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DOES DIVERSITY MATTER FOR HEALTH?

EXPERIMENTAL EVIDENCE FROM OAKLAND

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ABSTRACT

We study the effect of diversity in the physician workforce on the demand for preventive care among African-American men. Black men have the lowest life expectancy of any major demographic group in the U.S., and much of the disadvantage is due to chronic diseases, which are amenable to primary and secondary prevention. In a field experiment in Oakland, California, we randomize black men to black or non-black male medical doctors and to incentives for one of the five offered preventives - the flu vaccine. We use a two-stage design, measuring decisions about cardiovascular screening and the flu vaccine before (ex ante) and after (ex post) meeting their assigned doctor. Black men select a similar number of preventives in the ex ante stage, but are much more likely to select every preventive service, particularly invasive services, once meeting with a doctor who is the same race. The effects are most pronounced for men who have little experience obtaining routine medical care and among those who mistrust the medical system. Subjects are more likely to talk with a black doctor about their health problems and black doctors are more likely to write additional notes about the subjects. The results are most consistent with better patient-doctor communication during the encounter rather than discrimination or measures of doctor quality and effort. Our findings suggest black doctors could help reduce cardiovascular mortality by 16 deaths per 100,000 per year - leading to a 19% reduction in the black-white male gap in cardiovascular mortality.

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A data appendix is available at <http://www.nber.org/data-appendix/w24787>

A randomized controlled trials registry entry is available at <https://www.socialscienceregistry.org/trials/2497>

Does Diversity Matter for Health? Experimental Evidence from Oakland*

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September 2018

Abstract

We study the effect of diversity in the physician workforce on the demand for preventive care among African-American men. Black men have the lowest life expectancy of any major demographic group in the U.S., and much of the disadvantage is due to chronic diseases, which are amenable to primary and secondary prevention. In a field experiment in Oakland, California, we randomize black men to black or non-black male medical doctors and to incentives for one of the five offered preventives — the flu vaccine. We use a two-stage design, measuring decisions about cardiovascular screening and the flu vaccine before (ex ante) and after (ex post) meeting their assigned doctor. Black men select a similar number of preventives in the ex ante stage, but are much more likely to select every preventive service, particularly invasive services, once meeting with a doctor who is the same race. The effects are most pronounced for men who have little experience obtaining routine medical care and among those who mistrust the medical system. Subjects are more likely to talk with a black doctor about their health problems and black doctors are more likely to write additional notes about the subjects. The results are most consistent with better patient-doctor communication during the encounter rather than discrimination or measures of doctor quality and effort. Our findings suggest black doctors could help reduce cardiovascular mortality by 16 deaths per 100,000 per year — leading to a 19% reduction in the black-white male gap in cardiovascular mortality.

JEL CLASSIFICATION CODES: I12, I14, C93

KEYWORDS: Homophily, concordance, communication, behavioral misperceptions

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I. Introduction

African-American men have the lowest life expectancy of any major demographic group in the United States (Arias, Heron, and Xu 2017) and live on average 4.5 fewer years than non-Hispanic white men (Murphy et al. 2017). Reasons for this disparity are multifactorial and include lack of health insurance, lower socioeconomic status, and structural racism (IOM 2003). Approximately 60% of the difference in life expectancy between black and white men is attributable to chronic diseases, which are amenable to primary or secondary prevention (Harper, Rushani, and Kaufman 2012; Silber et al. 2014). Some examples are poorly controlled hypertension (associated with stroke and myocardial infarction), diabetes (associated with end organ disease including kidney failure), and delayed diagnosis of cancers. These data suggest at least part of the mortality disparity is related to underutilized preventive healthcare services.

One frequently discussed policy prescription put forth by the Institute of Medicine (IOM) as well as the National Medical Association (NMA), the Association of American Medical Colleges (AAMC), and the American Medical Association (AMA) to address racial health disparities is to diversify the healthcare profession by increasing the number of under-represented minorities.¹ Blacks comprise approximately 13% of the U.S. population but only 4% of physicians and less than 7% of recent medical school graduates (AAMC 2014, AAMC 2016). Evidence on whether patient and physician racial concordance improves satisfaction and health outcomes is mixed, perhaps due to methodological differences. Meghani et al. (2009) perform a meta-analysis of thirty observational studies in public health and medicine concerning four racial and ethnic groups. They conclude that the evidence in favor of patient-doctor concordance in medical care is inconclusive and recommend additional research.² We advance this literature by providing experimental evidence on whether and to what extent diversity in the physician workforce improves medical decisions and outcomes among minority populations.

Our study builds upon several findings in economics. First, randomized trials in development economics have demonstrated puzzlingly low demand for high return preventive healthcare services among low-income populations (for a review see Dupas 2011; Banerjee and Duflo 2011, Chapter 3). Similar patterns are found in the U.S. — compared to non-Hispanic white men, African-American men are six percentage points less likely to visit the doctor and eight percentage points less likely to report receipt of the flu shot; insurance and education do not fully explain these gaps.³

Many factors likely contribute to this puzzle including lack of information, inadequate or low quality healthcare supply, and misperceptions about the etiology of disease. Given the prominent history of neglect and exploitation of disadvantaged populations by health authorities, mistrust

¹See “Unequal Treatment: Confronting Racial and Ethnic Disparities in Health Care” (IOM 2003); “Addressing Racial Disparities in Health Care: A Targeted Action Plan for Academic Medical Centers” (AAMC 2009); “Major Minority Physician Associations Come Together” (NMA 2018); and “Reducing Disparities in Health Care” (AMA 2018).

²We summarize the public health literature on concordance in the Appendix. A recent study by Hill, Jones, and Woodworth (2018) reports that black doctors greatly reduce the likelihood that African-Americans who are admitted to Florida hospitals die during their hospitalization.

³Authors’ own calculations using the National Health Interview Survey (Blewett et al. 2018a).

of the medical establishment is sometimes invoked as a contributing factor. Evidence consistent with historical abuse dampening demand and increasing mistrust has been found specifically among African-American men in the immediate aftermath of the U.S. Public Health Service syphilis experiment in Tuskegee, Alabama, (Alsan and Wanamaker 2018) and persisting decades after colonial medical campaigns in Central Africa (Lowe and Montero 2018). Recent studies in public health demonstrate that African-American men continue to score higher on medical mistrust measures than other groups (Kinlock et al. 2017, Nanna et al. 2018, Hammond et al. 2010).

Second, contributions in cultural economics have highlighted how norms of behavior are influenced by social identity (Akerlof and Kranton 2000; Benjamin, Choi, and Strickland 2010). Most notably, Tabellini (2008) shows how cooperation can be sustained in a one-shot prisoner’s dilemma among agents who perceive a non-economic benefit from cooperating with those closer in social distance. Third, natural experiments in labor and education have underscored how diversity, or lack thereof, may be particularly relevant in asymmetrical power relationships. For instance, Glover, Pallais, and Pariente (2017) find that minority workers exert less on the job effort in grocery stores with biased majority managers.⁴ A spate of studies has found that same race or same gender teachers are positively correlated with grades and career path, potentially through a role model effect.⁵

There are several ways in which racial diversity could play a role in medicine, specifically as it relates to the patient-doctor relationship. Taste-based discrimination (Becker 1957) on the part of the patient or doctor could imply that individuals are averse to interacting with those who do not share their racial background. On the other hand, internalized racism, or negative beliefs about one’s racial group, could lead to the opposite phenomenon. Third, a common racial background might facilitate communication — a critical component of clinical care as both patient and physician have potentially life-saving information to exchange. Fourth, and not mutually exclusive, concordance may foster trust leading to cooperation (i.e. compliance with doctors’ advice or willingness to engage). As noted by Arrow (1963), “...it is a commonplace that the physician-patient relation affects the quality of the medical care product.”

In this study, we examine whether doctor race affects the demand for preventive care among African-American men. We induce exogenous variation by randomly assigning subjects to black and non-black (white or Asian) doctors. Our experiment was conducted in Oakland, California, where we recruited over 1,300 black men from about 20 local barbershops and two flea markets. At these recruitment sites, subjects filled out baseline questionnaires and received a coupon for a free health screening. To facilitate our experiment, we set up a clinic to provide preventive services to the subjects. The clinic was staffed with fourteen black and non-black male doctors from the Bay Area as well as a diverse team of receptionists. Doctors and staff were told the study was designed to improve the take-up of preventive care among black men in Oakland, but not specifically informed

⁴For more from industry, see: Stoll, Raphael, and Holzer (2004); Giuliano, Levine, and Leonard (2009); Hjort (2014); and Bertrand et al. (2018).

⁵See: Ehrenberg, Goldhaber, and Brewer (1995); Dee (2004); Dee (2005); Bettinger and Long (2005); Carrell, Page, and West (2010); Fairlee, Hoffmann, and Oreopoulos (2014); and Lusher, Campbell, and Carrell (2018).

about the role of doctor race. Subjects learned of their (randomly) assigned doctor via tablet in the privacy of their own patient room.

The experiment proceeded in two stages and cross-randomized doctor race with incentives for the flu vaccine at the individual level. In the first (ex ante) stage, patients were introduced to their doctor via the tablet by way of text and photo, both standardized as described in Section III below. Subjects were then provided the opportunity to select which, if any, of the four advertised cardiovascular screening services they would like to receive. These services included body mass index (BMI) measurement, blood pressure measurement, diabetes screening, and cholesterol screening. The last two tests required a blood sample, and subjects were made aware of this feature. After making their selections for cardiovascular screening, subjects were informed they could also elect to receive a flu shot, administered by their assigned doctor. For subjects randomized to receive a flu incentive to encourage vaccine selection, the incentive amount was also listed. We conjectured that if subjects disliked doctors who did not share their racial background, those randomly assigned to non-black doctors would, on average, demand fewer preventives simply based on the tablet photo.

In the second stage, subjects met their assigned doctor in person. We refer to this stage throughout the paper as ex post (since decisions occur *after* interacting with their doctor). Subjects could revise their choice of preventives during this stage, after which the doctor administered the selected services. We therefore measure how black vs. non-black doctors change demand between the ex ante and ex post stages. Following the patient-doctor interaction, subjects filled out feedback forms and exited the clinic.

It is important to note that the study provided only *preventive* (i.e. care recommended during a state of relatively good health to avoid future illness) as opposed to *curative* (i.e. care needed during a state of illness to restore health) interventions.⁶ Individuals often have imperfect knowledge regarding the health benefits of prevention, perhaps because they have been misinformed, never informed, or informed by someone they don't trust, which can dampen demand.⁷ Hence the role of study doctors was limited to information provision on the benefits of receiving care even when not feeling sick and then providing those chosen.

Approximately half of the subjects we recruited from the community visited our clinic, and those who presented were negatively selected. Subjects who redeemed the clinic coupon were 13 percentage points more likely to be unemployed (off of a baseline level of 18%) and 19 percentage points more likely to have a high school education or less (off of a baseline level of 44%). In terms of health and healthcare utilization, they had significantly lower self-reported health, were less likely to have a primary care doctor, and more likely to have visited the emergency room.

Once at the clinic, subjects randomly assigned to a black doctor elected to receive the same number of preventive services as those assigned to a non-black doctor in the ex ante stage. In sharp contrast, we find that subjects assigned to black doctors, upon interacting with their doctor, are 18 percentage points more likely to take up preventives relative to those assigned to non-black doctors.

⁶We use the term preventives to refer to screenings and immunizations.

⁷According to the CDC, up to 40% of annual deaths in the U.S. are deemed preventable (CDC 2014).

These findings are robust to corrections for multiple hypothesis testing; the inclusion of fixed effects for clinic date, field staff, and recruitment location; as well as various permutations of the study doctors, including dropping the “best” black and “worst” non-black doctor.

Why would black male subjects randomly assigned to black male doctors elect to receive more services upon interacting with them? We provide several pieces of evidence that better communication between black subjects and black doctors explains our results, and discuss alternative mechanisms below. First, in our controlled study environment, the role of the doctor was circumscribed to informing subjects about the benefits of preventive services, and then providing those chosen. Second, we find that subjects were 10 percentage points (29%) more likely to talk with black male doctors about other health problems. Black doctors were 11 percentage points (35%) more likely to write notes about black patients than non-black doctors. Third, for non-invasive tests (those that do not require blood or an injection), both non-black and black doctors shifted out demand in the ex post stage relative to the ex ante stage, though the effect was larger for the latter. Yet, for invasive tests, those that carry more risk and thus likely require more trust in the person providing the service, only subjects assigned to black doctors responded: increasing their take-up of diabetes and cholesterol screenings by 20 and 26 percentage points (47% and 72%), respectively.

The experimental findings highlighting improved communication for black male patients paired with black male doctors are consistent with those collected in a non-experimental manner. We surveyed 1,490 black and white adult males who matched our sample in terms of educational attainment. The respondents were asked to select a doctor of a particular race based on accessibility, quality, and communication. With respect to quality (i.e. which doctor is the most qualified) black and white respondents both selected doctors of the same race about 50% of the time. However, for questions regarding communication, in particular which doctor would understand your concerns, the proportion of respondents choosing doctors of their own racial background jumped to nearly 65% for blacks and 70% for whites.⁸

An alternative interpretation of our results is that the estimated treatment effect is picking up an attribute correlated with the race of the doctor in our sample and which affects the outcome of interest. Importantly, doctors were balanced on observables in age, experience, and medical school rank; however, a prominent candidate for a hard-to-measure characteristic that may correlate with doctor race is quality.⁹ The non-experimental findings cited above demonstrate black male respondents believe that non-black doctors are as qualified as black doctors. Yet, actual doctor quality within the context of our study could vary. If black doctors were higher quality than non-black doctors we would have expected them to be rated higher on the feedback forms, yet black and non-black doctors were rated equally (highly). This compression likely reflects the design. Differences in quality that would stem from diagnostic or treatment skills were not elicited in our study, which narrowly focused on encouraging the take-up of preventives. Furthermore if black

⁸These patterns are also found in nationally representative non-experimental data, see Appendix Table 11.

⁹This could arise if, for example, black doctors are more qualified than non-black doctors in the population and we failed to draw our sample from an area of overlapping support — or if the distributions were similar, but we drew from different tails.

doctors were higher quality, they should perform better with *all* patients. Although our recruitment efforts were focused on African-American men, 12 clients identified as from another racial or ethnic background.¹⁰ Among this out-of-sample group, individuals were 14 percentage points *less* likely to choose services in the ex post stage when randomized to black doctors (a finding that is more extreme than 93% of bootstrap coefficients on draws of 12 in-sample subjects). Thus, in order for an attribute correlated with the race of black doctors to be driving our results, it must manifest *only* when treating African-American male patients.

This leads to another competing explanation, perhaps black male doctors exerted more effort with patients who shared their racial background. Since communication requires some amount of effort, this is not an interpretation to which we object (though we note if communication is more natural within concordant pairs, black doctors might be expending less effort to achieve the same or better results — i.e. communication may be more efficient). Time spent with patients has been used as a proxy for provider effort (Das et al. 2016). Equating time spent with effort is problematic in our setting because it reflects many different factors. A longer time spent could simply reflect the treatment effect (i.e. subjects elect to receive more services from black doctors), low quality (i.e. difficulty performing the services), or communication (i.e. a better patient-doctor connection facilitating credible information exchange). We find that black doctors indeed spent more time with subjects, but this finding is driven by the treatment effect — the difference in visit lengths is small and statistically insignificant after adjusting for the selected services. If we examine another potential proxy for effort, the allocation of services to the “highest need” subjects, we fail to find evidence that doctors of either race were expending effort to target interventions. Lack of targeting also reflects our instruction to the study doctors to try and encourage all patients to take up preventives.

Lastly, we do not find evidence for the controversial hypothesis that subjects were prejudiced against non-black doctors. First, there was no race-preference elicited in the ex ante (tablet) stage. Second, the comments and ratings on feedback forms were consistently positive for both sets of doctors. As for non-black doctors discriminating against black male patients, this also appears unlikely. All doctors who were involved in the study knew the goal was to improve the preventive care of black men (though were blind to the notion that their race was being randomized, thus we could not perform implicit association tests). Taste-based discrimination by doctors would again be inconsistent with non-black doctors being rated as highly as black doctors.¹¹

Racial concordance between subjects and doctors appears to be a particular component of social distance that is influential in affecting demand. We fail to find evidence that alternative concordance measures, such as whether subjects and assigned doctors share approximately the same age or educational attainment, predict healthcare demand in any meaningful way. Nor does race interact with these other concordance measures. Such findings should be interpreted with caution since these characteristics were not randomized.

¹⁰To avoid conflict, we provided services for the handful of people from other backgrounds who were consented in to the study but deleted them from the main analytical sample. See Figure 1.

¹¹Chandra and Staiger (2010) also fail to find evidence of prejudice in heart attack treatment.

Similar to prior scholarship on incentives for preventives among low-income communities, (Banerjee et al. 2010; Cohen and Dupas 2010; Cohen, Dupas, and Schaner 2015; Thornton 2008) we find that financial incentives for the flu shot increased demand for the vaccine: by 19 percentage points for a \$5 incentive and 30 percentage points for a \$10 incentive in the ex ante stage. Yet not all those who selected an incentivized flu shot actually received it: about 18% of subjects randomized to black doctors and 26% randomized to non-black doctors declined the shot in the ex post stage (many cited contraindications). And regardless of incentive level, black doctors increased demand in the ex post stage — convincing about 26% of subjects who initially turned down an incentive and refused a flu shot to obtain it, suggesting subsidies and (meeting with) black doctors are not perfect substitutes.

In the setting of imperfect information regarding the benefit of healthcare, demand curves cease to be a sufficient statistic for welfare calculations (Pauly and Blavin 2008; Baicker, Mullainathan, and Schwartzstein 2015). Furthermore, we incentivized take-up for only one preventive yet demand for every preventive was affected by a black doctor treatment. Thus, to make progress on valuation, we combine published estimates on the health value of interventions offered in our clinic with results from our study. The health value estimates come from cost-effectiveness simulations in which the screen-positive population obtains and complies with guideline-recommended therapy. Using this approach, we calculate that black doctors would reduce mortality from cardiovascular disease by 16 deaths per 100,000 per year, accounting for 19% of the black-white gap in cardiovascular mortality (Kahn et al. 2010; Dehmer et al. 2017; Murphy et al. 2017; and Harper, Rushani, and Kaufman 2012). If these effects extrapolate to other leading causes of death amenable to primary or secondary prevention, such as HIV/AIDS or cancer, the gains would be even larger.

These calculations presume that there is a supply of black male doctors who could screen and treat black male patients. This might not be a safe assumption. Black males are especially under-represented in the physician workforce, comprising about 12% of the U.S. male population but only 3% of male doctors.¹² According to a recent report by the AAMC (2015), the number of black male medical students has been roughly constant since 1978 (when 542 matriculated into medical school compared to 515 in 2014). Returning to the non-experimental results, black male respondents were 26 percentage points less likely than white respondents to state that a doctor who matched their race and gender was available to them. Moreover, in many healthcare settings and insurance networks, patients have limited choice over their doctors.

The remainder of the paper proceeds as follows. Section II develops a simple framework for interpreting the results of the experiment. Section III describes the experimental design. Section IV describes the data, empirical approach, and the characteristics of study subjects. Section V presents the main findings and Section VI explores potential mechanisms and validity concerns. Section VII discusses health benefits and Section VIII concludes.

¹²Black females represent 13% of the U.S. population and 7% of the female physician workforce. Physician workforce figures are from AAMC (2014); population figures are from 2013 Census Bureau Population Estimates.

II. Framework

We develop a straightforward model that formalizes the hypotheses tested in the experiment and facilitates interpretation of the results. Recall that the experiment consists of two stages, the ex ante stage where subjects are introduced to their randomly assigned doctor via photo and text on a tablet and select preventives, and the ex post stage whereby the subject and the doctor interact and then subjects re-optimize based on the encounter. For ease of exposition, we use white instead of non-black and refer to subjects as patients in this section.

A. Ex Ante Stage (Period 0)

We incorporate insights from Pauly and Blavin (2008) and Baicker, Mullainathan, and Schwartzstein (2015), assuming patients have inaccurate beliefs about the value of preventive health benefits b discounting them by $\beta \sim U[0, 1]$. This assumption mirrors what we observed in the field with many patients expressing false beliefs or present-bias.¹³ For example, some thought flu shots caused sickness, or that other non-proven remedies could ward off the flu. Several said that they would get the shot later. One patient made a possible reference to the syphilis experiment in Tuskegee stating he did not want the flu shot out of “fear of being experimented on.” Another had been diagnosed with diabetes in the past but “refused to believe it.”

We incorporate race into the take-up decision as a non-negative psychic cost d associated with the assignment of doctor j from race group $\{black, white\}$, as $r_{j=b}$ and $r_{j=w}$, respectively (Becker 1957). This cost is additive to other utility costs c where $c + d \leq b$.¹⁴ The utility to taking up a preventive is therefore:

$$U_i^0 = \beta_i \cdot b - c - d_{r_j}. \quad (1)$$

Patients only choose preventives if the perceived benefits outweigh the costs. Since the experiment randomized subjects across arms, β_i should be similar on average across those who receive a black vs. white doctor. We consider three cases: $d > 0$ if $r_{j=w}$, $d > 0$ if $r_{j=b}$, and $d = 0 \forall r_j$ or $d > 0 \forall r_j$. $d = 0$ and $\beta = 1$ is the first best; patients only use services if the benefits outweigh the non-doctor race related costs.

Case 1: $d > 0$ if $r_{j=w}$ and $d = 0$ otherwise: If black male patients have an aversion for white doctors, then the fraction of black subjects that demand preventives in the ex ante stage will be strictly greater for those randomized to black versus white doctors (i.e. $\Pr(\beta_i > \frac{c+d_{r_{j=w}}}{b} | r_{j=w}) = 1 - \frac{(c+d_{r_{j=w}})}{b} < 1 - \frac{c}{b} = \Pr(\beta_i > \frac{c}{b} | r_{j=b})$).

