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The Economics of Sexual and Mental Health

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ABSTRACT

THE ECONOMICS OF SEXUAL AND MENTAL HEALTH

By

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In recent years, sexual and mental health have become increasingly important in both national discourse and policymaking. These shifting priorities present unique opportunities for economists to study the market contexts, incentives, and trade-offs faced by those making choices about their sexual and mental health. This dissertation's chapters utilize a mix of experimental and quasi-experimental methods to examine how changes in the health policy landscape affect choices about the treatment and prevention of sexually transmitted illnesses (STIs), and how patterns of discrimination affect access to mental healthcare markets.

Chapter 1 estimates the causal effect of a major eligibility expansion of a major expansion of eligibility for high-quality public health insurance on utilization of medications used to treat and prevent the viral STIs: Human Papilloma Virus, Genital Herpes, and viral Hepatitis. I find a significant increase in uptake of prescriptions used to treat viral STIs, principally driven by uptake in prescriptions to treat genital herpes. These increases appear to be driven primarily by these eligibility expansions, and not by trends of incidence of the illnesses themselves, suggesting pent-up demand for the treatment and prevention of common viral STIs.

Chapter 2 similarly estimates the causal effect of the same public health insurance eligibility expansion on the incidence of HIV and AIDS, mortality, and prescriptions for specialty combination drugs used to treat and prevent these diseases. Using matched data from the Centers for Disease Control and Prevention and the Centers for Medicare and Medicaid Services, I find that while Medicaid expansions had an ambiguous effect on the actual incidence and mortality of HIV and AIDS, it led to a modest uptake in prescriptions for these combination drugs.

Chapter 3 details the results of a field experiment aimed at detecting evidence of discrimination against transgender individuals, racial, and ethnic minorities in access to mental health appointments. Constructing a nationally-representative sample of mental health providers, we conduct an audit study in which fictitious prospective patients request mental health appointments. We find evidence of discrimination against both racial and ethnic minorities and transgender individuals, especially along intersectional lines.

THE ECONOMICS OF SEXUAL AND MENTAL HEALTH

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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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Dedication

I dedicate my dissertation first to my beloved mother, my first teacher, whose unfathomable love and devotion shaped the core of who I am. Secondly, I dedicate it to my father, who challenged me to ask questions, to think deeply, and who taught me the true meaning of "iron sharpens iron." Next, to my dearest friend Miles, who challenged me to dream this dream, but who passed from this world before the dream came to pass. He lives forever in my heart and memory. Next, I dedicate it to the faculty and staff of the College of Coastal Georgia, especially to Dr. Don Mathews, whose words tumble involuntarily from my mouth when I open it to teach, and to Dr. Marci Culley, whose voice (now internalized) points me always toward justice. Finally, I dedicate it to dear and precious friends: Chris and Tamara, who reminded me that old friends are better than new; Roby and Tyler, whose support and friendship sustained me in dire times; to Janackeh, Elijah, and Spencer, more than friends—family; and to my precious brothers, sisters, and nonbinary siblings in the Voices of Note family, with whom I hope to lift my voice again one day. To the innumerable others who have invested and believed in me, I send my humble thanks. What an embarrassment of riches!

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Table of Contents

List of Figures

List of Tables

Introduction

Healthcare is uncertain, contagious, and deeply connected to public finance (Bhattacharya, Hyde, and Tu, 2014). While risk and uncertainty define the central problems of many health decisions, when it comes to decisions about mental and sexual health, another problem emerges: stigma.

Choices about sexual partners and the kinds of interactions we have with them are driven by our most primal urges, urges that often cloud perceptions of risk, but these choices can have far-reaching, even dire consequences. Decisions about sexual health are often fraught with stigma and socio-cultural practices that can make them deeply private, siloing individuals from information that can help them make better choices. Mental health decisions are shrouded in a different kind of stigma, but also uncertainty about the science supporting or the efficacy of treatment (Beck, et al., 1995).

Since such stigma exists with regard to choices about sexual and mental health, there exists large incentives to obfuscate those choices. Consider, for example, underreporting taking drugs meant to treat mental illnesses. Bharadwaj, Pai, and Suziedelyte (2017) estimate that, conditional on taking prescription psychotropics, underreporting of taking those cases increases sharply over the lifespan, and especially for men and those with less education, and at far higher rates than non-psychotropic drugs. Similarly, Ramand (1980) and Schulz and Canning (2012) find that women underreport their contraceptive choices not only to researchers, but to their husbands as well.

These issues are particularly pronounced among members of the lesbian, gay, bisexual, transgender, and queer/questioning (LGBTQ) community. In addition to facing worse outcomes

with regard to both sexual health (CDC, 2018) and mental health (Burgess, et al., 2008), LGBTQ people face stigma and discrimination that compound into a phenomenon called "minority stress", which can exacerbate these already-poor outcomes. However, there exists a dearth of economic research evaluating sexual and mental healthcare choices in both the general case as well as the choices made by LGBTQ-identifying people.

The following three essays propose to answer economic questions about uptake, implementation, and retention in care for mental or sexual healthcare, where possible through the lens of LGBTQ-identifying individuals. Economic theory tells us how these individuals should behave in light of changing incentives, but combining quasi-experimental and experimental methods will allow us to test the extent to which the theory matches the evidence.

In the first two essays, I use quasi-experimental methods to examine the effect of expanding access to Medicaid, a government-provided health insurance program, on drugs used to treat certain sexually transmitted infections (STIs): first on the herpes, human papilloma virus, and hepatitis b, and then in the second essay, I zoom in on drugs used to treat and prevent human immunodeficiency virus (HIV) in addition to examining the effect of Medicaid expansion on the incidence and mortality of this illness. I use applied econometric techniques to compare states who expanded Medicaid under the Patient Protection and Affordable Care Act (ACA, also called "Obamacare") to those who didn't.

In the third and final essay, my coauthors and I present the preliminary findings of the first large-scale audit experiment aimed at uncovering evidence of discrimination against cisgender women, transgender women, transgender men, non-binary people, and racial and

ethnic minorities (African American and Hispanic individuals) in access to mental health services. As the experiment moves into its third phase, we classify the magnitude and differences of discrimination against these groups, summarize preliminary findings of the second phase of the experiment, and detail our planned next steps.

Chapter 1: Pent Up: Public Health Insurance and Prescription Medications for Sexually Transmitted Illnesses

1.1 Introduction

Among the most consequential decisions an individual will make about their health is the set of decisions about when, how, and with whom to have sex. Sex comes with many potential risks: unwanted or unplanned pregnancies, social stigma, psychological and physical pain, and potential sexually transmitted infections. Despite this risk, STIs in the United States are prevalent. By some estimates, as many as 1 in 2 individuals will contract an STI at some point in their life (CDC, 2018).

According to the National Institutes of Health (NIH, 2015), there are more than 20 types of STIs (also called sexually transmitted diseases or venereal diseases), the most common of which are: chlamydia, genital herpes, gonorrhea, HIV/AIDS, human papillomavirus (HPV), syphilis, and hepatitis. The overall health risk of STIs depends upon the specific infection, but can range from mild discomfort, to sterility, and even to psychosis. In addition, there exists social stigma not only toward the diagnosis of an STI, but toward screening for an STI (Hood and Friedman, 2011), which has been shown to decrease disclosure of previous diagnoses between partners in addition to decreasing uptake of screenings. Those living with STIs also report increased psychological distress, which can sometimes interfere with social responsibilities like work and family obligations.

STIs are shockingly prevalent in the United States. According to the Centers for Disease Control, nearly 20 million new STI cases will be reported each year (CDC, 2018). These are not

counting new infections of the most prevalent STIs like HPV and herpes for which the CDC does not collect data. HPV, the most common STI, will be contracted by virtually every sexually active adult at some point during their life. However, there is significant heterogeneity in the risk of infection across illnesses. For example, heterosexual women are most at risk for chlamydia, however young men of color who have sex with other men are most at risk for HIV. According to the Kaiser Family Foundation (2020), the annual direct costs in the U.S. associated with STIs is about $16 billion. Hence, while STIs impose heavy burdens on infected individuals, they also confer comparably heavy societal cost.

One possible contributing factor to the state of sexual health in the United States is public ignorance of data–most of the general public simply does not know how common STIs are. Recent polling (Kirzinger et. al, 2020) shows dismal public knowledge on the basic facts of STIs. Only 13% of US citizens can correctly guess the prevalence of STIs, while over half (57%) personally have known someone with an STI. Only about 1 in 10 of the general public fear that they are at risk of contracting an STI, and only about 1 in 5 young adults.

While many report that they are comfortable talking about their own sexual health with their doctor or sexual partners, some research contradicts this self-reported comfort. For example, Fortenbery, et al., (2001) find that 59% of surveyed men had never been tested for HIV due to fear of societal consequences of such a test, and another 44% stated that they would refuse a test if they were required to provide their name. Another study (Nack, 2000) interviewed women living with chronic STIs like herpes and HPV viewed their sexual selves as “damaged goods”, and since many were “passing” as healthy (presenting as asymptomatic), some reported lying about their sexual health to sexual partners. Overall, many find the

prospect or diagnosis of an STI fundamentally embarrassing, leading to negative perceptions of the testing process, lower willingness to seek treatment, and lower likelihood to share positive diagnoses with sexual partners (Balfe, et al., 2010). Despite ignorance and stigma, all STIs are preventable, and treatments exist that can cure many. However, one main reason for the high prevalence and spread of STIs is that many are initially asymptomatic, leaving many living with STIs to not become aware of their infection until individuals seek out treatment for other more serious. conditions. While many STIs can be cured, viral STIs such as herpes simplex viruses 1 and 2 (HSV-1 and HSV-2), HPV, HIV/AIDs, and viral hepatitis (Hepatitis A and B or HVA and HVB) can only be treated, though some STIs like HPV, HVA, and HVB may be prevented altogether with vaccines.

While administrative data regarding curable (bacterial) STIs are prevalent, less administrative data is collected on incurable (viral) STIs. For example, the Centers for Disease Control and Prevention publishes annual counts, prevalence, and infection rates for all major bacterial STIs: chlamydia, gonorrhea, syphilis, and cancroid. However, the CDC only directly tracks hepatitis B and HIV, leaving national data on herpes and HPV to be inferred from surveys (McQuillan, et al., 2018) despite these being among the most prevalent STIs. Indeed, the CDC does not recommend testing for genital herpes in particular unless individuals experience symptoms (CDC, 2015), citing potential ineffectiveness of serological testing.

Despite the efficacy of treatment and prevention measures (CDC, 2016), STI prevalence is particularly high among low-income and uninsured individuals. While a loose, ad hoc network of public and private support funds clinics and aid programs aimed at curbing STIs for this population, lack of insurance and inability to pay for treatment is a major predictor of new

infections (Lee, et al., 2018). Therefore, expanding free or low-cost insurance to low-income or uninsured people likely removes barriers that are cost-related to the uptake of treatment and prevention of STIs, in particular STIs that are chronic.

Starting in 2010, the United States government began the rollout of the Affordable Care and Patient Protection Act (ACA). This legislation, the largest of its kind in scope and execution in modern history, directly targeted inadequacies in existing U.S. healthcare markets. One principle aim of the ACA in its original form was provision of universal health insurance coverage by reducing the uninsurance rate to virtually zero. The mechanisms by which this would be achieved were twofold: the government would simultaneously require and subsidize employer-offered health insurance benefits as well as expand eligibility requirements for Medicaid, a public health insurance program for those living in poverty. Hence, those who could afford healthcare would have it subsidized by their employers and government tax credits while those who could not would have it subsidized by the federal government altogether. (Frean, Gruber, and Sommers, 2017). In the years prior to the rollout of the ACA, the uninsurance rate was just over 15%, and by 2018 it fell to 8.5% (Zammitti, Cohen, and Martinez 2017)

In this chapter, I attempt to quantify the effects of ACA-related Medicaid expansions that occurred between 2011 and 2018 on medications primarily used in the treatment of the viral STIs herpes, HPV, and hepatitis B for which Medicaid acted as a third-party payer. I use prescriptions per capita as a clinical proxy (Lehmann et al. 2014) for patient uptake of these medications. The fact that these medications are covered by Medicaid, taken together with the fact that out-of-pocket cost for these medications can cost as much as $1000 per prescription

implies that without the aid of Medicaid, an otherwise-uninsured Medicaid-elligible person could not reasonably afford them.

To estimate these effects, I combine administrative data on the universe of prescriptions obtained in outpatient, non-specialty settings; telemedicine, retail, and online pharmacies; both for fee for service and managed care plans for which Medicaid was a third party payer between 2011 and 2018 using a differences-in-differences (DD) design. I find that, post-expansion, prescriptions for medications used to treat the viral STIs herpes, HPV, and hepatitis increased by nearly 50% in expansion states compared to non-expansion states. Effects are similar in magnitude across drug types, but are primarily driven by drugs used to treat herpes.

The chapter proceeds as follows: in the first part of section 2, I discuss Medicaid, with emphasis on specific Medicaid expansions, the previous literature, and evidence relevant to those expansions, while in the second part I discuss the growing body of research specifically devoted to the effect of Medicaid expansion on sexual health and risky behavior. In section 3, I discuss the empirical strategy, data, variables, methods, as well as the pharmacological context of the specific drugs I study. Section 4 discusses the results of my estimation. Finally, in section 5, I conclude with a discussion first of limitations and extensions, and then policy relevance. Section 6, the appendix, contains tables and figures mentioned in subsequent sections.

1.2 Medicaid, ACA Expansions, and Related Literature

1.2.1 Medicaid Expansion Under the ACA: An Institutional Context

Medicaid was created in 1965 as part of the Johnson administration's "Great Society" suite of domestic antipoverty programs. It covers over 70 million low-income children, pregnant women, adults, senior citizens, and disabled people (about 1 in 5 Americans). It is the primary insurer for low-income people in the United States (Sommers and Grabowski, 2017). Medicaid is a joint program between states and the federal government, and prior to the ACA was largely federalist in nature with states granted wide latitude in the administration of Medicaid, including the freedom to set eligibility criteria as well as benefit structure.

