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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 mistrust the medical system and for those who experienced greater hassle costs associated with their visit. 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 differential quality of doctors or discrimination. 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

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), or 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 minority representation.[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). Rigorous experimental evidence on whether and to what extent diversity in the physician workforce improves medical decisions and outcomes among minority populations is currently lacking.[2] This is a notable gap in the literature, given how consequential such decisions may be for well-being.

Our study aims to fill this gap and 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). 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 minority populations by health authorities, mistrust of the medical establishment is sometimes invoked as a contributing factor. Evidence consistent with such an effect 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 (Lowes and Montero 2018). Second, contributions in cultural economics have highlighted how norms of behavior are influenced by identity (Akerlof and Kranton 2000; Benjamin, Choi, and Strickland 2010). Most notably, Tabellini (2008) shows how cooperation can be sustained in a one-shot prisoners dilemma among agents who perceive a non-economic benefit from cooperating with

[1] See "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).

[2] There are several observational studies in public health and medicine that address the topic. We summarize contributions from these fields in the Appendix.

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. A spate of studies in education has found that same race or same gender teachers are positively correlated with grades and career path, potentially through a role model effect.[3]

There are several ways in which diversity could play a role in medicine, specifically as it relates to the patient-doctor relationship. Taste-based discrimination (Becker 1971) 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. With regards to preventive care, individuals often have imperfect knowledge regarding the health benefits, perhaps because they have been misinformed, never informed, or informed by someone they don't trust, which can dampen demand (Pauly and Blavin 2008; Baicker, Mullainathan, and Schwartzstein 2015).[4] Fourth, and not mutually exclusive, homophily 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 doctors. Our experiment was conducted in Oakland, California, where we recruited over 1,300 black men from twenty local barbershops and two flea markets. At these recruitment sites, subjects filled out baseline questionnaires and received a voucher for a free health screening. To facilitate our experiment, we set up a clinic to provide the screenings to the subjects.[5] The clinic was staffed with 14 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 about the role of doctor race. Subjects discovered 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 included body mass index (BMI),

[3] See: Ehrenberg (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). For evidence from industry, see Stoll, Raphael, and Holzer (2004); Giuliano, Levine, and Leonard (2009); Hjort (2014); and Bertrand et al. (2018).

[4] Medical care is a classic credence good in which under- and over-treatment are possible.

[5] This allowed us to measure actual take-up rather than stated willingness.

blood pressure, diabetes, and cholesterol screenings. 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 interacted with their randomly assigned doctor. We refer to this stage throughout the paper as ex post (since decisions occur *after* meeting the doctor). Decisions about which services to obtain could be revised by the subject during the patient-doctor interaction. After the interaction, the actual administration of selected preventives occurred by the assigned doctor. It is important to note that the study provided only *preventive* (i.e. care recommended during a state of good health to avoid future illness) as opposed to *curative* (i.e. care needed during a state of illness to restore health) interventions.[6] Hence the role of study doctors was mainly limited to information provision on the benefits of receiving care even when not feeling sick. We therefore measure how black vs. non-black doctors change demand between the ex post and ex ante stages. Following the patient-doctor interaction, subjects filled out feedback forms and exited the clinic.

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 voucher were 13 percentage points more likely to be unemployed (off of a baseline level of 18%) and 19 percentage points less likely to have post high school education (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 elect to receive the same number of preventive services as those assigned to a non-black doctor in the ex ante period. In sharp contrast, we find that subjects assigned to black doctors, upon interacting with their doctor, increase their take up across all screening services by 16 percentage points relative to non-black doctors.[7] These findings are robust to corrections for multiple hypothesis testing; 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? Although mechanisms are difficult to precisely pin down, several pieces of evidence point to better trust and communication between black subjects and black doctors than between black subjects and non-black doctors. First, in our controlled study environment, the role of the doctor was circumscribed to communicating the benefits of preventive care to subjects, and then providing those chosen. Second, we find that subjects are 10 percentage

[6]We use the term preventives to refer to preventive services (i.e. screening and immunizations).

[7]This result is driven by subjects randomized to black doctors increasing their selected services, rather than reversals among subjects randomized to non-black doctors.

points (29%) more likely to talk with black male doctors about other health problems. Black doctors are 11 percentage points (34%) 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 shift out demand in the ex post period relative to the ex ante period, though the effect is 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 respond: increasing their take-up of diabetes and cholesterol screenings by 20 and 26 percentage points (47% and 72%), respectively.

To obtain further evidence on channels, we gathered non-experimental data through a survey of 1,490 black and white adult males who matched our experimental 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 would provide appropriate treatment or is the most qualified) black and white respondents both selected doctors of the same race about 50% of the time, with white respondents expressing preferences for homophily slightly more often than black respondents. However, for questions regarding communication, in particular which doctor would understand your concerns, the rates of respondents choosing doctors of their own racial background jumped to nearly 65% for blacks and 70% for whites.

An alternative interpretation of our results is that the estimated treatment effect is picking up an attribute correlated with doctor race and which affects the outcome of interest in our sample.[8] Importantly, doctors were balanced on observables in age, experience, birthplace, and medical school rank; however, a prominent candidate for a hard-to-measure characteristic that may correlate with doctor race is quality. The non-experimental findings cited above suggest perceived quality is not affected by race; however, actual doctor quality within the context of our study could vary.[9] 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 are rated equally (highly).[10] 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 screening. Furthermore if black 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.[11] 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 bootstrapped coefficients on draws of 12 in-

[8]This 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.

[9]Doctor quality is difficult to measure (AHRQ 2016). Common metrics include board scores (which include standardized patients), medical liability concerns (lack thereof), and more recently, doctor report cards.

[10]To assess patient satisfaction, we ask respondents to rate doctors on a scale of 1 to 5, similar to the Press-Ganey rating system often used in the medical field. For more, see: https://www.forbes.com/sites/kaifalkenberg/2013/01/02/why-rating-your-doctor-is-bad-for-your-health/.

[11]To avoid conflict in the field or the clinic, we provided services for the handful of people from other backgrounds but deleted them from the main analytical sample. See Figure 1.

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 due to homophily, black doctors might be expending *less* effort to achieve the same or better results — i.e. communication may be more efficient). In other settings, 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. This is because time spent during the visit is primarily influenced by the number of tests selected and the ease with which the doctors performed the tests. Thus a longer time could simply 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). We find that black doctors indeed spend more time with subjects, but this finding is mainly driven by the treatment effect — accounting for only one additional minute from a baseline level of twenty minutes after adjusting for selected tests. If we examine another potential proxy for effort, allocating screening 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.

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 interacted with these other concordance measures. Such findings should be interpreted with caution since these characteristics were not randomized.

Lastly, although difficult to completely exclude, we do not find evidence for the controversial hypothesis that subjects are prejudiced against non-black doctors. First, there was no race-preference elicited in the ex ante 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). Discrimination by doctors would again be inconsistent with non-black doctors being rated as highly as black doctors.

Similar to prior scholarship on incentives and preventives for the poor, (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 dollar incentive and 30 percentage points for a $10 incentive in the ex ante period. 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 (interactions 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 studies on the health value of interventions offered in our clinic with results from our study. The published 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 — 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 was available to them.

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 and empirical approach. Section V presents the main findings and Section VI explores potential mechanisms and validity concerns. Section VII discusses health benefits and Section VIII concludes.

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 tablet and select preventives, and the ex post stage whereby the subject and the doctor interact and then subjects re-optimize based on the interaction. 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 et al. (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.[12] For example, some thought flu shots caused sickness, or that other remedies could ward off the flu. Several said that they would get the shot later (with a range as early as tomorrow to next year). 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 $r_j = \{r_b, r_w\}$, where the subscripts b and w refer to black and white, respectively (Becker 1971). This cost is additive to other utility costs c where $c + d < b$.[13] The utility to taking up a preventive is therefore:

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

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 $r_j = r_w$, $d > 0$ if $r_j = r_b$, 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.

1. $d > 0$ if $r_j = r_w$ 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 period will be strictly greater for those randomized to black versus white doctors (i.e. $\Pr(\beta_i > \frac{c+d_w}{b} | r_j = r_w) = 1 - \frac{(c+d_w)}{b} < 1 - \frac{c}{b} = \Pr(\beta_i > \frac{c}{b} | r_j = r_b)$).

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

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 $\Pr(\beta_i > \frac{c+d}{b} | r_j = r_w) = \Pr(\beta_i > \frac{c+d}{b} | r_j = r_b)$ or $\Pr(\beta_i > \frac{c}{b} | r_j = r_w) = \Pr(\beta_i > \frac{c}{b} | r_j = r_b)$. 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

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

[13] For 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.

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 $\beta_i b + \epsilon_i^* = b \iff \epsilon_i^* = (1-\beta_i)b$. Consider all doctors communicate ϵ_i^* but whether the information is 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 $r_{j=w} = 0$.[14] Ex post utility is therefore given by:

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

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.

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, $\mathbb{E}[U|r_j = r_w] = b - c - \frac{\delta b}{2} < b - c = \mathbb{E}[U|r_j = r_b]$. Note that white doctors will also shift out demand for black patients relative to the ex ante stage if $\delta \neq 1$.

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 $\mathbb{E}[U|r_j = r_w] > \mathbb{E}[U|r_j = r_b]$.

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 period 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.

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 (see Figure 1 for study design and flow).[15] We recruited men from 20 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

[14] For a continuous social distance formulation, see Tabellini (2008).

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

written informed consent, the subject was given a short baseline survey.[16] The baseline survey included questions on socio-demographics, healthcare, and mistrust. For completing the survey, the men received a voucher (with up to $25) for their haircut or, in 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 Saturdays in the fall and winter of 2017–2018 (See Appendix Table 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.[17] 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).[18] The RO would then provide the subject with a tablet, which randomized the subject to a black or non-black doctor and a flu vaccine incentive. SurveyCTO programmed in-form randomization for the tablets. Note that the tablet was the first time subjects learned about the opportunity to receive a flu vaccine, since it was not advertised.[19] The RO would collect the coupon and give the subject 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.[20] 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 with generic language stating that *Dr. [Last Name] is a medical doctor licensed to practice in the state of California and currently practicing in the Bay Area.* This text was accompanied by a large headshot photo of the doctor in a white coat with a red background.[21] The next screen listed services 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 next 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

[16] Baseline survey included in the Appendix. Field officers were minority 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 15 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.

[17] Field officers used their own phones to obtain the rides.

[18] Receptionist 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.

[19] We were concerned, based on focus group work, that men would believe they had to receive a flu vaccine at the clinic and therefore would not attend.

[20] Fourteen 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).

[21] Tablet 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.

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. Subjects who were confused about whether they had a contraindication were encouraged to talk to their assigned doctor. Fourteen doctors participated in the experiment, including eight non-black and six black. The RO returned to the patient room, collected the tablet, recorded the responses, and handed a clipboard to the assigned doctor.[22]

Study doctors were instructed to encourage patients to receive all preventives. The doctors, subjects, and field staff were not explicitly informed that race was being randomized, though they could have inferred it over time. They were explicitly told that the purpose of the study was to improve the uptake 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 see 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 a 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 authorities at Berkeley and MIT ceded authority to Stanford.

IV. Empirical Strategy

The purpose of the study is to estimate the causal effect of doctor race on the preventive healthcare decisions of African-American men. We estimate the following equation:

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

where i is an individual subject. Y_i is the demand for preventives during various stages of the experiment. 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 incentive only affects 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 Ap-

[22] Doctor assignment was double checked by a second FO.

pendix Table 9.[23] In addition, we estimate stacked versions of Equation 3 where each observation is a subject-by-preventive.

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 survey was designed to capture information on respondents' preference for certain doctor characteristics. Using these data, we examine whether the preference for homophily (i.e. a racially concordant provider) is unique to black male respondents and whether it varies across healthcare domains. Specifically, we estimate the following equations:

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

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

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

where i indicates respondent, k signifies race (black or white) and l is one of three domains cited by the World Health Organization (WHO) as features of a responsive health system: access (choice in providers), quality, and communication (Gostin et al. 2003).[24] X_i in the above survey refers to respondent characteristics (i.e. age, education, and income). Equation 4a examines whether respondents have a preference for doctors of the same race, where *RaceMD* and *RaceResp* are either both black or both white. Equation 4b tests whether the preference for racial homophily differs between black and white respondents. Finally, Equation 4c investigates whether the importance of racial homophily differs across domains as well as by race of the respondent.

V. Results

We begin by describing the recruitment and selection of the subjects who chose to come to the clinic, then move on to balance tests and the study's main findings.

A. Recruitment and Selection

To examine selection, we modify Equation 3, regressing X_i on a dummy for *Clinic Show Up*.[25] These results are gathered in Table 1.[26] In general, those who came to the clinic were older, had lower self-reported health, visited the ER 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,

[23]We follow the Anderson (2008) procedure to adjust p-values for multiple hypothesis testing. Note we do not cluster standard errors as the treatment assignment mechanism was not clustered (Abadie et al. 2017).

[24]The other domains include respect, autonomy, confidentiality, timeliness, and familial support.

[25]See Data Appendix for variable definitions.

[26]This table was not part of our pre-analysis plan. Our main clinic sample includes all of those who identify as African-American and are 18 years old 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 including only those who explicitly checked African-American for the race question.

disability, or social security; were 19 percentage 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 is likely what led to this pattern of selection. We return to these results in Section VI concerning external validity.

B. 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 this covariate, among others, in Equation 3 does not alter our results (see Table 4). Appendix Table 1 demonstrates that the results are also balanced when examining the black doctor or flu incentive randomizations separately.