Case 2: $d > 0$ if $r_{j=b}$ and $d = 0$ otherwise: In contrast, if internalized racism leads black men to discriminate against doctors of their own race then $\Pr(\beta_i > \frac{c}{b} | r_{j=w}) > \Pr(\beta_i > \frac{c+d_{r_{j=b}}}{b} | r_{j=b})$.

¹³Or perhaps they lack perfect foresight in predicting the risks of chronic disease/influenza infection — see Gabaix and Laibson (2017).

¹⁴For a review of discrimination models and empirical literature, see Charles and Guryan (2011). In our setting, it is reasonable to characterize tablet choices as revealing generic race-based aversion since the patient and doctor are not interacting.

Case 3: $d = 0 \forall r_j$ or $d > 0 \forall r_j$. Finally, in the absence of aversion to doctors based on their race, or if patients have the same level of aversion to doctors regardless of their race, then $\Pr(\beta_i > \frac{c+d}{b} | r_{j=w}) = \Pr(\beta_i > \frac{c+d}{b} | r_{j=b})$ or $\Pr(\beta_i > \frac{c}{b} | r_{j=w}) = \Pr(\beta_i > \frac{c}{b} | r_{j=b})$. This implies that the fraction of patients who demand preventives will be equal across the two groups, though it will be higher in the absence than in the presence of aversion.

B. Ex Post Stage (Period 1)

In the ex post stage, patients interact with doctors and have an opportunity to revise their choices on preventives before receiving them. In particular, doctors can provide information that allows the patient to correct his false belief. Consistent with a behavioral framework, we do not assume patients are Bayesian. Rather, we model this correction as an additive term in the utility function, ϵ_i , and note that patients are completely disabused of false beliefs when $\beta_i b + \epsilon_i^* = b \iff \epsilon_i^* = (1 - \beta_i)b$.¹⁵ Consider all doctors provide information ϵ_i^* but whether that information is considered credible or comprehensible may depend on social distance, Δr_{ji} , which reflects the difference between the race of assigned doctor j and race of patient i (i.e. $|r_j - r_i|$), with $r_{j=b} = r_{i=b} = 1$ and $r_{j=w} = 0$.¹⁶ Ex post utility is therefore given by:

$$U_i^1 = \beta_i \cdot b - c + (1 - \delta \mathbb{1}^{\Delta r_{ij}}) \epsilon_i^* - d_{r_j}. \quad (2)$$

where $\delta \in [0, 1]$ captures the discounting of information received from a socially distant, less trusted, source. We again consider three cases, focusing on $d_{r_j} = 0$ and discussing other cost possibilities below.

Case 1: $\mathbb{1} = \begin{cases} 1 & \text{if } \Delta r_{ji} = 1 \\ 0 & \text{if } \Delta r_{ji} = 0 \end{cases}$ and $\delta \in (0, 1)$. If patients self-identify as black, then minimizing social distance by pairing such patients with black doctors dominates pairing such patients with white doctors, $\mathbb{E}[U^1 | r_{j=w}] = b - c - \frac{\delta b}{2} < b - c = \mathbb{E}[U^1 | r_{j=b}]$.¹⁷

Case 2: $\mathbb{1} = \begin{cases} 0 & \text{if } \Delta r_{ji} = 1 \\ 1 & \text{if } \Delta r_{ji} = 0 \end{cases}$ and $\delta \in (0, 1)$. In contrast, if white doctors are viewed as more credible sources of information than black doctors then $\mathbb{E}[U^1 | r_{j=w}] > \mathbb{E}[U^1 | r_{j=b}]$.

Case 3: $\delta = 0$ or $\delta = 1$ for all r_j . Finally, there will be no difference in demand for preventives across treatment arms of doctor race in the ex post stage if there is either no discounting of information by social distance, so that the first best is achieved no matter which doctor race is assigned, or the information from either source (black or white) is discounted fully.

¹⁵If $\beta_i = 1$, then the patient perceives the benefit of preventives accurately prior to interacting with a doctor.

¹⁶This assumption is supported by qualitative findings: an equal number of subjects from the treatment and control group stated their doctor provided useful information in their feedback forms. For a continuous social distance formulation, see Tabellini (2008).

¹⁷Note that white doctors increase demand too, just not as much as black doctors (and vice-versa for ex post case 2).

If there is an aversion to a particular race of doctor in the ex ante stage and this is followed by a lower perceived benefit, on average, from the same, this will reinforce the gap in demand across the two groups. If, on the other hand, aversion early on is countered by a less discounted health benefit ex post, the overall effect of doctor race on demand will be ambiguous.

III. Experimental Design

The experiment was conducted in Oakland, California, in the fall and winter of 2017–2018 (see Figure 1 for study design and flow).¹⁸ We recruited men from 19 black barbershops as well as two flea markets in and around the East Bay. Field officers (FO) approached men in the barbershops to enroll in the study. After obtaining written informed consent, the subject was given a short baseline survey.¹⁹ The baseline survey included questions on socio-demographics, healthcare, and mistrust. For completing the survey, the men received a coupon (worth up to \$25) for their haircut or, at the flea market, a cash incentive. After completing the baseline survey, the subjects were given a coupon to receive a free health screening for blood pressure, BMI, cholesterol, and diabetes at the clinic we operated on eleven Saturdays (see Appendix Figure 1). Subjects were encouraged to come to the clinic promptly, and subjects who did not have transport could receive a ride to the clinic courtesy of Uber.²⁰ Attendance at the clinic was encouraged with a \$50 incentive.

Upon arrival at the clinic, subjects who had a valid coupon were escorted into a waiting room where a ticket number was dispensed. Once their ticket number was called, they were led to a private patient room by a receptionist officer (RO).²¹ ROs wore crimson polo shirts with a Stanford - Bridge Clinical logo and khaki pants. The RO would then provide the subject with a tablet, which randomized the subject to a flu vaccine incentive and to a black or non-black doctor. Fourteen doctors participated in the experiment, including eight non-black and six black. SurveyCTO programmed in-form randomization using a computerized random assignment algorithm for the tablets.²² Note that the tablet was the first time subjects learned about the opportunity to receive a flu vaccine, since it was not advertised.²³ The RO would collect the coupon and give the subject

¹⁸Protocol information and links to the pre-analysis plan as well as other study documents are provided in the Appendix.

¹⁹Baseline survey included in the Appendix. Field officers were mostly minority or first-generation college students planning to apply to medical school. Six were black and three were Hispanic; most were male. FOs were encouraged to approach men who were black, the majority of clientele at the recruitment barbershops. However, they were also instructed they should not confront anyone who insisted on taking the survey and receiving the free haircut even if they do not appear to meet study criteria (i.e. individuals who self-identified as African-American males and who were at least 18 years of age). The net effect is that we were very successful at recruitment in the short amount of time (over 1,300 subjects in about three months) but 14 individuals who came to the clinic did not meet study criteria and were removed from the main analysis — see Figure 1. These out-of-sample subjects are used in the exploration of mechanisms discussed below.

²⁰Field officers used their own smart phones to obtain the rides. Forty-seven subjects used free rides as transport to the clinic; 64 accepted a ride after leaving the clinic.

²¹Receptionist officers were generally first-generation or minority college students planning to apply to medical school as well, including two white, two black, two Hispanic and one South Asian student; most were female.

²²At least four doctors were on site every Saturday (for reference, the median number of physicians at a community health center is five (Ku et al. 2015)). The algorithm ensured the per subject probability of receiving a black doctor was one-half and the probability of receiving a \$10, \$5, or no incentive for choosing flu was one-third (see Figure 1).

²³We were concerned, based on focus group work, that men would believe they had to receive a flu vaccine at the

his \$50 participation incentive, then instruct the subject on how to use the tablet. Two practice questions were answered by the subject with the RO present to make sure they could operate the tablet.²⁴ The RO then exited the patient room and allowed the subject to make their medical decisions in private.

The tablet introduced the subject to their assigned doctor and emphasized the doctor would be providing the services:

Your assigned doctor for today is Dr. [Last Name]. On the next page, you will be asked to select the services you wish to receive from Dr. [Last Name]. Dr. [Last Name] will administer all the services that you choose.

In addition, the same generic information about doctor training was provided:

Dr. [Last Name] is a medical doctor licensed to practice in the state of California and currently practicing in the Bay Area.

The text was accompanied by a large headshot photo of the doctor in a white coat with a red background.²⁵

The next screen listed four services (blood pressure measurement, body mass index measurement, cholesterol testing, and diabetes testing) as well as the doctor photo and queried the subjects on which services they would like to receive. The need for a finger prick of blood for diabetes and cholesterol was clearly demarcated. Selecting “none of the above” was also an option.

The following screen apprised the subject that they could also obtain the flu shot, which would “protect you and your community.” Those randomized to receive an incentive were then informed they would obtain \$5 or \$10 for selecting the flu shot. The doctor’s photo was shown for a third time and the subject was asked whether they would like to receive a shot from *Dr. [Last Name]*. If the subject responded affirmatively, a list of screening questions would appear for contraindications. Subjects were informed the \$5 or \$10 incentive would be given regardless of whether they had a contraindication. This was necessary to encourage reporting of any condition which could make flu vaccination potentially dangerous (e.g. allergic response). However, subjects who were reluctant to receive the shot in the first place could lie about having a problem. The RO returned to the patient room, collected the tablet, recorded the responses, and handed a clipboard to the assigned doctor.²⁶

Study doctors were instructed to encourage patients to receive all preventives.²⁷ The doctors, subjects, and field staff were not informed that doctor race was being randomized, though they clinic and therefore would not attend.

²⁴Fourteen subjects were illiterate and needed to have the RO read the tablet to them. We test for robustness to dropping those observations (see Appendix Table 7).

²⁵Tablet screenshots can be found in Appendix Figure 2. To protect the identity of the study doctors, there are no photos in the figure. The screenshots are not shown to scale, the tablet screen was approximately 10 inches.

²⁶Doctor assignment was double checked by a second FO. It’s possible that subjects could have doubted information on the tablet, such as whether the assigned doctor would actually meet them. Yet many seemed to believe (and respond) to the flu shot incentive by choosing it and our results on this subset are similar. Results available on request.

²⁷Similar to Coffman and Niehaus (2018) who study homophily in the context of the seller-buyer relationship, we did not provide a specific script for the doctors to use in their meetings. A script could have limited communication and made doctors appear less genuine/trustworthy in what was a real clinical encounter.

could have inferred it. They were explicitly told that the purpose of the study was to improve the take-up of preventive health screening services for African-American men (the study was officially labeled the “Oakland Men’s Health Disparities Project”). Doctors were aware that subjects were randomized, so that they would only meet with subjects assigned to them. Due to the nature of the malpractice coverage we were able to provide, study doctors were instructed not to provide medical care other than the services that were covered by the study. Subjects were also informed that the doctors were only able to provide the set of preventives listed on the tablet. If subjects had alarming values on any of their tests, there was an emergency protocol in place. After the visit was completed, subjects filled out a feedback form. They were then escorted out of the clinic by an RO and the ride-sharing service was called if needed. The study was approved by the IRB committee of Stanford and by the IRB committee at NBER for the non-experimental sample. The IRB committees at Berkeley and MIT ceded authority to Stanford.

IV. Empirical Strategy and Sample Characteristics

The purpose of the study is to estimate the causal effect of doctor race on the preventive healthcare decisions of African-American men. We begin by presenting an overview of our estimation framework and the data used in the study.²⁸ We then turn to describing characteristics of the study sample.

A. Estimating Equations

Using experimental data, we estimate the following equation:

$$Y_i = \alpha + \beta_1 \cdot \mathbb{1}_i^{BlackMD} + \beta_2 \cdot \mathbb{1}_i^{\$5} + \beta_3 \cdot \mathbb{1}_i^{\$10} + \Gamma' X_i + \epsilon_i \quad (3)$$

where i is an individual subject. Y_i is the demand for preventives during various stages of the experiment. $\mathbb{1}_i^{BlackMD}$, $\mathbb{1}_i^{\$5}$, and $\mathbb{1}_i^{\$10}$ are indicators for random assignment to black doctor, five dollar flu incentive and ten dollar flu incentive, respectively. X_i are characteristics of the subject and are included in some specifications to improve precision. In addition, to explore mechanisms, characteristics are interacted with randomized components. The results from our analysis of Equation 3 will show that the flu incentives only consistently affect demand for the flu, and thus we interact the black doctor treatment and flu incentive specifically when examining that outcome. We correct standard errors for multiple hypothesis testing in Appendix Table 9. In addition, we estimate stacked versions of Equation 3 where each observation is a subject-by-preventive service.²⁹

To further probe mechanisms, we collected non-experimental data from a survey of 1,490 other black and white male respondents whose education profile mirrored that of our experimental sample. The sampling frame was a panel of respondents managed by Qualtrics. We designed the survey to

²⁸A more detailed discussion of all data sources used in the analysis can be found in the Appendix.

²⁹p-values accounting for multiple hypothesis testing using sharpened false discovery rate adjusted q-values are reported in Appendix Table 9 (Anderson 2008). With the exception of panel data (Table 5 and parts of Table 8) standard errors are not clustered since neither the treatment assignment mechanism nor the sampling method were clustered (Abadie et al. 2017).

capture whether the preference for a racially concordant provider is unique to black male respondents and whether it varies across healthcare domains. Specifically, we estimate the following equations:

$$\mathbb{1}_i^{RaceMD=k} = \alpha + \beta_1 \cdot \mathbb{1}_i^{RaceResp=k} + \Gamma' X_i + \epsilon_i \quad (4a)$$

$$\mathbb{1}_i^{RaceMD=RaceResp} = \alpha + \beta_1 \cdot \mathbb{1}_i^{BlackResp} + \Gamma' X_i + \epsilon_i \quad (4b)$$

$$\mathbb{1}_{il}^{RaceMD=RaceResp} = \alpha + \beta_1 \cdot \mathbb{1}_i^{BlackResp} + \lambda_l \cdot \mathbb{1}_l^{Domain} + \Gamma' X_i + \epsilon_{il} \quad (4c)$$

where i indicates respondent, k signifies race (black or white) and l is one of the domains cited by the World Health Organization (WHO) as features of a responsive health system: access, quality, and communication (Gostin et al. 2003).³⁰ X_i in the above refers to respondent’s age, education, and income. Equation 4a examines whether respondents are relatively more likely to prefer doctors who share their racial background, where $RaceMD$ and $RaceResp$ are either both black or both white. Equation 4b tests whether the preference for racial concordance differs between black and white respondents. Finally, Equation 4c investigates whether the importance of concordance differs across domains as well as by race of the respondent.

B. Sample Characteristics

We first examine characteristics of the subjects who chose to come to the clinic, then proceed to check that observable characteristics are balanced across arms before turning to our main findings.

Recruitment and Selection — To examine selection, we modify Equation 3, regressing X_i on a dummy for *Clinic Presentation*.³¹ These results are gathered in Table 1.³² In general, those who came to the clinic were older, had lower self-reported health, visited the emergency room more in the past two years, and were less likely to have a primary medical doctor (PMD) compared to those that did not come. The selected men also had lower reported income; were less likely to be married; were more likely to be receiving unemployment, disability, or social security; were 19 percentage points more likely to have a high school diploma or less; and were 13 percentage points more likely to be unemployed.

Recall that the visit to the clinic was incentivized and barriers associated with not having a car or a license were alleviated by providing free transport to and from the clinic. The combined reduction in transport barriers and incentive to attend likely contributed to this pattern of selection.

Balance — Treatment groups are well-balanced on observables with two exceptions (see Table 2). The cell containing subjects who were randomized to a non-black doctor and \$10 incentive for flu are more likely to be uninsured and less likely to have good self-assessed health. The only significant joint F -test is on self-reported health, but including these two covariates, among others,

³⁰The other domains include respect, autonomy, confidentiality, timeliness, and familial support.

³¹See Data Appendix for variable definitions.

³²Our main clinic sample includes all of those who identify as African-American and are at least 18 years of age on the baseline survey as well as approximately 9% who skipped the demographic questions but were recruited in a black barbershop. In Appendix Table 7 we assess sensitivity to various sample restrictions.

in Equation 3 does not alter our results (see Table 4). Appendix Table 1 demonstrates that the groups are also balanced when examining randomization to a black doctor or a flu incentive amount separately.

V. Experimental Results

We now turn to our experimental results and the principle aim of our analysis. Do black male subjects randomized to black male doctors demand more preventives? Table 3 presents the main results conditioning only on the randomized treatments: doctor race and flu incentive.³³ In the ex ante stage, across every test offered, the race of the doctor in the photo did not influence demand in any significant way (see Columns (1), (4), and (7) in Panels (A) and (B)). These results are also apparent when comparing the means of ex ante take-up among black and non-black doctors in Figure 2 (the pair of vertical bars on the left side of each figure). Such findings are inconsistent with racial aversion playing a major role in take-up decisions. Rather, they are supportive of ex ante case 3 of the model — in which subjects do not add doctor-related costs to their utility calculation or add it equally for all doctors.

We find that the incentive influences ex ante demand for the flu shot. Approximately 20% of subjects selected the flu shot on the tablet in the absence of an incentive. A \$5 incentive increased flu take-up by about 19 percentage points, and a \$10 incentive increased it by 30 percentage points. The demand for flu vaccination in the ex ante stage is shown in Figure 4 Panel (A). With a \$10 incentive, almost 50% selected the flu shot on the tablet, though, as discussed further below, not all subjects who initially chose flu shots received it since subjects could revise their decision, usually by endorsing a contraindication.

In the ex post stage of the experiment, the effect of being randomized to a black doctor is statistically significant and, as we calculate below, medically meaningful. Table 3 Panel (A) Column (2) shows that subjects randomized to a black doctor are 11 percentage points more likely to demand a blood pressure measurement, an increase of 15% compared to the non-black doctor mean. According to the estimates in Panel (A) Column (5), the effect of a black doctor on BMI take-up is 16 percentage points or 27%. Note that, for both of these tests, subjects assigned to non-black doctors are also demanding more exams (see Figure 2 Panels (A) and (B)); however, those assigned to black doctors do so more frequently. These results are consistent with the conceptual framework in which all doctors relay basic information regarding the benefits of preventive care yet social distance acts to discount information from a discordant source (ex post case 1).

Moving to the invasive tests (those that required blood samples from the subject or involved an injection), the results demonstrate an even larger relative effect of black doctor assignment on demand for preventives among black male patients. A subject randomly assigned to a black doctor was 20 percentage points (47%) more likely to agree to a diabetes screening and 26 percentage points (72%) more likely to accept a cholesterol screening (Table 3 Panel (A) Column (8) and

³³In Appendix Table 4 we present baseline results with only the black doctor treatment.

Panel (B) Column (2)). Lastly with respect to the flu vaccine, which was cross-randomized with an incentive, subjects randomly assigned to a black male doctor were 10 percentage points more likely (56%) to agree to the flu shot relative to those who were assigned to a non-black doctor and no incentive. Interestingly, and in contrast to the non-invasive services, subjects assigned to non-black male doctors were not, on average, more likely to agree to the invasive services after meeting the doctor (See the light (gray) bars in Figure 2 Panels (C)–(F)). A simple extension to our basic framework demonstrates how, if the importance of social proximity varies by test characteristics, such a result could occur.³⁴

Figure 3 Panel (A) plots the black vs. non-black doctor difference in ex post screening by exam. The figure reveals the percent difference between black and non-black doctors is positively correlated with the invasiveness of the test. Blood pressure is a non-invasive test and was performed in the patient room. Therefore, it is unsurprising that this low risk and low hassle test had the lowest black doctor effect relative to non-black doctors. BMI measurement required the doctor to escort the subject down the hallway to a public room where there was a scale and height machine. The doctor used both devices to measure the height and weight of the subject and then calculated the BMI. Cholesterol and diabetes tests required a finger prick of blood (usually two separate sticks). The cholesterol and diabetes tests also took longer than other tests — on average, visit lengths for subjects who selected diabetes tests were about six minutes longer; a cholesterol screening added about three minutes. The results suggest the more invasive the test, the greater the advantage to being assigned a black doctor. To formally test this hypothesis, we stack the data to create a subject-screening panel. Table 5 Column (2) demonstrates that subjects assigned to black doctors were 10 percentage points more likely to demand invasive preventives in the ex post stage than those assigned to non-black doctors.³⁵

Columns (3), (6), and (9) of Table 3 present the difference between ex post and ex ante demand, which we refer to throughout the paper as the delta. This is similar to conditioning on the first choice, which, per above, was not statistically different across race of male doctor, and is a direct measure of how much demand shifts out after meeting the randomly assigned doctor. For instance, in Panel (B) Column (3), subjects assigned to a black doctor were 25 percentage points more likely to select a cholesterol screening after meeting their physician than those assigned to a non-black doctor.³⁶ Figure 3 Panel (B) plots the histogram of delta as a share of the four non-incentivized tests (i.e. excluding the flu). There is heaping on zero, reflecting the fact that many subjects did not change their choices. Most changes that did occur between the ex ante and ex post stages were from 0 to 1. In other words, subjects initially refused the screening but revised their decision after meeting with their assigned doctor, consistent with doctors' counseling increasing their perceived

³⁴Let I denote invasiveness with $\frac{d\delta}{dI} > 0$, implying that social distance matters more for invasive exams. Then the difference in ex post case 1 expected utility for subjects assigned to black vs. white doctors is $\frac{\delta b}{2}$ and the derivative of this term with respect to invasiveness will be positive.

³⁵This result is robust to including a control for the length of the physician visit.