A key feature of Medicaid is low cost-sharing among patients, and comparatively more services eligible for coverage than private insurance. However, Medicaid does not cover testing for some STIs. (KFF, 2016) Testing for herpes is not covered at all. Coverage for HPV tests is restricted to certain categories of women despite widespread infection among men. Coverage for hepatitis B testing is restricted to those under the age of 18. These testing restrictions ensure that those who suspect that they might have herpes, HPV, or hepatitis cannot be sure of infection until they experience symptoms. Hence, given CDC estimates of herpes, HPV, and hepatitis prevalence, it could be the case that a significant population of people have never been diagnosed with these STIs, but experience symptoms for which, without insurance, they cannot be treated.

When initially passed in 2010, the ACA mandated that all states expand eligibility for Medicaid, but in 2012, the Supreme Court ruled in National Federation of Independent Business v. Sebelius that this portion of the law (and only this portion of the law) was unconstitutional, leaving the ultimate decision of whether to expand Medicaid to individual states (Rosenbaum and Westmoreland, 2012). Beginning in 2014, 31 states and the District of Columbia chose to

expand Medicaid eligibility requirements to cover non-disabled adults as well as parents up to 138% of the Federal Poverty Level (FPL) (KFF, 2016). Newly eligible individuals are insured by expansion plans that cover treatment for STIs (though sometimes not testing, as aforementioned).

Economic theory suggests that Medicaid expansions will increase quantity demanded for prescriptions by reducing out-of-pocket prices for the newly enrolled. Moreover, since primary care visits are also covered by Medicaid expansion, individuals suffering from symptoms of viral STIs like herpes, HPV, and hepatitis may receive treatment for these STIs if their primary care physician determines such treatment is needed (even in the absence of a positive test). For example, a patient might a flare-up of genital herpes (the appearance of rashes and sores on the mouth, genitals, or rectum), and seek medical attention for their symptoms, wherein they might receive a prescription for Valtrex (one popular treatment) without the need for a blood test if their primary care physician diagnoses thinks that they may have genital herpes despite the lack of a serological test. However, since Medicaid covers some contraception as well, it could be the case that Medicaid expansion could reduce incidence of viral STIs, though given declining use of the most effective contraception (condoms) in the US, this seems unlikely (CDC, 2018). Another common inference from economic theory is that by lowering the relative risk of sexual activity, Medicaid expansion could cause ex ante moral hazard: the newly-enrolled perceive that the out-of-pocket cost of risky sex declines conditional on coverage, and thus engage in riskier sexual behavior.

However, many factors may mute these effects. For instance, widespread stigma surrounding STIs might prevent the newly enrolled from seeking treatment or filling

prescriptions, especially in rural areas in which they might have personal ties to their doctor or pharmacist. Similarly, patients might be discouraged from taking up treatment if they are unfamiliar with the healthcare system itself. Finally, limited participation in Medicaid by providers or shortages of providers (Decker, 2011) could maintain non-cost-related barriers into healthcare markets. Thus, the extent to which Medicaid expansions affect uptake of treatment for non-HIV viral STIs is an empirical question. I attempt to quantify this effect.

However, the newly-enrolled are not the only channel by which an effect could emerge. An important note is that all states experienced a 5% increase in income eligibility as part of the federal government's new method of measuring income: modified adjusted gross income (MAGI). Another potential effect is what Frean, Gruber, and Sommers (2017) call the "Welcome Mat" effect, in which ACA-driven outreach to Medicaid-eligible populations "scooped up" people who were previously eligible for Medicaid, but who never applied. Previous research has shown these effects (in similar contexts) to be relatively small compared to the effect driven by the newly-eligible (Maclean, et. al, 2018), so my main analysis principally leverages variation in Medicaid availability for the newly eligible.

However, for exploiting this variation to be plausible, it needs to be the case that Medicaid expansions meaningfully affected uninsurance rates in expansion states compared to nonexpansion states, and that the newly-insured actually increased uptake of the healthcare for which they became newly insured. Frean, Gruber, and Sommers (2017) along with others (Miller and Wherry, 2017) show the former: Medicaid expansions under the ACA led to among the largest policy-driven reductions in uninsurance in modern times. Similarly, previous literature shows that the newly enrolled (as well as those who received coverage via the private

portion of the expansions) became more likely to have a primary care physician and regular checkups (Sommers, et al., 2016; Sommers, et al., 2017), as well as experienced improved health overall (Simon, Soni, and Cawley, 2017; Courtemanche, et al., 2017).

Therefore, since Medicaid expansions have been shown to both increase the number of people with health insurance that covers drugs that treat hepatitis, HPV, and herpes as well as increases the overall utilization of healthcare services by those newly-insured people, it is plausible to assume that it could have an effect on uptake of prescriptions for those drugs. Moreover, a more narrow body of work within the extant Medicaid expansion literature offers some suggestive evidence of the kind of effect to expect.

1.2.2 Sex, Drugs, and Medicaid Expansion

Predictions of ex-ante moral hazard are ubiquitous in health economics literature, especially literature with regard to health insurance and risky behavior. Early economic theory by Ehrlich and Becker (1972), develops a notion in which forward-looking utility maximizers perceive the purchase of health insurance as lowering the present-discounted expected future cost of risky behavior, and are faced with contemporaneous incentives to engage in that risky behavior. However, empirical work often yields results inconsistent with this theory.

A common criticism of this empirical work, though, is that up until the ACA, most tests leverage the variation in coverage status driven by the Medicare eligibility age cutoff of 65. (Dave and Kastner, 2009), and that the studied age group (65 and older) are less prone to risky behavior, especially risky sexual behavior (De Preux, 2011). However, Medicaid expansion

under the ACA offers similar variation in coverage status for a broader age range, especially young people.

Concurrent with the limited evidence of ex ante moral hazard within the literature is a fairly consistent finding that, in certain contexts, lowering the cost of unprotected sex increases STIs (Chesson, 2012) while increasing the cost of unprotected sex decreases STIs (Levine, 2003; Klick and Stratmann, 2008). However, these studies examine variation in the cost of condoms (usually by making them free), and other studies show that condom effectiveness is moderated by sex education (Buckles and Hungerman, 2016). This paper contributes to this literature by adding to the growing body of work that utilizes variation in health insurance coverage instead of the price of condoms.

While there are relatively few papers directly examining the effect of Medicaid expansion on sexual health, related findings are sprinkled through the larger literature. Arora and Desai (2016) find limited evidence of uptake in contraceptive use. Study of earlier expansions in Medicaid eligibility prior to the ACA found robust evidence of declines in pregnancy among both teens and newly-eligible women (Kearny and Levine, 2009).

Two papers directly examine the effect of Medicaid expansions on STIs. Oney (2018) and Willage (2020) both find that Medicaid expansion increased certain bacterial STIs, both overall and specifically among young adults. A limitation of these studies is that neither are able to observe the effect of these expansions on viral STIs (due to limitations previously discussed), and both focus on relatively early expansion periods before many states rolled out expansion in earnest. I contribute to this literature by offering a proxy for the effect of these expansions on

non-HIV viral STIs by offering prescriptions used to treat them as a kind of instrumental variable.

However, my focus in this paper is not STI incidence directly, but rather the medications used to treat them, and while no previous research examines these drugs, a small body of research shows that Medicaid expansion is associated with increased uptake of prescription medications across a variety of contexts. In general, Medicaid expansion is associated with about a 19% increase in new prescriptions (about 9 per new enrollee), with the largest increases for drugs devoted to chronic conditions (Ghosh, Simon, Sommers, 2019). My paper follows the method of Maclean, et al., (2018), who find that Medicaid expansion is associated with a 22.3% increase in psychotropic medications, medications used to treat mental illnesses, in expansion states. I expand this literature by including a previously-unexamined drug market: antivirals.

1.3 Data and Methods

1.3.1 Antiviral Medications

To perform my primary analysis, I utilize data from the Medicaid State Drug Utilization Database (SDUD). This data, compiled by the Centers for Medicaid and Medicare (CMS), contains the universe of prescription drugs used in outpatient, non-specialty settings: telemedicine, retail, and online pharmacies. Medicaid covers these drugs as a third party under the Medicaid Drug Rebate Program (U.S. Department of Health and Human Services [HHS], 2012). This data is compiled by the federal government quarterly and allows HHS to determine state and federal rebates from nearly 600 distinct pharmaceutical companies who participate in

this program. I consider both fee-for-service and managed care reimbursement schemes. Since these data directly influence the rebates collected from manufacturers and whether states receive Medicaid drug funds from CMS, both are incentivized to ensure these data are of high quality.

Following Maclean, et al. (2019), I utilize data starting in 2011 due to the inclusion of prescriptions financed by managed care plans. This is important as states have shifted toward managed care plans over time (Hurley and Sommers, 2003). Additionally, the vast majority of expansion states' newly-eligible population will be enrolled in managed care plans (Paradise, 2017). Hence, including data from before 2011 would miss a significant share variation between states in counts of prescriptions.

I use SDUD for all quarters from Q1 2011 to Q4 2018 for all 50 states and the District of Columbia. My analysis captures overall prescriptions for antiviral medications with indications for STIs, and I consider heterogeneity across specific illnesses treated: herpes simplex virus (HSV) types 1 and 2, human papilloma virus (HPV), and viral hepatitis (hepatitis A and B). Medications are listed in Table 1.1.

Table 1.1: Antiviral Medications

STI Treated	Medications
Herpes	Acyclovir, Famciclovir, Valacyclovir, Zovirax, Famvir, Valtrex
HPV	Gardasil, Cervarix, Imiquimod, Zyclara, Aldara, Veregen
Hepatitis	Entecavir, Baraclude, Vemlidy, Viread, Epivir HBV, Hepsera

Note: Data source is the Center for Disease Control and National Hepatitis Foundation Physician Treatment Guidelines. Overall medications include the union of medications in this list. More information upon request

The CDC (2015) provides treatment guidelines for STIs. I form a set of drugs to examine by first using this treatment guide. Next, I cross-examine each drug's Medline webpage to expand the list (as many drugs have multiple brand names). However, only drugs with FDA indicators for treatment of the viral STIs aforementioned are included in my analyses. I identify medications in the SDUD by product name and re-check my set of drugs using crosswalks between National Drug Codes provided by the National Bureau of Economic Research (Roth, 2017).

At this point, two principle confounds are introduced. First, the list of drugs I compile is not the universe of drugs used to treat the viral STIs upon which my analysis is centered. I argue that, given treatment guidelines from the CDC, it is highly unlikely that there exist significant deviations from these drugs in treatment. Secondly, some of the drugs in the set I compile are prescribed to treat other illnesses. For instance, many of the drugs used to treat herpes simplex virus also treat herpes zoster (shingles). This is an inherent limitation of studying antiviral drugs.

Where possible, I attempt to mitigate this by eliminating altogether drugs that can treat the viral STIs I study, but are principally used to treat others (e.g. Tenofovir is eliminated since it treats hepatitis B, but is principally prescribed to treat HIV).

1.3.2 Medicaid Expansions and Outcomes

My classification of expansion states and expansion dates follows Maclean, et al., (2019) and is found in Table 1.2. One institutional feature of Medicaid expansion under the ACA is that, following the National Federation of Independent Business v. Sebelius decision, some states expanded Medicaid while others did not, but not all states who expanded Medicaid did so at the same time. Four states expanded prior to 2011, choosing to cover parents and childless adults up to 100% of the FPL or higher. These states are considered treated in all periods. Most states expanded on January 1, 2014. Michigan and New Hampshire expanded later in 2014, while Alaska, Indiana, Louisiana, Montana, and Pennsylvania expanded in 2015 and 2016. To identify states as expanding in the data, I match Medicaid expansion dates to the SDUD by state-year-quarter. If states expand within a quarter, I code the first fully-treated quarter as the quarter of expansion.

Table 1.2: Medicaid Expansion by State

Early Expansion States	
Delaware	Prior to 2011
District of Columbia	Prior to 2011
Massachusetts	Prior to 2011
New York	Prior to 2011
Vermont	Prior to 2011

Regular Expansion States	
Arizona	Q1 2014
Arkansas	Q1 2014
Colorado	Q1 2014
Connecticut	Q1 2014
Hawaii	Q1 2014
Illinois	Q1 2014
Iowa	Q1 2014
Kentucky	Q1 2014
Maryland	Q1 2014
Michigan	Q1 2014
Minesotta	Q1 2014
Nevada	Q1 2014
New Hampshire	Q1 2014
New Jersey	Q1 2014
New Mexico	Q1 2014
North Dakota	Q1 2014
Ohio	Q1 2014
Oregon	Q1 2014
Rhode Island	Q1 2014
Washington	Q1 2014
Virginia	Q1 2014
Late Expansion States	
Alaska	Q3 2015
Indiana	Q1 2015
Montana	Q1 2016
Louisville	Q3 2016
Pennsylvania	Q1 2015

Note: Expansion dates derived from Maclean, et al., 2018. States are counted as having expanded only if a "substantial" expansion occurred (i.e. if states covered both parents and children at least up to 100% of FPL.)

I construct the number of Medicaid-financed prescriptions used to treat non-HIV viral STIs regardless of managed care or fee for service utilization. For subsequent analyses, I also

construct average reimbursement rates for these prescriptions by dividing the total number amount reimbursed by Medicaid for a drug by the total number of prescriptions for that drug in a state-year-quarter.

1.3.3 Empirical Strategy

I estimate the causal effect of Medicaid expansions on prescriptions for drugs that treat non-HIV viral STIs using the differences-in-differences model specified in Equation (1):

$$Rx_{\{st\}} = \beta_0 + \beta_1(MedExp_{st}) + \beta_2'(Controls_{st}) + S_s + \tau_t + \varepsilon_{\{st\}} \quad (1)$$

Where $Rx_{\{st\}}$is the number of prescriptions reimbursed by Medicaid in state per 100,000 18-64 year-olds in state *s* at year-quarter *t*; $MedExp_{st}$is an indicator for whether or not state *s* has expanded its Medicaid program in period *t* (one can think of this as an interaction between a state's time-invariant expansion status and an indicator variable that returns a 1 if expansion has happened in the current term or earlier). $Controls_{st}$ is a vector of time-varying characteristics from the CPS (unemployment rate, average age, sex, race, ethnicity, non-U.S. birth, and education). $S_s$and $\tau_t$are state and year-quarter fixed effects, respectively. State fixed effects will control for time-invariant characteristics that are idiosyncratic to the state while year-quarter fixed effects control for time varying national trends in prescription of these drugs. I cluster standard errors at the state level and report 95% confidence intervals.