C. Demand for Preventives

We next turn to our main results and the principle aim of our study. 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.[27] In the ex ante stage, across every test offered, the race of the doctor photo did not influence demand in any distinguishable 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 five dollar incentive increased flu take-up by about 19 percentage points, and a ten dollar incentive increased it by 30 percentage points. The willingness to pay for flu vaccination in the ex ante period is shown in Figure 4 Panel (A). With a ten dollar incentive, almost 45% selected the flu shot on the tablet, though, as discussed in detail below, not all subjects who initially chose flu shots received it since subjects could endorse 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 increase their take-up of blood pressure measurement by 11 percentage points, an increase of 15% compared to the non-black doctor mean.

[27] In Appendix Table 4 we present baseline results with only the black doctor treatment.

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. This is consistent with the conceptual framework that social distance acts to discount information on the benefit of preventives provided by non-black doctors (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 Panel (B) Column (3)). 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. Interestingly, and in contrast to the non-invasive services, subjects assigned to non-black male doctors were not more likely to agree to the 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 behavioral bias varies by test characteristics, such a result could occur.[28]

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 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 seven minutes longer; a cholesterol screening added about four 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 after the encounter than those assigned to non-black doctors.

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

[28]Let I denote invasiveness and $\frac{d\delta}{dI} > 0$, 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.

to select a cholesterol screening after meeting their physician than those assigned to a non-black doctor. 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 minds. 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 changed their mind after meeting with their assigned doctor, consistent with doctors' counseling increasing their perceived 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 nonblack doctors.

Returning to the only incentivized test, the flu shot, we note in Table 3 Panel (B) Column (6), that a high-powered flu incentive ($10) decreases the total effect of either black or non-black doctors to increase demand compared to the ex ante period and relative to the $0 subsidy condition. Figure 4 Panels (D)–(F) separate out willingness to pay by assigned race of doctor. We show in Figure 4 Panel (F) that subjects assigned to a black doctor increased their demand for the flu shot in the ex post period at every incentive level. In contrast, particularly at $10 incentive levels, subjects who originally chose the flu then met with a non-black doctor often opted out, citing contraindications. The results are imprecise (see Table 5 Column (6) — the total effect on delta for black doctor when randomized to a $10 dollar incentive is 0.04 (s.e. 0.057) vs. -0.11 (s.e. 0.053) for non-black doctors) but consistent with the notion that subsidies and interactions 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. Appendix Table 2 reports the coefficients on all the covariates. As a robustness check, we include different fixed effects (FO, date, and recruitment location (Table 7 Panel (A)) and different samples (i.e. including everyone who consented, excluding those who could not read, including only those who responded to every demographic question (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).[29] Finally, race appears to be a special facet of social distance — sharing the same age or educational background as doctors does not seem to influence take-up (see Table 9). In sum, the results presented thus far reveal that, for low-income black men, interacting with a black male doctor has a consistent and robust positive effect on the demand for preventives.

[29] In unreported results, we do not find evidence that knowing someone else at the clinic, a practice question we asked to ensure subjects could operate the tablet, affected demand.

VI. Mechanisms

In this section, we explore potential mechanisms for our results. We do so in three 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; and third, by using non-experimental evidence from an additional survey we conducted on approximately 1,500 black and white men concerning preferences over doctors. We first examine the role of trust and communication. Then we discuss other possible interpretations of our results including physician effort, quality, and discrimination.

A. Trust and Communication Between Patients and Doctors

Our primary data source 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 screenings. These results are also robust for controlling for the time spent with subject 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 could write about any "notable" issues during the 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 12 percentage points more likely to discuss personal matters or health issues unrelated to the screening, conditional on the doctor writing a note.

Qualitative evidence from the subject feedback forms and doctors' notes also support the mechanism of improved communication and correcting 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.[30] We find that low-income subjects, defined as those that report annual household 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. For the interaction of black

[30]Ex ante results are in Appendix Table 3.

doctor with low education (an indicator for a high school degree or less) and age (an indicator for younger than 40) we fail to find strong evidence of an important interaction effect.

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 prior healthcare experience, respectively. In particular, subjects who were randomized to a black doctor but had longer 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 eight 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 a black doctor than a non-black doctor.[31]

African-Americans visit the emergency room more often than non-Hispanic whites, which some have linked to lack of insurance, 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.[32] Similarly, those who had no recent screening had a heightened response.

Research in medicine finds that black men are more likely to endorse medical mistrust than their white counterparts, and that mistrust is correlated with delayed preventive care 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 (5) and (6)). 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.[33] 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 on 1,490 African-American and white (self-identified) males. We matched the survey sample to our clinic data in terms of education, so that approximately half of the survey respondents had a high school education or less. From a set of black, white, and Asian male doctors, respondents were asked to choose which

[31]The wait time has fewer observations due to missing data for the first two clinic days. All three variables are balanced across black and non-black doctor treatment, despite the fact that subjects assigned to black doctors spent more time with them.

[32]We also asked a question about usual source of care in the baseline survey, but many subjects selected multiple options making this variable difficult to code.

[33]If we interact legal mistrust with black doctor treatment instead of medical mistrust — we obtain a statistically insignificant coefficient though it is imprecise.

doctor ranked the highest across three WHO domains: quality, communication, and accessibility. The results are reported in Table 8.

First we examine preferences for homophily (Equation 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 more likely to report that a same race physician would understand them better (Columns (4) and (5)) and was more likely to be accessible (Columns (7) and (8)).

Second, we examine whether preferences for homophily vary across race (Equation 4b). Column (3) tests whether black respondents were more or less likely to rate doctors who share their racial background 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. There is no racial difference for communication (Column (6)), which both sets of respondents reported as important (control mean of 0.69). For accessibility, there is a large racial gap, a point we return to when discussing external validity.

Third, we estimate Equation 4c, which tests whether racial preference is stronger for some domains than others. In Column (10) we find that black and white respondents were both much more likely to select a doctor of the same race when the question was about communication as opposed to when the question referred to quality.

Figure 5 succinctly presents the highlights from Table 8. The figure graphs the percent of respondents from a race selecting their own race across the three domains. We find a slight preference for same race when it comes to quality, though both sets of respondents are very close to the (red) 50 percent line, indicating that they were as likely to select own race as another race doctor. In sharp contrast, for questions related to communication, both black and white respondents shift to the right. There is a clear preference for same race doctors when the questions concerned communication. Nearly 65% of black respondents and 70% of white respondents reported that a doctor of their own race would understand their concerns best.

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 et al. 2017). 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 according to a Press-Ganey rating scale, 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 for black and non-black doctors. Furthermore, the mean doctor rating was about 4.7 with 85% of subjects giving their doctor 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, adding an indicator for black doctor increases the R-squared of the regression by 85 percent. The results suggest having a black doctor was equivalent to a doctor moving from the 80th ranked medical school to the top ranked medical school.

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-American, but were still seen at the clinic because they had been consented to participate. Moreover, these clients were randomized across eight of the fourteen study doctors, equally balanced by race. These out-of-sample subjects were 14 percentage points *less* likely to choose services from black doctors in the ex post period. 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 the bootstrap coefficients (see Appendix Figure 3).[34] 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.

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 five 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 1.4 minutes, compared to an average visit length of 20.5 minutes. We also examine whether study doctors exerted more effort by

[34] If we examine delta share invasive services received, the coefficient for the 12 out-of-sample subjects is -0.05 which is more negative than 91% of other subjects.

targeting services to the "highest need" subjects. 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 statistical differences in observables in terms of experience, age, or medical school ranking (if anything, the set of black doctors attended lower ranked medical schools, 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 two outliers (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 fairly consistent (i.e. 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 period, we find that subjects assigned to non-black doctors increased their demand relative to the ex ante period (see 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 extrapolating 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. (17% and 6%, respectively).[35] 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 46 years and 61% with a high school education or less in the U.S.).

[35] Calculations on the U.S. population come from 2016 1-year American Community Survey data.

Turning to health characteristics, the average value for systolic blood pressure was 132.7 mm Hg consistent with stage 1 hypertension, the average BMI value was 27.4 kg/m^2 consistent with an overweight categorization, and 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 A1c value in the seriously elevated range (i.e. >9%).

In terms of 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 (see Figure 7).[36] 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 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.[37]

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* non-black doctor distribution. The doctors also gave up their Saturdays in exchange for a fixed hourly compensation that they received through direct deposit or check.[38] 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. This is not reflective of the general black male population. Returning to our non-experimental evidence in Figure 8, 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%). This finding is robust to the inclusion of basic demographic controls. 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

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

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

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

available, conditional on age, income, and education.

As stated in the introduction, African-Americans comprise only 4% of practicing physicians in the U.S. Moreoever, the pipeline of African-American medical students is relatively flat — hovering around 6% for the last decade, an increasingly lower share than the growing African-American population (see Appendix Figure 5). In contrast, the share of medical students who are white is declining at about the same rate as the non-Hispanic white percent of the population. 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; 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 no screening conditions, 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.[39] 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 period.

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) over about a 40-year time horizon.[40] 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%.

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 2017). 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.

[39] The 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.

[40] We 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.

Harper et al. (2012) calculate that 41% of the life-expectancy gap between black and white males in 2008 is 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 (5 years) 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 preventable illnesses account for another 26% of the black-white male life expectancy gap.

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 a chance to interact, those 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 were more likely to talk to black doctors and that black doctors made more notations about the subjects.

These findings are consistent with a framework in which agents underestimate the value of preventive care, and thus have lower 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 on 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 and a 8% decline in the black-white male life expectancy gap. 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			
	Self-Reported Health	*Any Health Problem*	*Hospital Visits*	*ER Visits*	*Nights Hospital*	*Medical Mistrust*	*Has Primary MD*
Clinic Presentation	-0.126***	0.033	0.244	0.513***	-0.332	-0.011	-0.072**
	(0.025)	(0.028)	(0.469)	(0.183)	(0.746)	(0.042)	(0.029)
Mean	0.812	0.574	4.738	1.242	1.925	1.639	0.691
Observations	1,148	1,241	935	1,031	1,041	1,232	1,096
				PANEL B			
	Uninsured	*Age*	*Married*	*Unemployed*	*High School Education*	*Low Income*	*SSI/DI/UI*
Clinic Presentation	0.038	3.411***	-0.058***	0.129***	0.190***	0.198***	0.113***
	(0.027)	(0.811)	(0.022)	(0.025)	(0.029)	(0.027)	(0.024)
Mean	0.242	41.055	0.202	0.177	0.439	0.250	0.179
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. *, **, *** indicate significance at the 10, 5, or 1% level.

Table 2: Balance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Mean (S.D.)	*Non-Black MD - $5*	*Non-Black MD - $10*	*Black MD - $0*	*Black MD - $5*	*Black MD - $10*	*F-test*	*p-value*	*N*
Self-Reported Health	0.72	-0.033	-0.181***	0.007	-0.016	0.004	2.075	0.067	563
	(0.45)	(0.066)	(0.067)	(0.065)	(0.064)	(0.063)			
Any Health Problem	0.62	-0.026	0.036	-0.015	-0.025	-0.021	0.250	0.940	614
	(0.49)	(0.068)	(0.065)	(0.069)	(0.067)	(0.066)			
ER Visits	1.69	-0.149	0.867	-0.212	0.145	-0.391	1.336	0.247	511
	(3.54)	(0.434)	(0.609)	(0.443)	(0.558)	(0.419)			
Nights Hospital	1.20	-0.392	0.839	1.956	-0.214	0.230	1.332	0.249	511
	(3.52)	(0.415)	(0.734)	(1.490)	(0.466)	(0.663)			
Has Primary MD	0.63	-0.042	0.033	-0.059	0.008	-0.019	0.415	0.838	537
	(0.49)	(0.074)	(0.070)	(0.073)	(0.070)	(0.071)			
Medical Mistrust	1.61	0.162	-0.046	0.032	0.016	-0.034	0.979	0.430	611
	(0.74)	(0.105)	(0.100)	(0.105)	(0.105)	(0.100)			
Age	44.96	-1.051	-0.100	-0.261	-1.109	-0.495	0.109	0.990	620
	(14.76)	(1.973)	(2.001)	(1.982)	(2.048)	(1.944)			
Married	0.12	0.062	-0.028	0.088	0.003	0.032	1.388	0.227	586
	(0.33)	(0.051)	(0.043)	(0.054)	(0.046)	(0.048)			
Unemployed	0.32	-0.045	-0.008	-0.051	0.008	0.025	0.394	0.853	570
	(0.47)	(0.066)	(0.066)	(0.065)	(0.065)	(0.065)			
High School Education	0.62	0.006	-0.006	-0.029	0.055	0.034	0.344	0.886	556
	(0.49)	(0.070)	(0.070)	(0.072)	(0.068)	(0.068)			
Low Income	0.47	-0.026	-0.033	-0.043	0.022	-0.042	0.258	0.936	571
	(0.50)	(0.072)	(0.071)	(0.072)	(0.070)	(0.069)			
Uninsured	0.22	0.042	0.146**	0.112	0.057	0.010	1.398	0.223	517
	(0.42)	(0.066)	(0.067)	(0.070)	(0.064)	(0.062)			
Attrition	0.06	0.009	0.059	0.034	0.025	-0.034	1.755	0.120	702
	(0.23)	(0.036)	(0.039)	(0.038)	(0.036)	(0.030)			