³⁶To benchmark the results, we follow DellaVigna and Kaplan (2007) and calculate persuasion rates as a measure for how much subjects changed their behavior upon exposure to a black doctor. Appendix Figure 7 demonstrates that the persuasion rate is high relative to other published studies (summarized in DellaVigna and Gentzkow (2010)).

benefit. Black doctors shifted more of the distribution right, in the direction of obtaining more exams. There were a handful of reversals: reflecting subjects who chose the screening test initially, then declined after meeting the doctor. These are represented as mass left of zero in Figure 3 Panel (B), and, while very rare for non-incentivized exams, were more frequent for subjects assigned to non-black doctors.

Returning to the only incentivized test, the flu shot, we note in Table 3 Panel (B) Column (5), there is no statistical difference in the effect of a \$5 or a \$10 incentive on the receipt of a flu injection after interacting with their assigned doctor (in the ex post stage). Figure 4 Panels (D) and (E) separate out demand by assigned race of doctor. We show in Figure 4 Panel (F) that subjects assigned to a black doctor increased their take-up of the flu shot in the ex post stage at every incentive level. In contrast, particularly at \$10 incentive levels, subjects who originally chose the flu shot then met with a non-black doctor often reversed their decision. The results are imprecise (the total effect on delta for black doctor when randomized to a \$10 incentive is 0.05 (s.e. 0.047) vs. -0.11 (s.e. 0.042) for non-black doctors) but consistent with the notion that subsidies and interacting with a black doctor are not perfect substitutes for increasing demand.

In Table 4, we probe whether our results are sensitive to the inclusion of covariates thought to influence health, such as subject age (and its square), having a regular PMD, insurance, the month of the screening, education, income, and self-assessed health. The results are very similar to those presented in Table 3 and Figure 2.³⁷ As a robustness check, we include different fixed effects (RO, date, and recruitment location (Appendix Table 7 Panel (A)) and different samples (i.e. including everyone who consented regardless of their race or ethnicity, excluding those who could not read, including only those who responded to every demographic question (Appendix Table 7 Panel (B))); again the results are very similar. We also show that the results are not sensitive to dropping indicators for flu incentive levels (Appendix Table 4).³⁸ Finally, race appears to be a special facet of social distance — sharing the same age or educational background as doctors does not seem to positively influence take-up (see Table 9).³⁹ In sum, the results presented thus far reveal that, for African-American men in our study, the opportunity to meet with a black male doctor has a consistent, large, and robust positive effect on the demand for preventives.

VI. Mechanisms

In this section, we explore potential mechanisms for our results. We do so in four ways: first, by using data from the physician notes and subject feedback forms to further our understanding of the clinical encounter; second, by examining heterogeneity across subjects; third, by using non-experimental evidence from an additional survey we conducted on approximately 1,500 black and white men concerning preferences over doctors; and fourth by using publicly available, nationally

³⁷ Appendix Table 2 reports the coefficients on all the covariates.

³⁸In unreported results, we do not find evidence that knowing another subject at the clinic, a practice question we asked to ensure subjects could operate the tablet, affected demand.

³⁹Caution should be used in interpreting these results as neither education nor age was randomly assigned.

representative data from a survey of health utilization. We begin by examining the role of communication. Then we discuss other possible interpretations of our results including physician effort, quality, and discrimination.

A. Communication Between Patients and Doctors

Our primary data sources for understanding what transpired during the clinical encounter are doctors' notes on the patient and subject feedback forms about their clinical experience. As mentioned above, doctors were instructed to provide only the advertised services to subjects. In Table 7 Column (3) we find evidence that subjects assigned to black doctors were 10 percentage points more likely to try and talk to their doctor about issues unrelated to the provided services. These results are robust to controlling for the time spent with subjects and test fixed effects (see Appendix Table 8). Thus, subjects discussed other health problems with black doctors conditional on the number of minutes they spent in the room together. The doctors also indicated whether there was anything "notable" about the patient encounter on the patient files. Subjects were 11 percentage points more likely to have this section filled in if their assigned doctor was black (Column (4)). We analyzed the content of these notes by having three students who were blinded to the treatment hand code them as related or unrelated to the screening. Subjects assigned to black doctors were 9 percentage points more likely to discuss personal matters or health issues unrelated to the screening.⁴⁰

Qualitative evidence from the subject feedback forms and doctors' notes also support the mechanism of improved communication and the correction of false beliefs. One subject randomized to a black doctor wrote: "Dr. XXYY was excellent, he talked me into getting a flu shot and the conspiracy theories. I said 'Oh!' Great visit and putting me on track to monitor my sugar and cholesterol. Thanks!" As for the doctors' notes, a frequent phrase was "initially refused but agreed after counseling."

In Table 6, we test whether subjects assigned to black doctors were more responsive to the treatment based on their baseline demographic characteristics (Panel (A)), study clinic experience (Panel (B)), or past healthcare experience (Panel (C)). We focus on the delta demand of non-flu preventives — abstracting away from the interaction with incentives.⁴¹ Low-income subjects, defined as those that report an annual individual income below \$5,000 (over 40% of the sample), were more likely to take up non-flu preventive services if assigned to a black doctor than higher-income subjects, though this result is only marginally significant. We fail to find strong evidence of an important interaction effect between black doctor and either low education (an indicator for a high school degree or less) or age (an indicator for younger than 40).

In contrast, both Panels (B) and (C) reveal significant interactions between the black doctor treatment and either hassle costs associated with the study clinic or limited prior healthcare experience, respectively. In particular, subjects who were randomized to a black doctor but had longer

⁴⁰See also Bertrand et al. (2010) for randomized evidence on the importance of "intuitive" responses to informational content on the demand for loans.

⁴¹Ex ante results are in Appendix Table 3.

wait times (an indicator for over an hour) demanded more services than those exposed to a similarly lengthy wait time, but who were assigned to a non-black doctor. Subjects who experienced high congestion (greater than nine people in the waiting room, the 50th percentile) or those who were recruited from farther away locations (longer than 18 minutes by car, the 50th percentile) also elected to receive more services when randomized to a black doctor than a non-black doctor.⁴²

African-Americans visit the emergency room more often than non-Hispanic whites, which some have linked to lack of insurance, lower socioeconomic status, and mistrust that precludes healthcare utilization until an advanced stage of illness (Arnett et al. 2016, Brown et al. 2012). Panel (C) demonstrates that those who use the emergency room more often increased their demand for services when randomized to a black doctor. This result is particularly strong for the uninsured: in unreported results, the coefficient on the interaction between black doctor and number of ER visits is roughly seven times greater if a subject reported having no insurance.⁴³ Similarly, those who had no recent screening had a heightened response.

Research in medicine finds that black men have higher levels of medical mistrust than their white counterparts, and this mistrust is correlated with delays in care, lower healthcare utilization, and worse health outcomes (Kinlock et al. 2017, Nanna et al. 2018, Hammond et al. 2010). As alluded to above, we find that subjects increased their demand of all preventive services when assigned to a black doctor, and this effect was heightened if the screening test was invasive (see Table 5 Columns (2) and (3)). More invasive procedures, such as taking blood or providing injections, require a higher degree of trust between doctor and patient. As seen in Panel (C) Column (3) of Table 6, subjects were 6 percentage points more likely to obtain preventive services per a one unit increase in medical mistrust (on a scale of 1–3) when randomized to a black vs. non-black doctor.⁴⁴ Taken together, these results suggest that black men who had an inferior clinical experience (characterized by lengthy wait times and congestion) or those who were relatively inexperienced with respect to regular outpatient care were those who responded most strongly to a black doctor treatment.

An additional source of data we use to inform mechanisms is from a survey we conducted on 1,490 African-American and white (self-identified) males. We matched the survey sample to the recruited participants in terms of education, so that approximately half of the survey respondents had a high school education or less. Given a choice between a black, white, or Asian male doctor, respondents were asked to choose which doctor ranked the highest across three WHO domains: quality, communication, and accessibility.⁴⁵ The results are reported in Table 8.

First we examine respondent preferences for a doctor of the same race, i.e. concordance (Equa-

⁴²The wait time and congestion interactions have fewer observations due to missing data for the first two clinic days. All three variables are balanced across black and non-black doctor treatment.

⁴³We also asked a question about usual source of care in the baseline survey, but many subjects selected multiple options making their responses difficult to interpret. As in Zhou et al. (2017), we find that the uninsured use the ER at a similar rate to the insured, though they have fewer total hospital admissions and doctor visits. Results available on request.

⁴⁴If we interact legal mistrust with black doctor treatment instead of medical mistrust — we obtain a statistically insignificant and imprecise coefficient.

⁴⁵Questions were multiple choice asking if, holding constant male sex and age, respondents preferred a doctor who was either black, Asian, or white. No other information was provided.

tion 4a). In Column (1), we find that black respondents were more likely than white respondents to select black male doctors as the most qualified. Column (2) demonstrates that white respondents selected white doctors more often than black respondents. This finding is consistent across other domains, whereby both sets of respondents were relatively more likely to choose a concordant physician rather than a discordant physician (see Columns (4) and (5) and Columns (7) and (8)).

Second, we examine whether preferences for concordance vary across race (Equation 4b). Column (3) tests whether respondents were more or less likely to rate concordant doctors as most qualified. We find that white respondents were 6 percentage points more likely to select white doctors as most qualified than black respondents select black doctors as the most qualified. Both sets of respondents view concordance as important for communication (about 69%, see Column (6)) and there is no difference between the two groups. Turning to accessibility, responses from the two groups differ significantly (Column (9)), a point we return to when discussing external validity.

Third, we estimate Equation 4c, which tests whether concordance is stronger for some domains than others. In Column (10) we find that black and white respondents were 17 percentage points more likely to select a concordant doctor when the question was about communication as opposed to when the question referred to quality.

Figure 5 summarizes the results from Table 8. The figure plots the percent of respondents from a given race selecting a doctor of their own race across the three domains. We find a slight preference for concordance when it comes to quality, though both sets of respondents are very close to the (red) 50 percent line, indicating that, on average, respondents were as likely to select concordant physicians as they were to select discordant physicians. In sharp contrast, for questions related to communication, both black and white respondents shift rightwards: reflecting a clear preference for concordant doctors. Nearly 65% of black respondents and 70% of white respondents reported that a doctor of their own race would understand their concerns best.

To understand whether these patterns are also found in nationally representative data, we use the Medical Expenditure Panel Survey (MEPS), which queries individuals on characteristics of their doctor as well as utilization (Blewett et al. 2018b). Respondents were more likely to see a doctor of their own racial/ethnic group — though that varies across the race of the respondent. Specifically, 85% of white respondents and 71% of Asian respondents reported their primary doctor was of the same race (see Appendix Table 10). Although more black respondents report their doctor is black than respondents of other backgrounds, only 26% of black respondents said they had black doctors. The pattern for Hispanics is similar. This may reflect under-representation of blacks and Hispanics in the physician workforce, a point we return to when discussing external validity below.

Appendix Table 11 reports correlations between patient-doctor concordance and three outcomes: whether a respondent would go to their doctor for preventive care, whether they think their doctor listens to them carefully, and whether their doctor’s instructions were easy to understand. The sample is limited to adult males. The estimating equation includes indicators for patient and doctor race/ethnicity as well as concordant interactions. The interaction between black male patient and black doctor is consistently positive and significant, indicating that said patients are more likely to

seek out preventive care, feel their concerns are understood, and comprehend medical advice when paired with a black doctor.⁴⁶

B. Threats to Internal Validity

In this section, we consider whether doctor race represents a causal effect. Race is not randomly assigned in the population. Thus, in the sample of doctors we hired, race could be correlated with a characteristic that influences the ability of doctors to encourage subjects to take up preventives (i.e. our outcome of interest). Prominent potential omitted variables include quality and effort, which are hard to measure outside of the clinic context. In addition, with a finite number of physicians, the findings might be driven by outliers in either group. Finally, there is the concern that either subjects or doctors discriminate. We discuss each of these possible interpretations in turn.

Physician Quality — Physician quality is thought to influence patient outcomes, but is acknowledged to be complex and difficult to measure, particularly in primary care (Young, Roberts, and Holden 2017; AHRQ 2016). Some measures of quality include malpractice complaints, physician report cards, and rank of medical school. In this study, all doctors were vetted by a medical liability company and Stanford attorneys as a requirement of their participation. To measure physician quality, we asked subjects to fill out a feedback form before leaving the clinic. They rated their experience on a scale of 1 to 5 and were asked whether they would recommend their doctor to a friend. As seen in Table 7 Columns (6) and (7), there were no statistical differences between ratings and recommendations among those assigned to black or non-black doctors. Furthermore, the mean experience rating was about 4.8 with 85% of subjects characterizing it as excellent (a rating of 5) and 99% saying they would recommend their doctor to a friend. These findings are inconsistent with differential quality across doctor race.

To further analyze quality, we modify Equation 3 replacing the black doctor indicator with a fixed effect for each study doctor. We then examine what explains the correlation between doctor attributes and the fixed effect estimates (see Table 10). Experience and medical school ranking do little to explain the variation in fixed effects. In contrast race accounts for about 85 percent of the R-squared in Column (4). The results suggest that, for a black male patient, having a black doctor was equivalent to a doctor moving from about the 80th ranked medical school to the top ranked medical school.⁴⁷

If race of doctor in the study was highly correlated with quality, then we should find black doctors perform better on subjects from all backgrounds. Twelve individuals did not identify as African-

⁴⁶The main effect of black doctor is negative though not consistently significant — a finding that does not support differential quality of doctors across race. See also the next subsection for further evidence on (lack of) differential quality across race.

⁴⁷We also tested whether physicians improved over time at the clinic. Specifically, we totaled the number of patients seen by each doctor, and assessed whether the average effect of non-black and black doctors was higher for each doctor's second set of patients. We find no evidence that black doctors improved over time and, although sample sizes are small, non-black doctors' effect decreased slightly.

We also test whether the set of black doctor or non-black fixed effects are jointly different than zero. Although we reject the null when including all doctors, when dropping the “best” doctor of each group, the F-statistic for black doctors is 26.4 but for non-black doctors falls to 1.35 as is indistinguishable from zero.

American, but were still seen at the clinic because they had been consented to participate during recruitment. These clients were randomized across eight of the fourteen study doctors, equally balanced by race, and were 14 percentage points *less* likely to choose services from black doctors in the ex post stage. We compare this result to a placebo test where we randomly select 12 in-sample subjects and regress the share of services received on black doctor. We find that the coefficient on black doctor for the out-of-sample group is lower than 93 percent of these bootstrap coefficients (see Appendix Figure 3). To the extent that quality is a relatively stable attribute of a clinician, this finding is inconsistent with a correlation between doctor race and quality confounding the interpretation of our results. As a final indication of quality, we tabulate the number of mechanical errors on the diabetes and cholesterol machines by doctor. There were very few errors in total and they did not vary across race.

Physician Effort — Another potential explanation is that black doctors exerted more effort when working with black patients than non-black doctors. Similar to quality, physician effort is difficult to measure. Often, time spent with the patient is used as a metric, but in our study this equivalence is complicated. As mentioned in the introduction, a longer time could reflect the treatment effect (i.e. subjects elect to receive more services from black doctors), low quality (i.e. difficulty performing the test), or communication (i.e. a better patient-doctor connection facilitating credible information exchange). In Table 7 Column (1) we find that black doctors spent approximately four more minutes with subjects. However, this finding is mainly related to our treatment effect, in Column (2) when we condition on fixed effects for each test, the point estimate is about one minute (compared to an average visit length of 20.5 minutes), and is not statistically significant. We also examine whether study doctors exerted more effort by targeting services to the those at increased risk for disease (as defined by national guidelines — see Data Appendix for details). Such targeting would require clinical acumen and effort since doctors were provided no information on the subjects’ medical histories prior to their brief encounter. Results in Appendix Table 5 fail to find evidence of targeting.

Outliers — A third possibility is that our results are driven by outliers. As noted above, there are no prominent differences in observables (if anything, the set of black doctors attended lower ranked medical schools and were slightly less likely to be internists, see Appendix Table 6). To test whether any particular physician is driving our results, we estimate the black doctor effect dropping one doctor at a time. The results gathered in Figure 6 demonstrate that the results are remarkably stable across the leave-one-out estimates. If we drop the “best” black and non-black doctor, we obtain a consistent coefficient of 0.148 (s.e. 0.023). In the most stringent condition, we omit the “best” black and the “worst” non-black doctor. We still find our treatment effect is highly significant though the coefficient declines by 50% when estimating on the set of *all screening tests*. However, for *invasive tests* the magnitude is 0.170 (s.e. 0.022) for all doctors and 0.108 (s.e. 0.023) when omitting the “best” black and the “worst” non-black doctor. For comparison, if we dropped the “worst” black doctor and the “best” non-black doctor the treatment effect would be roughly doubled, and for invasive testing the treatment effect would be 0.221 (s.e. 0.023).

Discrimination — A fourth possibility is that subjects derive disutility from non-black doctors thus decreasing demand (i.e. ex ante case 1). Our results suggest this is unlikely. First, if aversion for a particular race was strong, we would have expected to observe this in the ex ante stage, when subjects were first introduced to the doctor by tablet photo. As previously noted, though, we find no statistical differences in the ex ante tablet selections (Table 3). Second, in the ex post stage, we find that, on average, subjects assigned to non-black doctors increased their demand relative to the ex ante stage (see light (gray) bars in Figure 2), just not as much of an increase as with black doctors (and not at all with invasive exams). Lastly, we note that if discrimination by patients or doctors were an important part of the explanation for our results, we would have expected variation in subject feedback across doctor race and lower scores for non-black doctors. Instead we find that the average ratings were very high and there was no difference across doctor race.

C. Threats to External Validity

In order to benchmark our results and assess their relevance for the larger discussion on reducing health disparities in the U.S., it’s important to compare our study doctors and sample to the general population, bearing in mind that extrapolation should be done with caution.

Subjects — In terms of demographic characteristics, our study subjects were more likely to be uninsured (28%) and unemployed (31%), as compared to black men in the U.S. (about 17% and 7%, respectively).⁴⁸ However, they are very similar in terms of average age and education (43 years and 63% with a high school education or less in our sample versus 43 years and 58% with a high school education or less in the U.S.).

Turning to health characteristics, the average value for systolic blood pressure was 132.7 mm Hg consistent with stage 1 hypertension (distributions of medical screening results are displayed in Figure 7). The average BMI value was 27.4 kg/m² consistent with an overweight categorization. The average hemoglobin A1c was 5.8%, consistent with a diagnosis of pre-diabetes. About 1.4% of the sample had a hypertensive crisis — a critically high value of blood pressure requiring urgent care, 4.4% were morbidly obese, and 3.1% of the subjects had a hemoglobin A1c value in the seriously elevated range (i.e. >9%).

In terms of disease prevalence, about 30% of the screened study sample had values of blood pressure, BMI, and cholesterol consistent with hypertension, obesity, and dyslipidemia, respectively; and 15% had hemoglobin A1c levels diagnostic of diabetes.⁴⁹ Despite our sample having higher rates of unemployment and uninsurance, these figures are unfortunately very similar to the prevalence of the aforementioned conditions among black men in the U.S. more broadly, as seen in Figure 8. If anything our screened study sample was slightly healthier than the average African-American male in the U.S. Specifically, the prevalence of high blood pressure in black men in the U.S. is 41%, compared to 30% for white men, the prevalence of hypercholesterolemia is 33% for black men

⁴⁸Calculations on the U.S. population come from 2016 1-year American Community Survey data (Ruggles et al. 2017).

⁴⁹Some subjects indicated that they were on medications for these conditions; we only include them in the estimate if they chose to receive a screening.

compared to 37% for white men, and the prevalence of diabetes is 18% for black men vs. 9% for white men (Fryar et al. 2017; Hales et al. 2017; CDC 2017b; and CDC 2017c). These comparisons suggest that our findings are not due to a sample of individuals with worse health on average.⁵⁰

Doctors — How representative were the doctors hired for our study? All doctors who participated knew the clinic provided preventive services to black men, many of whom lacked alternative medical options. Therefore, these doctors are plausibly drawn from the *least prejudiced* doctor distribution. The doctors also gave up their Saturdays in exchange for a fixed hourly compensation that they received through direct deposit or check.⁵¹ Doctors of both races attended highly ranked medical schools. Across all 14 study doctors, 11 graduated from schools ranked in the top 25 of the U.S. News Research Rankings, a much higher share of graduates relative to the population at large. Black doctors in the study graduated from slightly lower ranked schools, also consistent with the national data (see Appendix Figure 4 — a higher share of black graduates attend unranked schools relative to white graduates).

One way our study was unique, however, was that subjects had easy access to a black male doctor once randomized to them. Several studies report that minority doctors are more likely than white doctors to work in underserved areas and see patients who share their racial background (Moy and Bartman 1995; Komaromy et al. 1996; Cantor et al. 1996; Walker, Moreno, Grumbach 2012). Yet despite this allocation, there remains a difference in access. Returning to our non-experimental evidence in Figure 5, by far the largest divide between black and white male respondents is with regards to accessibility of a doctor who is of their same race and sex background (37% vs. 62%). In Table 8 Column (9), black male respondents were 26 percentage points less likely to respond that a black male doctor is available near them than white males report white male doctors are available, conditional on age, income, and education.⁵²

As stated in the introduction, African-Americans comprise only 4% of practicing physicians in the U.S. Both African-American and Hispanic physicians are significantly under-represented if comparing the ratios of the share of the recent medical school graduates to their share in the U.S. population. Non-Hispanic white physicians approach a ratio of one and Asian physicians approach a ratio of four (see Appendix Figure 5). Moreover, the pipeline of African-American medical school graduates is relatively flat — hovering around 6% for the last decade, an increasingly lower share of the African-American population (see Appendix Figure 6). This aspect of the study was also noted by one of the subjects: “Really excited about the black male doctors!!!”