1.3.4 Internal Validity

A key feature of the Differences-in-Differences model (DD) is that it requires the assumption of pre-treatment parallel trends in outcomes in order to plausibly recover causal

estimates. In other words, it needs to be the case that in the (unobservable) counterfactual state, treatment (expansion states) and control (non-expansion states) groups would have followed the same trend in the post treatment period. However, since this counterfactual is inherently unobservable, this assumption is not directly testable. I attempt to overcome this challenge in two ways.

First, I examine unadjusted trends in pre-expansion counts of per-capita prescriptions for both expansion and non-expansion states. If I find that these pre-treatment trends are similar regardless of expansion status (even if levels are not exactly the same), such trends suggest that the parallel trends assumption is satisfied. Second, I use pre-Medicaid-expansion data for the treatment group and 2011-2013 data for non-expansion states and estimate the regression in equation (2):

$$Rx_{\{st\}} = \gamma_0 + \gamma_1(Treat_s \times Trend_t) + \gamma_2{}'(Controls_{st} + S_s + \tau_t + \varepsilon_{\{st\}} \quad (2)$$

In this event study specification, $(Treat_s \times Trend_t)$ is an interaction between an indicator for the treatment group (expansion states) and a linear time trend. Failure to reject the null hypothesis that the coefficient on this variable is zero supports that our data satisfy the parallel trends assumption. I exclude states with expansions prior to 2011 from my event study as they are coded as treated in all periods.

1.3 Results

1.4.1 Summary Statistics and Internal Validity

Table 1.3 reports summary statistics for pre-treatment years: average prescription use per 100,000 18-64 year-olds by expansion status (excluding early expanders). Notable level

differences exist in demographic variables by expansion status, but I control for these variables. Also-notable level differences exist in counts of prescriptions. In particular, expansion states report nearly 5 times the number of prescriptions to treat hepatitis.

Table 1.3. Average Annual Antiviral Drug Usage

Average Number (Counts) of Prescriptions per Year

	Expansion States		Non-Expansion States	
	Pre-2014	Post-2014	Pre-2014	Post-2014
Drug class				
Herpes	5668.284	7003.24	4468.566	3767.29
HPV	471.0637	401.779	349.1806	236.8
Hepatitis	1573.333	1767.01	322.0179	275.834

Average Number (per 100,000 18-64-year olds) of Prescriptions per Year

	Expansion States		Non-Expansion States	
	Pre-2014	Post-2014	Pre-2014	Post-2014
Drug class				
Herpes	87.27568	107.754	73.45259	49.1654
HPV	6.099136	6.04046	5.185205	3.47867
Hepatitis	13.45058	15.603	4.161251	3.62018

Figure 1.1 maps out trends in prescriptions, which are aggregated to the state-year-quarter level, and Figure 1.2 maps average trends in per-capita terms (per 100,000 18-64 year-olds). Broadly, these trends move in parallel in the pre-treatment period, then sharply diverge following 2014. Figure 1.3 formalizes this in plotting the results of the regression model estimated from equation (2), and table 4 lists these coefficient estimates and reports their standard errors. Both visually and statistically, the results are striking: near-zero and insignificant estimates prior to Q1 of 2014 with sharp increases following Q1.

Figure 1.1: Total Prescription Utilization (per capita)

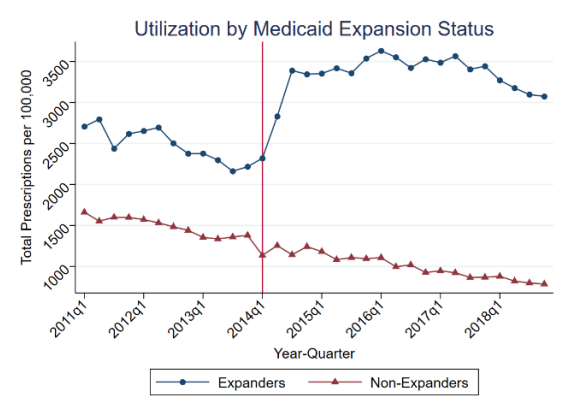


Figure 1.2: Average Prescription Utilization (per capita)

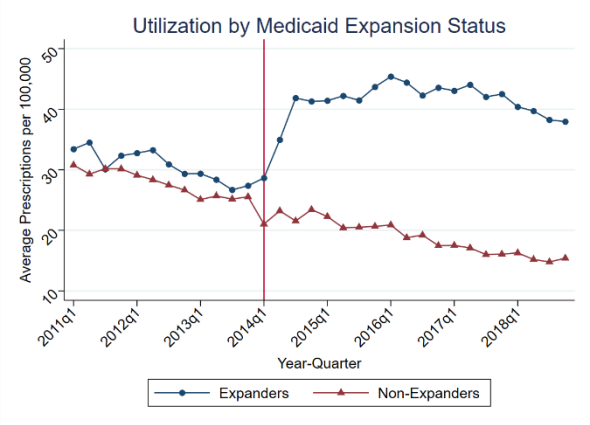


Figure 1.3: All Drugs Event Study

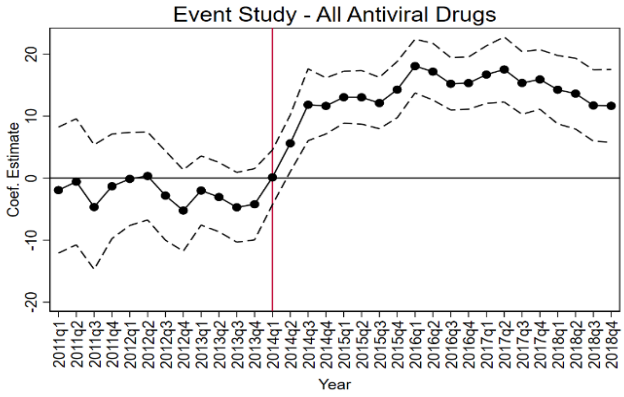


Table 1.4 Antiviral Drug Regression Estimates

	(1) With State Controls	(2) With State Controls & logged Rx	(3) + State & YQ FE	(4) + State, YQ FE, & logged Rx
dd	58.90***	0.582***	49.66***	0.453***
	(13.40)	(0.140)	(12.81)	(0.112)
N	1631	1599	1631	1599
adj. R^2	0.196	0.153	0.722	0.790

Standard errors in parentheses. All regressions clustered at the state level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

1.4.2 Difference-in-Differences: Prescriptions

In my primary analysis, I estimate regression equation (1) and recover the causal estimates for the effect of Medicaid expansion on per-capita prescriptions for antiviral drugs used to treat viral STIs. These results are listed in table 1.5. In the fully-specified model, I find that Medicaid expansion is associated with an increase of 49.66 prescriptions per 100,000 18-64 year-olds (about a 52% increase 12 over the pre-treatment baseline of 96.14 per 100,000).

However, given differences in per-capita uptake of these antiviral drugs by type shown in Table 1.3, it could be the case that there is significant heterogeneity in the effect of Medicaid expansion by drug type. To test for this heterogeneity, I estimate 3 separate regressions in which I restrict my sample to drugs treating each individual type of STI. Recall that since my event study for hepatitis treating antivirals rejects the null hypothesis of a pre-treatment effect, we must exercise caution in interpreting the results of the third regression, which estimates the

effect of Medicaid expansion on drugs treating hepatitis. Results of these regressions are detailed in Table 1.5.

Table 1.5 Antiviral Drug Regression Estimates per 100,000 by Drug Class

	(1) Herpes	(2) Herpes (with logged Rx)	(3) HPV	(4) HPV (with logged RX)	(5) Hepatitis	(6) Hepatitis (with logged Rx)
dd	44.75***	0.473***	2.136***	0.490***	3.652***	0.432***
	(12.43)	(0.122)	(0.781)	(0.160)	(0.957)	(0.0842)
N	1599	1599	1578	1578	1592	1592
adj. R^2	0.640	0.766	0.719	0.704	0.915	0.894

Standard errors in parentheses. All regressions clustered at the state level with CPS controls, State, and YQ FE.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

There is a marked similarity in the individual effects on each drug type in percent-change terms. Antivirals treating herpes increased by about 47%, those treating HPV increased by about 49%, and those treating hepatitis can cautiously be interpreted to have increased by 43%. However, it is important to note that drugs treating herpes are prescribed far more than drugs treating HPV or hepatitis, and so their increase in levels is much larger (an increase of 44.75 prescriptions per 100,000 compared to 2.13 for HPV-treating drugs and 3.65 for hepatitis-treating drugs).

1.4.3 Difference-in-Differences

A central question of this design is whether or not increases in prescriptions was financed by new enrollees or state Medicaid programs themselves. To answer this question, I next consider equation (1) with total and Medicaid payments to consider the costs of increased

prescriptions for these drugs. Following the example of Maclean, et al., 2018, I use these estimates to examine the extent to which individual state Medicaid programs bore the burden of costs over enrollees. Since these regressions are not in per-capita terms, I add the additional control of state population to the regressions.

I begin by converting all Medicaid reimbursement to 2017 terms using the CPI. Next I construct total and Medicaid payments for drugs treating viral STIs. Pre-expansion, total payments for these drugs was $1,509,800 per year, per state while Medicaid payments was $1,498,763 per year, per state. Medicaid expansion is responsible for a roughly 45% increase in total payments and about a 41% increase in Medicaid payments. These are remarkably similar percentage increases in payments when compared with the increases in prescriptions discussed in the previous subsections. However, the principle conclusion of these estimates is that since patient share of payments relative to Medicaid payments did not substantially increase for these drugs due to Medicaid expansion. Following previous research, I interpret this to mean Medicaid, not patients, provided the majority of expansion-attributable prescriptions.

1.5 Conclusion

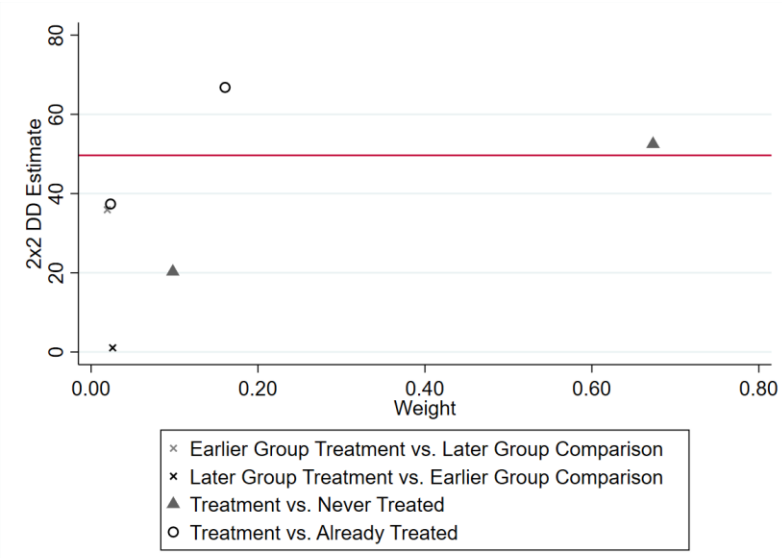
1.5.1 Robustness and Limitations

Absent non-parallel trends in pre-treatment periods, the principle threat to the difference-in-differences design employed in this paper is the staggered timing in treatment by way of heterogeneity in Medicaid expansion dates. If a state expanded, it did so as part of one of three groups: early (pre-2011) expansion, standard (Q1 of 2014) expansion, or late expansion

(after Q1 of 2014). This gives rise to the troublesome econometric condition of some treated units being compared to other treated units as a control.

Previous literature (Goodman-Bacon, 2018) points out that under the conditions of differential timing in treatment, the DD estimator derived in section 4 is the weighted average of all 2x2 estimators implicit in the data. In other words, my results in section 4 are the mean effect of comparing early expansion states to late expansion states, late expansion states to regular expansion states, states that never expanded to states that expanded at any time, and states that expanded late to states that never expanded. Careful interpretation of the causal effect of Medicaid expansion requires that we decompose these individual effects from the weighted average. Each of these estimates are plotted in Figure 1.4.

Figure 1.4: Coefficient Weights



The first decomposed 2x2 estimator compares per-capita prescriptions in states that expanded early to those who never expanded, but only in post-treatment years of the expansion. This estimate only accounts for about 2% of the weighted average of the DD

estimate, and tells us that early expanders experienced an increase of about 35.95 prescriptions per 100,000 over late-period nonexpansion states (compared to the weighted average of 49.65).

The second decomposed 2x2 estimator compares per-capita prescriptions in states that expanded late to per-capita prescriptions in non-expansion states, but only in pre-treatment years. This estimate only accounts for about 2.6% of the weighted average, and tells us that late expanders experienced an increase of only about 1 prescription relative to pre-treatment non-expansion states. This is not surprising as the event study shows a downward trend in expansion status interacted with a linear time trend, which implies an attenuation of the treatment effect over time.

The third decomposed 2x2 estimator compares per-capita prescriptions among expansion states and nonexpansion states overall, and is the primary decomposed estimator with which I am interested. It accounts for about 77% of the weighted average, and tells us that expansion states saw uptake of 48.415 prescriptions per capita. This is remarkably similar to the estimate of 49.65 derived from the (weighted average) DD estimator derived from equation (1).

Finally, the fourth decomposed 2x2 estimator compares treated units to already-treated units. It accounts for about 18.4% of the weighted average and tells us that states expanding later in the sample picked up about 63.06 more per capita prescriptions than those who expanded earlier. This is driven more by the comparison of regular expanders to early expanders than by late expanders to regular expanders given the much-larger share of regular expanders.

Hence, the results of the DD estimator derived from equation (1) is biased slightly upward, but remains a good estimate of the average treatment effect of Medicaid expansion on the number of prescriptions for drugs treating non-HIV viral STIs. For additional robustness, I simplify my model to ignore heterogeneity in treatment timing. I mark any state ever expanding Medicaid as treated and set Q1 of 2014 as the beginning of the treatment period for every treated unit. In this simplified 2x2 design, I re-estimate equation (1) and find very similar results: an increase of 45.91 prescriptions per capita.

There are some limitations to using my current design. Primarily, many drugs in the set I construct are used to treat other conditions such as STIs I do not consider (like HIV) and unrelated illnesses (like shingles and chickenpox.) I argue that this is a fundamental limitation of using SDUD for causal inference. Furthermore, I point out that overall incidence of HIV (CDC, 2018) is falling, and while incidence of herpes zoster is increasing (Harpaz, 2019), it has only increased by a small percentage even in the most at-risk category (adults over 65), a group not considered in the construction of my sample. Overall incidence of chickenpox is simply too low to account for increases in my results, and is concentrated in the very young.

