hospitals
A. Full Sample			
Patient Same Race as Physician	-0.0018	-0.0022*	-0.0033**
	(0.0013)	(0.0013)	(0.0014)
Observations	78,619	76,422	62,256
R-squared	0.2096	0.2039	0.2226
B. Excluding Patients with Zero-Mortality Diagnoses			
Patient Same Race as Physician	-0.0030	-0.0034*	-0.0041**
	(0.0019)	(0.0018)	(0.0017)
Observations	55,107	56,205	51,157
R-squared	0.2302	0.2315	0.2356
Year×Quarter FE	Y	Y	Y
Patient Age	Y	Y	Y
Patient Gender	Y	Y	Y
Patient Race	Y	Y	Y
Diagnosis FE	Y	Y	Y
Physician FE	Y	Y	Y
Hospital FE	Y	Y	Y
Patient ZIP FE	Y	Y	Y

Notes. In Columns 1 and 2, a condition is defined as "High Variance" if the variance of the length of stay or number of procedures for patients diagnosed with that condition is above the median variance of the length of stay or number of procedures across all conditions. In Column 3, a condition is defined as "High Variance in Total Charges in Most Hospitals" if, in more than 50% percent of hospitals, the variance of the total charges for patients diagnosed with that condition is above the within-hospital median variance of total charges across all conditions. Medians are calculated by weighting each encounter equally, so conditions that have more encounters (are more common) carry more weight.

Heteroskedastic-robust standard errors clustered by hospital in parentheses

*** p<0.01, ** p<0.05, * p<0.1