VII. Health Valuation

In behavioral hazard models, individuals may underuse medical care due to misperceptions; thus the demand curve ceases to be a sufficient statistic for welfare calculations (Pauly and Blavin 2008;

⁵⁰For a detailed review of recent trends in African-American health, see Simon et al. (2016).

⁵¹The compensation was competitive with the market rate for moonlighting physicians in the Bay Area <https://www.whitecoatinvestor.com/forums/topic/moonlighting-rates/>.

⁵²In the baseline survey, we asked how much choice individuals had in where they go for medical care — only 37% of respondents answered that they had a “great deal of choice.”

Baicker, Mullainathan, and Schwartzstein 2015). In addition, most of the preventives we offered were not cross-randomized with incentives. Thus, we value the effect of a black doctor in preventing cardiovascular-disease-related deaths using recently published medical studies (Kahn et al. 2010, Dehmer et al. 2017). Both Kahn et al. and Dehmer et al. perform a Monte-Carlo simulation on a representative U.S. population to compare screening to a no screening condition, and assume that those who screen positive receive guideline-recommended therapy. Since both studies were published relatively recently, treatment efficacy is likely to reflect the current state of care, though varying the fraction of screen-positive who obtain and follow appropriate treatment recommendations will alter the results, particularly if this fraction also interacts with doctor race.⁵³ Since these models are concerned with the effect of screening on health, we combine their estimates with the coefficient on black doctor in the ex post stage.

We find that black doctors reduce myocardial infarctions by 1,082 per 100,000 and cardiovascular-related deaths by 628 per 100,000 (or 15.7 per year) for black men over about a 40-year time horizon.⁵⁴ The difference in annual age-adjusted mortality rates for cardiovascular disease between non-Hispanic white (268.4 per 100,000) and non-Hispanic black males (350.3 per 100,000) in the U.S. is 81.9 per 100,000 (Murphy et al. 2017). Therefore, the treatment effect we estimate for black doctors could reduce this gap by approximately 19%.⁵⁵

The difference in annual age-adjusted mortality rates for influenza and pneumonia between non-Hispanic white and non-Hispanic males in the United States is 2.7 per 100,000 (20.3 versus 17.6). Flu vaccination for adults over the age of 18 is estimated as averting 2.7 deaths per 100,000 per year (based on CDC 2016 and CDC 2017a). Multiplying the treatment effect of black doctors by the efficacy of flu vaccination to prevent flu deaths among adults, we obtain 0.27, which is roughly 10% of the gap in mortality for this cause of death.

Harper, Rushani, and Kaufman (2012) calculate that 41% of the life-expectancy gap between black and white males in 2008 was due to cardiovascular disease and diabetes. Therefore, our estimates of the black doctor treatment effect suggest the overall life-expectancy gap between black and white males exclusive of infant mortality could be reduced by approximately 8% or 5 months from cardiovascular disease and diabetes alone. If we extrapolate the screening benefit to other preventable leading causes of death and health disparities among African-American men (i.e. HIV and cancer), the life expectancy gain could be even larger since these chronic illnesses account for another 26% of the black-white male life expectancy gap.⁵⁶

⁵³The Dehmer et al. study assumes only 90% of those offered screening take it up, thus we divide by 0.9 to make the results consistent with the Kahn et al. study. The Dehmer et al. study also provides estimates of the effects of screening subdivided by race and gender. Such stratification is not available in Kahn et al. Further details on the studies and the calculation can be found in the Appendix.

⁵⁴We use a 40-year time horizon since screenings for blood pressure, cholesterol, and diabetes are modeled as beginning at 18, 20, and 30 years of age.

⁵⁵Garthwaite, Gross, and Notowidigdo (2018) calculate a substantial cost to hospitals from uncompensated care, in particular uninsured visits to the ER. To the extent preventive services reduce ER visits, our intervention could translate into cost savings for hospitals. As noted above, we found that those who were uninsured and used the ER were particularly sensitive to the black doctor treatment.

⁵⁶Certain types of cancer can be prevented through care and treatment adherence (e.g. HPV vaccine, tobacco cessation, earlier stage diagnoses).

VIII. Conclusion

In this study, we examine the effect of diversity of the physician workforce on the demand for preventive care among African-American men using a randomized trial. We find that, when patients and doctors had an opportunity to meet in person, patients assigned to a black doctor increased their demand for preventives, particularly those which were invasive. These findings were stronger among subjects who had high mistrust of the medical system as well as those who had limited prior experience with routine medical care. Data from the clinical encounter demonstrate that subjects brought up more issues and were more likely to seek advice from black doctors, as reflected in the doctors' notes.

These findings are consistent with a framework in which agents underestimate the benefit of preventive care, and thus have low demand. Physicians, through their counseling and rapport with patients, which varies by social distance, can help correct false beliefs and increase demand. Subsidies also increase demand, though we find financial incentives do not completely substitute for information from a trusted source. Some subjects who selected flu shots initially, encouraged by the incentive, declined to actually receive them (often citing contraindications). Moreover, black doctors continued to increase demand even among subjects who initially refused a flu shot despite a financial incentive.

Our back of the envelope calculations suggest the increased demand induced by black doctors could reap substantial health benefits. Specifically, we calculate that increased screening could lead to a 19% reduction in the black-white male cardiovascular mortality gap and a 8% decline in the black-white male life expectancy gap. Given the current supply of black doctors, a more diverse physician workforce might be necessary to realize these gains.

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Table 1: Selection into Experiment

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---------------------|-----------------------------|---------------------------|------------------------|---------------------|--------------------------------|-------------------------|-----------------------|
| PANEL A | | | | | | | |
| | <i>Self-Reported Health</i> | <i>Any Health Problem</i> | <i>Hospital Visits</i> | <i>ER Visits</i> | <i>Nights Hospital</i> | <i>Medical Mistrust</i> | <i>Has Primary MD</i> |
| Clinic Presentation | -0.126*** (0.025) | 0.033 (0.028) | 0.244 (0.469) | 0.513*** (0.183) | -0.332 (0.746) | -0.011 (0.042) | -0.072** (0.029) |
| Mean | 0.81 | 0.57 | 4.74 | 1.24 | 1.93 | 1.64 | 0.69 |
| Observations | 1,148 | 1,241 | 935 | 1,031 | 1,041 | 1,232 | 1,096 |
| PANEL B | | | | | | | |
| | <i>Uninsured</i> | <i>Age</i> | <i>Married</i> | <i>Unemployed</i> | <i>≤ High School Education</i> | <i>Low Income</i> | <i>SSI/DI/UI</i> |
| Clinic Presentation | 0.038 (0.027) | 3.411*** (0.811) | -0.053** (0.022) | 0.129*** (0.025) | 0.190*** (0.029) | 0.198*** (0.027) | 0.113*** (0.024) |
| Mean | 0.24 | 41.06 | 0.20 | 0.18 | 0.44 | 0.25 | 0.18 |
| Observations | 1,074 | 1,241 | 1,201 | 1,176 | 1,141 | 1,171 | 1,198 |

Note: Table reports results from a regression of various baseline characteristics on clinic presentation. Observation count varies due to missing responses in the baseline survey. Reported mean is among subjects that did not present to the clinic. See Data Appendix for other variable definitions. Robust standard errors in parentheses. *, **, *** indicate significance at the 10, 5, or 1% level.

Table 2: Balance

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|-------------------------|------------------------|-------------------------------|--------------------------------|---------------------------|---------------------------|----------------------------|---------------|----------------|----------|
| | <i>Mean (S.D.)</i> | <i>Non-Black MD - \$5</i> | <i>Non-Black MD - \$10</i> | <i>Black MD - \$0</i> | <i>Black MD - \$5</i> | <i>Black MD - \$10</i> | <i>F-test</i> | <i>p-value</i> | <i>N</i> |
| Self-Reported Health | 0.72 (0.45) | -0.033 (0.066) | -0.181*** (0.067) | 0.007 (0.065) | -0.016 (0.064) | 0.004 (0.063) | 2.075 | 0.067 | 563 |
| Any Health Problem | 0.62 (0.49) | -0.026 (0.068) | 0.036 (0.065) | -0.015 (0.069) | -0.025 (0.067) | -0.021 (0.066) | 0.250 | 0.940 | 614 |
| ER Visits | 1.69 (3.54) | -0.149 (0.434) | 0.867 (0.609) | -0.212 (0.443) | 0.145 (0.558) | -0.391 (0.419) | 1.336 | 0.247 | 511 |
| Nights Hospital | 1.20 (3.52) | -0.392 (0.415) | 0.839 (0.734) | 1.956 (1.490) | -0.214 (0.466) | 0.230 (0.663) | 1.332 | 0.249 | 511 |
| Medical Mistrust | 1.61 (0.74) | 0.162 (0.105) | -0.046 (0.100) | 0.032 (0.105) | 0.016 (0.105) | -0.034 (0.100) | 0.979 | 0.430 | 611 |
| Has Primary MD | 0.63 (0.49) | -0.042 (0.074) | 0.033 (0.070) | -0.059 (0.073) | 0.008 (0.070) | -0.019 (0.071) | 0.415 | 0.838 | 537 |
| Uninsured | 0.22 (0.42) | 0.042 (0.066) | 0.146** (0.067) | 0.112 (0.070) | 0.057 (0.064) | 0.010 (0.062) | 1.398 | 0.223 | 517 |
| Age | 44.96 (14.76) | -1.051 (1.973) | -0.100 (2.001) | -0.261 (1.982) | -1.109 (2.048) | -0.495 (1.944) | 0.109 | 0.990 | 620 |
| Married | 0.14 (0.35) | 0.043 (0.052) | -0.037 (0.045) | 0.069 (0.055) | -0.015 (0.047) | 0.024 (0.050) | 1.120 | 0.348 | 586 |
| Unemployed | 0.32 (0.47) | -0.045 (0.066) | -0.008 (0.066) | -0.051 (0.065) | 0.008 (0.065) | 0.025 (0.065) | 0.394 | 0.853 | 570 |
| ≤ High School Education | 0.62 (0.49) | 0.006 (0.070) | -0.006 (0.070) | -0.029 (0.072) | 0.055 (0.068) | 0.034 (0.068) | 0.344 | 0.886 | 556 |
| Low Income | 0.47 (0.50) | -0.026 (0.072) | -0.033 (0.071) | -0.043 (0.072) | 0.022 (0.070) | -0.042 (0.069) | 0.258 | 0.936 | 571 |
| Attrition | 0.03 (0.18) | 0.022 (0.033) | 0.045 (0.034) | 0.031 (0.034) | 0.015 (0.031) | -0.029 (0.025) | 1.715 | 0.129 | 684 |

Note: Columns (2)–(6) report regression coefficients and standard errors for each randomization group relative to the omitted group (Column (1), the non-black doctor and no incentive group). Columns (7) and (8) show the F-statistic and associated *p*-value testing whether the treatment arms are jointly equal to zero. Observation count varies due to missing responses in the baseline survey. Attrition is an indicator for the 47 subjects that did not complete the study because they left before the clinic encounter (3 of the 50 subjects who attrited did not self-identify as black men and are therefore excluded). See Data Appendix for other variable definitions. Robust standard errors in parentheses. *, **, *** indicate significance at the 10, 5, or 1% level.

Table 3: Ex Ante, Ex Post, and Delta Demand for Preventives

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|----------------------------|-----------------------|---------------------|---------------------|------------------------|---------------------|---------------------|---|---------------------|---------------------|
| | Ex Ante | Ex Post | Delta | Ex Ante | Ex Post | Delta | Ex Ante | Ex Post | Delta |
| PANEL A | | | | | | | | | |
| | <i>Blood Pressure</i> | | | <i>BMI</i> | | | <i>Diabetes</i> | | |
| Black Doctor | 0.025 (0.039) | 0.107*** (0.033) | 0.082** (0.034) | 0.023 (0.040) | 0.161*** (0.036) | 0.138*** (0.033) | 0.050 (0.039) | 0.201*** (0.039) | 0.151*** (0.029) |
| \$5 Incentive | 0.028 (0.048) | 0.044 (0.040) | 0.017 (0.043) | -0.059 (0.049) | 0.019 (0.045) | 0.078* (0.043) | 0.085* (0.048) | 0.105** (0.048) | 0.020 (0.036) |
| \$10 Incentive | -0.023 (0.048) | -0.026 (0.041) | -0.003 (0.040) | -0.009 (0.048) | -0.010 (0.044) | -0.001 (0.038) | 0.028 (0.047) | 0.050 (0.047) | 0.021 (0.035) |
| \$5 = \$10 <i>p</i> -value | 0.295 | 0.082 | 0.646 | 0.300 | 0.521 | 0.053 | 0.238 | 0.240 | 0.977 |
| Control Mean | 0.56 | 0.72 | 0.16 | 0.50 | 0.60 | 0.11 | 0.37 | 0.43 | 0.05 |
| Observations | 637 | 637 | 637 | 637 | 637 | 637 | 637 | 637 | 637 |
| PANEL B | | | | | | | | | |
| | <i>Cholesterol</i> | | | <i>Flu Vaccination</i> | | | <i>Share of All Non-Incentivized Tests (Excludes Flu)</i> | | |
| Black Doctor | 0.010 (0.038) | 0.260*** (0.038) | 0.250*** (0.032) | -0.009 (0.037) | 0.100*** (0.038) | 0.108*** (0.033) | 0.027 (0.030) | 0.182*** (0.029) | 0.155*** (0.023) |
| \$5 Incentive | 0.067 (0.047) | 0.061 (0.048) | -0.006 (0.038) | 0.192*** (0.043) | 0.221*** (0.045) | 0.029 (0.039) | 0.030 (0.037) | 0.057 (0.035) | 0.027 (0.028) |
| \$10 Incentive | -0.014 (0.045) | -0.013 (0.047) | 0.001 (0.039) | 0.299*** (0.043) | 0.219*** (0.044) | -0.080* (0.041) | -0.004 (0.036) | -0.00005 (0.035) | 0.004 (0.026) |
| \$5 = \$10 <i>p</i> -value | 0.083 | 0.113 | 0.856 | 0.026 | 0.974 | 0.010 | 0.366 | 0.112 | 0.423 |
| Control Mean | 0.35 | 0.36 | 0.01 | 0.20 | 0.18 | -0.02 | 0.44 | 0.53 | 0.08 |
| Observations | 637 | 637 | 637 | 637 | 637 | 637 | 637 | 637 | 637 |

Note: Table reports OLS estimates of Equation 3. The outcome varies by column heading. Ex ante refers to demand upon viewing assigned doctor photo on tablet, but before meeting doctor in person. Ex post refers to demand after meeting doctor in person. Delta is ex post - ex ante demand. Control mean refers to subjects randomized to a non-black doctor for the non-flu screenings and to subjects randomized to a non-black doctor and no incentive for the flu vaccination. Robust standard errors in parentheses. *p*-values corrected for multiple hypothesis testing are found in Appendix Table 9. *, **, *** indicate significance at the 10, 5, or 1% level.

Table 4: Ex Ante, Ex Post, and Delta Demand for Preventives with Controls

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|----------------|-----------------------|---------------------|---------------------|------------------------|---------------------|---------------------|---|---------------------|---------------------|
| | Ex Ante | Ex Post | Delta | Ex Ante | Ex Post | Delta | Ex Ante | Ex Post | Delta |
| PANEL A | | | | | | | | | |
| | <i>Blood Pressure</i> | | | <i>BMI</i> | | | <i>Diabetes</i> | | |
| Black Doctor | 0.030 (0.039) | 0.103*** (0.033) | 0.073** (0.034) | 0.019 (0.039) | 0.158*** (0.036) | 0.139*** (0.034) | 0.051 (0.038) | 0.204*** (0.038) | 0.153*** (0.029) |
| \$5 Incentive | 0.023 (0.048) | 0.041 (0.040) | 0.018 (0.042) | -0.070 (0.049) | 0.005 (0.045) | 0.075* (0.044) | 0.091** (0.046) | 0.107** (0.047) | 0.017 (0.036) |
| \$10 Incentive | -0.020 (0.048) | -0.029 (0.042) | -0.009 (0.040) | -0.005 (0.046) | -0.015 (0.044) | -0.010 (0.038) | 0.035 (0.045) | 0.045 (0.047) | 0.010 (0.034) |
| Control Mean | 0.56 | 0.72 | 0.16 | 0.50 | 0.60 | 0.11 | 0.37 | 0.43 | 0.05 |
| Observations | 637 | 637 | 637 | 637 | 637 | 637 | 637 | 637 | 637 |
| PANEL B | | | | | | | | | |
| | <i>Cholesterol</i> | | | <i>Flu Vaccination</i> | | | <i>Share of All Non-Incentivized Tests (Excludes Flu)</i> | | |
| Black Doctor | 0.013 (0.036) | 0.262*** (0.038) | 0.249*** (0.033) | -0.006 (0.037) | 0.103*** (0.038) | 0.109*** (0.034) | 0.028 (0.029) | 0.182*** (0.028) | 0.153*** (0.022) |
| \$5 Incentive | 0.078* (0.044) | 0.062 (0.047) | -0.016 (0.038) | 0.181*** (0.043) | 0.205*** (0.045) | 0.024 (0.039) | 0.030 (0.035) | 0.054 (0.035) | 0.024 (0.027) |
| \$10 Incentive | -0.003 (0.044) | -0.020 (0.045) | -0.017 (0.039) | 0.299*** (0.043) | 0.208*** (0.045) | -0.091** (0.041) | 0.002 (0.035) | -0.005 (0.034) | -0.006 (0.026) |
| Control Mean | 0.35 | 0.36 | 0.01 | 0.20 | 0.18 | -0.02 | 0.44 | 0.53 | 0.08 |
| Observations | 637 | 637 | 637 | 637 | 637 | 637 | 637 | 637 | 637 |

Note: Table reports OLS estimates of Equation 3 including controls (indicators for month of clinic visit, age, age squared, high school education, low income, self-assessed health, has primary medical doctor, and uninsured). Missing values of the controls coded as -9 and a missing indicator included when relevant. The outcome varies by column heading. Ex ante refers to demand upon viewing assigned doctor photo on tablet, but before meeting doctor in person. Ex post refers to demand after meeting doctor in person. Delta is ex post - ex ante demand. See text for further details. Control mean refers to subjects randomized to a non-black doctor for the non-flu screenings and to subjects randomized to a non-black doctor and no incentive for the flu vaccination. Robust standard errors in parentheses. p -values corrected for multiple hypothesis testing are found in Appendix Table 9. *, **, *** indicate significance at the 10, 5, or 1% level.

Table 5: Black Doctor and Invasive/Incentive Test Interactions

| | (1) | (2) | (3) | (4) | (5) | (6) |
|-----------------------|----------------------|----------------------|----------------------|---------------------|---------------------|--------------------|
| | Ex Ante | Ex Post | Delta | Ex Ante | Ex Post | Delta |
| | <i>Invasive</i> | | | <i>Incentive</i> | | |
| Black Doctor | 0.022 (0.034) | 0.133*** (0.030) | 0.110*** (0.028) | -0.033 (0.053) | 0.098* (0.058) | 0.131** (0.053) |
| \$5 Incentive | 0.030 (0.037) | 0.057 (0.035) | 0.027 (0.028) | 0.113* (0.060) | 0.192*** (0.061) | 0.079 (0.049) |
| \$10 Incentive | -0.004 (0.036) | -0.00005 (0.035) | 0.004 (0.026) | 0.337*** (0.061) | 0.243*** (0.060) | -0.094* (0.054) |
| Invasive Test | -0.167*** (0.023) | -0.265*** (0.024) | -0.099*** (0.021) | | | |
| Black * Invasive Test | 0.010 (0.034) | 0.099*** (0.033) | 0.089*** (0.032) | | | |
| Black * \$5 | | | | 0.155* (0.086) | 0.055 (0.091) | -0.099 (0.078) |
| Black * \$10 | | | | -0.071 (0.086) | -0.046 (0.089) | 0.025 (0.083) |
| Control Mean | 0.53 | 0.66 | 0.13 | 0.20 | 0.18 | -0.02 |
| Observations | 2,548 | 2,548 | 2,548 | 637 | 637 | 637 |

Note: Columns (1)–(3) report OLS estimates on the delta in demand for the share of four non-incentivized preventives (blood pressure, body mass index, cholesterol and diabetes) using a subject-screening panel and including interactions between an indicator for black doctor and an indicator for an invasive exam. Columns (4)–(6) report OLS estimates on flu demand including interactions between black doctor and indicators for different incentive levels. The outcome varies by column heading. Ex ante refers to demand upon viewing assigned doctor photo on tablet, but before meeting doctor in person. Ex post refers to demand after meeting doctor in person. Delta is ex post - ex ante demand. See text for further details. Control mean refers to non-invasive tests for those randomized to a non-black doctor for Columns (1)–(3) and to those randomized to a non-black doctor and no incentive for Columns (4)–(6). Clustered standard errors in parentheses for panel analysis (Columns (1)–(3)) as treatment assignment is correlated within subject. Robust standard errors in parentheses in Columns (4)–(6). *p*-values corrected for multiple hypothesis testing are found in Appendix Table 9. *, **, *** indicate significance at the 10, 5, or 1% level.