The second limitation to my design is that I lack data on patients. This is related to the first limitation in that I cannot guarantee that a prescription for a drug was guaranteed to treat the an STI, nor can I determine if the medication was prescribed appropriately or if it improved a patient's symptoms. I am also not able to precisely infer incidence of herpes, HPV, and hepatitis. Currently, I am only able to refer to other data (CDC, 2018) that show herpes incidence has decreased from 59% to 48% from 1999-2016. It is unclear whether or not Medicaid expansion plays a role in that decline

1.5.2 Policy Relevance

My work suggests that there exists much latent demand for STI treatment. Given existing research already discussed in this paper, this is not surprising. Those suffering from untreated STIs face many disincentives to taking up treatment, both with regard to stigma and cost. While public health policy can likely do little (at least in the short run) to reduce stigma, the principle incentive policymakers can target is cost. This is the story that plays out across many of the empirical papers examining the overall health effects of Medicaid expansions (Simon, Soni, and Cawley, 2017; Courtemanche, et al., 2017): eliminating or reducing the cost burden of healthcare improves overall health. Also given that many of these treatments provide a prophylactic effect (Corey, et al., 2004) when used during an outbreak. In other words, these treatments are also effective as preventing spread of the disease. It could be the case that by expanding access to these drugs to new enrollees, Medicaid expansions could attenuate infections in the long run, but more research is needed.

Chapter 2: Medicaid Expansion and the Treatment and Prevention of HIV (joint with Derek Hoodin)

2.1 Introduction

HIV constitutes a public health crisis in the US. Over 1.1 million people were living with HIV at the end of 2016, and there were nearly 39,000 new infections in the last year (CDC, 2018). HIV overwhelmingly affects men (80% of all new infections), disproportionately affects African Americans and Latinx individuals (73% of all new infections), and disproportionately affects the young (nearly 56% of all new infections are below 34).

For much of the past fifty years, HIV was a death sentence. The virus attacks the immune system of its host at the genetic level, using the body's own self-replication abilities to propagate itself, and is passed on sexually, by sharing needles for intravenous drug use, or through contact with the blood, saliva, or other bodily fluids of an HIV-infected person. By the time it reaches its third and final stage, HIV will have ravaged its host's immune system beyond the point at which the host can protect itself against external pathogens. This final stage is referred to as AIDS, and the subsequent lack of any ability of the body to fight off opportunistic infections usually results in death. (Adler, et. al, 2012)

After over a decade and activism and public pressure, government investment in pharmaceutical research and development gave rise to highly active antiretroviral therapy (HAART). HAART combined a cocktail of three or more potent antriretrovirals that inhibited HIV's ability to replicate in the body. Though the first iteration of HAART had devastating (sometimes even fatal) side effects for some, wide-spread adoption in the U.S. saw the first

major decline in HIV-related fatalities. However, it soon became clear that HAART had an additional benefit: in addition to suppressing viral loads to non-fatal levels, the cocktail could suppress viral loads beyond the ability of conventional blood tests to detect. Reaching this ”undetectable” status leaves an HIV-infected individual unable to transmit the virus.

The benefits of HAART were fully realized in the early 2000s when researchers began to notice that non-infected partners in serodiscordant relationships (relationships between an HIV-infected person and non-HIV-infected person) remained non-infected even with partners who had not achieved undetectable status by sharing their partner's antiretroviral drugs. Grant, et al. (2010) showed that by taking a modified version of an existing antiretroviral cocktail, non-infected people at risk for HIV could reduce their risk of infection to effectively 0. This advancement was called Pre-Exposure Prophylaxis (PrEP), and currently only two drugs (Truvada and Descovy, both produced by Gilead Sciences) have been authorized for use as PrEP. The subsequent decline in HIV infection rates in the U.S. directly attributable to PrEP are well-documented (Grant, et. al, 2014).

However, HAART and PrEP are extremely expensive. HAART and PrEP both retail roughly between $16,000-$20,000 a year without insurance (Gebo, et. al, 2010; Horberg and Raymond, 2013). To date, HIV treatment and prevention is the largest line item in the Medicaid drug budget and the fifth largest for private insurers (Medicaid NADAC, 2018; ExpressScripts, 2017). While these costs are expected to go down with the expiration of Truvada's patent in 2021, they remain a significant barrier to entry for many.

2.1.2 HIV-Related Economics Literature

Though it is sparse, much of the extant literature with regard to HIV follows one of two main streams: empirical work examining behavioral effects of variation in HIV incidence or prevalence, theoretical work estimating moral hazard or elasticities, and some field work aimed at improving outcomes.

Lakdawalla et al., 2006 use variation in Medicaid eligibility legislation as an instrument for HIV treatment. They find that treating HIV positive individuals subsequently doubles their sexual partners, thus increasing the risk of infection to HIV negative individuals and lowering their expected welfare. This result is likely not reasonable given the contemporary state of HIV treatment in which almost all HIV-treated individuals become undetectable quite quickly (Grant, et al, 2014) due to the efficacy of modern HAART.

Auld's (2006) structural model similarly approaches the HIV epidemic from a similar perspective of moral hazard. Using data from the San Francisco Men's Health Survey (SFMHS), Auld estimates that risky sexual behavior is prevalence inelastic: men only reduced their risky sexual behavior by half a percentage point for every 1-point increase in the prevalence of HIV. Perhaps presciently, Auld suggests that (in the context of his model) a prophylactic vaccine would increase risky behavior. This is born out in the Public Health literature, which suggests that PrEP uptake is coupled with a spike in risky sexual behavior (Oldenberg, et al., 2019; Koester, et al., 2017).

Other economists have used the SFMHS to draw similar conclusions. Dow and Phillipson (1996) consider the extent to which HIV positive men match with other HIV positive men as

sexual partners. This seroconcordant matching generates a positive externality: new HIV infections are reduced by about a third. Dow and Phillipson also find that HIV positive men are about twice as likely to match with other HIV positive men, likely due to stigma surrounding serodiscordant relationships. Francis (2008) proposes that high incidence may cause individuals to pursue heterosexual relationships instead of homosexual ones. While he finds some limited evidence of this, it is worth noting that such behavior is likely driven on the intensive margin among bisexual or pansexual men (Kenneady, 2014).

Bhattacharya et al. (2003) aim to disentangle the effects of insurance and HIV-related mortality, jointly estimating demand for insurance alongside treatment. Bhattacharya and authors find that expanded access to insurance (public health insurance) in particular dramatically decrease the probability of HIV-related mortality (principally by expanding access to HAART). Given that these results are pre-PrEP, it seems reasonable to assume that they are an extreme lower bound on the benefits of public health insurance with regard to HIV-related mortality. Only one other study examines HIV and AIDS through the lens of Medicaid expansion. Gai and Marthinson (2019) finds evidence that Medicaid expansion is associated with a 3.22 percentage point increase in HIV testing rates.

2.1.2 Medicaid Expansions, Sexual Health, and Contribution to the Literature

Few papers examine the effect of Medicaid expansion on sexual health directly, but findings that elucidate the topic are part of the larger literature. Courtemanche, et al. (2017) find that expansion states saw larger relative amounts of HIV testing. Arora and Desai (2016) find limited evidence of uptake in contraceptive use. Studies of earlier expansions prior to the

ACA found evidence of declines in pregnancy among both teens and newly-eligible women (Kearny and Levine, 2009).

Oney (2018) and Willage (2020) both find that Medicaid expansion increased certain bacterial STIs, both overall and specifically among young adults, but these results are limited in that neither are able to observe the effect of these expansions on viral STIs, and both focus on relatively early expansion periods before some states expanded eligibility. Previous work shows a link between access to insurance and changes in HIV risk: Bhattacharya and coauthors (2003) and Lakdawalla and coauthors (2006) suggest that expanding access to insurance (especially public insurance) can save lives. Previous work also suggests that there could be some ambiguity to the direction of the effect: Gai and Marthinson suggest that expanding Medicaid increases HIV testing, which would increase the number of HIV-positive people in expansion states (since about 15% of those living with HIV are undiagnosed), while Bhattacharya suggests increased access to HAART should drive that number down.

We contribute to this literature by offering a proxy for the effect of these expansions on non-HIV viral STIs by offering prescriptions used to treat them as a kind of instrumental variable in addition to directly examining the effect on incidence and mortality and by showing that there is some effect on uptake of drugs used in the treatment and prevention of HIV. It stands to reason that Medicaid expansion directly pulls two different levers: it pushes HIV infections up due to increasing the number of people being tested for HIV, but it also pulls HIV infections down through increased access to PrEP and HAART, both of which are covered by Medicaid. It could also indirectly push the number of HIV infections up by lowering the perceived relative risk of a risky sexual act. If a person living in an expansion state perceives that his or her

potential sexual partners are now relatively safer because of improved health outcomes (inasmuch as Medicaid saliently provides those), he or she might be incentivized to be more sexually risky.

Finally, since it is well-known that increased respective effectiveness and availability of HAART and PrEP are correlated to increases in other STIs (CDC, 2018), it could be the case that Medicaid expansion comes with some moral hazard. Our results show that the direct effect of Medicaid expansions on actual incidence and mortality due to HIV is unclear (noisy, imprecise estimates), and similarly, its effects on the medications used to treat and prevent HIV are sensitive to the specification: the standard two way fixed effects (TWFE) estimator yields insignificant results under the standard staggered treatment specification absent "clean" controls or stacking, methods from the TWFE econometric literature.

2.2 Data and Methods

Data for this work comes from three sources: (1) the Centers for Disease Control and Prevention (CDC) annual HIV/AIDS and STD Surveillance Reports and (2) the Medicaid State Drug Utilization Database (SDUD).

Data from the CDC HIV/AIDS Surveillance Reports includes state aggregates for HIV and AIDS-related outcomes: new infections and mortality as well as breakdowns by race/ethnicity, and other demographic characteristics. We use provided per-capita counts of new HIV cases, new AIDS classifications, and mortality due to HIV or AIDS. We use data from 2010-2018 state level aggregates.

Data from the Medicaid SDUD include the universe of all filled prescriptions covered by the Medicaid Drug Rebate Program. We use data from 2011-2018 (both fee for service and managed care), and restrict our sample to the 20 combination drugs licensed by the Food and Drug Administration (FDA) for HIV treatment and prevention (shown in Table 2.1). HAART drugs are categorized under 7 different classes based on the method by which the drug interferes with the viral replication process. Combination drugs are single pills that contain combination of other drugs and represent a more holistic drug regimen than ad-hoc cocktails. We focus only on the 20 combination drugs as the most recent physician guidance from the federal government (HHS, 2021) recommends their prescription over ad-hoc cocktails of other drugs, and since the utilization of these drugs represents the vast majority of HAART reimbursed by Medicaid. There are currently only two drugs approved for use as PrEP in our treatment period, Truvada, though a second drug, Descovy has since transitioned out of rotation as treatment and is now used as prevention.

We also include controls from the ACS include state population estimates from 2011 to 2018 from all 50 states and the District of Columbia. Also included are estimates of proportion of state population that is African American or Hispanic, as well as the proportion of the state living below the poverty line. Finally, for robustness, we include state expenditures on the Ryan White program, a separate, large federal program providing HIV treatment and related medical care to low-income individuals. Over half the people diagnosed with HIV in the U.S. utilize this program. We define Medicaid expansion according to the timeline provided in Table 1.2 in chapter 1 of this work.

2.2.1 Empirical Strategy

For our preliminary analysis, we use a simple Difference-in-Differences framework to estimate the effect of Medicaid Expansion on HIV and AIDs:

$$Y_{\{st\}} = \beta_0 + \beta_1(MedExp_{st}) + \beta_2'(Controls_{st} + S_s + \tau_t + \varepsilon_{\{st\}} \quad (1)$$

Where $Y_{\{st\}}$is the number of prescriptions used for (a) HIV treatment and/or prevention reimbursed by Medicaid in state per 100,000 18-64 year-olds in state *s* at year-quarter *t* or (b) (HIV/AIDS) incidence and/or mortality in state *s* at year *t*); $MedExp_{st}$is an indicator for whether or not state *s* has expanded its Medicaid program in period *t* (one can think of this as an interaction between a state's time-invariant expansion status and an indicator variable that returns a 1 if expansion has happened in the current term or earlier). $Controls_{st}$ is a vector of time-varying characteristics from the CPS (unemployment rate, average age, sex, race, ethnicity, non-U.S. birth, and education) and Ryan White expenditures by state (for HIV/AIDS surveillance data). $S_s$and $\tau_t$are state and year-quarter (year) fixed effects, respectively. State fixed effects will control for time-invariant characteristics that are idiosyncratic to the state while year-quarter (year) fixed effects control for time varying national trends in prescription of these drugs. I cluster standard errors at the state level and report 95% confidence intervals.

2.2.2 Internal Validity

The Differences-in-Differences model (DD) requires the assumption of pre-treatment parallel trends in outcomes in order to plausibly recover causal estimates. This counterfactual is inherently unobservable, but we attempt to overcome this challenge in two ways.

First, we examine unadjusted trends in pre-expansion outcome variables of interest (HIV/AIDs incidence/mortality vs.per-capita counts of prescriptions) both expansion and non-expansion states. If I find that these pre-treatment trends are similar regardless of expansion status (even if levels differ), such trends suggest that the parallel trends assumption is satisfied. Second, I use pre-Medicaid-expansion data for the treatment group and 2011-2013 data for non-expansion states and estimate the regression in equation (2):

$$Y_{\{st\}} = \gamma_0 + \gamma_1(Treat_s \times Trend_t) + \gamma_2'(Controls_{st} + S_s + \tau_t + \varepsilon_{\{st\}} \qquad (2)$$

In this event study specification, ($Treat_s \times Trend_t$) is an interaction between an indicator for the treatment group (expansion states) and a linear time trend. Failure to reject the null hypothesis that the coefficient on this variable is zero supports that our data satisfy the parallel trends assumption. I exclude states with expansions prior to 2011 from my event study as they are coded as treated in all periods.

2.2.3 Robustness

Previous literature (Goodman-Bacon, 2018) points out that under the conditions of differential timing in treatment, the DD estimator derived by the aforementioned model is the weighted average of all 2x2 estimators implicit in the data. In other words, the results derived under a standard differences in differences two way fixed effects model are the mean effect of comparing early expansion states to late expansion states, late expansion states to regular expansion states, states that never expanded to states that expanded at any time, and states that expanded late to states that never expanded. Careful interpretation of the causal effect of Medicaid expansion requires accounting for staggered treatment timing. We account for this

staggered timing in two ways: first, we use the "clean controls" approach of Cengiz, Dube, Lindner, and Zipperer (2019) in which only treated units which have never been previously considered controls or who have only ever been considered as treated units (e.g. late expanders and early expanders) are only compared to untreated units.