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 $0 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 65 subjects that did not complete the study either because they did not meet criteria or they left before the clinic encounter. See Data Appendix for other variable definitions. *, **, *** 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									
	Blood Pressure			*BMI*			*Diabetes*		
Black Doctor	0.025	0.107***	0.082**	0.023	0.161***	0.138***	0.050	0.201***	0.151***
	(0.039)	(0.033)	(0.034)	(0.040)	(0.036)	(0.033)	(0.039)	(0.039)	(0.029)
$5 Incentive	0.028	0.044	0.017	-0.059	0.019	0.078*	0.085*	0.105**	0.020
	(0.048)	(0.040)	(0.043)	(0.049)	(0.045)	(0.043)	(0.048)	(0.048)	(0.036)
$10 Incentive	-0.023	-0.026	-0.003	-0.009	-0.010	-0.001	0.028	0.050	0.021
	(0.048)	(0.041)	(0.040)	(0.048)	(0.044)	(0.038)	(0.047)	(0.047)	(0.035)
$5 = $10 *p*-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									
	Cholesterol			*Flu Vaccination*			*Share of All Non-Incentivized Tests (Excludes Flu)*		
Black Doctor	0.010	0.260***	0.250***	-0.009	0.100***	0.108***	0.027	0.182***	0.155***
	(0.038)	(0.038)	(0.032)	(0.037)	(0.038)	(0.033)	(0.030)	(0.029)	(0.023)
$5 Incentive	0.067	0.061	-0.006	0.192***	0.221***	0.029	0.030	0.057	0.027
	(0.047)	(0.048)	(0.038)	(0.043)	(0.045)	(0.039)	(0.037)	(0.035)	(0.028)
$10 Incentive	-0.014	-0.013	0.001	0.299***	0.219***	-0.080*	-0.004	-0.000	0.004
	(0.045)	(0.047)	(0.039)	(0.043)	(0.044)	(0.041)	(0.036)	(0.035)	(0.026)
$5 = $10 *p*-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 expressed on tablet. Ex post refers to demand after meeting doctor. 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 a $0 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									
	Blood Pressure			*BMI*			*Diabetes*		
Black Doctor	0.031	0.103***	0.072**	0.018	0.157***	0.139***	0.051	0.204***	0.153***
	(0.039)	(0.033)	(0.034)	(0.039)	(0.036)	(0.034)	(0.038)	(0.038)	(0.029)
$5 Incentive	0.021	0.043	0.022	-0.069	0.006	0.075*	0.083*	0.105**	0.022
	(0.048)	(0.040)	(0.043)	(0.049)	(0.045)	(0.044)	(0.046)	(0.047)	(0.036)
$10 Incentive	-0.021	-0.028	-0.007	-0.004	-0.016	-0.011	0.029	0.043	0.014
	(0.048)	(0.042)	(0.041)	(0.046)	(0.044)	(0.038)	(0.045)	(0.047)	(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									
	Cholesterol			*Flu Vaccination*			*Share of All Non-Incentivized Tests (Excludes Flu)*		
Black Doctor	0.013	0.261***	0.248***	-0.006	0.104***	0.110***	0.028	0.181***	0.153***
	(0.036)	(0.038)	(0.033)	(0.037)	(0.037)	(0.034)	(0.029)	(0.028)	(0.022)
$5 Incentive	0.074*	0.062	-0.013	0.177***	0.196***	0.019	0.027	0.054	0.026
	(0.045)	(0.047)	(0.038)	(0.043)	(0.044)	(0.038)	(0.036)	(0.035)	(0.028)
$10 Incentive	-0.006	-0.020	-0.015	0.296***	0.201***	-0.094**	-0.001	-0.005	-0.005
	(0.044)	(0.046)	(0.039)	(0.043)	(0.045)	(0.041)	(0.035)	(0.034)	(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 (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 expressed on tablet. Ex post refers to demand after meeting doctor. 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 a $0 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
		Invasive			*Incentive*	
Black Doctor	0.024	0.131***	0.108***	-0.035	0.096*	0.131**
	(0.028)	(0.025)	(0.024)	(0.053)	(0.058)	(0.052)
$5 Incentive	0.024	0.049**	0.025	0.117*	0.193***	0.076
	(0.024)	(0.022)	(0.020)	(0.060)	(0.061)	(0.049)
$10 Incentive	-0.002	-0.006	-0.003	0.338***	0.233***	-0.105**
	(0.024)	(0.022)	(0.019)	(0.060)	(0.059)	(0.053)
Invasive Test	-0.167***	-0.265***	-0.099***			
	(0.027)	(0.026)	(0.020)			
Black * Invasive Test	0.010	0.099***	0.089***			
	(0.039)	(0.036)	(0.032)			
Black * $5				0.157*	0.054	-0.103
				(0.086)	(0.091)	(0.077)
Black * $10				-0.060	-0.047	0.013
				(0.085)	(0.088)	(0.082)
Total Black MD & Invasive	-0.133***	-0.035	0.098***			
	(0.028)	(0.027)	(0.023)			
Total Black MD & $5				0.239***	0.344***	0.105*
				(0.061)	(0.060)	(0.054)
Total Black MD & $10				0.243***	0.283***	0.040
				(0.060)	(0.060)	(0.057)
Control Mean	0.54	0.63	0.10	0.20	0.18	-0.02
Observations	2,548	2,548	2,548	637	637	637

Note: Columns (1)–(3) report OLS estimates from a modified version of Equation 3 on a subject-screening panel (without the flu shot) including interactions between black doctor and an indicator for an invasive preventive (i.e. requiring blood). Columns (4)–(6) report OLS estimates on flu demand from a modified version of Equation 3 including interactions between black doctor and indicators for different incentive levels. The outcome varies by column heading. Ex ante refers to demand expressed on tablet. Ex post refers to demand after meeting doctor. 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 and $0 incentive for Columns (1)–(3) and to those randomized to a non-black doctor and $0 incentive for Columns (4)–(6). Total Black MD & Invasive adds together the Black Doctor, Invasive, and Black Doctor * Invasive coefficients. Other rows in gray are similarly defined. 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 6: Heterogeneity by Demographics, Hassle Costs, and Medical Care Experience

	(1)	(2)	(3)
	Outcome = Delta Share Non-Incentivized Preventives		
	PANEL A: Demographics		
X =	*Low Income*	*High School Education*	*Younger than 40*
Black Doctor * *X*	0.087*	-0.074	0.037
	(0.047)	(0.049)	(0.047)
X	0.057*	0.109***	-0.034
	(0.029)	(0.028)	(0.029)
Black Doctor	0.113***	0.192***	0.135***
	(0.029)	(0.037)	(0.028)
Observations	571	556	620
	PANEL B: Hassle Costs		
X =	*Long Wait Time*	*High Congestion*	*Long Commute*
Black Doctor * *X*	0.175***	0.128***	0.106**
	(0.055)	(0.049)	(0.045)
X	-0.005	0.028	0.010
	(0.030)	(0.027)	(0.027)
Black Doctor	0.114***	0.103***	0.108***
	(0.029)	(0.032)	(0.028)
Observations	455	451	637
	PANEL C: Medical Care Experience		
X =	*ER Visits*	*No Recent Screening*	*Medical Mistrust*
Black Doctor * *X*	0.012**	0.146**	0.061**
	(0.006)	(0.067)	(0.031)
X	-0.000	-0.032	-0.017
	(0.003)	(0.040)	(0.019)
Black Doctor	0.134***	0.122***	0.058
	(0.028)	(0.024)	(0.053)
Observations	511	604	611

Note: Table reports OLS estimates from a modified version of of Equation 3 including interactions between black doctor and certain baseline characteristics. The outcome variable for every specificaiton 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. 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)
	Length Visit, Minutes	*Length Visit, Test Controls*	*Subject Talk to MD*	*Doctor Notes About Subject*	*Non-Preventive Notes*	*Subject Rating of Experience*	*Subject Recommend MD*
Black Doctor	4.918***	1.441*	0.100***	0.111***	0.123**	-0.014	0.000
	(1.044)	(0.843)	(0.039)	(0.038)	(0.049)	(0.049)	(0.010)
$5 Incentive	2.537*	0.235	-0.072	0.055	-0.014	0.026	0.009
	(1.345)	(1.123)	(0.048)	(0.047)	(0.059)	(0.068)	(0.013)
$10 Incentive	-0.097	-1.004	-0.085*	0.016	-0.002	0.080	0.010
	(1.303)	(1.067)	(0.047)	(0.046)	(0.061)	(0.056)	(0.012)
Control Mean	20.53	20.53	0.35	0.32	0.19	4.80	0.99
Observations	498	498	637	637	312	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)
	Quality			*Communication*			*Access*			*Communication vs. Quality*
	Which MD most qualified?			Which MD understands me?			Which MD available near me?			
	Black MD	White MD	Race Match	Black MD	White MD	Race Match	Black MD	White MD	Race Match	Race Match
Black Respondent	0.350***		-0.055*	0.531***		-0.001	0.241***		-0.255***	-0.047
	(0.025)		(0.030)	(0.024)		(0.029)	(0.024)		(0.029)	(0.030)
White Respondent		0.273***			0.479***			0.175***		
		(0.029)			(0.027)			(0.030)		
Communication										0.144***
										(0.023)
Mean	0.11	0.27	0.54	0.12	0.19	0.69	0.11	0.43	0.62	0.48
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 control 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); and the average white respondents who prefer white doctors in colums (3), (6), and (9). See Data Appendix and text for variable definitions. Robust standard errors in parentheses. *, **, *** indicate significance at the 10, 5, or 1% level.

Table 9: Take-Up with Alternative Concordance Measures

	(1)	(2)	(3)	(4)	(5)	(6)
X =	*Age, 5 Years*		*Age, 10 Years*		*Education*	
X	-0.044*	-0.029	-0.013	-0.023	-0.005	0.015
	(0.026)	(0.027)	(0.023)	(0.026)	(0.052)	(0.090)
X * 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

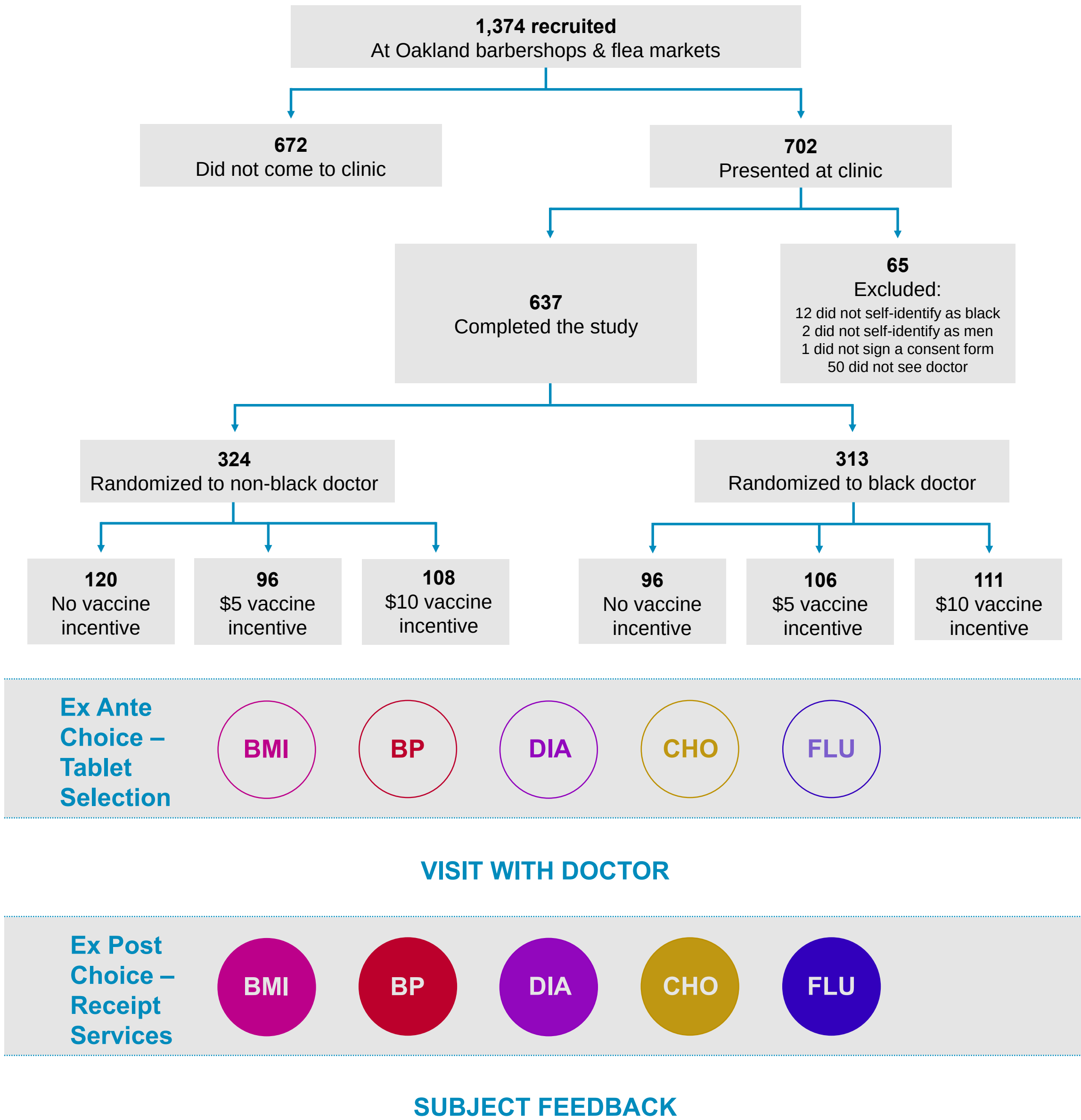
Note: Table reports OLS estimates of Equation 3. The outcome is the delta share of the four non-incentivized preventives selected (blood pressure, body mass index, cholesterol, and diabetes). Columns (1) and (2) explore age concordance (i.e. doctor and subject born within five years of each other), Columns (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 non-concordant doctor in Columns (1), (3), and (5) and a non-concordant 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)
		Doctor Fixed Effects		
Black Doctor				0.162**
				(0.064)
Medical School Rank			-0.001	-0.002*
			(0.001)	(0.001)
Experience		0.002	0.002	0.001
		(0.004)	(0.004)	(0.002)
Constant	0.126***	0.094	0.112	0.074
	(0.035)	(0.055)	(0.066)	(0.061)
Adj R-square	0.000	0.033	0.061	0.418
Observations	14	14	14	14