Table 6: Heterogeneity by Demographics, Hassle Costs, and Medical Care Experience

| | (1) | (2) | (3) |
|---|-----------------------|-------------------------------------|-------------------------|
| <i>Outcome = Delta Share Non-Incentivized Preventives</i> | | | |
| PANEL A: Demographics | | | |
| <i>X =</i> | <i>Low Income</i> | \leq <i>High School Education</i> | <i>Younger than 40</i> |
| Black Doctor * <i>X</i> | 0.087* (0.047) | -0.074 (0.049) | 0.037 (0.047) |
| <i>X</i> | 0.057* (0.029) | 0.109*** (0.028) | -0.034 (0.029) |
| Black Doctor | 0.113*** (0.029) | 0.192*** (0.037) | 0.135*** (0.028) |
| Observations | 571 | 556 | 620 |
| PANEL B: Hassle Costs | | | |
| <i>X =</i> | <i>Long Wait Time</i> | <i>High Congestion</i> | <i>Long Commute</i> |
| Black Doctor * <i>X</i> | 0.179*** (0.055) | 0.140*** (0.050) | 0.090* (0.046) |
| <i>X</i> | -0.005 (0.030) | 0.014 (0.027) | 0.021 (0.026) |
| Black Doctor | 0.111*** (0.029) | 0.102*** (0.031) | 0.108*** (0.028) |
| Observations | 451 | 451 | 618 |
| PANEL C: Medical Care Experience | | | |
| <i>X =</i> | <i>ER Visits</i> | <i>No Recent Screening</i> | <i>Medical Mistrust</i> |
| Black Doctor * <i>X</i> | 0.012** (0.006) | 0.146** (0.067) | 0.061** (0.031) |
| <i>X</i> | -0.0004 (0.003) | -0.032 (0.040) | -0.017 (0.019) |
| Black Doctor | 0.134*** (0.028) | 0.122*** (0.024) | 0.058 (0.053) |
| Observations | 511 | 604 | 611 |

Note: Table reports OLS estimates from a modified version of Equation 3 including interactions between black doctor and certain baseline characteristics. The outcome variable for every specification is the delta in demand for the share of the four non-incentivized preventives selected (blood pressure, body mass index, cholesterol, and diabetes). Observation count varies due to missing responses in the baseline survey. Indicators for incentive levels are included but not reported. See Data Appendix and text for variable definitions. Robust standard errors in parentheses. *p*-values corrected for multiple hypothesis testing are found in Appendix Table 9. *, **, *** indicate significance at the 10, 5, or 1% level.

Table 7: Time Spent, Communication, and Satisfaction with Doctor

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|----------------|----------------------------------|--|-------------------------------|---------------------------------------|---------------------------------|---|---------------------------------|
| | <i>Time</i> | | <i>Communication</i> | | | <i>Satisfaction</i> | |
| | <i>Length Visit, Minutes</i> | <i>Length Visit, Test Controls</i> | <i>Subject Talk to MD</i> | <i>Doctor Notes About Subject</i> | <i>Non-Preventive Notes</i> | <i>Subject Rating of Experience</i> | <i>Subject Recommend MD</i> |
| Black Doctor | 4.384*** (0.897) | 0.981 (0.717) | 0.100*** (0.039) | 0.111*** (0.038) | 0.089*** (0.026) | -0.019 (0.048) | -0.0005 (0.010) |
| \$5 Incentive | 3.275*** (1.126) | 0.949 (0.885) | -0.072 (0.048) | 0.055 (0.047) | 0.001 (0.033) | 0.029 (0.065) | 0.009 (0.013) |
| \$10 Incentive | 0.617 (1.088) | -0.357 (0.861) | -0.085* (0.047) | 0.016 (0.046) | -0.016 (0.031) | 0.078 (0.056) | 0.010 (0.012) |
| Control Mean | 20.53 | 20.53 | 0.35 | 0.32 | 0.08 | 4.80 | 0.99 |
| Observations | 498 | 498 | 637 | 637 | 637 | 574 | 597 |

Note: Table reports OLS estimates of Equation 3. The outcome variables include time the subject spent with the doctor (Columns (1) and (2)), communication (Columns (3)–(5)), and subject feedback (Columns (6) and (7)). Observation count varies due to missing values. Results from adding test controls to Columns (3)–(7) can be found in Appendix Table 8. See Data Appendix and text for variable definitions. Control mean refers to subjects randomized to a non-black doctor. Robust standard errors in parentheses. *p*-values corrected for multiple hypothesis testing are found in Appendix Table 9. *, **, *** indicate significance at the 10, 5, or 1% level.

Table 8: Perceptions of Doctors among Black and White Male Respondents

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|------------------|--------------------------|---------------------|--------------------|--------------------------|---------------------|-------------------|-----------------------------|---------------------|----------------------|----------------------------------|
| | <i>Quality</i> | | | <i>Communication</i> | | | <i>Access</i> | | | <i>Communication vs. Quality</i> |
| | Which MD most qualified? | | | Which MD understands me? | | | Which MD available near me? | | | Concordance |
| | Black MD | White MD | Concordance | Black MD | White MD | Concordance | Black MD | White MD | Concordance | Concordance |
| Black Respondent | 0.350*** (0.025) | | -0.055* (0.030) | 0.531*** (0.024) | | -0.001 (0.029) | 0.241*** (0.024) | | -0.255*** (0.029) | -0.028 (0.025) |
| White Respondent | | 0.273*** (0.029) | | | 0.479*** (0.027) | | | 0.175*** (0.030) | | |
| Communication | | | | | | | | | | 0.171*** (0.014) |
| Mean | 0.11 | 0.27 | 0.54 | 0.12 | 0.19 | 0.69 | 0.11 | 0.43 | 0.62 | 0.54 |
| R-squared | 0.12 | 0.08 | 0.03 | 0.23 | 0.24 | 0.04 | 0.09 | 0.04 | 0.07 | 0.06 |
| Observations | 1,490 | 1,490 | 1,490 | 1,490 | 1,490 | 1,490 | 1,490 | 1,490 | 1,490 | 2,980 |

Note: Columns (1), (2), (4), (5), (7), and (8) report OLS estimates of Equation 4a, testing whether respondents have a preference for doctors of the same race with respect to three domains of healthcare: quality, communication, and access, respectively. Columns (3), (6), and (9) report OLS estimates of Equation 4b testing whether preference for own race varies across black and white respondents. Column (10) reports OLS estimates of Equation 4c comparing preference across domain and race. The comparison group mean is the average white respondents who prefer black doctors in Columns (1), (4), and (7); the average black respondents who prefer white doctors in Columns (2), (5), and (8); the average white respondents who prefer white doctors in Columns (3), (6), and (9); and the average white respondents who select concordance in regards to quality in Column (10). See Data Appendix and text for variable definitions. All specifications include categorical controls for age, education, and household income levels. Robust standard errors in parentheses for Columns (1)–(9). Clustered standard errors in parentheses for panel analysis (Column (10)) as treatment assignment is correlated within subject. *, **, *** indicate significance at the 10, 5, or 1% level.

Table 9: Take-Up with Alternative Concordance Measures

| | (1) | (2) | (3) | (4) | (5) | (6) |
|-------------------------|---------------------|----------|----------------------|----------|------------------|----------|
| <i>X</i> = | <i>Age, 5 Years</i> | | <i>Age, 10 Years</i> | | <i>Education</i> | |
| <i>X</i> | -0.044* | -0.029 | -0.013 | -0.023 | -0.005 | 0.015 |
| | (0.026) | (0.027) | (0.023) | (0.026) | (0.052) | (0.090) |
| <i>X</i> * Black Doctor | | -0.044 | | -0.008 | | -0.080 |
| | | (0.049) | | (0.044) | | (0.110) |
| Black Doctor | | 0.159*** | | 0.153*** | | 0.153*** |
| | | (0.026) | | (0.030) | | (0.025) |
| Control Mean | 0.17 | 0.09 | 0.16 | 0.09 | 0.15 | 0.08 |
| Observations | 620 | 620 | 620 | 620 | 556 | 556 |

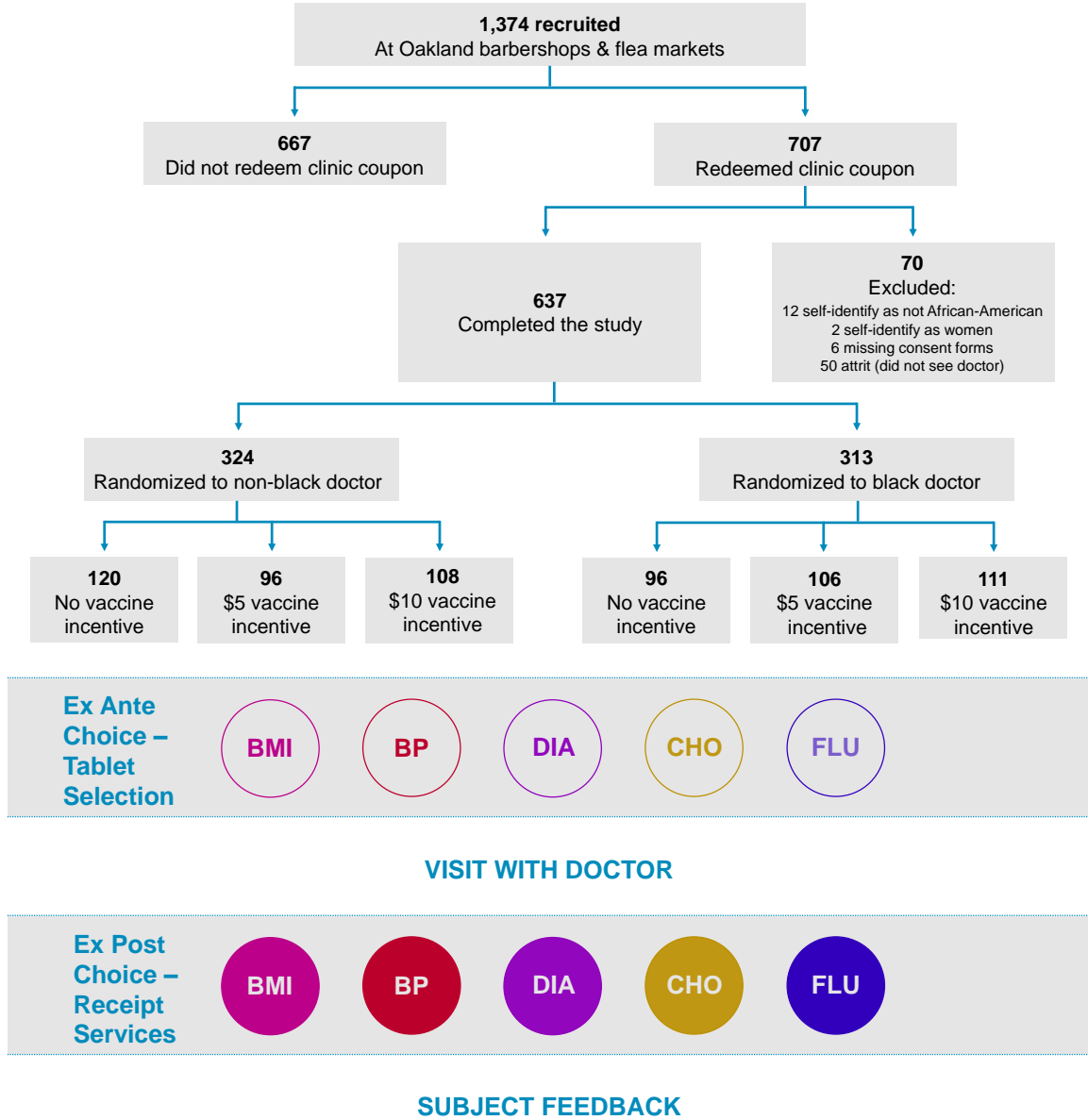
Note: Table reports OLS estimates of Equation 3. The outcome is the delta share of the four non-incentivized preventives selected (blood pressure, body mass index, cholesterol, and diabetes). Columns (1) and (2) explore age concordance (i.e. doctor and subject born within five years of each other), Columns (3) and (4) examine concordance within a wider age window (i.e. doctor and subject born within 10 years of each other), and Columns (5) and (6) explore concordance across educational attainment (i.e. subject has at least a bachelor of arts degree). Control mean refers to subjects randomized to a discordant doctor in Columns (1), (3), and (5) and a discordant and non-black doctor in Columns (2), (4), and (6). Robust standard errors in parentheses. *p*-values corrected for multiple hypothesis testing are found in Appendix Table 9. *, **, *** indicate significance at the 10, 5, or 1% level.

Table 10: Examining Doctor Effectiveness

| | (1) | (2) | (3) | (4) |
|-----------------------------|---------------------|------------------|-------------------|--------------------|
| <i>Doctor Fixed Effects</i> | | | | |
| Black Doctor | | | | 0.162** (0.064) |
| Experience | | 0.002 (0.004) | 0.002 (0.004) | 0.001 (0.002) |
| Medical School Rank | -0.001 (0.001) | | -0.001 (0.001) | -0.002* (0.001) |
| Constant | 0.140*** (0.044) | 0.094 (0.055) | 0.112 (0.066) | 0.074 (0.061) |
| R-squared | 0.034 | 0.033 | 0.061 | 0.418 |
| Observations | 14 | 14 | 14 | 14 |

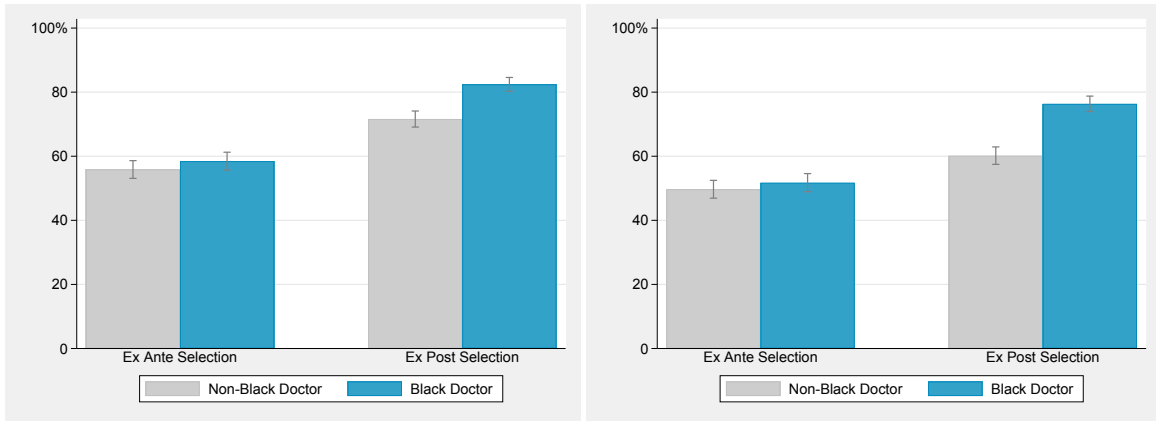
Note: Table reports OLS estimates. The outcome variable is doctor fixed effects. See Data Appendix and text for further details on the baseline doctor characteristics. Robust standard errors in parentheses. *, **, *** indicate significance at the 10, 5, or 1% level.

Figure 1: Study Design and Flow



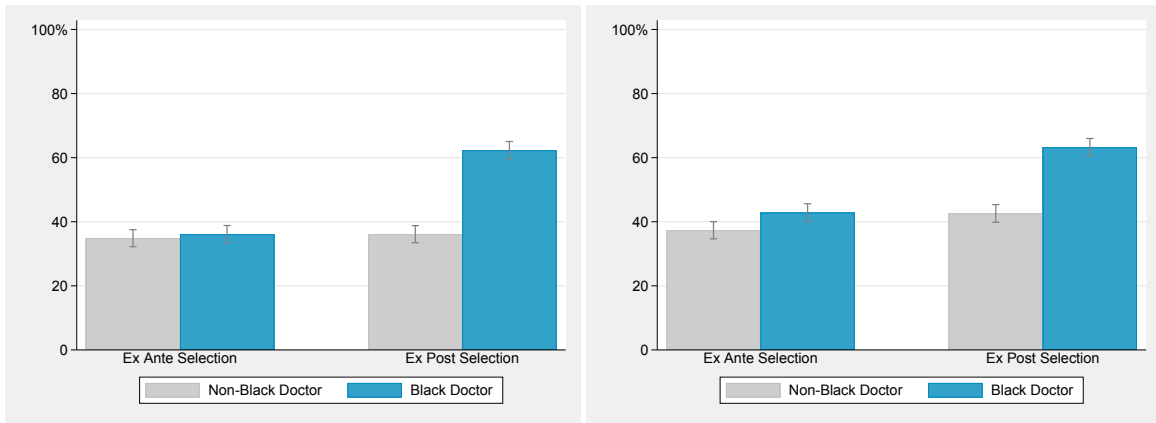
Note: Two-stage cross-randomization design and flow of subjects from recruitment through clinical encounter. Note that 70 subjects were randomized but are not included in the analysis study either because they did not meet criteria (i.e. they self-identified as a different race/ethnicity or as a female, were underage, or did not consent) or they left before the clinic encounter.

Figure 2: Demand for Preventives



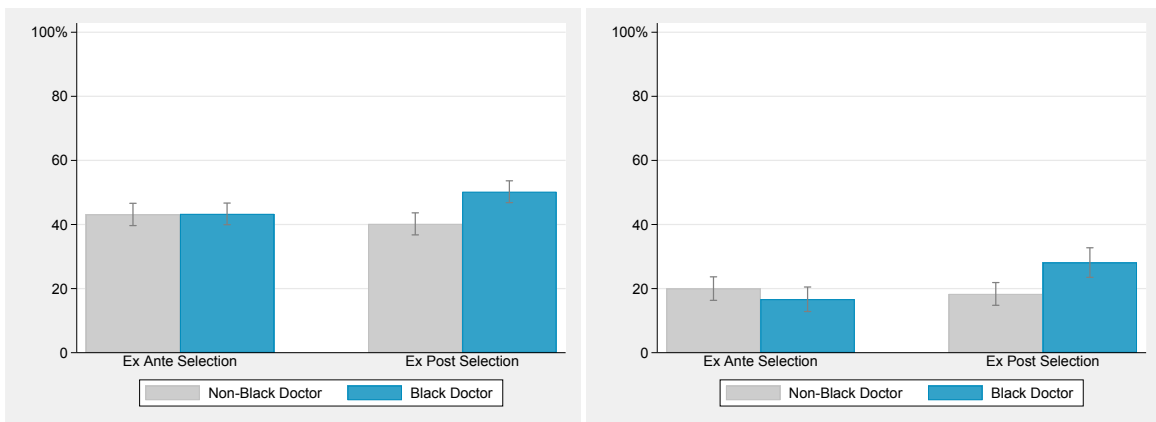
(a) Blood Pressure

(b) BMI



(c) Cholesterol

(d) Diabetes

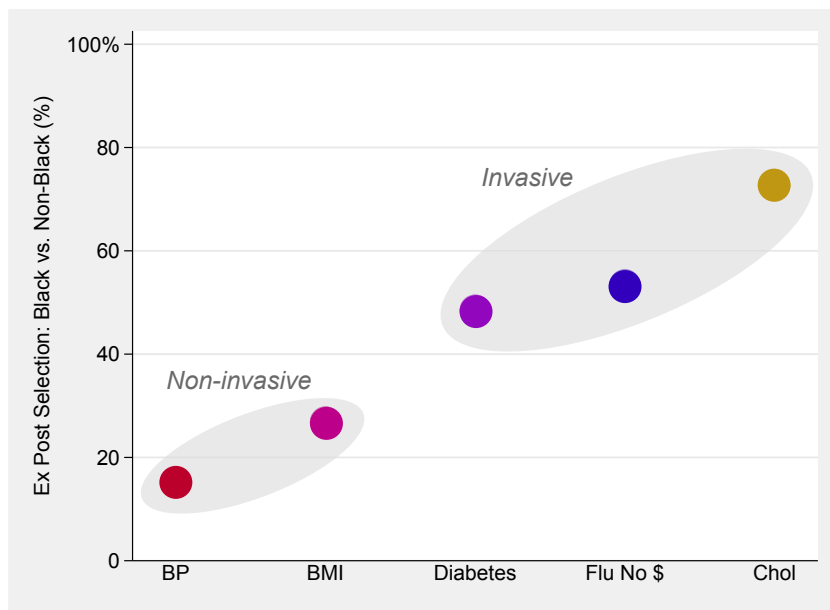


(e) Flu Shot: With Incentive

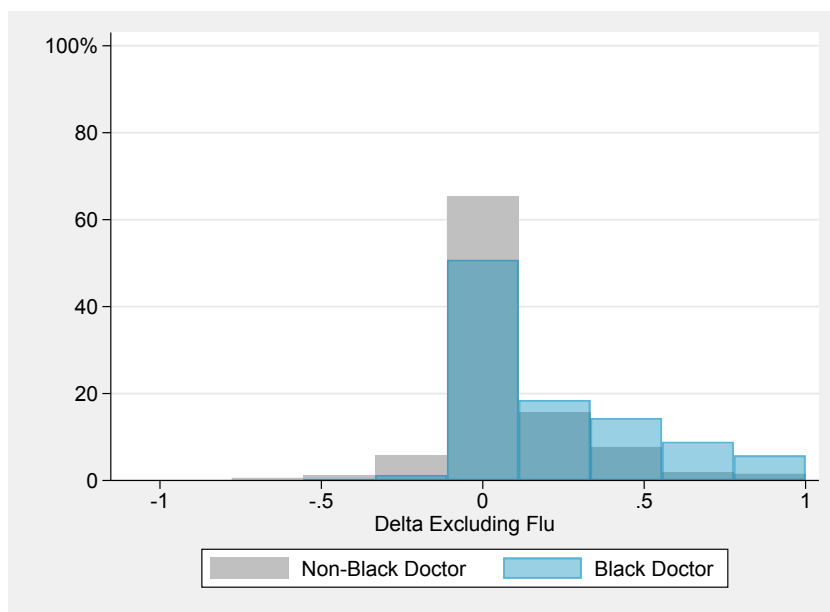
(f) Flu Shot: Without Incentive

Note: Ex ante and ex post selection for preventives by randomized doctor race.