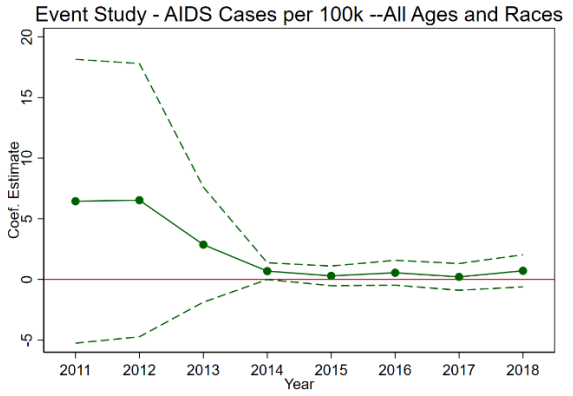
Secondly, we use the "stacked DD" approach of Abraham and Sun 2018 and Deshpande and Li 2017 in which we form a new dataset by transforming the year—quarter (year) variable into a timing variable with the year of treatment as the relative reference year (for treated units). This solves the fundamental problem of considering already-treated units as a control by reshaping the data. We do find that our results are only significantly different from zero if we address the bias caused by staggered rollout of Medicaid Expansion.

2.3 Results and Discussion

2.3.1 HIV/AIDS Incidence and Mortality

Given our model specification, the effect of Medicaid expansion on HIV incidence and mortality is unrecoverable since Figures 2.1 and 2.2 reveals that incidence and mortality measures fail the aforementioned event study design, and thus have non-parallel trends. Event studies reveal pre-treatment coefficients are noisy, and while post-treatment coefficients are far more precisely-measured, coefficients are not different than zero.

Figure 2.1: per Capita AIDS cases and Mortality Event Studies



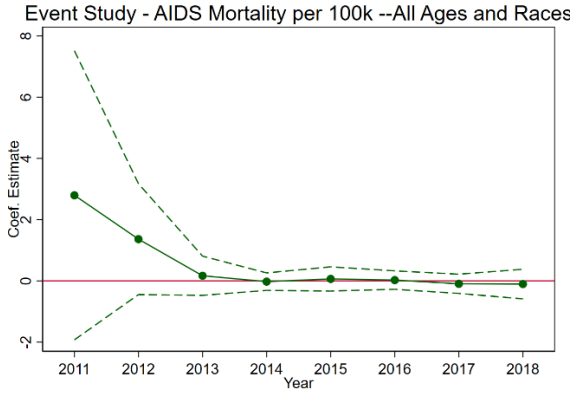
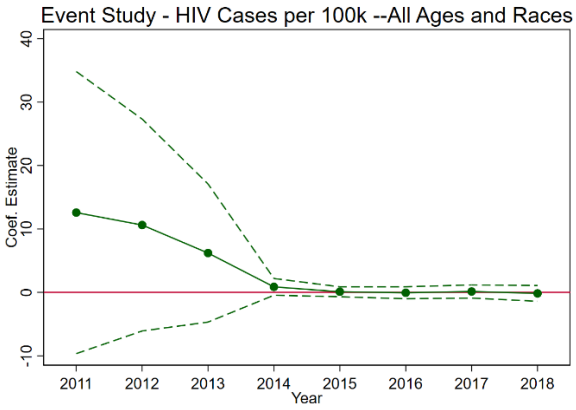
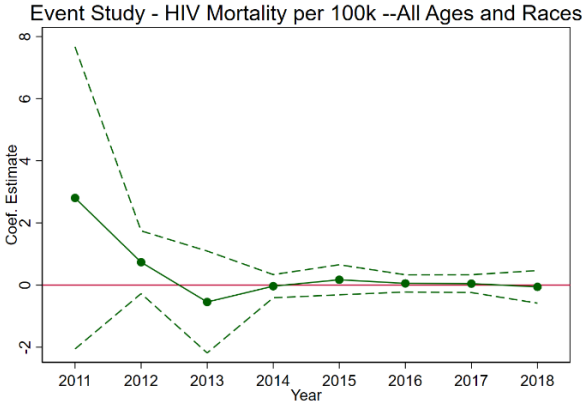


Figure 2.2: per Capita HIV cases and Mortality Event Studies





There are several possible explanations for this. The first is the most straightforward: Medicaid expansions simply didn't move the needle on the dynamics of HIV treatment and prevention all that much. The second, also likely explanation is that there is simply too little variation to precisely measure much given that the data used is from state-level aggregates of already-small measures. A third explanation is that other sources of treatment funding beyond Medicaid (like Ryan White) are more responsible for variation in HIV/AIDS-related outcomes. This is plausible given the share of HIV-infected individuals who receive funding from these sources (over 50%). A final, albeit untestable explanation for observed event study results is that Medicaid expansion had a dual effect on HIV/AIDS-related outcomes. One could imagine

the case of access to public health insurance increasing the probability of reporting a positive HIV result by way of linkage to care while also decreasing the probability of reporting a positive HIV result by way of expanded access to treatment and prevention. This duality of effects could cause the observed effect to tend toward zero.

2.3.2 HIV Treatment and Prevention

Figures 2.3, 2.4, and 2.5 reveal graphically the results of the event-study design which tests the feasibility of using the DD design for prescriptions used to treat and prevent HIV, first aggregated and for treatment and prevention separately. While coefficients in the year-quarters prior to expansion are somewhat noisily-estimated, the remain statistically not different from zero in pre-treatment periods, while post-treatment periods reveal a modest positive coefficient just outside the 95 percent confidence intervals.

Table 2.1 shows DD regression estimates for HIV treatment and prevention show that Medicaid expansion is associated with an increase of about 16 prescriptions per 100,000 overall (if adjusted for staggered treatment). Disaggregating treatment and prevention separately, Table 2.1 shows that this effect can be disaggregated into an increase of about 10 prescriptions per 100,000 with regard to prevention and an increase of about 21 prescriptions per 100,000 with regard to treatment. In percentage terms, these effects represent about a 23 percent increase over the mean of 68 prescriptions per 100,000 for all drugs, a 27%

Figure 2.3-Event Study All Drugs

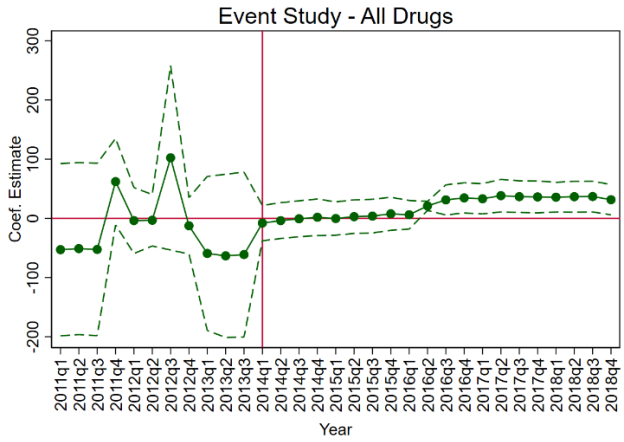


Figure 2.4-Event Study PrEP

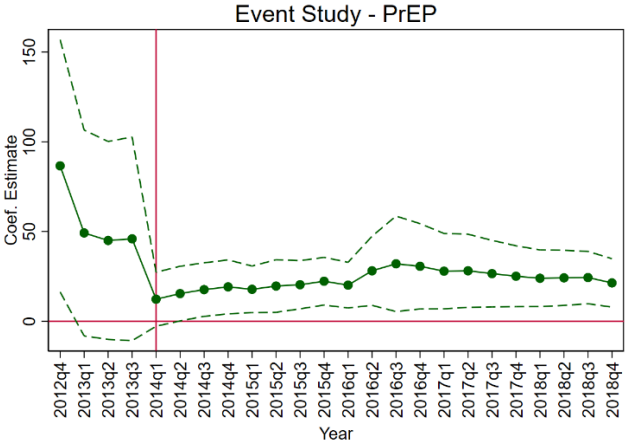


Figure 2.5-Event Study HIV Treatment

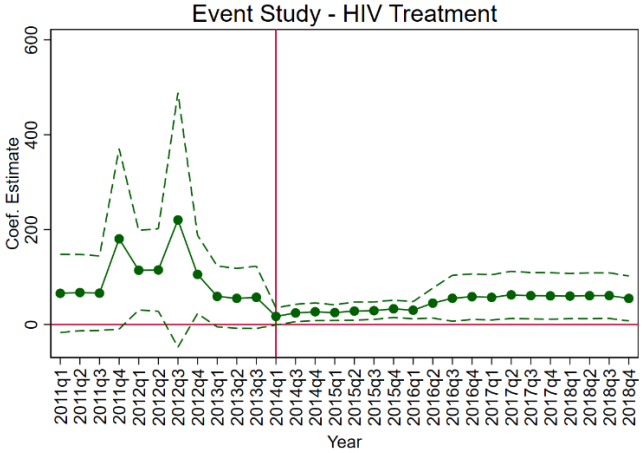


Table 2.1
Regression Estimates—All Treatment and Prevention

	(1) Basic TWFE Estimate	(2) TWFE with "Clean Controls"	(3) "Stacked" TWFE
MedExp	7.73	16.28 ***	15.95***
	(11.58)	(2.81)	(2.79)
N	1632	1268	1036
adj. R^2	0.4147	0.6403	0.7001

Standard errors in parentheses. All regressions clustered at the state level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.2
Regression Estimates—Prevention

	(1) Basic TWFE Estimate	(2) TWFE with "Clean Controls"	(3) "Stacked" TWFE
MedExp	6.79	9.82***	9.75***
	(4.72)	(2.38)	(2.56)
N	1632	1268	1036
adj. R^2	0.465	0.6125	0.6626

Standard errors in parentheses. All regressions clustered at the state level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.3
Regression Estimates—Treatment

	(1) Basic TWFE Estimate	(2) TWFE with "Clean Controls"	(3) "Stacked" TWFE
MedExp	15.44	21.73***	20.39***
	(11.21)	(5.30)	(3.79)
N	1632	1268	1036
adj. R^2	0.5353	0.6201	0.6620

Standard errors in parentheses. All regressions clustered at the state level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

increase over the mean for PrEP, and about a 20% increase over the mean for HAART. Again, all of these effects are sensitive to accounting for staggered treatment, but they do align with mid-tier estimates (Maclean, et al., 2019) of the effect of Medicaid expansion on some drug markets, though these estimates significantly undershoot the effects observed in chapter 1.

A comprehensive interpretation of these results is challenging. Given the lack of evidence of an increase (or decrease) in HIV and AIDS incidence and mortality due to the policy change, we are unable to connect the increase in utilization of these prescriptions directly to an increase in diagnosis. However, there are other explanations. Given the results discussed in Chapter 1, it could be the case that there is a pent-up demand for HIV treatment and prevention (especially prevention). For example, it could be the case that those newly-eligible for Medicaid would have previously financed these drugs out of pocket (not very plausible), or with the assistance of public and private funding sources (such as charities, pharmaceutical company rebates, or other income-contingent programs).

The imprecision of these estimates make policy recommendations difficult and imply that Medicaid expansion had an ambiguous effect on the dynamics of HIV and AIDS in the US. There is no evidence that it changed new infections or classifications of HIV or AIDS, but there is limited evidence that it modestly increased the number of prescriptions for HIV treatment and prevention reimbursed by Medicaid in expansion states relative to non-expansion states. Taken together with other evidence presented in chapter 1, we cautiously interpret these results to show a pent-up demand for drugs used in HIV treatment and prevention.

Chapter 3: Gender Identity, Race, and Ethnicity Discrimination in Access to Mental Health Care: Preliminary Evidence from a Multi-Wave Audit Field Experiment (joint with Patrick Button, Eva Dils, Luca Fumarco, and David Schwegman)

3.1 Introduction

Individuals who identify as transgender and non-binary[1] (TNB individuals) face socioeconomic status and health disparities as well as confront considerable stigma and discrimination[2] in their everyday lives (Badgett et al. 2009; Grant et al. 2011; Hughto, Reisner, and Pachankis 2015; James et al. 2016; Carpenter, Eppink, and Gonzales 2020). Compared to cisgender[1] people in the United States, TNB individuals are more likely to live in poverty, to be food insecure (Russomanno et al. 2019), to have been incarcerated, to have been the victim of an assault (particularly intimate partner violence), to be unemployed (Badgett, Carpenter, and Sansone 2020; Leppel 2020), and to lack health insurance (Carpenter, Eppink, and Gonzales 2020; James et al. 2016; Liszewski et al. 2018; Waters and Yacka-Bible 2017). TNB individuals are especially more likely to experience mental illness and severe psychological stress: TNB individuals have higher rates of anxiety, depression, substance misuse, and suicidality than non-TNB individuals (Grossman and D'Augelli 2007; Lagos 2018; Meyer et al. 2017; Miller and Grollman 2015; Mustanski et al. 2010; Nuttbrock et al. 2010; Scanlon et al. 2010; Streed et al.

[1] Throughout the paper, we will discuss transgender and non-binary (TNB) individuals together; however, these are separate gender identities and our experimental design allows us to differentially test between binary transgender and non-binary individuals. Liszewski et al. (2018) propose useful definitions that we adopt. Someone who is transgender identifies with a gender identity that does not exclusively match their gender assigned at birth. Someone who is transgender may identify as a gender that is different than the one assigned at birth, with both genders, or no gender. Non-binary individuals identify neither as exclusively male nor exclusively female, may identify as something other than male or female, may identify as multi-gendered, or may not identify with any gender. Cisgender individuals have a gender identify that matches their sex assigned at birth.

[2] Following Lahey and Oxley (2018), the term "discrimination" used throughout this paper refers to "differential treatment by demographic characteristics(s)." It does not refer solely to animus or taste-based discrimination. See footnote 11 for discussion of the different types of discrimination.

2018; Su et al. 2016). These disparities are stark. In a sample of 1,053 transgender persons, for example, 41 percent report having attempted suicide. This rate is 26 times higher than the general population (Safer et al. 2016).[3]

Despite an increased need for general and mental health services, real or perceived gender identity discrimination[4] by mental health care professionals may affect a TNB individual's ability to access (or desire to seek) appropriate mental health care services and treatment. Previous research found that approximately one-fourth of transgender individuals opted not to seek health care when needed for fear of being mistreated due to their gender identity. One-third of transgender individuals report having had a negative experience related to identifying as transgender (James et al. 2016).