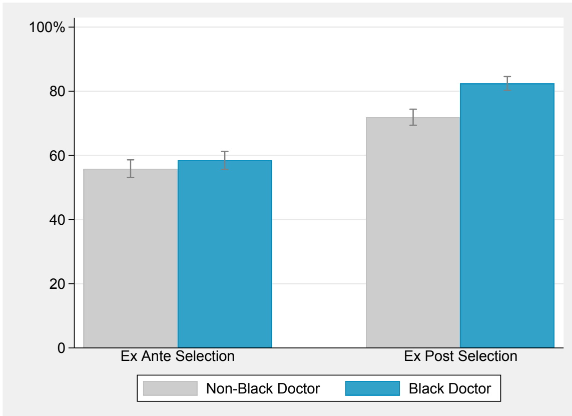
Note: Table reports OLS estimates. The outcome variable is the coefficient for the share of the four non-incentivized preventives selected (blood pressure, body mass index, cholesterol, and diabetes) regressed on indicators for each doctor. See Data Appendix and text for further details on the baseline doctor characteristics. Robust standard errors in parentheses. *, **, *** indicate significance at the 10, 5, or 1% level.

Figure 1: Study Design and Flow

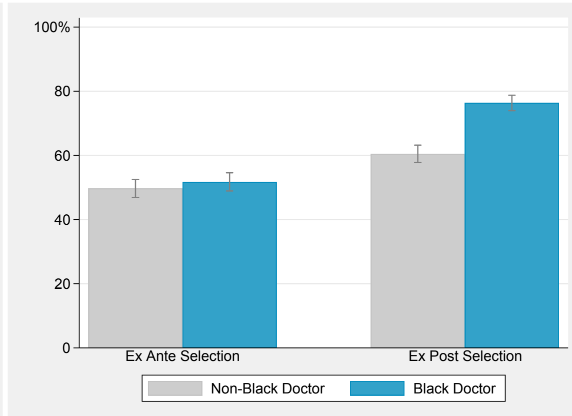


Note: Two-stage cross-randomization design and flow of subjects from recruitment through clinical encounter. Note that 65 subjects were randomized but did not complete the study either because they did not meet criteria (i.e. they self-identified as a different race/ethnicity or as a female) or they left before the clinic encounter.