Figure 3: Delta and Ex Post Differences, Black vs. Non-Black Doctors



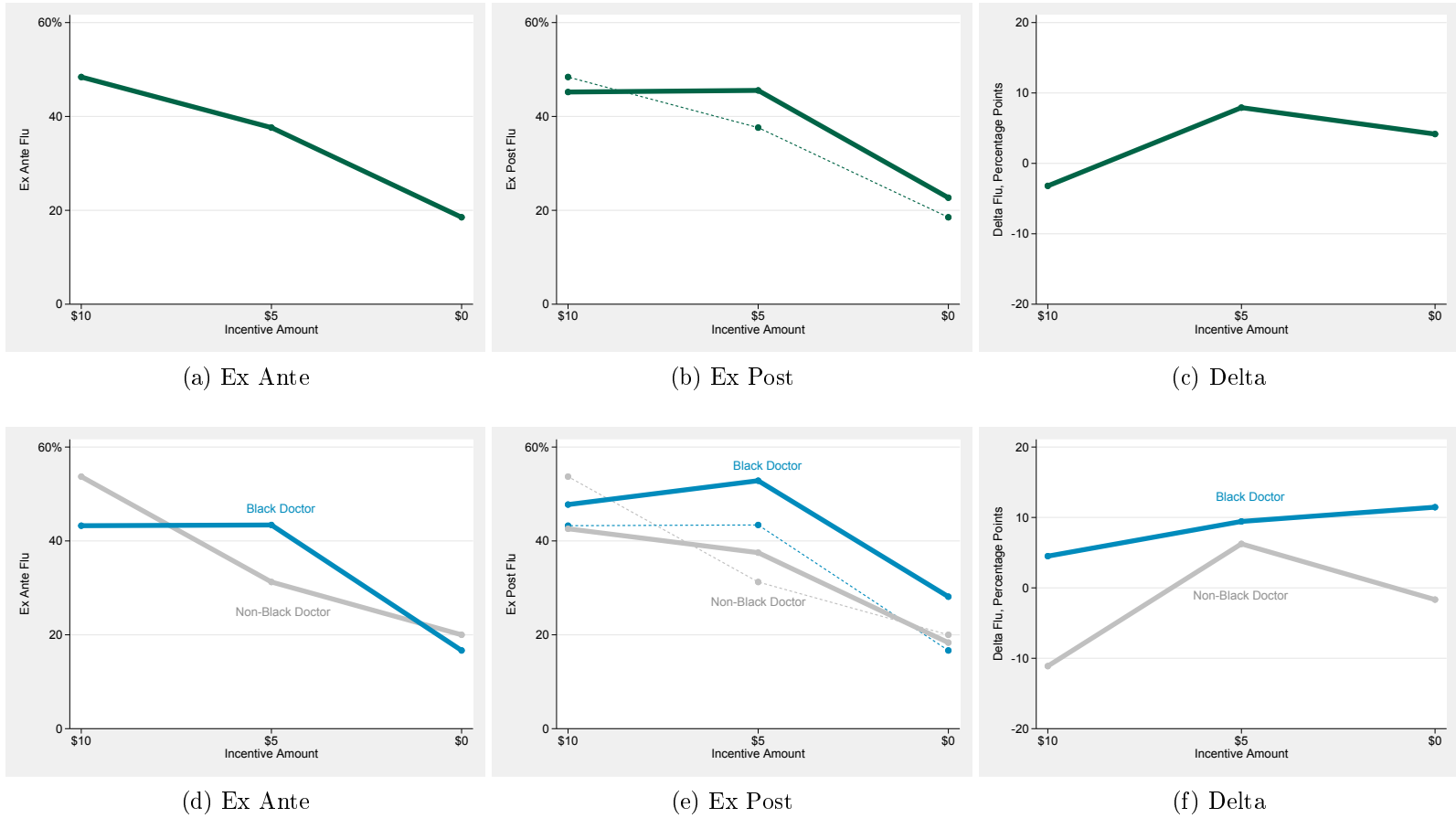
(a) Ex Post % Differences by Preventives



(b) Delta for Black vs. Non-Black Doctors

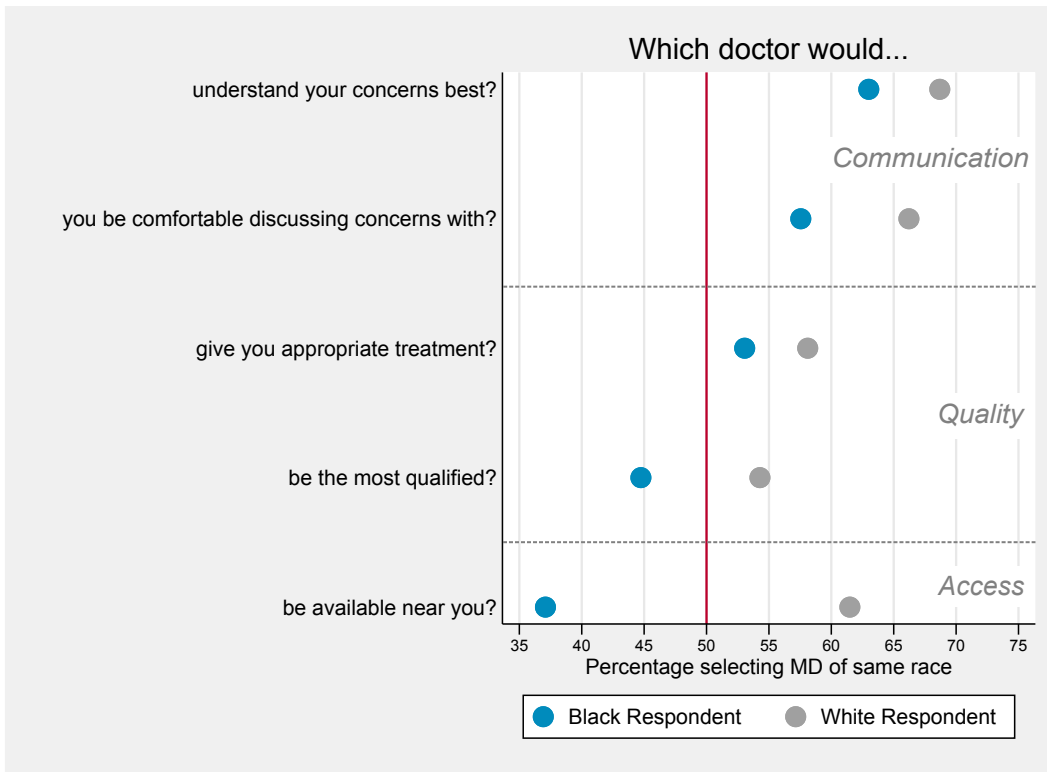
Note: Panel (A) plots the percent difference between black doctors vs. non-black doctors in ex post demand by preventive. Note that the percent difference in demand for the flu with an incentive (not shown) is equal to about 25%. Panel (B) plots the delta distribution (ex post - ex ante) for the four non-incentivized preventives.

Figure 4: Flu Vaccination Demand



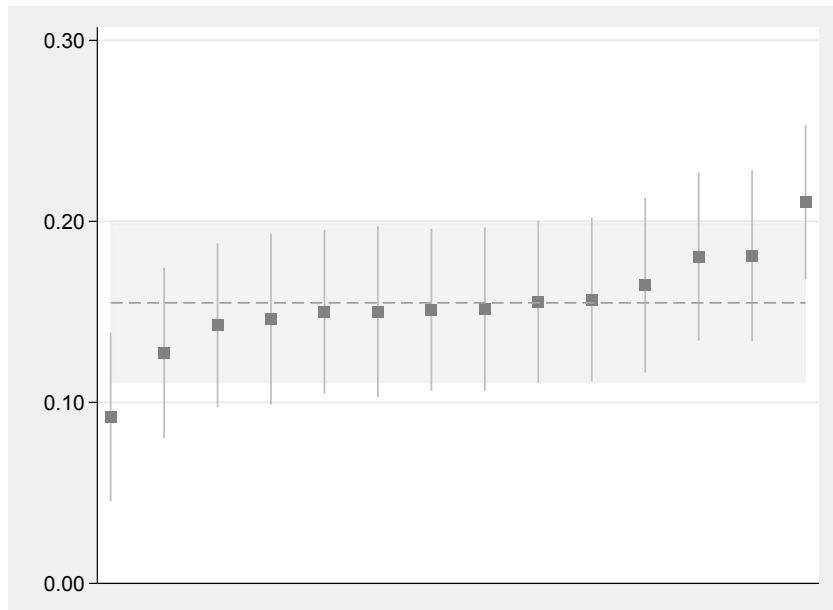
Note: Flu vaccination demand by treatment arm and experimental stage. Dashed lines indicate ex ante demand in Panels (b) and (e).

Figure 5: Non-Experimental Preference for Concordance



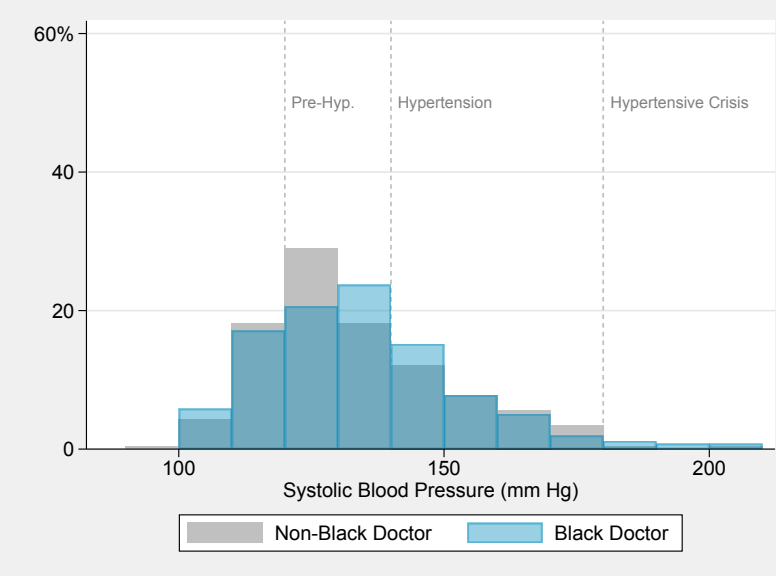
Note: Figure plots the percent of black and white survey respondents who select a doctor of the same race in response to various questions. Choice set included black, white, or Asian male doctors.

Figure 6: Plot of Leave-One-Out Estimates

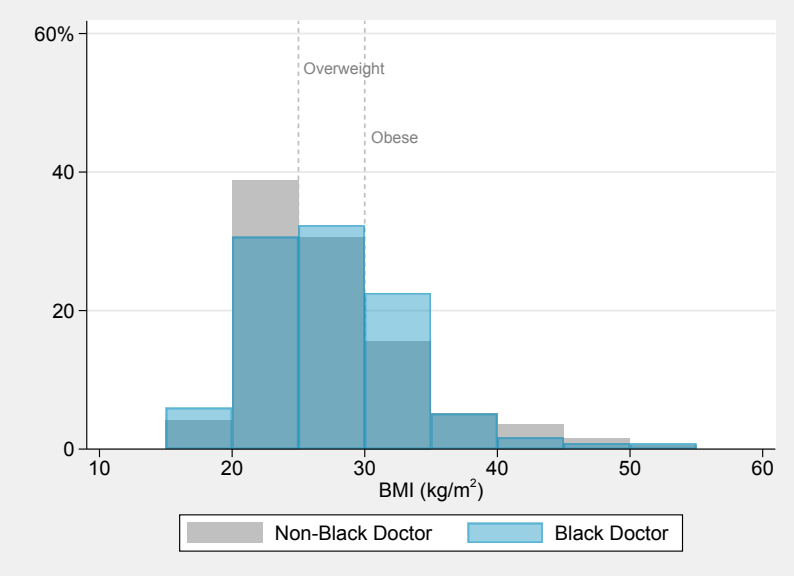


Note: Figure plots the leave-one-out coefficients for the main treatment effect of black doctor and their 95% confidence intervals. The treatment effect reported in Table 3 Panel (B) Column (9) (dashed line) and 95% confidence intervals (shaded area) are drawn for reference.

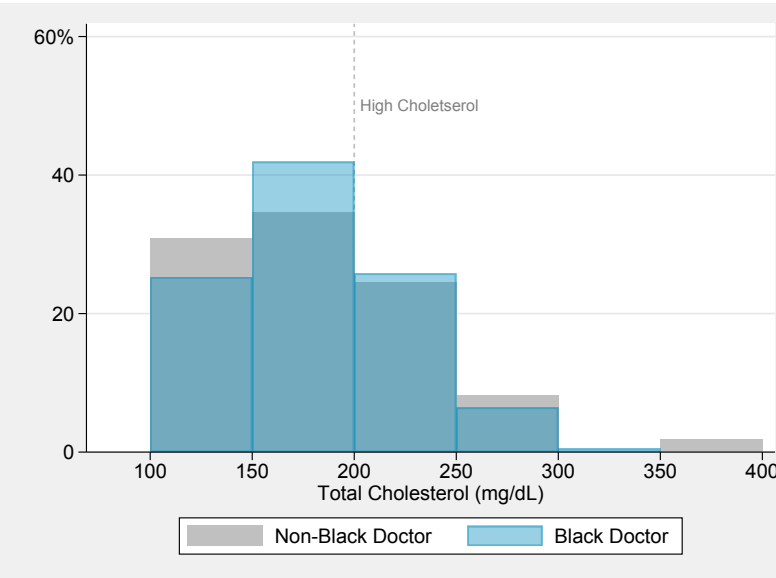
Figure 7: Distribution of Medical Screening Results



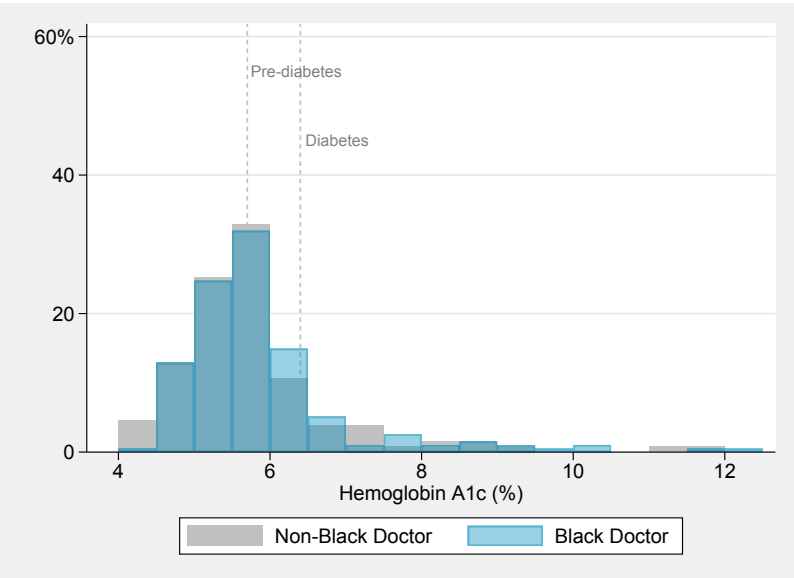
(a) Blood Pressure: Systolic



(b) Body Mass Index

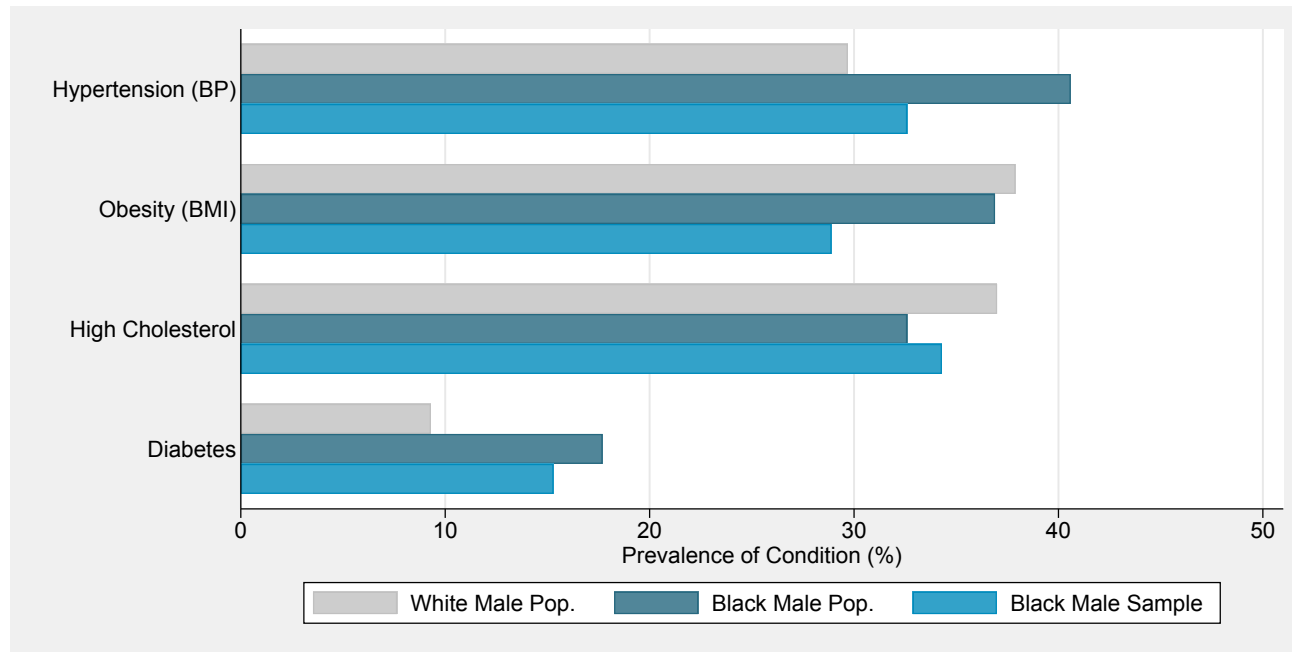


(c) Cholesterol



(d) Diabetes

Figure 8: Health of Study Sample vs U.S. Population



Note: Figure plots the percentage of each population group diagnosed with the listed conditions. Hypertension is defined as a systolic blood pressure value greater or equal to 140 mm Hg, obesity as a BMI greater or equal to 30 kg/m², high cholesterol as a cholesterol value greater or equal to 200 mg/dL, and diabetes as an A1c value greater or equal to 6.5%. Study sample values are for subjects who opted to receive a screening. Values for the U.S. population are from Fryar et al. (2017), Hales et al. (2017), and CDC (2017b, 2017c).

Appendix Table 1: Separate Balance Tests

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|-------------------------|------------------------------|-------------------|----------|--------------------------------------|--------------------|--------------------|---------------|----------------|----------|
| | <i>Doctor Randomization</i> | | | <i>Incentive Level Randomization</i> | | | | | |
| | <i>Mean</i> <i>(S.D.)</i> | <i>Black MD</i> | <i>N</i> | <i>Mean</i> <i>(S.D.)</i> | <i>\$5</i> | <i>\$10</i> | <i>F-test</i> | <i>p-value</i> | <i>N</i> |
| Self-Reported Health | 0.65 (0.48) | 0.068* (0.039) | 563 | 0.73 (0.45) | -0.027 (0.047) | -0.092* (0.048) | 1.934 | 0.146 | 563 |
| Any Health Problem | 0.62 (0.49) | -0.025 (0.040) | 614 | 0.61 (0.49) | -0.019 (0.049) | 0.014 (0.048) | 0.230 | 0.795 | 614 |
| ER Visits | 1.95 (3.65) | -0.406 (0.285) | 511 | 1.60 (2.97) | 0.099 (0.332) | 0.371 (0.358) | 0.552 | 0.576 | 511 |
| Nights Hospital | 1.39 (4.42) | 0.433 (0.572) | 511 | 2.08 (8.85) | -1.175* (0.702) | -0.312 (0.803) | 2.849 | 0.059 | 511 |
| Medical Mistrust | 1.64 (0.74) | -0.031 (0.061) | 611 | 1.62 (0.74) | 0.071 (0.076) | -0.054 (0.072) | 1.437 | 0.238 | 611 |
| Has Primary MD | 0.63 (0.48) | -0.020 (0.042) | 537 | 0.60 (0.49) | 0.012 (0.052) | 0.034 (0.051) | 0.223 | 0.800 | 537 |
| Uninsured | 0.28 (0.45) | -0.006 (0.040) | 517 | 0.27 (0.45) | 0.0005 (0.049) | 0.028 (0.048) | 0.216 | 0.806 | 517 |
| Age | 44.61 (14.53) | -0.286 (1.167) | 620 | 44.84 (14.28) | -0.966 (1.437) | -0.183 (1.408) | 0.251 | 0.778 | 620 |
| Married | 0.14 (0.35) | 0.023 (0.030) | 586 | 0.17 (0.38) | -0.017 (0.037) | -0.037 (0.036) | 0.534 | 0.586 | 586 |
| Unemployed | 0.30 (0.46) | 0.011 (0.039) | 570 | 0.29 (0.46) | 0.005 (0.047) | 0.032 (0.047) | 0.255 | 0.775 | 570 |
| ≤ High School Education | 0.62 (0.49) | 0.022 (0.041) | 556 | 0.61 (0.49) | 0.044 (0.050) | 0.027 (0.050) | 0.379 | 0.684 | 556 |
| Low Income | 0.45 (0.50) | -0.002 (0.042) | 571 | 0.45 (0.50) | 0.018 (0.051) | -0.019 (0.050) | 0.258 | 0.773 | 571 |

Note: Table reports balance tests separately by doctor, Column (2), and incentive, Columns (5) and (6). Control mean in Column (1) refers to those randomized to a non-black doctor. Control mean in Column (4) refers to those randomized to no incentive. Observation count varies due to missing responses in the baseline survey. See Data Appendix for variable definitions. Robust standard errors in parentheses. *, **, *** indicate significance at the 10, 5, or 1% level.