If mental health care providers (MHPs) behave in a manner that limits access to mental health services for TNB individuals or discourages them from seeking treatment, it will worsen mental health disparities in several ways. First, discrimination by MHPs further contributes to minority stress. Second, discrimination delays treatment, which negatively impacts health and increases treatment costs (Boudreau et al. 2004; Himelhoch et al. 2004). Third, difficulties in securing appointments lead many patients to discontinue the search for treatment altogether (James et al. 2016; Lambda Legal 2010). Fourth, discrimination may reduce match quality between the MHP and patient by forcing the patient to select a therapist who is trans-friendly

[3] African American and Hispanic people also face significant health and socioeconomic disparities which we summarize in the background section.

[4] Small and Pager (2020) note that economics has generally focused on measurable discrimination (on differences in wages, employment, mortgage rates, or other economic outcomes) rather than perceived discrimination, for "a potential's victim's mental health, depression, stress, and related health outcomes, perceiving that it happened is everything. Perceptions of discrimination can have an effect regardless of whether the perpetrator discriminated or instead seemed to discriminate but did not actually do so." (Small and Pager 2020; 63).

but is otherwise not as suitable for the patient (e.g., less experienced in the patient's area of concern, MHP practices farther away from where the patient lives) (Mizock and Lundquist 2016). Patient-MHP mismatch negatively affects care since a high-quality match is crucial for effective care (Budge and Moradi 2018; Kantrowitz 2016).

Despite ample observational evidence that TNB individuals face substantial mental health disparities and survey evidence that TNB individuals report facing significant discrimination by health care professionals, no study has quantified the actual level of gender identity discrimination within the mental health care system against TNB individuals.[5]

We measure discrimination in access to mental health care using an audit field experiment. Audit field experiments are considered the "gold standard" for measuring discrimination (Al-Ubaydli and List 2016; Bertrand and Duflo 2017; Gaddis 2018; Neumark 2018) because they allow researchers to study discrimination in actual behavior and they allow researchers to calculate an unbiased estimate of discrimination by holding all factors other than minority status constant.

This paper provides the first experimental evidence of gender identity discrimination in the mental health care system. We further examine if this discrimination varies by race, by ethnicity, by the intersection of gender identity *and* race or ethnicity, and by common mental health concern. To do this, we conduct a large-scale experimental field study of mental health care providers throughout the United States. Specifically, we request appointments from

[5] The most relevant existing studies link the mental health disparities that TNB people face to self-reported measures of discrimination (Bockting et al. 2013; Clements-Nolle, Marx, and Katz 2006; Hendricks and Testa 2012; Miller and Grollman 2015; Perez-Brumer et al. 2015; Reisner et al. 2016; Tebbe and Moradi 2016; Testa et al. 2017). While informative, these studies do not observe actual discriminatory behavior, do not capture how often this discrimination occurs, and do not often capture whether discrimination occurs in access to health care.

mental health providers, including psychologists, counselors, social workers, and psychiatrists, using a popular online website.

In these requests, we randomly assign names to signal race and gender. Specifically, in the first wave of the study, we use masculine and feminine names that signal a prospective patient is African American, Hispanic, or white. In the text of these appointment requests, our fictitious TNB patients disclose their gender identity by including a short statement like: "*I am [a transgender woman]/[a transgender man]/[non-binary] and am looking for a trans-friendly therapist*."[6] We also randomly assign the specific mental health concern that the individual is seeking treatment (anxiety, stress, or depression). In the subsequent waves of the study, we will add names to signal that a prospective patient is Chinese American and we will also randomize a signal of insurance status to study how insurance status affects access.[7] In our appointment requests, we provide both a return email address and phone number.

We record several different categories of MHP responses to our appointment inquiries, including the offer of an appointment, a call or consultation offer, offering a placement on a waitlist, a referral to a different provider, a rejection, as well as no reply. Based on the results of the first wave of a multi-wave study, our key result is that African American and Hispanic TNB people, particularly Hispanic transgender women and non-binary African Americans, face discrimination in access mental health care. We also find discrimination against cisgender women, compared to cisgender men, and a preference for prospective patients that mention

[6] Disclosing gender identity and inquiring about LGBTQ+-friendly providers is a common and recommended practice for TNB individuals seeking mental health services (Kassel 2018).
[7] We will randomize on five insurance statuses: no mention of insurance status, self-pay with no reference to a sliding scale, self-pay with a reference to paying through a sliding scale, Medicaid, and private insurance.

depression over anxiety or stress, although these results are not robust to all possible control variables.

When complete, our study will make several contributions to the existing literature on mental health care discrimination and gender identity discrimination. This is the first paper to provide causal estimates of gender identity discrimination in the U.S. health care system. Moreover, this study is also one of the few audit studies that explicitly tests for gender identity discrimination in any context (Bardales 2013; Granberg, Andersson, and Ahmed 2020; Levy at al. 2017; Make the Road New York 2010; Rainey, Imse, and Pomerantz 2015) with most of these studies having small sample sizes or being reports or honors theses (with the exception being Granberg, Andersson, and Ahmed 2020).[8]

Our study also improves on the three existing audit field experiments of discrimination in access to mental health care (Kugelmass 2016, 2019; Shin et al. 2016).[9] First, our largest contribution is that even with the preliminary data from only the first wave of our experiment – 1,000 observations – we already have a much larger sample size than these prior studies, which had sample sizes of between 300 to 400 each. Second, our sample is nationally representative, including MHPs in every state, proportional to their population, and including MHPs from

[8] There are a few studies that are not audit field experiments that also focus on discrimination or disparities faced by transgender people. Van Borm and Baert (2018) conduct a vignette experiment to quantify hiring discrimination against transgender women, compared to cisgender women, in fictional employment hiring scenarios. Van Borm et al. (2020) explore the mechanisms of hiring discrimination against transgender men using a similar vignette study. Reed, Franks, and Scherr (2015) conduct a small vignette study to quantify hiring discrimination against transgender people and to what extent hiring discrimination is based on assumptions about transgender people having mental illness. Geijtenbeek and Plug (2018) study the earnings of transgender people compared to cisgender people, and compared to before and after their administrative gender transition. Schilt and Wiswall (2008) study how workplace experiences change after transitioning. Drydakis (2019) discusses mental health, life satisfaction, and job satisfaction before and after transitioning.

[9] Olin et al. (2016) is another audit field experiment of access to mental health care, but focuses on access to care for youth in New York state and quantifies general access rates and wait times.

across the state, rather than just selecting individual cities (New York City, Kugelmass 2016) or states (an "East Coast, Mid-Atlantic state", Shin et al. 2016). Third, we follow Kugelmass (2019) and send appointment requests in a more externally-valid way by sending emails through a common MHP listing and appointment request service.[10] Fourth, we plan to leverage the detailed data in the publicly-posted MHP profiles to understand the sources of discrimination, where and when discrimination occurs the most, and what factors may reduce discrimination (see the final "Next Steps" section). This detailed data is not available, or is not used, to near this extent in virtually all other audit field experiments.

Lastly, to our knowledge, we are the first study to use experimental methods to examine how race, ethnicity, and gender identity interact to moderate or exacerbate discrimination. This adds to the limited experimental research on intersectional discrimination in general (Bourabain and Verhaeghe 2018; Burn et al. 2020; Lahey and Oxley 2018; Lauster and Easterbrook 2011; Pedulla 2014; Schwegman 2019). There is ample reason to believe that TNB people of color will experience greater discrimination than their cisgender non-white or white transgender/non-binary peers. In the United States, anti-transgender violence, which includes physical and sexual violence, is highly racialized (Jefferson, Neilands, and Sevelius 2013; Lombardi et al. 2002; Stotzer 2009). For example, 61 percent of lethal anti-LGBTQ+ hate in the

[10] Prior studies (Shin et al. 2016; Kugelmass 2016) left voicemails for MHPs to request an appointment, usually intentionally calling at times when they knew the MHP would not pick up the phone to avoid the risk of the MHP picking up the phone (e.g., "Sunday evening", Shin et al. 2016, p. 1196; "at night", Kugelmass 2016, p. 173). While this is an experimentally valid approach, our approach, following Kugelmass (2019), is to send appointment requests through email, through the most comprehensive and popular online therapist database. This avoids the less externally valid approach of calling and leaving voicemails, opting to mirror one of the most common ways that prospective patients find MHPs.

U.S. during 2016 resulted in the murder of transgender women of color, rates well beyond their proportion of the general population (Waters and Yacka-Bible 2017).

As we proceed through the next waves of the experiment, we will expand the study and further contribute to the literature. We discuss the details of the multiple extensions of this study in the concluding section of this paper.

At the time of writing this paper, our experiment is ongoing. This working paper presents the results from the first wave of the study, where we contact 1,000 mental health care providers (MHPs) who post their contact information on a popular online platform. We send each MHP an appointment request from one prospective patient, with randomly assigned race or ethnicity (white, African American, or Hispanic), gender identity (transgender, non-binary, or presumed to be cisgender), and common mental health concern (depression, anxiety, or stress).

3.2 Background

3.2.1 Mental Health Disparities among Racial, Ethnic, and Gender Minorities

There is a complex relationship between race, ethnicity, and mental health, with sometimes conflicting research on the direction of mental health disparities. For example, Hispanics, African Americans, and Asian Americans report having lower current, last-year, and lifetime rates of major depression and other psychiatric disorders than whites (Miranda et al. 2008; Williams et al. 2007; Williams 2018). However, when African Americans and Hispanics experience a mental disorder, their mental health episode tends to be more severe, persist for longer, and be more debilitating than for whites (Breslau et al. 2005). African Americans

reporting an episode of depression are more likely to be chronically or persistently depressed, have more severe symptoms of depression, and be less likely to receive treatment (Williams et al. 2007; Williams 2018).

While the relationship between race, ethnicity, and mental health is complex, there is more clear evidence that TNB people have worse mental health, higher rates of major psychiatric disorders, and higher rates of substance misuse than the general population. TNB individuals report higher rates of suicidal ideation and attempted suicide, as well as significantly higher rates of clinical depression (Clements-Nolle et al. 2001; Grossman and D'Augelli 2007; Haas et al. 2011; Hoffman 2014; Mustanski et al. 2010; Nuttbrock et al. 2010; Scanlon et al. 2010; Su et al. 2016).

Moreover, there is broad consensus that exposure to chronic and acute stressors—such as poverty, neighborhood violence, or discrimination—can negatively affect mental health (Pearlin et al. 1981, 2005; Turner 2013; Vega and Rumbaut 1991). Racial and gender minorities face higher rates of "traditional" stress than whites. Notably, they are more likely to be unemployed, uninsured, exposed to neighborhood violence, and involved in the criminal justice system (James et al. 2016; Williams 2018).

Economic precariousness, increased exposure to violence, social stigma, and explicit discrimination creates a unique set of psychological pressures and stresses for racial and gender minorities that is often referred to as "minority stress" (Arbona and Jimenez 2014; Hendricks and Testa 2012; Kelleher 2009; Tebbe and Moradi 2016; Testa et al. 2017). Minority stress is positively correlated with worse mental health outcomes, including higher rates of distress and

depression (Lewis, Cogburn, and Williams 2015; Paradies et al. 2015; Pascoe and Richman 2009; Schulz et al. 2006; Wallace, Nazroo, and Becares 2016; Williams and Mohammed 2009).

Specifically, explicit discrimination and other stressors can negatively affect mental health through several different pathways. Discrimination can increase stress, which puts pressure on the body's cardiovascular system and heightens vigilance, i.e., a state of psychological arousal designed to monitor and protect oneself from threats (Williams, Lavizzo-Mourey, and Warren 1994; Sawyer et al. 2012). Heightened violence is positively associated with depressive symptoms and was found to contribute to the African American-white disparity in depression (LaVeist et al. 2014; Testa et al. 2012).

Moreover, structural and institutional racism can give rise to the "stress proliferation process" (Pearlin et al. 2005) in which an initial stressor can initiate or exacerbate stressors in other aspects of life (Williams 2018). Previous research finds evidence of racial discrimination in the labor market (e.g., Bertrand and Mullainathan 2004; Gaddis 2015; Pager and Shepherd 2008), the housing market (e.g., Gaddis and Ghoshal 2020; Hanson and Hawley 2011; Hanson et al. 2016; Murchie and Pang 2018), physical and online stores or marketplaces (e.g., Bourabain and Verhaeghe 2018; Doleac and Stein 2013), and the public sector (e.g., Bergman and McFarlin 2020; Giulietti, Tonin, and Vlassopoulos 2019; Mujcic and Frijters 2020), among other areas and markets.

There is also evidence of TNB individuals facing significant discrimination in the labor market, in secondary and postsecondary schools, when accessing health care, when accessing housing, and in the criminal justice system (Baumle, Badgett, and Boutcher, 2020; BreakOUT! and National Council on Crime & Delinquency 2014; Glick et al. 2019; Grant et al. 2011;

Hanssens et al. 2014; James et al. 2016; Levy et al. 2017; Mallory, Hasenbush, and Sears 2015; Romero et al. 2016; Sears and Mallory 2014; Stotzer 2014; Stroumsa 2014). This systematic discrimination and inequality not only causes stress, but it can both cause and contribute to economic insecurity, which is a significant source of stress (Williams 2018).

For TNB individuals and cisgender racial minorities facing acute psychological stressors, counseling and therapy are effective and common strategies for helping with numerous mental health concerns, such as stress, anxiety, depression, and substance misuse. However, if providers of these mental health services discriminate against TNB individuals and racial minorities by restricting access to these services, then this discrimination may partially cause and likely exacerbate underlying race and gender identity-related mental health disparities.

3.2.2 Mental Health Care Providers in the United States

Mental health care providers supply and regulate access to mental health care services in the United States. Problematically, there is no universally agreed-upon definition of a "mental health care provider," nor is there a consensus on which provider types make up the mental health workforce in the United States (Heisler 2018). Mental health care services are provided by a wide range of licensed professionals, including primary care physicians, psychologists, psychiatrists, nurses, mental health and substance abuse counselors, family and marriage counselors, and social workers. Specific education and licensure requirements can vary from state to state, whereas other licensure requirements are more uniform across states. For example, to be a clinical psychologist requires a doctoral degree in psychology (Ph.D. or Psy.D) and passage of a certification exam (e.g., the Professional Practice in Psychology Exam).