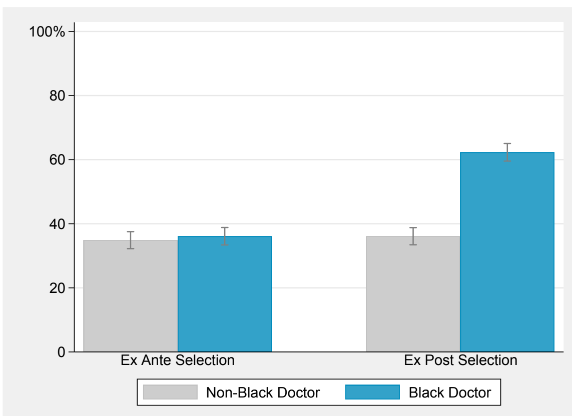
Figure 2: Demand for Preventives



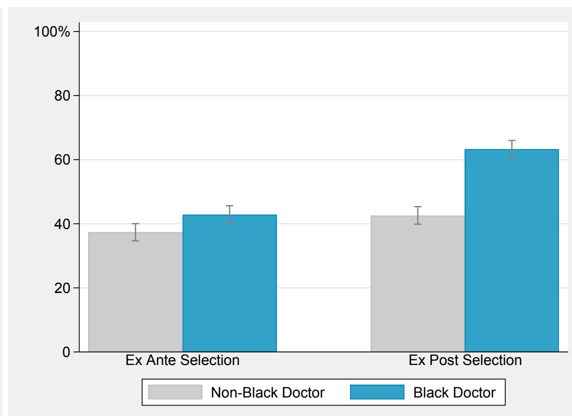
(a) Blood Pressure



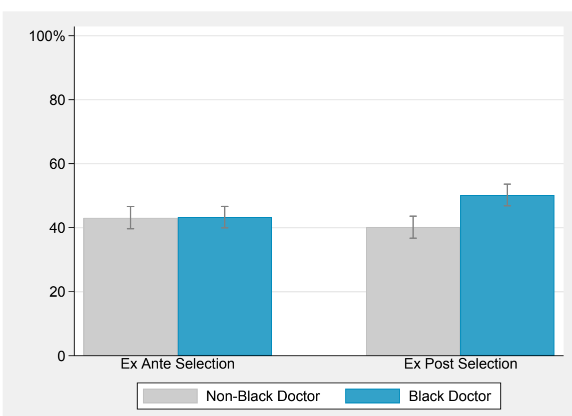
(b) BMI



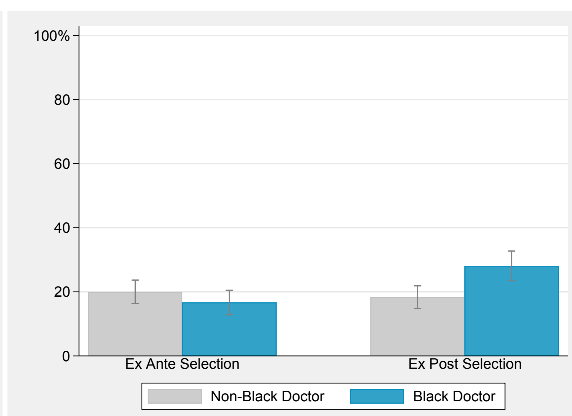
(c) Cholesterol



(d) Diabetes



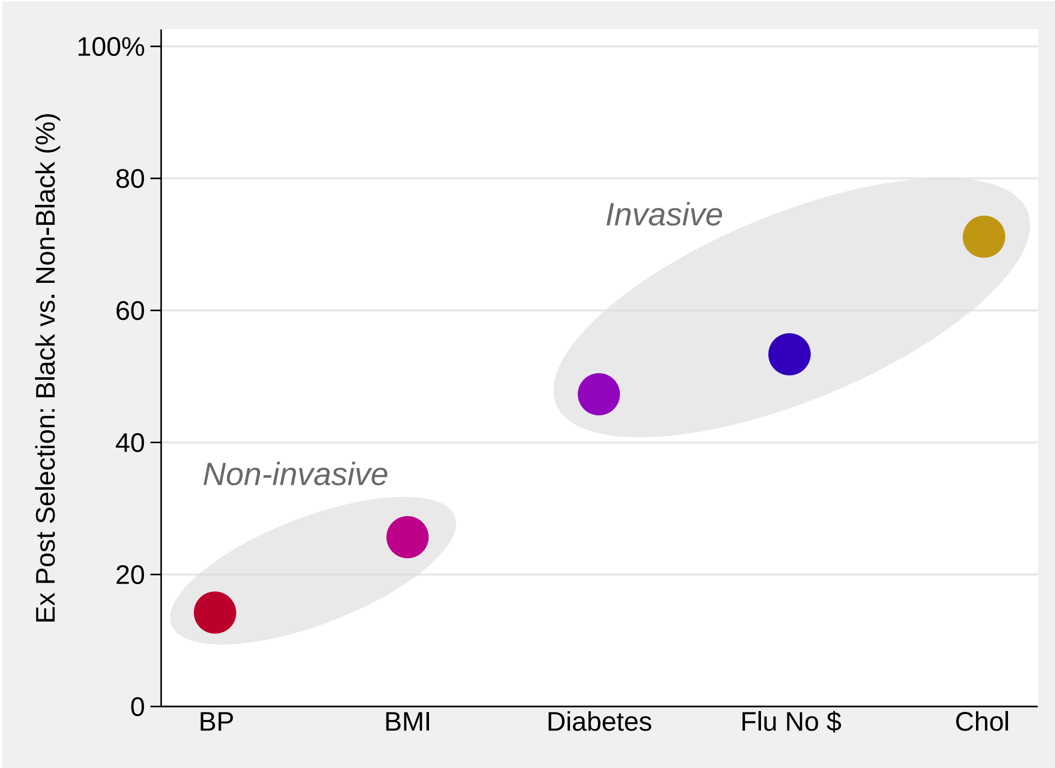
(e) Flu Shot: With Incentive



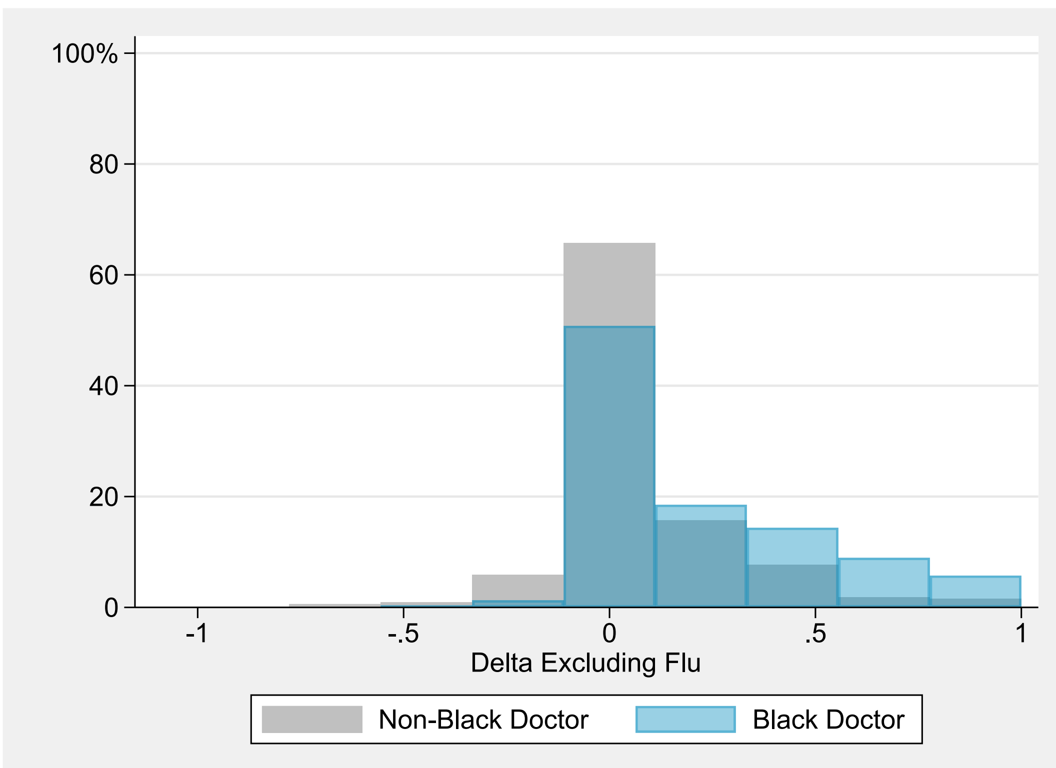
(f) Flu Shot: Without Incentive

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



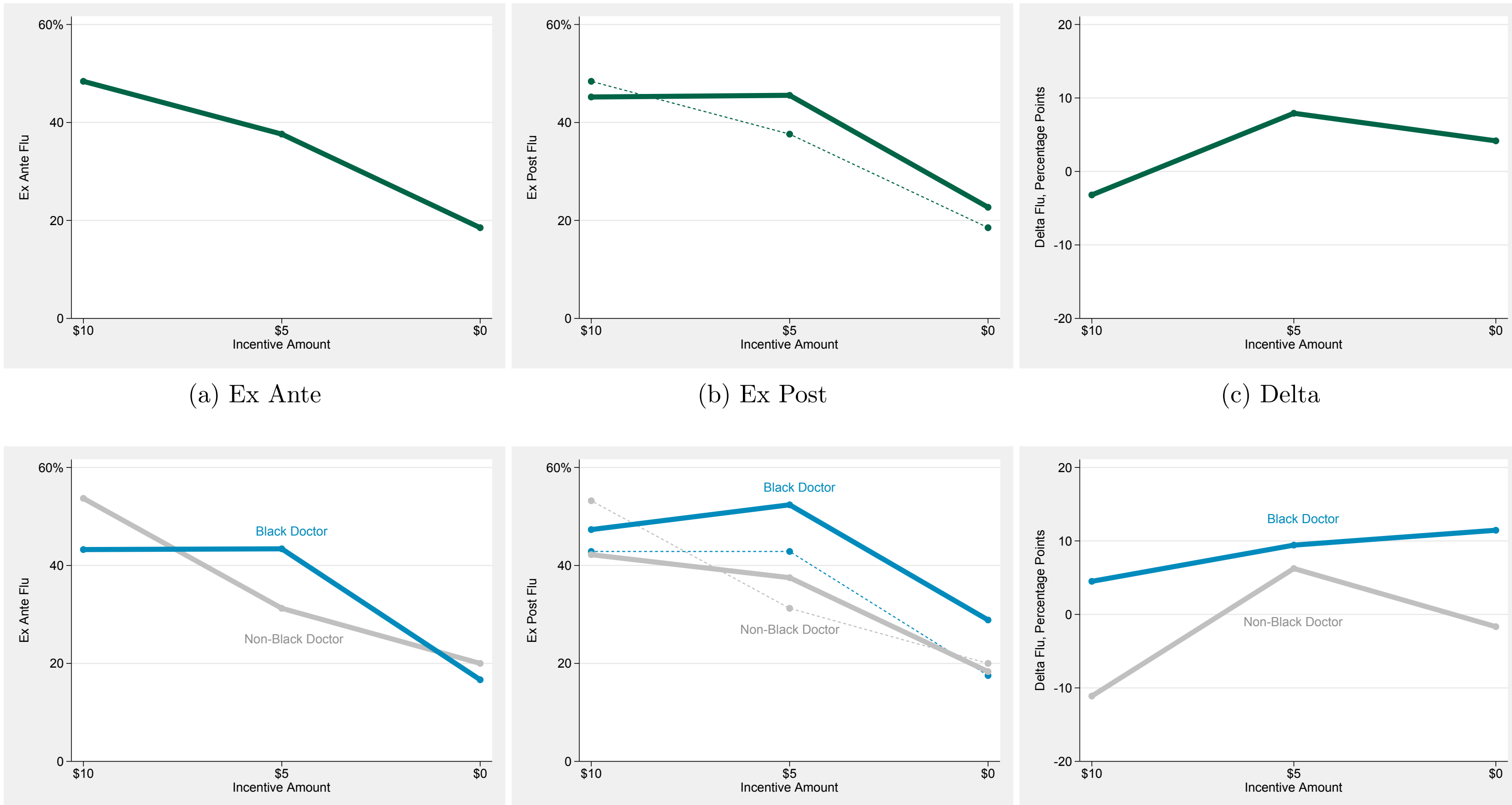
(a) Ex Post % Differences by Preventives



(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

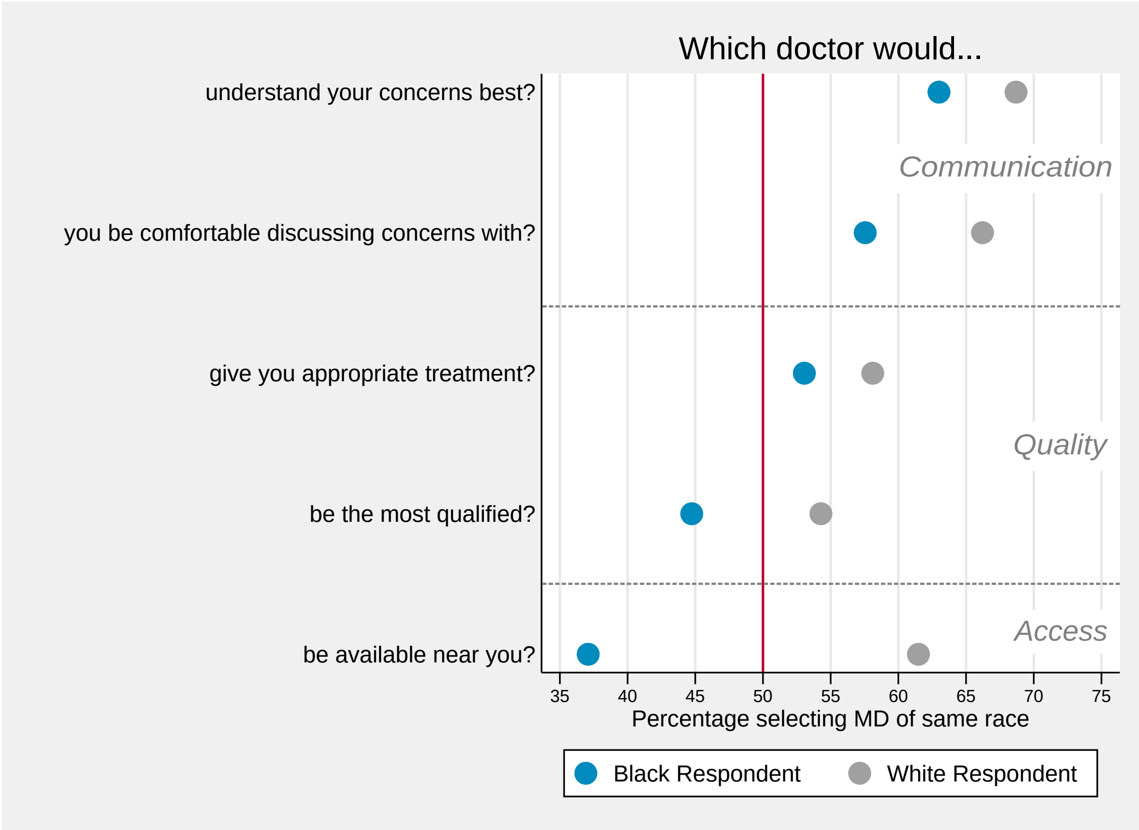


(a) Ex Ante (b) Ex Post (c) Delta

(d) Ex Ante (e) Ex Post (f) Delta

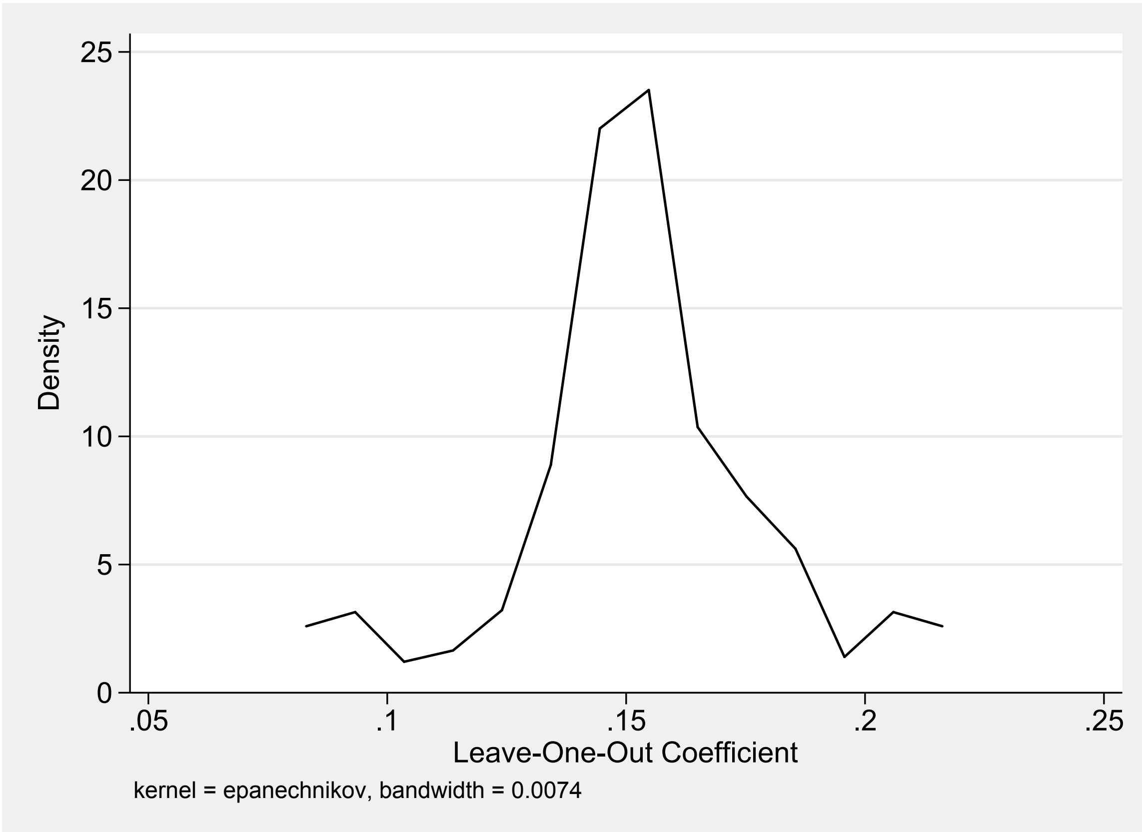
Note: Flu vaccination demand by treatment arm and experimental stage. Dashed lines indicate ex ante demand in Panels (b) and (e).

Figure 5: Non-Experimental Preference for Homophily



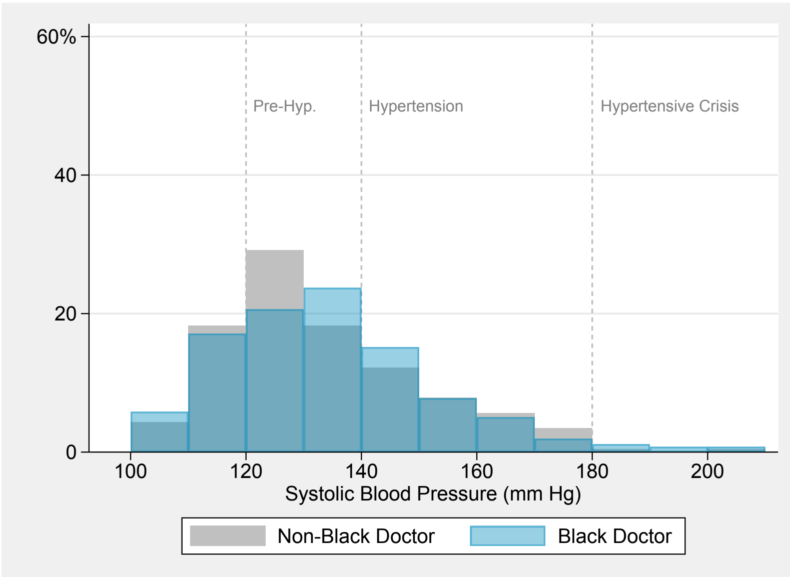
Note: Figure plots the percent of black and white survey respondents who select a doctor of the same race in response to various questions. Choice set included black, white, or Asian male doctors.

Figure 6: Distribution of Leave-One-Out Estimates

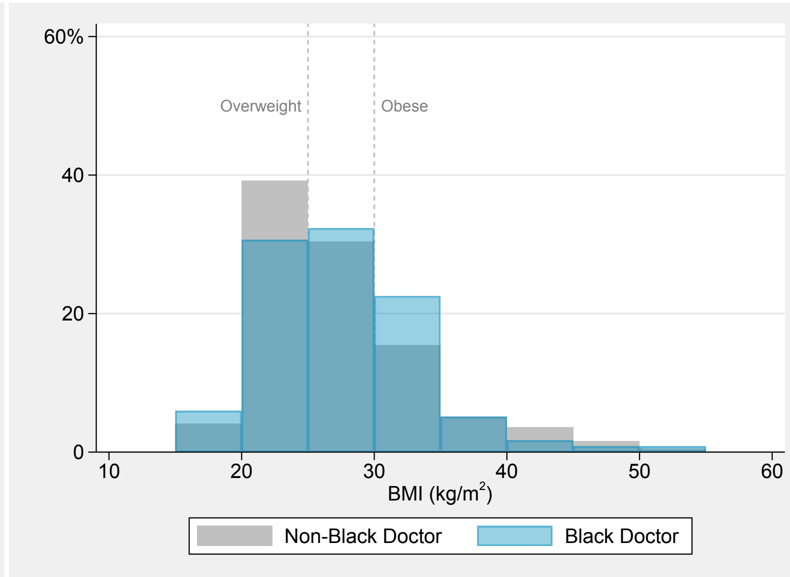


Note: Figure plots the leave-one-out coefficient for the main treatment effect of black doctor.