Appendix Table 2: Take-Up with Controls, Extended

| | (1) | (2) | (3) | (4) |
|-------------------------|---|---|---------------------------------------|---------------------------------------|
| | <i>Ex Ante Share, Excluding Flu</i> | <i>Ex Post Share, Excluding Flu</i> | <i>Delta Share, Excluding Flu</i> | <i>Delta Share, Including Flu</i> |
| Black Doctor | 0.028 (0.029) | 0.182*** (0.028) | 0.153*** (0.022) | 0.145*** (0.020) |
| \$5 Incentive | 0.030 (0.035) | 0.054 (0.035) | 0.024 (0.027) | 0.024 (0.024) |
| \$10 Incentive | 0.002 (0.035) | -0.005 (0.034) | -0.006 (0.026) | -0.023 (0.023) |
| Age | -0.003 (0.006) | -0.006 (0.006) | -0.003 (0.005) | -0.003 (0.004) |
| Age Squared | 0.00003 (0.00007) | 0.00008 (0.00007) | 0.00005 (0.00005) | 0.00004 (0.00005) |
| ≤ High School Education | -0.106*** (0.035) | -0.058* (0.033) | 0.048* (0.025) | 0.032 (0.022) |
| Low Income | -0.166*** (0.033) | -0.089*** (0.031) | 0.076*** (0.025) | 0.055** (0.022) |
| Self-Assessed Health | 0.021 (0.035) | -0.003 (0.034) | -0.024 (0.026) | -0.019 (0.023) |
| Has Primary MD | -0.069** (0.034) | -0.106*** (0.032) | -0.037 (0.027) | -0.047** (0.023) |
| Uninsured | 0.001 (0.040) | -0.015 (0.038) | -0.016 (0.028) | -0.020 (0.025) |
| Month Fixed Effects | Yes | Yes | Yes | Yes |
| Control Mean | 0.44 | 0.53 | 0.08 | 0.05 |
| Observations | 637 | 637 | 637 | 637 |

Note: Table reports OLS estimates of Equation 3 with controls and associated coefficients. Missing values of the controls coded as -9 and a missing indicator included when relevant. The outcome varies by column heading. Ex ante refers to demand upon viewing assigned doctor photo on tablet, but before meeting doctor in person. Ex post refers to demand after meeting doctor in person. Delta share is ex post - ex ante demand: Column (3) excludes the flu test, Column (4) includes it. See text for further details. Control mean refers to subjects randomized to a non-black doctor for Columns (1)–(3) and to subjects randomized to a non-black doctor and no incentive for Column (4). Robust standard errors in parentheses. *, **, *** indicate significance at the 10, 5, or 1% level.

Appendix Table 3: Heterogeneity by Demographics, Hassle Costs, and Medical Care Experience

| | (1) | (2) | (3) |
|---|-----------------------|--------------------------------|-------------------------|
| <i>Outcome = Ex Ante Share Non-Incentivized Preventives</i> | | | |
| PANEL A: Demographics | | | |
| <i>X =</i> | <i>Low Income</i> | <i>≤ High School Education</i> | <i>Younger than 40</i> |
| Black Doctor * <i>X</i> | 0.016 (0.061) | 0.072 (0.067) | -0.067 (0.064) |
| <i>X</i> | -0.204*** (0.043) | -0.196*** (0.046) | 0.071 (0.045) |
| Black Doctor | 0.023 (0.044) | -0.013 (0.055) | 0.053 (0.039) |
| Observations | 571 | 556 | 620 |
| PANEL B: Hassle Costs | | | |
| <i>X =</i> | <i>Long Wait Time</i> | <i>High Congestion</i> | <i>Long Commute</i> |
| Black Doctor * <i>X</i> | -0.043 (0.075) | 0.076 (0.071) | 0.054 (0.061) |
| <i>X</i> | -0.102* (0.053) | -0.187*** (0.049) | -0.166*** (0.042) |
| Black Doctor | 0.059 (0.044) | 0.010 (0.050) | 0.018 (0.040) |
| Observations | 451 | 451 | 618 |
| PANEL C: Medical Care Experience | | | |
| <i>X =</i> | <i>ER Visits</i> | <i>No Recent Screening</i> | <i>Medical Mistrust</i> |
| Black Doctor * <i>X</i> | -0.014 (0.009) | 0.084 (0.092) | -0.025 (0.042) |
| <i>X</i> | 0.001 (0.007) | -0.096 (0.072) | 0.020 (0.030) |
| Black Doctor | 0.034 (0.039) | 0.014 (0.034) | 0.058 (0.075) |
| Observations | 511 | 604 | 611 |

Note: Table reports OLS estimates from a modified version of Equation 3 including interactions between black doctor and certain baseline characteristics. The outcome variable for every specification is the ex ante demand for the share of the four non-incentivized preventives selected (blood pressure, body mass index, cholesterol, and diabetes). Observation count varies due to missing responses in the baseline survey. Indicators for incentive levels are included but not reported. See Data Appendix and text for variable definitions. Robust standard errors in parentheses. *, **, *** indicate significance at the 10, 5, or 1% level.

Appendix Table 4: Preventive Demand, Doctor Only

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|----------------|-----------------------|---------------------|---------------------|------------------------|---------------------|---------------------|---|---------------------|---------------------|
| | Ex Ante | Ex Post | Delta | Ex Ante | Ex Post | Delta | Ex Ante | Ex Post | Delta |
| PANEL A | | | | | | | | | |
| | <i>Blood Pressure</i> | | | <i>BMI</i> | | | <i>Diabetes</i> | | |
| Black Doctor | 0.026 (0.039) | 0.108*** (0.033) | 0.082** (0.034) | 0.021 (0.040) | 0.162*** (0.036) | 0.141*** (0.033) | 0.055 (0.039) | 0.207*** (0.039) | 0.152*** (0.029) |
| Control Mean | 0.56 | 0.72 | 0.16 | 0.50 | 0.60 | 0.11 | 0.37 | 0.43 | 0.05 |
| Observations | 637 | 637 | 637 | 637 | 637 | 637 | 637 | 637 | 637 |
| PANEL B | | | | | | | | | |
| | <i>Cholesterol</i> | | | <i>Flu Vaccination</i> | | | <i>Share of All Non-Incentivized Tests (Excludes Flu)</i> | | |
| Black Doctor | 0.012 (0.038) | 0.262*** (0.038) | 0.250*** (0.032) | 0.006 (0.038) | 0.114*** (0.038) | 0.108*** (0.034) | 0.028 (0.030) | 0.185*** (0.029) | 0.156*** (0.022) |
| Control Mean | 0.35 | 0.36 | 0.01 | 0.35 | 0.32 | -0.03 | 0.44 | 0.53 | 0.08 |
| Observations | 637 | 637 | 637 | 637 | 637 | 637 | 637 | 637 | 637 |

Note: Table reports OLS estimates of Equation 3, without including indicators for the incentive levels. The outcome varies across columns (see headings). Ex ante refers to demand upon viewing assigned doctor photo on tablet, but before meeting doctor in person. Ex post refers to demand after meeting doctor in person. Delta is ex post - ex ante demand. Control mean refers to subjects randomized to a non-black doctor. See text for further details. Robust standard errors in parentheses. *, **, *** indicate significance at the 10, 5, or 1% level.

Appendix Table 5: Heterogeneity by Increased Risk

| | (1) | (2) | (3) | (4) | (5) | (6) |
|-------------------------|---|---------------------|---------------------|---------------------------------|---------------------|---------------------|
| | Ex Ante | Ex Post | Delta | Ex Ante | Ex Post | Delta |
| <i>X</i> = | <i>Increased Risk, High Cholesterol</i> | | | <i>Increased Risk, Diabetes</i> | | |
| Black Doctor * <i>X</i> | 0.039 (0.088) | 0.011 (0.090) | -0.028 (0.076) | -0.160 (0.184) | -0.150 (0.192) | 0.010 (0.140) |
| <i>X</i> | 0.018 (0.062) | 0.051 (0.062) | 0.034 (0.049) | 0.031 (0.129) | -0.020 (0.129) | -0.050 (0.095) |
| Black Doctor | -0.022 (0.076) | 0.248*** (0.078) | 0.269*** (0.066) | 0.058 (0.043) | 0.199*** (0.043) | 0.141*** (0.032) |
| Observations | 620 | 620 | 620 | 561 | 561 | 561 |

Note: Table reports OLS estimates from a modified version of Equation 3 including interactions between black doctor and an indicator for whether the subject was at increased risk for high cholesterol or diabetes. See Data Appendix and text for details on the increased-risk groups. Ex ante refers to demand upon viewing assigned doctor photo on tablet, but before meeting doctor in person. Ex post refers to demand after meeting doctor in person. Delta is ex post - ex ante demand. Columns (1)–(3) report the demand for the cholesterol screening. Columns (4)–(6) report the demand for the diabetes screening. Observation count varies due to missing responses in the baseline survey. See Data Appendix and text for variable definitions. Robust standard errors in parentheses. *, **, *** indicate significance at the 10, 5, or 1% level.

Appendix Table 6: Doctor Characteristics

| | (1) | (2) | (3) | (4) |
|-----------------|------------|-------------------|----------------------------|------------------|
| | <i>Age</i> | <i>Experience</i> | <i>Medical School Rank</i> | <i>Internist</i> |
| Black Mean | 43.50 | 15.17 | 24 | 0.67 |
| Non-Black Mean | 41.13 | 12.25 | 11 | 1.00 |
| <i>p</i> -value | .604 | .741 | .846 | .089 |
| Observations | 14 | 14 | 14 | 14 |

Note: Table reports mean doctor characteristics by race. See Data Appendix and text for variable definitions. Wilcoxon rank-sum test *p*-values are reported in row 3.

Appendix Table 7: Fixed Effects and Alternative Samples

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|-------------------------------------|--------------------------|---------------------|---------------------|----------------------------------|---------------------|---------------------|-----------------------------|---------------------|---------------------|
| | Ex Ante | Ex Post | Delta | Ex Ante | Ex Post | Delta | Ex Ante | Ex Post | Delta |
| PANEL A: FIXED EFFECTS | | | | | | | | | |
| | <i>Reception Officer</i> | | | <i>Study Date</i> | | | <i>Recruitment Location</i> | | |
| Black Doctor | 0.036 (0.031) | 0.191*** (0.029) | 0.155*** (0.022) | 0.032 (0.030) | 0.178*** (0.029) | 0.147*** (0.022) | 0.035 (0.030) | 0.185*** (0.029) | 0.150*** (0.022) |
| \$5 Incentive | 0.027 (0.037) | 0.060* (0.035) | 0.032 (0.027) | 0.032 (0.036) | 0.047 (0.035) | 0.015 (0.027) | 0.026 (0.037) | 0.045 (0.036) | 0.019 (0.028) |
| \$10 Incentive | -0.007 (0.036) | 0.008 (0.034) | 0.015 (0.025) | -0.005 (0.036) | -0.008 (0.034) | -0.003 (0.025) | -0.011 (0.036) | -0.011 (0.035) | 0.0002 (0.027) |
| Control Mean | 0.44 | 0.53 | 0.08 | 0.44 | 0.53 | 0.08 | 0.44 | 0.53 | 0.08 |
| Observations | 637 | 637 | 637 | 637 | 637 | 637 | 618 | 618 | 618 |
| PANEL B: ALTERNATIVE SAMPLES | | | | | | | | | |
| | <i>All Subjects</i> | | | <i>Without Assisted Subjects</i> | | | <i>Strict Specification</i> | | |
| Black Doctor | 0.023 (0.030) | 0.177*** (0.029) | 0.153*** (0.022) | 0.016 (0.031) | 0.178*** (0.029) | 0.162*** (0.023) | 0.031 (0.032) | 0.179*** (0.030) | 0.149*** (0.023) |
| \$5 Incentive | 0.038 (0.037) | 0.064* (0.035) | 0.026 (0.028) | 0.027 (0.038) | 0.064* (0.036) | 0.038 (0.028) | 0.033 (0.039) | 0.067* (0.037) | 0.035 (0.028) |
| \$10 Incentive | -0.002 (0.036) | 0.002 (0.034) | 0.004 (0.026) | -0.009 (0.037) | 0.004 (0.035) | 0.013 (0.026) | -0.023 (0.038) | -0.009 (0.037) | 0.014 (0.027) |
| Control Mean | 0.44 | 0.53 | 0.08 | 0.45 | 0.53 | 0.08 | 0.45 | 0.53 | 0.08 |
| Observations | 651 | 651 | 651 | 623 | 623 | 623 | 578 | 578 | 578 |

Note: Table reports OLS estimates of Equation 3, adding in various fixed effects (Panel A) or estimated on alternative data samples (Panel B). In Panel A, Columns (1)–(3) add in indicators for student reception officer; Columns (4)–(6) add in indicators for the date of the study; Columns (7)–(9) add in indicators for the location where the subject was recruited from. In Panel B, Columns (1)–(3) include all subjects, regardless if they met study criteria; Columns (4)–(6) remove observations where a reception officer assisted the subject because of issues of illiteracy or blindness; Columns (7)–(9) drop subjects who did not answer questions relating to race, age, or gender. The outcome is the share of the four non-incentivized preventives selected (blood pressure, body mass index, cholesterol, and diabetes) and the stage varies by column heading. Ex ante refers to demand upon viewing assigned doctor photo on tablet, but before meeting doctor in person. Ex post refers to demand after meeting doctor in person. Delta is ex post - ex ante demand. See Data Appendix and text for further details. Control mean refers to subjects randomized to a non-black doctor. Robust standard errors in parentheses. *, **, *** indicate significance at the 10, 5, or 1% level.

Appendix Table 8: Communication and Satisfaction with Doctor, Controlling for Testing

| | (1) | (2) | (3) | (4) | (5) |
|----------------|-------------------------------|---------------------------------------|---------------------------------|-----------------------|---------------------------------|
| | <i>Communication</i> | | | <i>Satisfaction</i> | |
| | <i>Subject Talk to MD</i> | <i>Doctor Notes About Subject</i> | <i>Non-Preventive Notes</i> | <i>Subject Rating</i> | <i>Subject Recommend MD</i> |
| Black Doctor | 0.094** (0.045) | 0.092** (0.044) | 0.069** (0.028) | -0.047 (0.049) | -0.007 (0.011) |
| \$5 Incentive | -0.082 (0.054) | 0.058 (0.054) | 0.007 (0.038) | -0.019 (0.077) | 0.005 (0.017) |
| \$10 Incentive | -0.080 (0.054) | 0.018 (0.053) | -0.048 (0.035) | 0.089 (0.059) | 0.010 (0.016) |
| Control Mean | 0.35 | 0.32 | 0.08 | 4.80 | 0.99 |
| Observations | 498 | 498 | 498 | 453 | 469 |

Note: Table reports OLS estimates of Equation 3, adding in fixed effects for each screening and controlling for the length of the clinic visit. The outcome variables include communication (Columns (1)–(3)) and subject feedback (Columns (4) and (5)). Observation count varies due to missing values. See Data Appendix and text for variable definitions. Control mean refers to subjects randomized to a non-black doctor. Robust standard errors in parentheses. *, **, *** indicate significance at the 10, 5, or 1% level.

Appendix Table 9: q-values on Significant Results

| <i>(Table 3)</i> | | <i>(Table 4)</i> | | <i>(Table 5)</i> | | <i>(Table 6)</i> | | <i>(Table 7)</i> | | <i>(Table 9)</i> | |
|------------------|----------------|------------------|----------------|------------------|----------------|-------------------|----------------|-------------------|----------------|-------------------|----------------|
| <i>BP</i> | 0.107 | <i>BP</i> | 0.103 | <i>Flu</i> | 0.113 | <i>Delta Sh.</i> | 0.087 | <i>Length</i> | 4.384 | <i>Delta Sh.</i> | -0.044 |
| — | {0.001} | — | {0.002} | — | {0.061} | — | {0.066} | — | {<0.001} | — | {0.090} |
| <i>Black MD</i> | {0.005} | <i>Black MD</i> | {0.008} | <i>\$5</i> | {0.158} | <i>Black * LI</i> | {0.170} | <i>Black MD</i> | {0.001} | <i>Age, 5 Yrs</i> | {0.203} |
| <i>BP</i> | 0.082 | <i>BP</i> | 0.073 | <i>Flu</i> | 0.337 | <i>Delta Sh.</i> | 0.113 | <i>Length</i> | 3.275 | <i>Delta Sh.</i> | 0.159 |
| — | {0.018} | — | {0.034} | — | {<0.001} | — | {<0.001} | — | {0.004} | — | {<0.001} |
| <i>Black MD</i> | {0.053} | <i>Black MD</i> | {0.094} | <i>\$10</i> | {0.001} | <i>Black MD</i> | {0.001} | <i>\$5</i> | {0.013} | <i>Black MD</i> | {0.001} |
| <i>BMI</i> | 0.161 | <i>BMI</i> | 0.158 | <i>Flu</i> | 0.155 | <i>Delta Sh.</i> | 0.192 | <i>Subj. Talk</i> | 0.100 | <i>Delta Sh.</i> | 0.153 |
| — | {<0.001} | — | {<0.001} | — | {0.073} | — | {<0.001} | — | {0.010} | — | {<0.001} |
| <i>Black MD</i> | {0.001} | <i>Black MD</i> | {0.001} | <i>Black *</i> | {0.179} | <i>Black MD</i> | {0.001} | <i>Black MD</i> | {0.030} | <i>Black MD</i> | {0.001} |
| <i>BMI</i> | 0.138 | <i>BMI</i> | 0.139 | <i>Flu</i> | 0.098 | <i>Delta Sh.</i> | 0.135 | <i>Subj. Talk</i> | -0.085 | <i>Delta Sh.</i> | 0.153 |
| — | {<0.001} | — | {<0.001} | — | {0.093} | — | {<0.001} | — | {0.069} | — | {<0.001} |
| <i>Black MD</i> | {0.001} | <i>Black MD</i> | {0.001} | <i>Black MD</i> | {0.208} | <i>Black MD</i> | {0.001} | <i>Black MD</i> | {0.176} | <i>Black MD</i> | {0.001} |
| <i>BMI</i> | 0.078 | <i>Chol.</i> | 0.075 | <i>Flu</i> | 0.192 | <i>Delta Sh.</i> | 0.179 | <i>MD Notes</i> | 0.111 | | |
| — | {0.072} | — | {0.086} | — | {0.002} | — | {0.001} | — | {0.004} | | |
| <i>\$5</i> | {0.179} | <i>\$5</i> | {0.198} | <i>\$5</i> | {0.007} | <i>Bl. * Wait</i> | {0.005} | <i>Black MD</i> | {0.014} | | |
| <i>Diabetes</i> | 0.085 | <i>Diabetes</i> | 0.091 | <i>Flu</i> | 0.243 | <i>Delta Sh.</i> | 0.111 | <i>Non-Prev.</i> | 0.089 | | |
| — | {0.077} | — | {0.049} | — | {<0.001} | — | {<0.001} | — | {<0.001} | | |
| <i>\$5</i> | {0.189} | <i>\$5</i> | {0.131} | <i>\$10</i> | {0.001} | <i>Black MD</i> | {0.001} | <i>Black MD</i> | {0.003} | | |
| <i>Diabetes</i> | 0.201 | <i>Diabetes</i> | 0.204 | <i>Flu</i> | 0.131 | <i>Delta Sh.</i> | 0.140 | | | | |
| — | {<0.001} | — | {<0.001} | — | {0.014} | — | {0.006} | | | | |
| <i>Black MD</i> | {0.001} | <i>Black MD</i> | {0.001} | <i>Black MD</i> | {0.043} | <i>Bl. * Con.</i> | {0.018} | | | | |
| <i>Diabetes</i> | 0.105 | <i>Diabetes</i> | 0.107 | <i>Flu</i> | -0.094 | <i>Delta Sh.</i> | 0.102 | | | | |
| — | {0.028} | — | {0.022} | — | {0.080} | — | {<0.001} | | | | |
| <i>\$5</i> | {0.080} | <i>\$5</i> | {0.065} | <i>\$10</i> | {0.193} | <i>Black MD</i> | {0.005} | | | | |
| <i>Diabetes</i> | 0.151 | <i>Diabetes</i> | 0.153 | | | <i>Delta Sh.</i> | 0.090 | | | | |
| — | {<0.001} | — | {<0.001} | | | — | {0.053} | | | | |
| <i>Black MD</i> | {0.001} | <i>Black MD</i> | {0.001} | | | <i>Bl. * Dri.</i> | {0.135} | | | | |
| <i>Chol.</i> | 0.260 | <i>Chol.</i> | 0.078 | | | <i>Delta Sh.</i> | 0.108 | | | | |
| — | {<0.001} | — | {0.081} | | | — | {<0.001} | | | | |
| <i>Black MD</i> | {0.001} | <i>\$5</i> | {0.193} | | | <i>Black MD</i> | {0.001} | | | | |

| | | | | | |
|------------------|----------------|------------------|----------------|--------------------|----------------|
| <i>Chol.</i> | 0.250 | <i>Chol.</i> | 0.262 | <i>Delta Sh.</i> | 0.012 |
| — | {<0.001} | — | {<0.001} | — | {0.049} |
| <i>Black MD</i> | {0.001} | <i>Black MD</i> | {0.001} | <i>Bl. * ER</i> | {0.131} |
| | | | | | |
| <i>Flu</i> | 0.192 | <i>Chol.</i> | 0.249 | <i>Delta Sh.</i> | 0.134 |
| — | {<0.001} | — | {<0.001} | — | {<0.001} |
| <i>\$5</i> | {0.001} | <i>Black MD</i> | {0.001} | <i>Black MD</i> | {0.001} |
| | | | | | |
| <i>Flu</i> | 0.299 | <i>Flu</i> | 0.181 | <i>Delta Sh.</i> | 0.146 |
| — | {<0.001} | — | {<0.001} | — | {0.029} |
| <i>\$10</i> | {0.001} | <i>\$5</i> | {0.001} | <i>Bl. * Scr.</i> | {0.083} |
| | | | | | |
| <i>Flu</i> | 0.100 | <i>Flu</i> | 0.299 | <i>Delta Sh.</i> | 0.122 |
| — | {0.008} | — | {<0.001} | — | {<0.001} |
| <i>Black MD</i> | {0.026} | <i>\$10</i> | {0.001} | <i>Black MD</i> | {0.001} |
| | | | | | |
| <i>Flu</i> | 0.221 | <i>Flu</i> | 0.103 | <i>Delta Sh.</i> | 0.061 |
| — | {<0.001} | — | {0.006} | — | {0.048} |
| <i>\$5</i> | {0.001} | <i>Black MD</i> | {0.020} | <i>Bl. * Mist.</i> | {0.131} |
| | | | | | |
| <i>Flu</i> | 0.219 | <i>Flu</i> | 0.205 | | |
| — | {<0.001} | — | {<0.001} | | |
| <i>\$10</i> | {0.001} | <i>\$5</i> | {0.001} | | |
| | | | | | |
| <i>Flu</i> | 0.108 | <i>Flu</i> | 0.208 | | |
| — | {0.001} | — | {<0.001} | | |
| <i>Black MD</i> | {0.005} | <i>\$10</i> | {0.001} | | |
| | | | | | |
| <i>Flu</i> | -0.080 | <i>Flu</i> | 0.109 | | |
| — | {0.051} | — | {0.001} | | |
| <i>\$10</i> | {0.132} | <i>Black MD</i> | {0.005} | | |
| | | | | | |
| <i>Delta Sh.</i> | 0.182 | <i>Flu</i> | -0.091 | | |
| — | {<0.001} | — | {0.028} | | |
| <i>Black MD</i> | {0.001} | <i>\$10</i> | {0.080} | | |
| | | | | | |
| <i>Delta Sh.</i> | 0.155 | <i>Delta Sh.</i> | 0.182 | | |
| — | {<0.001} | — | {<0.001} | | |
| <i>Black MD</i> | {0.001} | <i>Black MD</i> | {0.001} | | |
| | | | | | |
| | | <i>Delta Sh.</i> | 0.153 | | |
| | | — | {<0.001} | | |
| | | <i>Black MD</i> | {0.001} | | |

Note: Table reports *q*-values corrected for multiple hypothesis testing. Columns display significant results from each primary paper table. For each listing, coefficients are in row 1, unadjusted *p*-values are in row 2 in brackets, and adjusted *q*-values are in row 3 in bold brackets.