Regardless of their professional training and qualifications, mental health providers have a significant degree of professional autonomy. MHPs are significantly more likely to be in solo practice than physicians or other healthcare providers. While only one in five physicians work by themselves, almost half of all MHPs do (Kane and Emmons 2013; Kugelmass 2016, 2019; Michalski, Mulvey, and Kohout 2009). Thus, MHPs face fewer formal and institutional constraints on their ability to make decisions consistent with their explicit or implicit biases.

Previous experimental and observational studies establish that health care providers, including MHPs, make decisions about patients that are shaped by their perceptions of a patient's race, social class, and gender (van Ryn and Burke 2000; Kugelmass 2016, 2019). This research primarily focuses on how race-based explicit or implicit biases affect diagnosis, treatment recommendations, and patient management (Arber et al. 2006; Green et al. 2007; Haider et al. 2011; Kikano et al. 1996; Lutfey et al. 2008, 2010; McKinlay et al. 1997; Stepanikova 2012; van Ryn et al. 2011). These explicit or internalized biases and prejudices result in African Americans and other minorities receiving fewer procedures and poorer quality medical care than whites across virtually every medical intervention (Smedley, Stith, and Nelson 2003).

These disparities may be driven by a personal aversion or a "taste-based animus" against working with gender and racial minorities. Health care providers have been found to ascribe negative characteristics to African American patients and lower-class patients, and they often perceive African American patients as implicitly less cooperative and more hostile (Abreu 1999; Green et al. 2007; van Ryn and Burke 2000).

Few studies examine if health care providers hold explicitly negative anti-transgender or anti-non-binary views. However, medicine and medical providers have historically treated TNB bodies as abnormal, unhealthy, diseased, and in need of corrective treatment (Davis et al. 2015). The American Psychiatric Association's Diagnostic and Statistical Manual of Mental Disorders considered being transgender a mental disorder from 1980 until 2012 (Heffernan 2012), and the World Health Organization considered identifying being transgender as a mental illness until 2018 (Papenfuss 2019). Many MHPs continue to view TNB people as mentally ill, delusional, or self-destructive because of their gender identity (Mizock and Fleming 2011).

There is also ample evidence to suggest that MHPs seek to cultivate a group of desirable patients by "cream skimming," or explicitly or implicitly choosing to provide services to a specific group of patients. That is, MHPs could choose to only provide services only to patients based on several non-mutually exclusive characteristics, including gender or race homophily, type of services the patient is seeking (e.g., the severity of the mental illness), or insurance status (i.e., the likelihood of payment, amount of payment, timeliness of payment). For example, there is evidence that therapists prefer to see YAVIS (young, attractive, verbal, intelligent, successful) patients (Teasdale and Hill 2006; Tyron 1986). Previous experimental audit and correspondence studies document cream-skimming based on a patient's socioeconomic status (Angerer, Waibel, and Stummer 2019; Kugelmass 2016; Olah et al. 2013), insurance status (Bisgaier and Rhodes 2011; Olin et al. 2016; Polsky et al. 2015; Rhodes et al. 2014; Werbeck et al. 2019), race (Leech, Irby-Shasanmi, and Mitchell 2019; Sharma et al. 2015, 2018; Wisniewski and Walker 2020), and perceived gender (Olah et al. 2013; Sharma et al. 2015).

Cream skimming could be rooted in different sources of discrimination, such as taste-based discrimination (i.e. MHPs are transphobic), statistical discrimination (MHPs use minority status to make assumptions about the prospective patient), or implicit bias (unconscious bias).[11] An MHP could exhibit statistical discrimination in appointment allocation in numerous ways. First, MHPs could assume that TNB prospective patients are more likely to have a severe mental health issue, which requires more time and effort to treat and potentially poses greater liability.[12] Alternatively, MHP my perceive TNB individuals as less likely to be insured or being less able to pay standard out-of-pocket rates.[13] Thus, MHPs could perceive TNB patients to be less desirable, causing MHPs to respond less favorably to appointment inquiries from TNB prospective patients. If this cream skimming is driving the behavior of MHPs, then including elements that increase the desirability of the patient (e.g., ability to pay) should differentially increase positive response rates for TNB prospective patients compared to presumed cisgender prospective patients.

Lastly, mental health care providers may hold implicit, unconscious biases about racial and gender minorities (Devine 1989; Greenwald and Banaji 1995). Devine (1989) notes that it is possible for individuals who are not explicitly prejudiced and who may deliberately try to avoid

[11] Economics typically conceptualizes discrimination in terms of taste-based discrimination (Becker 1971) and statistical discrimination (Arrow 1973; Phelps 1972). Taste-based discrimination, or animus, occurs when MHPs gain disutility from interacting with specific groups of patients (or alternatively, they gain utility from the act of discrimination) (Lahey and Oxley 2018). Statistical discrimination occurs when minority status is used as a proxy for missing information about the prospective patient, such as assuming that minorities have lower socio-economic status, people of color are more likely to have Medicaid, or TNB individuals have more severe conditions.

[12] Numerous studies find that TNB individuals face more severe mental health conditions (Grossman and D'Augelli 2007; Lagos 2018; Meyer et al. 2017; Miller and Grollman 2015; Mustanski et al. 2010; Nuttbrock et al. 2010; Scanlon et al. 2010; Streed et al. 2018; Su et al. 2016), which could lead MHPs to statisticaly discriminate against TNB prospective patients if they prefer patients with less severe conditions.

[13] Several studies find that TNB individuals are less likely to have health insurance (Carpenter, Eppink, and Gonzales 2020; James et al. 2016; Liszewski et al. 2018; Waters and Yacka-Bible 2017) and have lower income (Badgett, Carpenter, and Sansone 2020; Carpenter, Eppink, and Gonzales 2020), which could lead to MHPs statistically discriminating on this basis.

stereotypes and prejudice to still make decisions based on internalized biases or stereotypes. Numerous studies find that health care providers hold implicit biases and stereotypes about racial minorities that result in unequal treatment (Green et al. 2007; McKinlay et al. 1996). There are few studies documenting implicit stereotypes held about gender identity. However, a recent study found that people tend to express implicit and explicit preferences for cisgender over transgender people (Axt et al. 2020).

Regardless of the cause of any underlying discrimination, if MHPs are less responsive to and less helpful towards racial and gender minorities, this behavior will decrease access and reduce the probability that these individuals receive timely and necessary medical care. The concluding section of this paper describes the steps that we will take to better understand the mechanisms behind discrimination.

3.3 Experimental Design

Audit field experiments are the gold standard for detecting and measuring discrimination (Al-Ubaydli and List 2016; Bertrand and Duflo 2017; Gaddis 2018; Neumark 2018). Experimental studies are practically the only method for causally measuring discrimination against groups for which there is very little administrative or survey data, e.g., TNB individuals.[14] In this section, we outline the details of our experimental design. We discuss ethics in audit studies in Appendix B.

[14] See Badgett, Carpenter, and Sansone (2020) for a helpful overview of the (often lack of) data on LGBTQ+ individuals in socio-economic and health surveys.

3.3.1 Sampling Frame and Power Analysis

We use a popular online therapist search database to collect our sample of auditable mental health care providers. In order to be included in our sample, an MHP: (1) must not specialize exclusively on specific types of patients who are outside of the scope of our experiment (e.g., children, adolescents, or couples therapy), (2) must not be specialized in a type of therapy (e.g., grief, domestic violence) that would not deal with the common mental health conditions that we signal: anxiety, depression, and stress, (3) must list an individual's profile (e.g., it cannot be the profile of a clinic), (4) must provide an email option through a web form, and (5) must be accepting patients (i.e., we do not contact MHPs that indicate that they are not currently accepting patients). After accounting for these characteristics, we select MHPs proportionately to state populations. Within states, we select MHPs proportionally to the population of each ZIP code.

We collect information about each MHP from their publicly-posted profile to use in future analysis. We will use this to control for variation in MHPs' characteristics that affects MHP responses, thus increasing the precision of our estimates of discrimination. We will also use this data on MHP characteristics to investigate moderators of discrimination. Specifically, we record the MHPs state, their ZIP code, the number of years in practice, their cost per session, and their titles, licenses, and degrees. We also note whether each MHP specializes in anxiety, depression, and stress. Lastly, we record whether each MHP lists "transgender ally," "non-binary ally," “LGBT-ally,” and/or a transgender specialty on their profile. We also saved each MHP’s publicly-posted profile so we can extract more data from it later. We discuss our

plans for using MHP characteristics and other factors to explore the moderators and sources of discrimination in the final “Next Steps” section in the paper, and in Table 9.

3.3.2 Patient Profiles and Email Scripts

If a mental health care provider meets the inclusion criteria for this experiment, we send a message to them through an “Email Me” webform. In these inquiries, we use names to signal the fictitious prospective patient's race, ethnicity, and gender. We randomly assign various other aspects of the email to signal gender identity, mental health concern, and, in future waves of our experiment, insurance status. Figure 3.1 provides the general structure of our appointment inquiry emails, and Figure 3.2 summarizes the randomized options that we assign to each email.

Figure 3.1: Structure of the Emails to MHPs

1.) **[EMAIL SUBJECT LINE]** *Legend: ( ): denotes motivating verbiage, not exact phrasing*
Hi,/Hello, *[ ]: denotes randomized input*
My name is 2) **[NAME]**. (I'm contacting you because) 3) **[MENTAL HEALTH CONCERN]** (and would like to talk to a therapist). *If transgender or non-binary:* I am 4) **[GENDER IDENTITY]** and am looking for a therapist who is trans-friendly. 5) **[APPOINTMENT REQUEST]**.
6) **[VALEDICTION]**
2) **[NAME]**

Figure 3.2: Randomized Components of the Emails to MHPs

1) **[EMAIL SUBJECT LINE]**

-Seeking therapy

-Looking for a therapist

-Therapy inquiry

3) **[MENTAL HEALTH CONCERN]**

-I've been feeling anxious lately.

-I've been feeling stressed all the time.

-I think I might be depressed.

4) **[GENDER IDENTITY]**

-a transgender woman

-a transgender man

-non-binary

2) **[NAME]**

Afr.-Am.	*Hispanic*	*White*
Male-Coded First Names		
Darius	Alejandro	Brian
DeShawn	Luis	Kevin
Female-Coded First Names		
Ebony	Mariana	Amanda
Lakeisha	Valentina	Heather
Last Names		
Washington	Hernandez	Anderson
Jefferson	Garcia	Thompson

5) **[APPOINTMENT REQUEST]**

-Can we set up an appointment? -When could I see you?

6) **[VALEDICTION]**

-Sincerely, -Thanks, -Best, -[None]

Notes: Ethnic and race specific first names are from Barlow and Lahey (2018), Gaddis (2017).

We use names from two previous audit studies (Barlow and Lahey 2018; Gaddis 2017a) to signal race and gender. We present these names in Figure 3.1, box 2. Each name is either stereotypically masculine (signaling that the sender identifies as a male) or feminine (signaling that the sender identifies as female). We assign transgender and cisgender women (men) a feminine (masculine) first name. Non-binary prospective patients are assigned either feminine names or masculine names, each with a 50 percent probability.[15]

Each MHP will receive one inquiry from one prospective patient who identifies either as transgender (25 percent of the time), non-binary (25 percent of the time), or cisgender (50 percent of the time). Specifically, TNB prospective patients the following statement in their appointment request email: "*I am [a transgender woman]/[a transgender man]/[non-binary] and I am looking for a therapist who is trans-friendly."* Cisgender prospective patients do not include any statement about gender identity and are thus presumed to be cisgender.

We believe that signaling TNB status in this way is common and externally valid. For a TNB individual seeking mental health services, finding a therapist who will not discriminate against them (i.e., a "trans-friendly" therapist) or stop them from being transgender[16] is essential. Disclosing transgender status and inquiring about trans-friendly services is common

[15] Many non-binary people keep their names assigned at birth, or otherwise have names that are more feminine or masculine, especially since few names are non-gender specific. We considered including some non-gender specific names for non-binary people but decided not to since there is no clear naming convention or way that non-binary people select non-gender specific names. Also, including another set of names would have added another difference between our non-binary prospective patients and our transgender and cisgender prospective patients, which may have made it more different to compare results.

[16] Almost 1 in 10 respondents to the 2015 U.S. Transgender Survey report that at least one MHP has tried to stop them from being TNB (James et al. 2016). Those who have experienced a professional try to stop them from being TNB report worse mental health outcomes, including higher rates of psychological distress and attempted suicide.

and is recommended by experts who provide advice on how to find trans-affirming care (e.g., Kassel 2018).

We selected names that clearly signal gender, race (African American or white), and ethnicity (Hispanic) from studies that carefully test how names signal race, ethnicity, and socioeconomic status (Barlow and Lahey 2018; Gaddis 2017a).[17] Figure 2 presents these names. In the next waves of our experiment, we will add Chinese American names, as discussed in our concluding section.

In the first wave of the study, the results of which we present in this paper, we randomly assign an MHP to receive an inquiry containing a white name approximately 50 percent of the time, an inquiry containing an African American name approximately 25 percent of the time, and an inquiry containing a Hispanic name approximately 25 percent of the time.

We also randomly assign one of the following mental health conditions: stress, anxiety, or depression. We use these conditions since they are the most common, virtually all MHPs are qualified to treat them, and they do not suggest that the mental health concern is trans-specific. We focus this study on quantifying access to mental health care for common mental health conditions rather than quantifying access to trans-specific care, which is a separate research question requiring a different research design.

[17] Using these names helps us confront the criticism that using African-American first names to signal race over-estimate discrimination and confuses racial discrimination for socio-economic status discrimination because some names also have negative socioeconomic status signals (Barlow and Lahey 2018; Darolia et al. 2016; Fryer and Levitt 2004; Gaddis 2017a; 2017b; Ghoshal 2019).

3.3.3 Coding Mental Health Provider Responses

Each appointment request email contained both the fictitious patient's email and phone number. MHPs are thus able to respond via phone, text message, and email. We consider a (non-automated) email, a text message, or a voicemail to be a response.[18]

We coded each MHP response into one of the following seven mutually exclusive outcome categories: appointment offered, call or consultation offer, screening question(s), referral, waitlist, rejection, and no response. These seven, mutually-exclusive categories[19] capture the variation in the quality of response. See Table 1 for each outcome's more detailed definition.