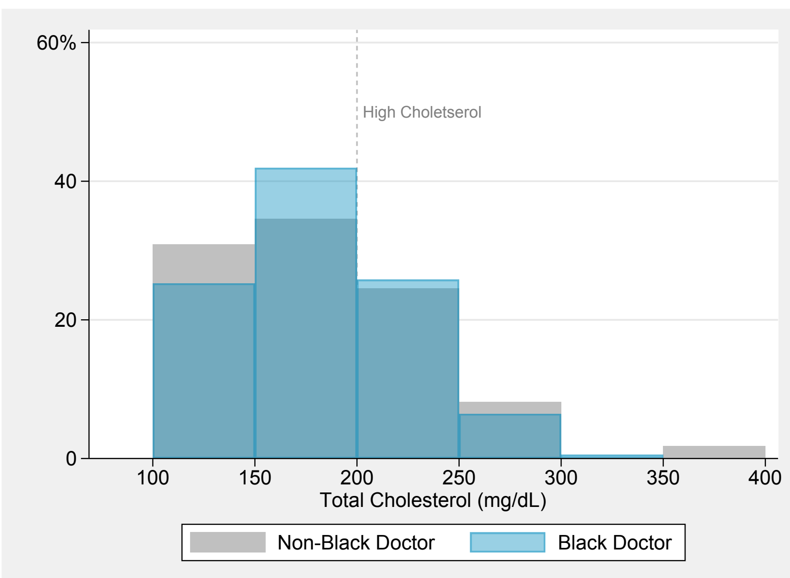
Figure 7: Results from Clinic Encounter



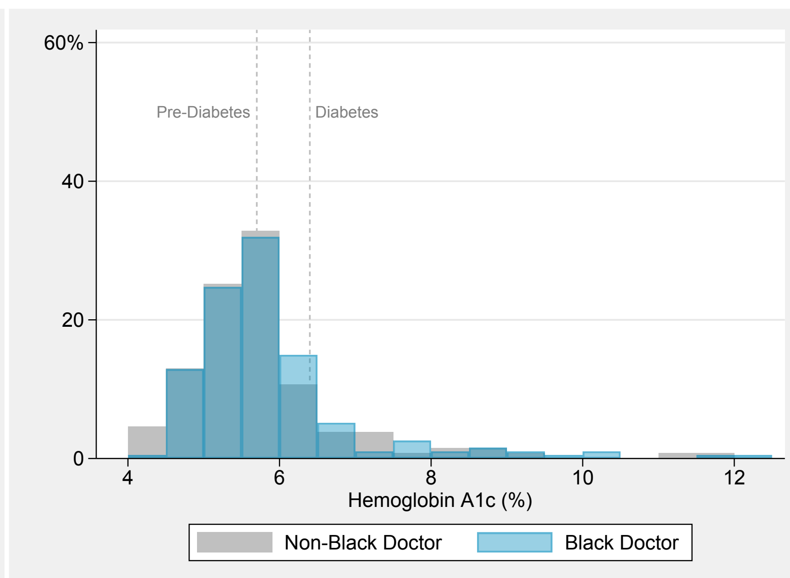
(a) Blood Pressure: Systolic



(b) Body Mass Index



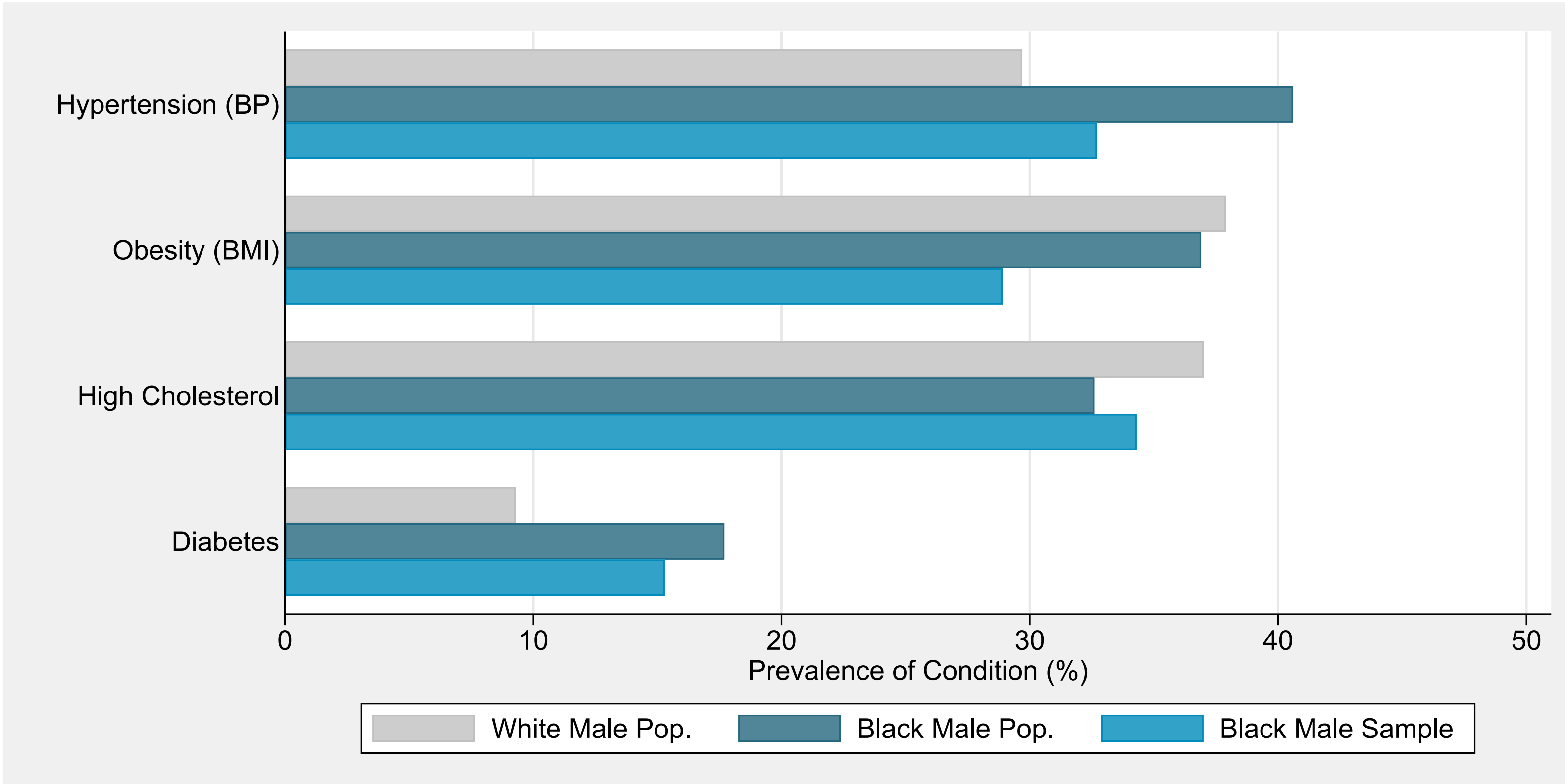
(c) Cholesterol



(d) Diabetes

Note: Distribution of medical screening results for subjects who elected to receive preventives by race of doctor.

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



Note: Figure plots the percentage of each population group afflicted with the listed conditions. We define hypertension as a systolic blood pressure value greater or equal to 140 mm Hg, obesity as a BMI greater or equal to 30 kg/m^2, high cholesterol as 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)
	Doctor Randomization			*Incentive Level Randomization*					
	Mean (S.D.)	*Black MD*	*N*	*Mean (S.D.)*	*$5*	*$10*	*F-test*	*p-value*	*N*
Self-Reported Health	0.65	0.068*	563	0.73	-0.027	-0.092*	1.934	0.146	563
	(0.48)	(0.039)		(0.45)	(0.047)	(0.048)			
Any Health Problem	0.62	-0.025	614	0.61	-0.019	0.014	0.230	0.795	614
	(0.49)	(0.040)		(0.49)	(0.049)	(0.048)			
ER Visits	1.95	-0.406	511	1.60	0.099	0.371	0.552	0.576	511
	(3.65)	(0.285)		(2.97)	(0.332)	(0.358)			
Nights Hospital	1.39	0.433	511	2.08	-1.175*	-0.312	2.849	0.059	511
	(4.42)	(0.572)		(8.85)	(0.702)	(0.803)			
Has Primary MD	0.63	-0.020	537	0.60	0.012	0.034	0.223	0.800	537
	(0.48)	(0.042)		(0.49)	(0.052)	(0.051)			
Medical Mistrust	1.64	-0.031	611	1.62	0.071	-0.054	1.437	0.238	611
	(0.74)	(0.060)		(0.74)	(0.076)	(0.072)			
Age	44.61	-0.286	620	44.84	-0.966	-0.183	0.251	0.778	620
	(14.53)	(1.167)		(14.28)	(1.437)	(1.408)			
Married	0.13	0.029	586	0.16	-0.007	-0.037	0.652	0.522	586
	(0.34)	(0.029)		(0.37)	(0.037)	(0.035)			
Unemployed	0.30	0.011	570	0.29	0.005	0.032	0.255	0.775	570
	(0.46)	(0.039)		(0.46)	(0.047)	(0.047)			
High School Education	0.62	0.022	556	0.61	0.044	0.027	0.379	0.684	556
	(0.49)	(0.041)		(0.49)	(0.050)	(0.050)			
Low Income	0.45	-0.002	571	0.45	0.018	-0.019	0.258	0.773	571
	(0.50)	(0.042)		(0.50)	(0.051)	(0.050)			
Uninsured	0.28	-0.006	517	0.27	0.000	0.028	0.216	0.806	517
	(0.45)	(0.040)		(0.45)	(0.049)	(0.048)			

Note: Table reports balance tests separately by doctor, Columns (1)–(3), and incentive, Columns (4)–(9). Control mean is randomization to a non-black doctor for Columns (1)–(3) and to a $0 incentive for Columns (4)–(9). Observation count varies due to missing responses in the baseline survey. See Data Appendix for variable definitions. *, **, *** indicate significance at the 10, 5, or 1% level.

Appendix Table 2: Take-Up with Controls, Extended

	(1)	(2)	(3)	(4)
	Ex Ante Share, Excluding Flu	*Ex Post Share, Excluding Flu*	*Delta Share, Excluding Flu*	*Delta Share, Including Flu*
Black Doctor	0.028	0.181***	0.153***	0.144***
	(0.029)	(0.028)	(0.022)	(0.020)
$5 Incentive	0.027	0.054	0.026	0.025
	(0.036)	(0.035)	(0.028)	(0.024)
$10 Incentive	-0.001	-0.005	-0.005	-0.023
	(0.035)	(0.034)	(0.026)	(0.023)
Month: October	0.034	0.122***	0.089**	0.110***
	(0.047)	(0.046)	(0.039)	(0.033)
Month: November	0.094**	0.157***	0.064**	0.071***
	(0.041)	(0.041)	(0.030)	(0.026)
Month: December	0.139***	0.156***	0.016	0.019
	(0.037)	(0.038)	(0.027)	(0.024)
Age	-0.002	-0.001	0.001	0.000
	(0.004)	(0.004)	(0.004)	(0.003)
Age Squared	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
High School Education	-0.105***	-0.058*	0.048*	0.032
	(0.035)	(0.033)	(0.025)	(0.022)
Low Income	-0.169***	-0.092***	0.077***	0.055**
	(0.033)	(0.031)	(0.025)	(0.022)
Self-Assessed Health	0.017	-0.003	-0.020	-0.017
	(0.035)	(0.034)	(0.027)	(0.023)
Has Primary MD	-0.073**	-0.104***	-0.031	-0.044*
	(0.034)	(0.032)	(0.027)	(0.024)
Uninsured	0.000	-0.015	-0.015	-0.020
	(0.040)	(0.038)	(0.028)	(0.025)
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 share refers to demand expressed on tablet as a share of all non-incentivized tests. Ex post share refers to demand after meeting doctor as a share of all non-incentivized tests. 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 $0 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)
	Outcome = Ex Ante Share Non-Incentivized Preventives		
	PANEL A: Demographics		
X =	*Low Income*	*High School Education*	*Younger than 40*
Black Doctor * *X*	0.016	0.072	-0.067
	(0.061)	(0.067)	(0.064)
X	-0.204***	-0.196***	0.071
	(0.043)	(0.046)	(0.045)
Black Doctor	0.023	-0.013	0.053
	(0.044)	(0.055)	(0.039)
Observations	571	556	620
	PANEL B: Hassle Costs		
X =	*Long Wait Time*	*High Congestion*	*Long Commute*
Black Doctor * *X*	-0.037	0.032	0.029
	(0.075)	(0.070)	(0.061)
X	-0.103*	-0.185***	-0.141***
	(0.053)	(0.049)	(0.043)
Black Doctor	0.053	0.029	0.018
	(0.044)	(0.052)	(0.040)
Observations	455	451	637
	PANEL C: Medical Care Experience		
X =	*ER Visits*	*No Recent Screening*	*Medical Mistrust*
Black Doctor * *X*	-0.014	0.084	-0.025
	(0.009)	(0.092)	(0.042)
X	0.001	-0.096	0.020
	(0.007)	(0.072)	(0.030)
Black Doctor	0.034	0.014	0.058
	(0.039)	(0.034)	(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 specificaiton 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. 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									
	Blood Pressure			*BMI*			*Diabetes*		
Black Doctor	0.026	0.108***	0.082**	0.021	0.162***	0.141***	0.055	0.207***	0.152***
	(0.039)	(0.033)	(0.034)	(0.040)	(0.036)	(0.033)	(0.039)	(0.039)	(0.029)
Control Mean	0.56	0.72	0.16	0.50	0.61	0.11	0.37	0.43	0.05
Observations	637	637	637	637	637	637	637	637	637
PANEL B									
	Cholesterol			*Flu Vaccination*			*Share of All Non-Incentivized Tests (Excludes Flu)*		
Black Doctor	0.012	0.262***	0.250***	0.006	0.114***	0.108***	0.028	0.185***	0.156***
	(0.038)	(0.038)	(0.032)	(0.038)	(0.038)	(0.034)	(0.030)	(0.029)	(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 by column heading. Ex ante refers to demand expressed on tablet. Ex post refers to demand after meeting doctor. 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: Medical Necessity

	(1)	(2)	(3)	(4)	(5)	(6)
	Ex Ante	Ex Post	Delta	Ex Ante	Ex Post	Delta
X =	*At-Risk, Cholesterol*			*At-Risk, Diabetes*		
Black Doctor * *X*	0.022	-0.011	-0.033	-0.046	-0.125	-0.079
	(0.088)	(0.090)	(0.076)	(0.135)	(0.132)	(0.082)
X	0.030	0.063	0.034	0.233**	0.176*	-0.056
	(0.062)	(0.062)	(0.049)	(0.102)	(0.101)	(0.062)
Black Doctor	-0.009	0.264***	0.273***	0.047	0.209***	0.162***
	(0.077)	(0.078)	(0.066)	(0.041)	(0.041)	(0.032)
Observations	620	620	620	627	627	627