Appendix Table 10: MEPS Patient-Doctor Race

| | (1) | (2) | (3) | (4) |
|-------------------------|-----------------|-----------------|--------------------|-----------------|
| | <i>White MD</i> | <i>Black MD</i> | <i>Hispanic MD</i> | <i>Asian MD</i> |
| <i>White Patient</i> | 0.851 | 0.017 | 0.039 | 0.093 |
| <i>Black Patient</i> | 0.527 | 0.257 | 0.065 | 0.151 |
| <i>Hispanic Patient</i> | 0.381 | 0.029 | 0.439 | 0.151 |
| <i>Asian Patient</i> | 0.254 | 0.009 | 0.027 | 0.710 |

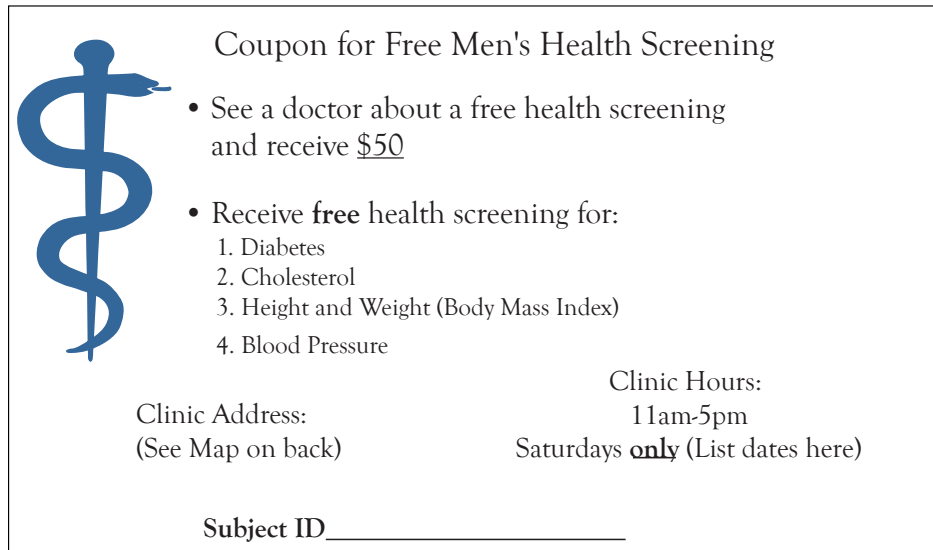
Note: Table reports the share of male respondents in the Medical Expenditure Panel Survey (MEPS) who have a doctor of a particular race or ethnicity. Gray cells highlight respondents with a concordant medical doctor.

Appendix Table 11: MEPS Patient-Doctor Concordance

| | (1) | (2) | (3) |
|--------------------------|---|-----------------------|--------------------------|
| | <i>Go To Doctor for Preventive Care</i> | <i>Doctor Listens</i> | <i>Understand Doctor</i> |
| Black Respondent | -0.008* (0.005) | -0.013 (0.012) | -0.015 (0.014) |
| Black MD | -0.012 (0.009) | -0.064** (0.025) | -0.066* (0.040) |
| Black Resp * Black MD | 0.020** (0.009) | 0.082*** (0.026) | 0.080* (0.041) |
| Any Insurance | 0.004 (0.003) | 0.051*** (0.010) | 0.022 (0.013) |
| Resp. Race Indicators | Yes | Yes | Yes |
| MD Race Indicators | Yes | Yes | Yes |
| Concordance Interactions | Yes | Yes | Yes |
| Control Mean | 0.99 | 0.94 | 0.97 |
| Observations | 32,189 | 22,118 | 7,649 |
| Years | 2005–2015 | 2005–2015 | 2011–2015 |

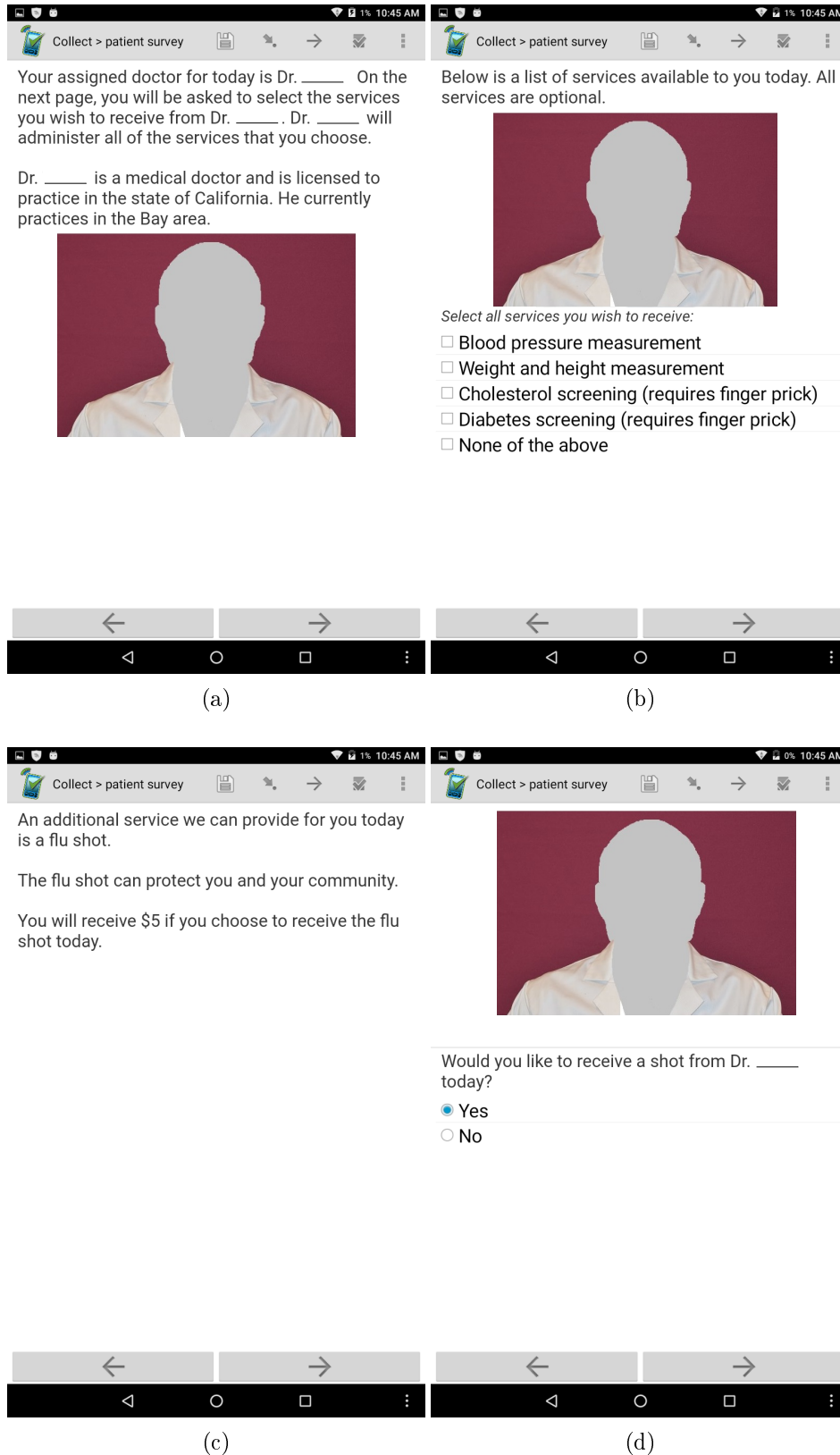
Note: Table reports WLS estimates using data from the Medical Expenditure Panel Survey and provided survey weights. The outcome variable varies by column and includes responses to the following survey questions: would the respondent go to their medical doctor for preventive care (Column (1)), whether the respondent said their doctor “usually” or “always” listens to them carefully (Column (2)), and whether the respondent said their doctor’s instructions regarding a specific illness or health condition were “usually” or “always” easy to understand (Column (3)). The sample is restricted to males over the age of 18 who identify as either white, black, Hispanic, or Asian and who report having a medical doctor who is white, black, Hispanic, or Asian. Control mean refers to white male subjects. Controls in every specification include indicator variables for: insurance, age greater than 65, education less than or equal to high school, and household income below 125% of the federal poverty line. In addition we include respondent race/ethnicity indicators, doctor race/ethnicity indicators, and concordant patient-doctor interactions for each race/ethnicity (not reported). Robust standard errors in parentheses. *, **, *** indicate significance at the 10, 5, or 1% level.

Appendix Figure 1: Clinic Coupon



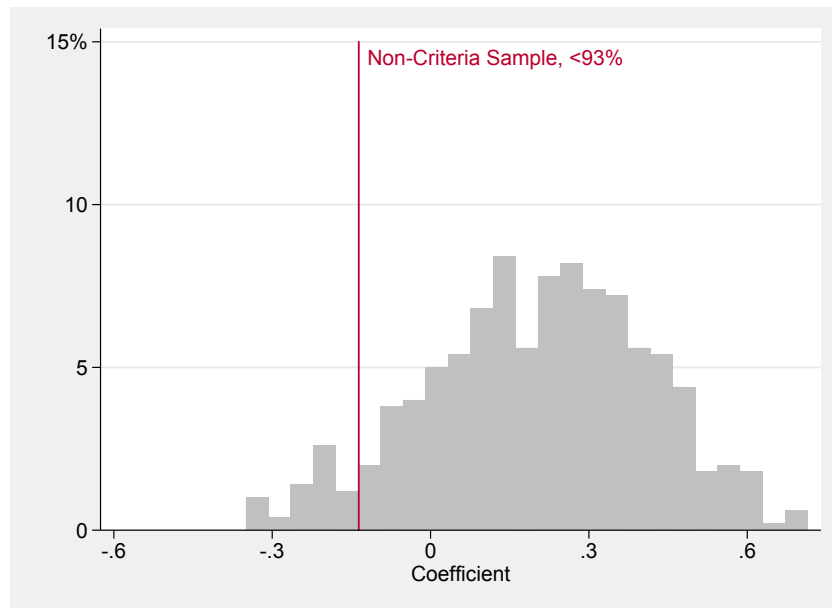
Note: Image of coupon subjects received in barbershops, which served as their ticket to the clinic.

Appendix Figure 2: Tablet Photos



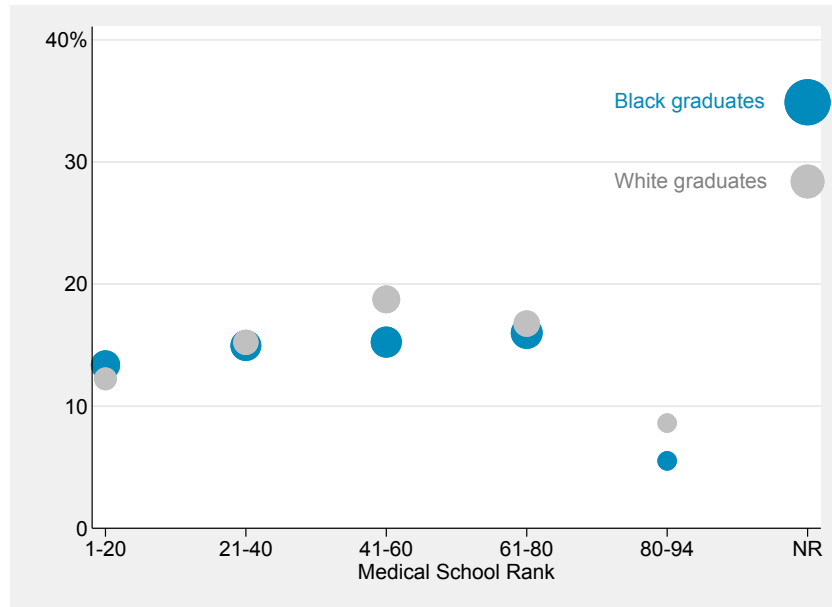
Note: Screenshots of clinic survey tablet: Panel (a) introduces subject's doctor; Panel (b) presents the non-incentivized screenings available; Panel (c) informs the subject about the flu shot and associated incentive (if applicable); Panel (d) asks the subject whether he would like to receive a flu vaccination. Screenshots not shown to scale; tablet screen was approximately 10 inches.

Appendix Figure 3: Permutation Test of Black Doctor Effect



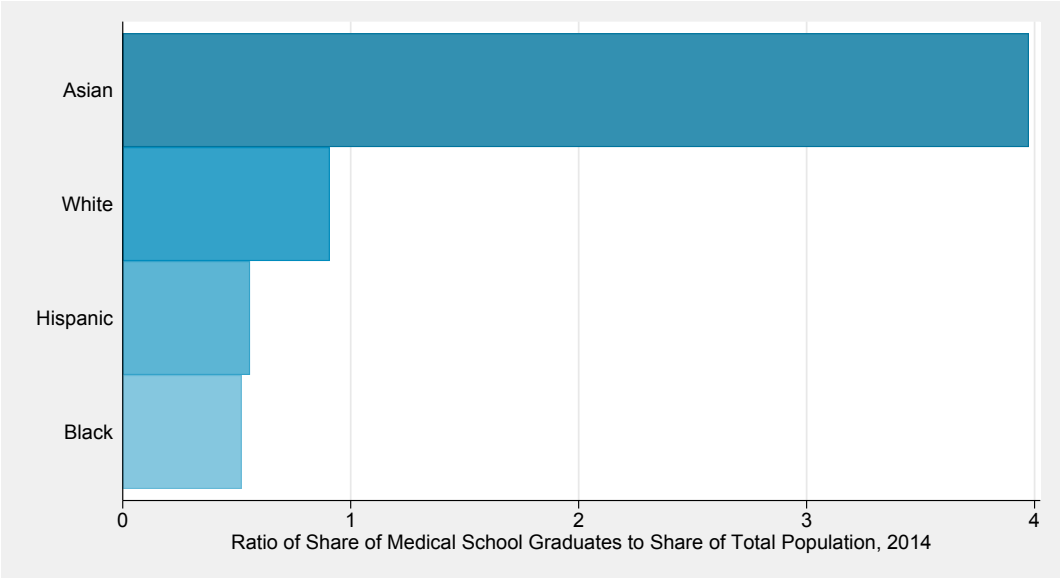
Note: Figure plots the black doctor coefficient on a random selection of N subjects with replacement, where $N = 12$. Permutation test runs main regression 500 times. Vertical (red) line signifies the coefficient from the subjects who did not meet study criteria.

Appendix Figure 4: Medical School Graduates by School Rank, 2016–17



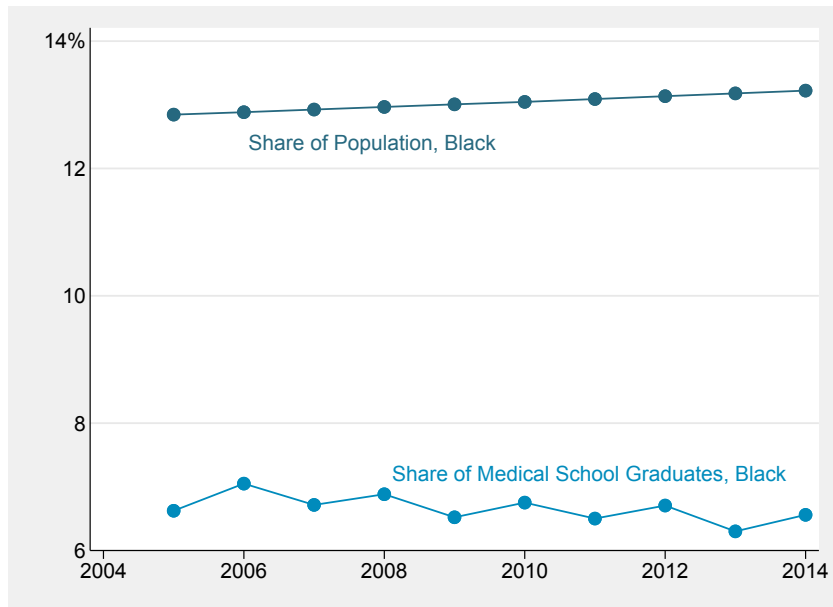
Note: Graduates data is from the Association of American Medical Colleges; medical school rank data is from U.S. News 2018 research rankings. See Data Appendix for more details. Figure plots the share of medical school graduates in each category of school rank by race for 2016–17. U.S. News rankings stop at number 94; NR stands for “not ranked.” Size of the bubble reflects the percent of the race-specific medical school graduate population in each category relative to all race-specific medical school graduates.

Appendix Figure 5: Ratio of the Share Medical School Graduates to Share Population



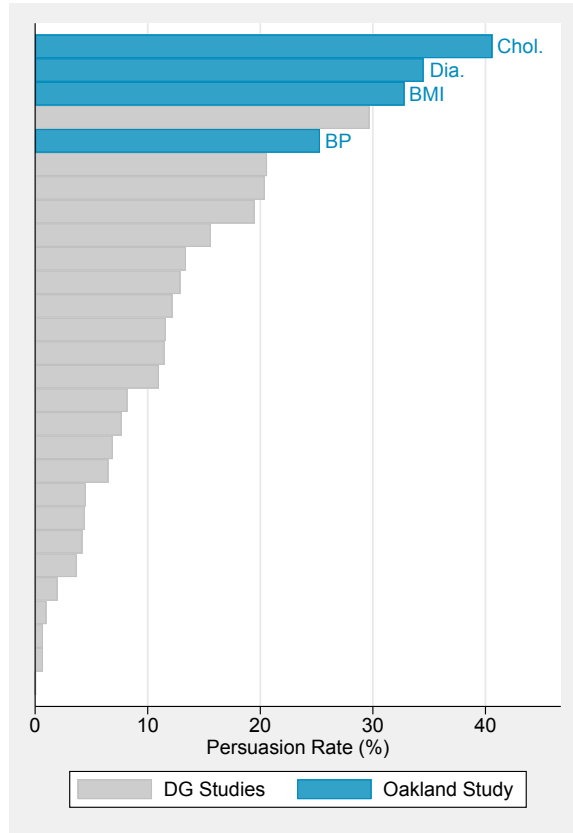
Note: Data from the Association of American Medical Colleges and Census Bureau Population Estimates. See Data Appendix for more details. Figure plots the ratio of the share of a given race/ethnicity among medical school graduates to their respective share in the population.

Appendix Figure 6: Trends in Medical Students and Population



Note: Data from the Association of American Medical Colleges and Census Bureau Population Estimates. See Data Appendix for more details. Figure plots black medical school graduates as a share of all graduates and the share of U.S. population that is black over time.

Appendix Figure 7: Persuasion Rates



Note: Figure plots persuasion rates defined as $f = 100 \cdot \frac{y_T - y_C}{e_T - e_C} \cdot \frac{1}{1 - y_0}$, where y is the share adopting the behavior (*ex post take-up* in our context), e is the share receiving the message (everyone, in our case), $1 - y_0$ is the share left to be convinced ($1 - \text{ex ante take-up}$), and T (randomized to black doctor) and C (randomized to non-black doctor) represent treatment and control groups, respectively (DellaVigna and Kaplan 2007, DellaVigna and Gentzkow 2010). Each blue bar represents the persuasion rate for one of the four non-incentivized clinic screenings. Gray bars represent persuasion rates of studies from Table 1 of DellaVigna and Gentzkow (2010).