Note: Table reports OLS estimates from a modified version of of Equation 3 including interactions between black doctor and an indicator for whether the subject was at-risk for cholesterol or diabetes. See Data Appendix and text for details on the at-risk groups. The outcome varies by column heading. Ex ante refers to demand expressed on tablet. Ex post refers to demand after meeting doctor. 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)
	Age	*Experience*	*Medical School Rank*	*From California*
Black Mean	43.50	15.17	24	0.50
Non-Black Mean	41.13	12.25	11	0.25
p -value	.370	.322	.181	.186
Observations	14	14	14	14

Note: Table reports estimates from a t-test of doctor race on other doctor characteristics. See Data Appendix and text for variable definitions. Difference in mean 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								
	Reception Officer			*Study Date*			*Recruitment Location*		
Black Doctor	0.037	0.192***	0.155***	0.032	0.178***	0.147***	0.035	0.184***	0.149***
	(0.031)	(0.029)	(0.022)	(0.030)	(0.029)	(0.022)	(0.030)	(0.029)	(0.022)
$5 Incentive	0.027	0.059*	0.032	0.032	0.047	0.015	0.026	0.043	0.017
	(0.037)	(0.035)	(0.027)	(0.036)	(0.035)	(0.027)	(0.037)	(0.037)	(0.028)
$10 Incentive	-0.006	0.010	0.016	-0.005	-0.008	-0.003	-0.011	-0.012	-0.001
	(0.036)	(0.034)	(0.025)	(0.036)	(0.034)	(0.025)	(0.036)	(0.035)	(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								
	All Subjects			*Without Assisted Subjects*			*Strict Specification*		
Black Doctor	0.022	0.178***	0.156***	0.016	0.178***	0.162***	0.031	0.179***	0.149***
	(0.030)	(0.028)	(0.022)	(0.031)	(0.029)	(0.023)	(0.032)	(0.030)	(0.023)
$5 Incentive	0.040	0.062*	0.022	0.027	0.064*	0.038	0.033	0.067*	0.035
	(0.037)	(0.035)	(0.028)	(0.038)	(0.036)	(0.028)	(0.039)	(0.037)	(0.028)
$10 Incentive	-0.000	0.001	0.001	-0.009	0.004	0.013	-0.023	-0.009	0.014
	(0.036)	(0.034)	(0.026)	(0.037)	(0.035)	(0.026)	(0.038)	(0.037)	(0.027)
Control Mean	0.44	0.53	0.08	0.45	0.53	0.08	0.45	0.53	0.08
Observations	652	652	652	623	623	623	578	578	578

Note: Table reports OLS estimates of Equation 3, adding in certain fixed effects (Panel A) or 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 varies by column heading. Ex ante refers to demand expressed on tablet. Ex post refers to demand after meeting doctor. 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: Time Spent, Communication, and Satisfaction with Doctor

	(1)	(2)	(3)	(4)	(5)
	Subject Talk to MD	*Doctor Notes About Subject*	*Non-Preventive Notes*	*Subject Rating*	*Subject Recommend MD*
Black Doctor	0.092**	0.089**	0.100*	-0.044	-0.007
	(0.045)	(0.044)	(0.053)	(0.050)	(0.011)
$5 Incentive	-0.078	0.062	0.004	-0.022	0.006
	(0.054)	(0.054)	(0.069)	(0.081)	(0.017)
$10 Incentive	-0.077	0.022	-0.074	0.094	0.010
	(0.054)	(0.053)	(0.067)	(0.059)	(0.016)
Control Mean	0.35	0.32	0.19	4.80	0.99
Observations	498	498	247	453	469

Note: Table reports OLS estimates of Equation 3, adding in indicators for each screening and controlling for the length of the clinic visit. 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. 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.

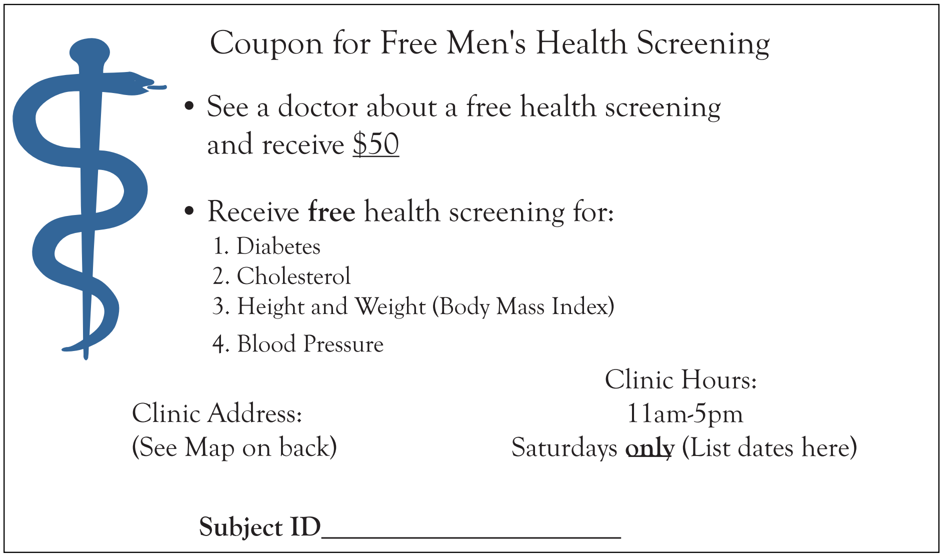
Appendix Table 9: q-values on Significant Results

(Table 3)		*(Table 4)*		*(Table 5)*		*(Table 6)*		*(Table 7)*		*(Table 9)*	
BP — Black MD	0.107 {0.001} **{0.006}**	BP — Black MD	0.103 {0.002} **{0.008}**	Flu — $5	0.117 {0.054} **{0.138}**	Delta Sh. — Black * LI	0.087 {0113} **{0.167}**	Length — Black MD	4.918 {0.000} **{0.001}**	Delta Sh. — Black MD	0.159 {0.000} **{0.001}**
BP — Black MD	0.082 {0.018} **{0.054}**	BP — Black MD	0.072 {0.037} **{0.101}**	Flu — $10	0.338 {0.000} **{0.001}**	Delta Sh. — Black MD	0.113 {0.000} **{0.001}**	Length — $5	2.537 {0.060} **{0.151}**	Delta Sh. — Black MD	0.153 {0.000} **{0.001}**
BMI — Black MD	0.161 {0.000} **{0.001}**	BMI — Black MD	0.157 {0.000} **{0.001}**	Flu — Black * $5	0.157 {0.069} **{0.170}**	Delta Sh. — Black MD	0.192 {0.000} **{0.001}**	Length — Black MD	1.441 {0.088} **{0.204}**	Delta Sh. — Black MD	0.153 {0.000} **{0.001}**
BMI — Black MD	0.138 {0.000} **{0.001}**	BMI — Black MD	0.139 {0.000} **{0.001}**	Flu — Black MD	0.096 {0.097} **{0.217}**	Delta Sh. — Black MD	0.135 {0.000} **{0.001}**	Subj. Talk — Black MD	0.100 {0.010} **{0.032}**		
BMI — $5	0.078 {0.072} **{0.173}**	Chol. — $5	0.075 {0.097} **{0.194}**	Flu — $5	0.193 {0.002} **{0.006}**	Delta Sh. — Bl. * Wait	0.175 {0.001} **{0.005}**	Subj. Talk — Black MD	-0.085 {0.069} **{0.170}**		
Diabetes — $5	0.085 {0.077} **{0.182}**	Diabetes — $5	0.083 {0.071} **{0.171}**	Flu — $10	0.233 {0.000} **{0.001}**	Delta Sh. — Black MD	0.114 {0.000} **{0.001}**	MD Notes — Black MD	0.111 {0.004} **{0.014}**		
Diabetes — Black MD	0.201 {0.000} **{0.001}**	Diabetes — Black MD	0.204 {0.000} **{0.001}**	Flu — Black MD	0.131 {0.012} **{0.037}**	Delta Sh. — Bl. * Con.	0.128 {0.010} **{0.032}**	Non-Prev. — Black MD	0.123 {0.012} **{0.037}**		
Diabetes — $5	0.105 {0.028} **{0.079}**	Diabetes — $5	0.105 {0.026} **{0.074}**	Flu — $10	-0.105 {0.048} **{0.128}**	Delta Sh. — Black MD	0.103 {0.002} **{0.007}**				
Diabetes — Black MD	0.151 {0.000} **{0.001}**	Diabetes — Black MD	0.153 {0.000} **{0.001}**			Delta Sh. — Bl. * Dri.	0.106 {0.019} **{0.057}**				
Chol. — Black MD	0.260 {0.000} **{0.001}**	Chol. — $5	0.074 {0.097} **{0.217}**			Delta Sh. — Black MD	0.108 {0.000} **{0.001}**				

Chol. — *Black MD*	0.250 {0.000} **{0.001}**	*Chol.* — *Black MD*	0.261 {0.000} **{0.001}**	*Delta Sh.* — *Bl. * ER*	0.012 {0.049} **{0.128}**
Flu — *$5*	0.192 {0.000} **{0.001}**	*Chol.* — *Black MD*	0.248 {0.000} **{0.001}**	*Delta Sh.* — *Black MD*	0.134 {0.000} **{0.001}**
Flu — *$10*	0.299 {0.000} **{0.001}**	*Flu* — *$5*	0.177 {0.000} **{0.001}**	*Delta Sh.* — *Bl. * Scr.*	0.146 {0.029} **{0.081}**
Flu — *Black MD*	0.100 {0.008} **{0.028}**	*Flu* — *$10*	0.296 {0.000} **{0.001}**	*Delta Sh.* — *Black MD*	0.122 {0.000} **{0.001}**
Flu — *$5*	0.221 {0.000} **{0.001}**	*Flu* — *Black MD*	0.104 {0.005} **{0.019}**	*Delta Sh.* — *Bl. * Mist.*	0.061 {0.048} **{0.128}**
Flu — *$10*	0.219 {0.000} **{0.001}**	*Flu* — *$5*	0.196 {0.000} **{0.001}**		
Flu — *Black MD*	0.108 {0.001} **{0.006}**	*Flu* — *$10*	0.201 {0.000} **{0.001}**		
Flu — *$10*	-0.080 {0.051} **{0.115}**	*Flu* — *Black MD*	0.110 {0.001} **{0.005}**		
Delta Sh. — *Black MD*	0.182 {0.000} **{0.001}**	*Flu* — *$10*	-0.094 {0.021} **{0.062}**		
Delta Sh. — *Black MD*	0.155 {0.000} **{0.001}**	*Delta Sh.* — *Black MD*	0.181 {0.000} **{0.001}**		
		Delta Sh. — *Black MD*	0.153 {0.000} **{0.001}**		

Note: Table reports q-values corrected for multiple hypothesis testing. Columns refer to 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 Figure 1: Clinic Coupon



Coupon for Free Men's Health Screening

- See a doctor about a free health screening and receive $50

- Receive **free** health screening for:
  1. Diabetes
  2. Cholesterol
  3. Height and Weight (Body Mass Index)
  4. Blood Pressure

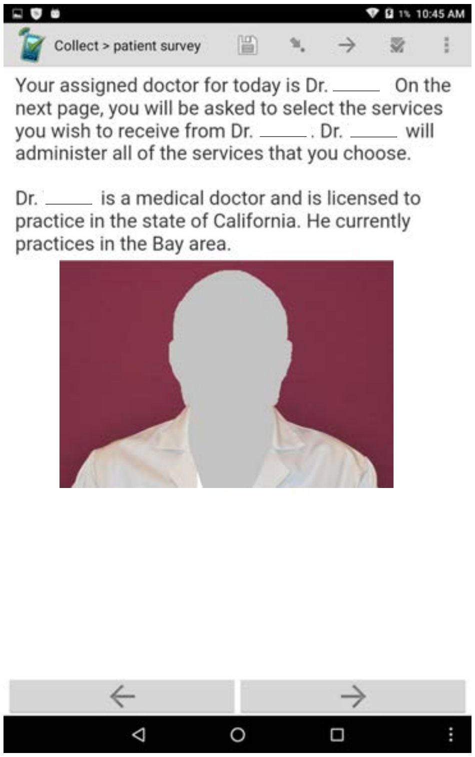
Clinic Address:
(See Map on back)

Clinic Hours:
11am-5pm
Saturdays **only** (List dates here)

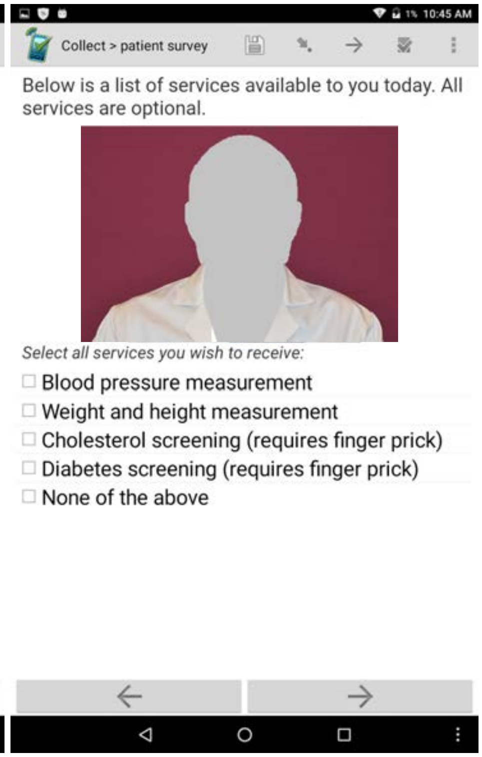
Subject ID______________________

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

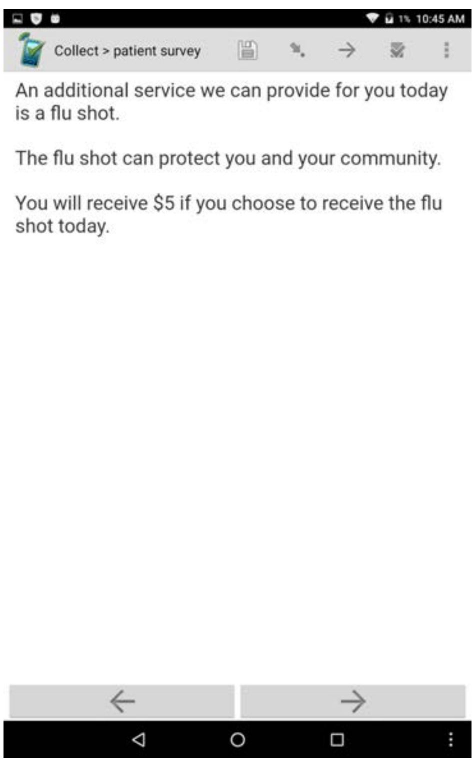
Appendix Figure 2: Tablet Photos



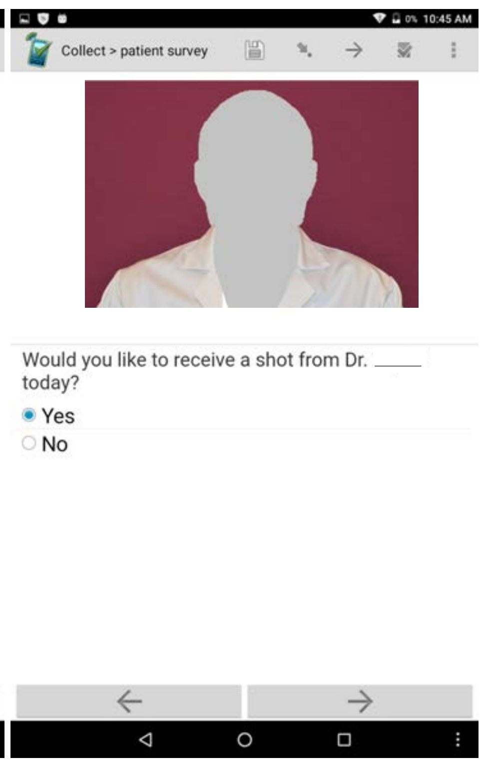
(a)



(b)



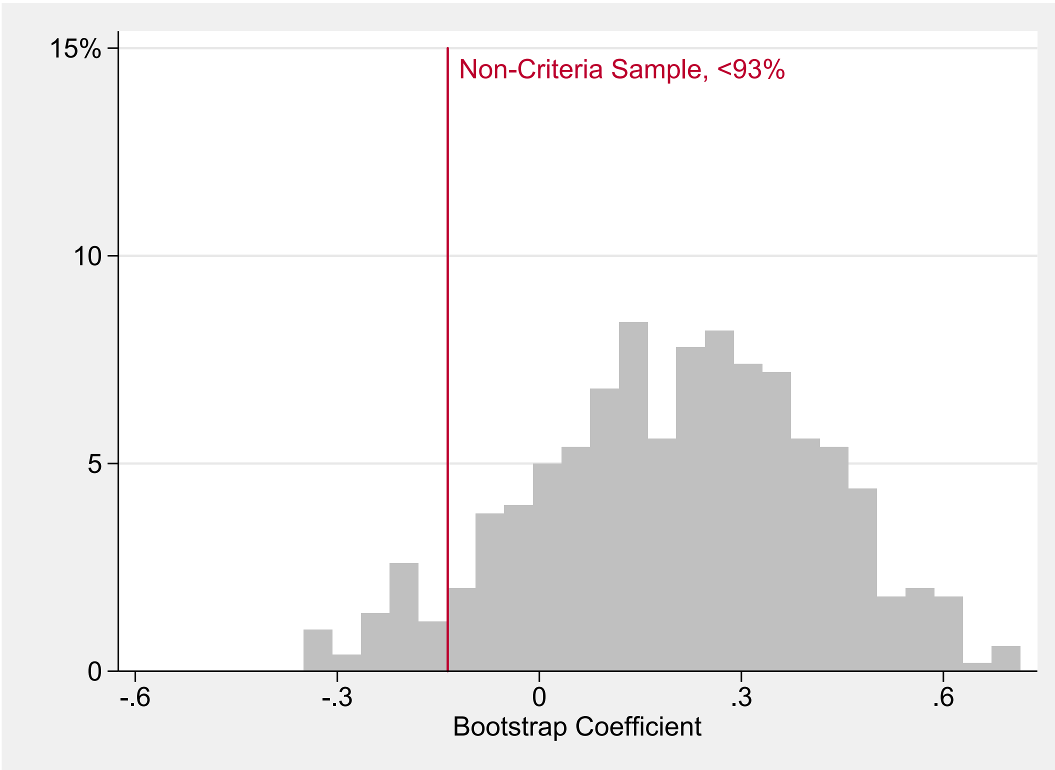
(c)



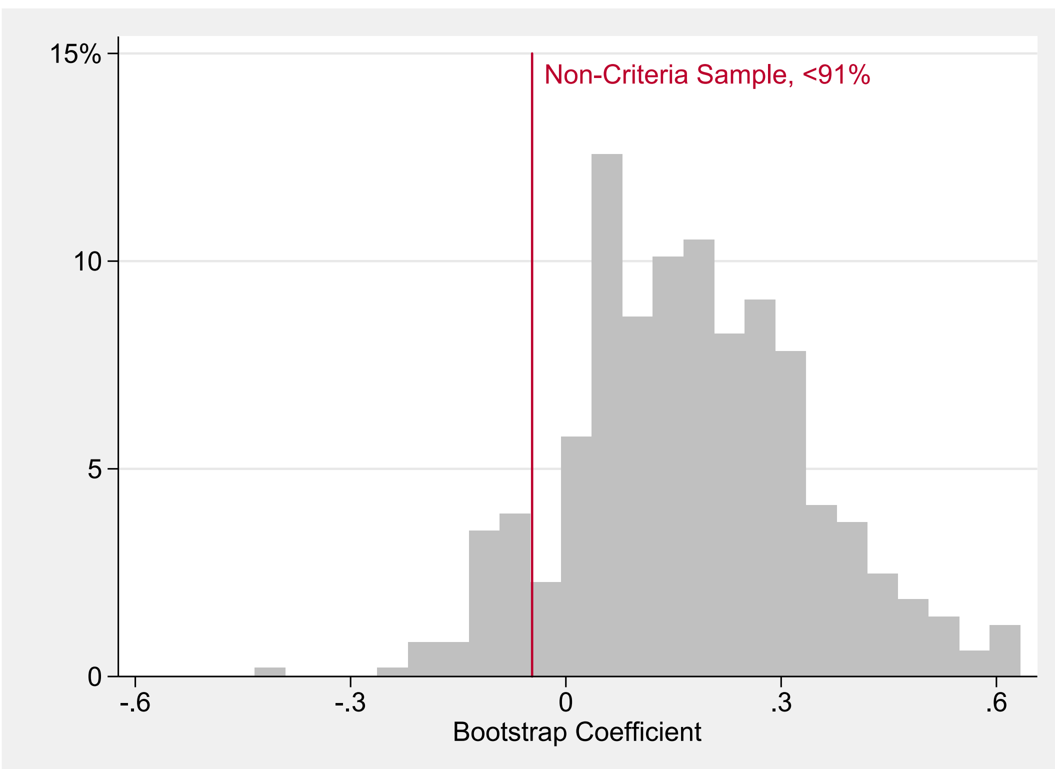
(d)

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 approximately 10 inches.

Appendix Figure 3: Permutation Test of Black Doctor Effect



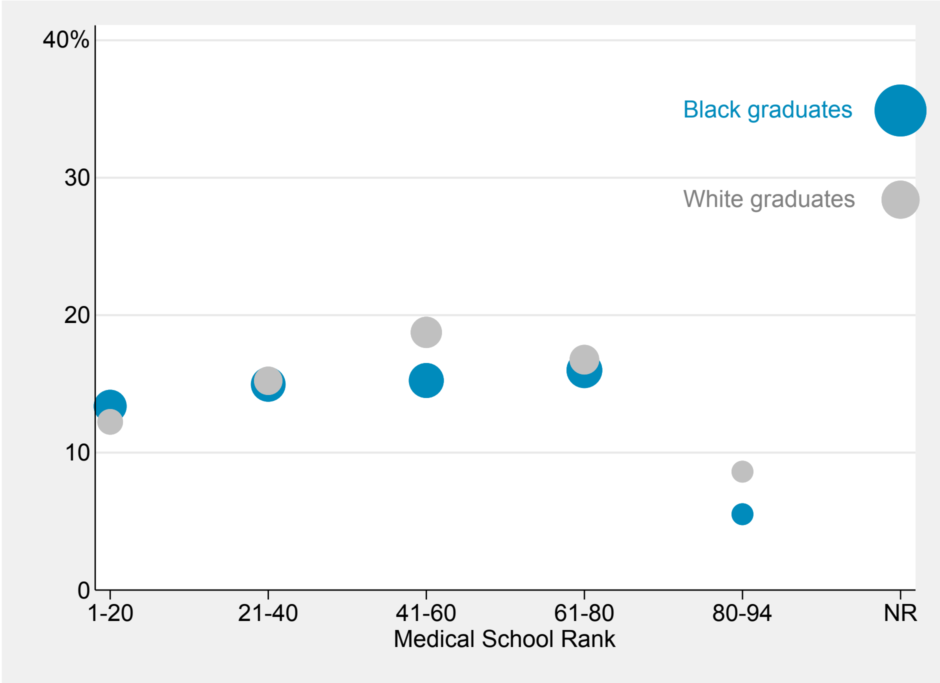
(a) Ex Post



(b) Delta Invasives

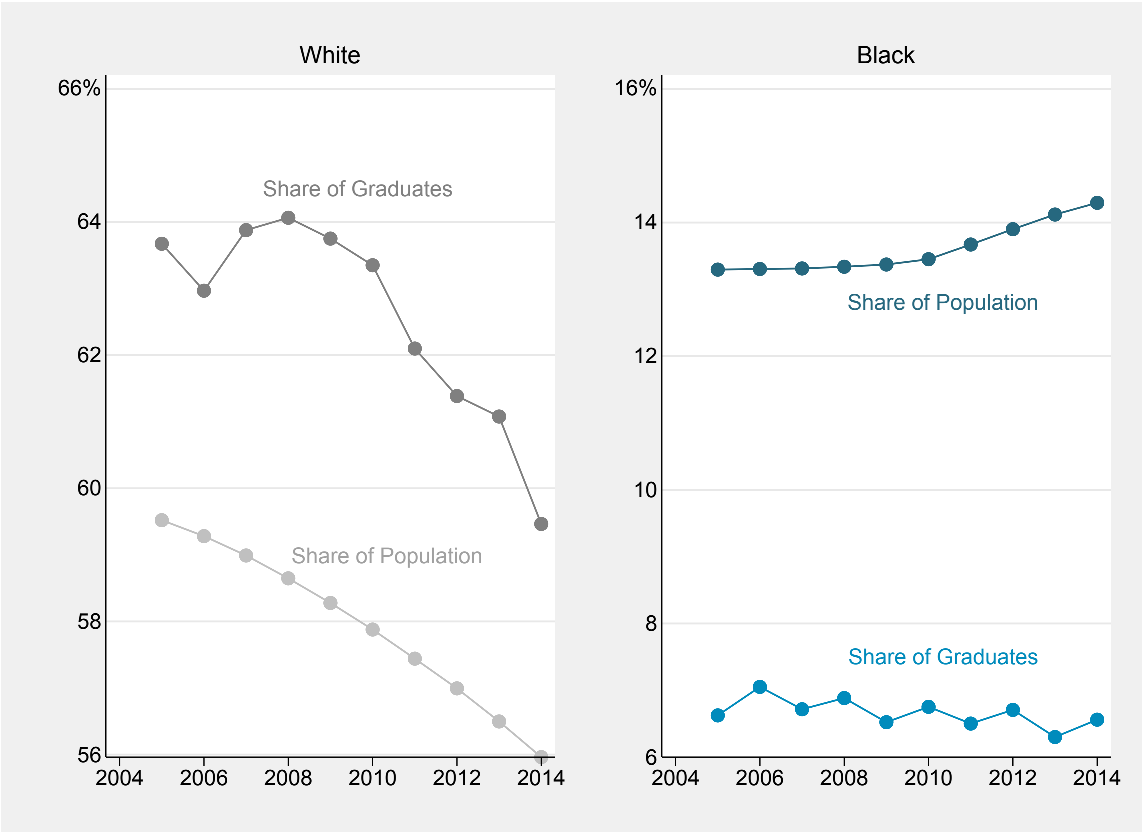
Note: Figure plots the black doctor coefficient on a random selection of N subjects without replacement, where N = 12. Permutation test runs main regression 500 times. Vertical (red) line signifies the coefficient from the subjects who did not meet study criteria.

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 race-specific medical school graduate population in each category.

Appendix Figure 5: 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 race-specific medical school graduates as a share of all graduates ("Share of Graduates") and race-specific 20–29-year-olds as a share of all 20–29-year-olds ("Share of Population").