To improve power and increase our results' interpretability, we collapse these categories into a binary variable, called "positive response," that adopts the value one if the MHP's response was normatively positive (the sum of appointment offer and call or consultation offer) and zero otherwise.[20] Categorizing responses as positive or not positive is a standard approach in audit studies (e.g., Kugelmass 2019; Neumark, Burn, and Button 2019), but in future waves of the study, we will conduct an analysis that better explores differences between frequencies in different response categories.

[18] We record MHP's phone numbers and cross-reference those with any missed calls, but we find only perhaps one instance of an MHP calling without leaving a voicemail.
[19] MHPs of course often provide more than one type of response, such as a referral and a consultation offer. If an MHP's response falls into more than one category, it is coded as the best category. For example, a referral and a consultation offer is coded as consultation offer, and a rejection and a referral is coded as a referral.
[20] This is the same binary categorization as Kugelmass (2019). Our results are generally similar if we use an alternative binary categorization that deems screening questions and referrals to be positive responses as well. We discuss these results in a robustness sub-section within the results section.

Table 3.1: Descriptive Statistics of Outcomes

		Binary Coding			Gender Identity		Race and Ethnicity		
Outcome	**Description**	*Default*	*Alt.*	*Overall*	*Cisgender*	*Trans or Non-Binary*	*White*	*African American*	*Hispanic*
Appointment Offer	The MHP explicitly offers an appointment.	+	+	33.3%	33.2%	33.4%	33.4%	32.4%	34.0%
Call or Consultation Offer	The MHP offers to speak on the phone but does not offer an appointment.	+	+	23.3%	27.3%	19.6%	24.6%	23.2%	20.5%
Screening Question	The MHP requests additional information but does not offer an appointment.	-	+	6.0%	7.1%	5.0%	5.9%	7.0%	5.0%
Referral	The MHP gives a referral but does not offer an appointment.	-	+	4.8%	3.8%	5.8%	4.9%	5.9%	3.2%

Waitlist	The MHP offers to put the prospective patient on a waitlist.	-	-	2.1%	1.3%	2.9%	2.1%	0.7%	0.4%
Rejection	The MHP rejects the prospective patient and does not offer an alternative provider.	-	-	6.0%	6.5%	5.6%	5.8%	6.6%	5.5%
No Response	No response from the MHP within one week.	-	-	24.5%	20.9%	27.6%	23.0%	24.0%	28.2%
			N	1,000	480	520	500	270	230

Notes: These categorizations are mutually exclusive. For example, a response is coded as an appointment offer even if a referral is also provided. Our default binary coding treats appointment offer and call or consultation offer as the only positive outcomes, while our alternative binary coding also considers screening questions and referrals as positive outcomes.

3.4 Empirical Strategy

We use regression analysis to quantify differences in outcomes. We start first by testing for differences in our broader categories, using the binary "positive" outcome variable and a linear probability model[21] as follows:

$$Positive_i = \beta_0 + \beta_1 TransOrNonBinary_i + \beta_2 AfricanAmerican_i + \beta_3 Hispanic_i + \beta_4 Depression_i + \beta_5 Anxiety_i + \varepsilon_i \quad [1]$$

$Positive_i$ equals one for positive responses to the appointment inquiry (appointment offer or call or consultation offer), and $TransOrNonBinary_i$, $AfricanAmerican_i$, and $Hispanic_i$ are indicator variables for each randomized status, with the excluded category being cisgender white people. $Depression_i$ and $Anxiety_i$ capture differences in the positive response rate between those who mention depression or anxiety in their appointment request, compared to those who just mention having stress. In our preferred specifications, we also include state fixed effects and fixed effects for the week and day of the week when we sent the appointment request. We cluster our standard errors at the patient level since, while each MHP only gets one email, each patient emails appointment requests to up to ten MHPs in their assigned area.

We then extend equation [1] to explore intersectional groups, such as individuals by gender identity (e.g., transgender wo(men) vs. cisgender wo(men) vs. non-binary people) and by race, ethnicity, and gender identity intersectionality (e.g., TNB people of color).

[21] Our main results are similar using a probit model (see Table Appendix Table A1). We discuss the minor differences in the robustness sub-section of the results section.

In subsequent analyses, when we have a larger sample size and have collected more data, we will control for MHP characteristics. We anticipate that this would increase precision in addition to showing how MHP characteristics affect access to appointments in general. We will also conduct a more in-depth analysis of differences in the types of responses, such as using multinomial models to determine if there are differences within our binary categorization that our analysis does not pick up. For example, are TNB individuals more likely to get a call or consultation offer instead of an outright appointment? Or are TNB individuals more likely to get referrals instead of being outright rejected? This analysis will provide a deeper understanding of how MHPs react to prospective patients.[22]

3.5 Preliminary Results

Between January 28, 2020 and May 15, 2020, we sent appointment requests to 1,000 MHPs. Before proceeding, it is important to note that, although our sample size is small, it is only the first wave of a multi-wave study. Thus, these results are preliminary and subject to change based on the results of subsequent waves.

We receive responses to 75.5 percent of our appointment request emails. This response rate is comparable to other email correspondence audit studies (Hanson et al. 2016; Kugelmass 2019). Among these responses, 80 percent of MHPs responded via email, and the remainder left a voicemail (or, in a few instances, a text message only).

[22] Our results, however, are similar using our alternative binary coding, which also considers screening questions and referrals as positive outcomes. But a multinomial model would allow for a finer study of differences by response categories.

In Table 1, we categorize the responses into seven mutually exclusive outcome categories. In particular, we received an appointment offer from one third of our appointment requests and we received a call or consultation offer 23.3 percent of the time. We code both of these as positive responses in our binary coding, so the positive response rate is 56.6 percent. For the responses we code as negative, the most common situation was that we did not receive a response at all (24.5 percent), followed by a response with a rejection (6.0 percent), the MHP asks a screening question but does not offer an appointment, call, or consultion (6.0 percent), the MHP offers a referral only (4.8 percent), or the MHP offers to put the prospective patient on a waitlist (2.1 percent). See Table 1 for each outcome's frequency by gender identity, race, and ethnicity.

3.5.1 Differences in Positive Response Rates

We then collapse this more detailed coding of responses into our binary positive response coding (positive responses are appointment, call, or consultation offers) to present raw differences in positive response rates. Table 2 presents positive response rates by gender identity, first for the aggregated grouping of cisgender prospective patients versus TNB prospective patients. Cisgender prospective patients received a positive response 60.6 percent of the time while TNB prospective patients only received a positive response 52.8 percent of the time—a statistically significant 7.8 percentage point difference (*p-value* = 0.013, using a two-sided t-test). We then compare positive response rates by our finer categorizations of gender identity. Cisgender men have the highest positive response rate (61.6 percent) followed by cisgender women (58.8 percent), transgender women (55.8 percent), non-binary individuals

(51.9 percent), and transgender men (50.7 percent). These finer categorizations have less precision, given our smaller sample size, so only the response rate difference between cisgender and transgender men – where transgender men have an 10.9 percentage point lower response rate – is statistically significant (*p-value* = 0.030).

Table 3.2 Positive Response Rates by Gender Identity

Response Rates by Trans/Cis Status:	Positive	Negative	Total		
Cisgender	60.6% (291)	39.4% (189)	480		
Transgender or Non-binary	52.8% (275)	47.2% (245)	520		
Total	56.6% (566)	43.4% (434)	1,000		
Test of independence, p-value	0.013				
Response Rates by Gender Identity:					
Cisgender men	61.6% (191)	38.4% (119)	310		
Cisgender women	58.8% (100)	41.2% (70)	170		
Transgender men	50.7% (71)	49.3% (69)	140		
Transgender women	55.8% (95)	44.2% (75)	170		
Non-binary	51.9% (109)	48.1% (101)	210		
Tests of independence, p-values	Cis men	Cis women	Trans men	Trans women	Non-binary
Cisgender men	…				
Cisgender women	0.551	…			
Transgender men	0.030	0.151	…		
Transgender women	0.222	0.585	0.365	…	
Non-binary	0.028	0.179	0.829	0.441	…

Notes: Responses are coded as positive if the MHP's response was an appointment offer or a call or consultation offer. P-values come from a t-test (two-sided).

Table 3.3 presents positive response rates by race and ethnicity. White prospective patients have the highest positive response rate (58.0 percent) followed by African Americans (55.5 percent) and Hispanics (54.8 percent). None of these differences are statistically significant in this raw data. Table 4 presents positive response rates for groups by the

intersection of race, ethnicity, and gender identity. We find no statistically significant differences in raw response rates between whites, African Americans, or Hispanics *with* the same transgender/cisgender status. However, we find differences by race and ethnicity across transgender/cisgender status.

Table 3.3 Positive Response Rates by Race or Ethnicity

	Positive	Negative	Total
White	58.0% (290)	42.0% (210)	500
African American	55.5% (150)	45.5% (120)	270
Hispanic	54.8% (126)	45.2% (104)	230
Total	56.6% (566)	43.4% (434)	1,000

Tests of independence, p-values	White	African American	Hispanic
White	…	…	…
African American	0.514	…	…
Hispanic	0.415	0.862	…

Notes: Responses are coded as positive if the MHP's response was an appointment offer or a call or consultation offer. P-values come from a t-test (two-sided).

Table 3.4 Positive Response by Race or Ethnicity, for Cisgender and Transgender or Non-Binary Patients Separately

Response rates for cisgender only:	Positive	Negative	Total
White	61.5% (160)	38.5% (100)	260
African American	60.7% (85)	39.3% (55)	140
Hispanic	57.5% (46)	42.5% (34)	80
Total	60.6% (291)	39.4% (189)	480
Test of independence, p-values	White	African American	Hispanic
White	…	…	…
African American	0.872	…	…
Hispanic	0.519	0.642	…
Response rates for transgender or non-binary only:			
White	54.2% (130)	47.8% (110)	240
African American	50.0% (65)	50.0% (65)	130
Hispanic	53.3% (80)	46.7% (70)	150
Total	52.9% (275)	47.1% (245)	520
Test of independence, p-values	White	African American	Hispanic
White	…	…	…
African American	0.445	…	…
Hispanic	0.873	0.579	…

Transgender or non-binary vs. Cisgender: Tests of independence, p-values			
	Cisgender White	Cisgender African American	Cisgender Hispanic
Transgender or non-binary White	0.096	…	…
Transgender or non-binary African American	0.030	0.077	…
Transgender or non-binary Hispanic	0.105	…	0.547

Notes: Responses are coded as positive if the MHP's response was an appointment offer or a call or consultation offer. P-values come for a t-test (two-sided).

Cisgender African Americans have a higher positive response rate (60.7 percent) than TNB African Americans (50.0 percent, *p-value* = 0.077), and cisgender whites have a higher positive response rate (61.5 percent) than TNB whites (54.2 percent, *p-value* = 0.096).

However, we find the largest positive response rate differences by comparing TNB African Americans and Hispanics to cisgender whites. TNB African Americans face the lowest positive response rate (50.0 percent) compared to cisgender whites, who face the highest rate (61.5 percent, *p-value* = 0.030). For TNB Hispanics, this response rate is 53.3 percent (p = 0.105). Thus, it appears that the discrimination against TNB prospective patients is largely discrimination against TNB African-Americans and Hispanics.

Table 3.5 presents regression estimates of the differences in response rate by race, ethnicity and gender identity derived from the linear probability model specified in equation [1]. Without any control variables, the regression estimates show that prospective patients who signal TNB status have between a 6.5 and 7.5 percentage point lower positive response rate, significant at the 5 percent level (columns (1) and (2)). Without control variables, there is also no difference in response rates between white, African American, and Hispanic prospective patients. These results mirror the raw differences in positive response rates seen in Tables 3.2 and 3.3. We also find that those who mention anxiety in their appointment request, rather than stress, have a 10.4 percentage point lower response rate.

Table 3.5: Differences in Positive Response Rates, Results for Aggregated Groups and by Mental Health Concern

	(1)	(2)	(3)	(4)	(5)
Transgender or Non-binary	- .07446**	-.0657**	-.0334	-.0084	.0123
	(.0317)	(.0320)	(.0429)	(.0429)	(.0426)
African American	- .0245	-.0226	-.1091**	-.1492**	-.1333***
	(.0374)	(.0374)	(.0432)	(.0419)	(.0404)
Hispanic	-.0195	-.0278	-.0209	-.0911*	-.1302**
	(.0398)	(.0399)	(.0526)	(.0465)	(.0495)
Depression	…	- .0201	.0450	.1366**	.1459**
		(.0385)	(.0502)	(.0587)	(.0576)
Anxiety	…	-.1040**	-.0011	.0139	.0111
		(.0449)	(.0524)	(.0527)	(.0527)
State fixed effects:			X	X	X
Week-sent fixed effects:				X	X
Day-of-the-week-sent fixed effects:					X
Mean positive response rate for excluded category (cisgender whites w/ stress):	.6158	.6479	.6487	.6473	.6353
N	1,000	1,000	1,000	1,000	1,000
Adjusted R^2	0.0063	0.0076	0.0808	0.0986	0.1070

Notes: Regression estimates based on the linear probability model in equation (1). Standard errors, clustered at the patient level, in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

Adding state fixed effects (column (3)) changes these estimates significantly.[23] With state fixed effects, the positive response rate is only 3.3 percentage points lower and statistically insignificant for TNB individuals compared to cisgender individuals. Adding state fixed effects reveals discrimination against African American prospective patients, with a 10.9 percentage point lower response rate, statistically significant at the 5 percent level. Adding state fixed effects also removes the estimated positive response rate difference between prospective patients who mention stress versus anxiety.

We then add week-sent and day-of-the-week-sent fixed effects to control for random variation from the time that the emails were sent (although this is random with respect to prospective patient demography). In our preferred specification with all these controls (column (5)), we find no evidence of differential positive response rates between cisgender and TNB prospective patients. However, we do find that, on average, MHPs respond to both African American and Hispanic patients about 13 percentage points less often than white patients (significant at the 1 percent and 5 percent levels, respectively). In this preferred specification, we also find that prospective patients reporting depression as the primary mental health concern in an inquiry to the MHP increases the probability of a positive response by 14.6 percentage points relative to the prospective patient mentioning stress.

In Table 3.6, we disaggregate the cisgender and TNB groups to quantify differences in positive response rates to prospective patients of specific gender identities: binary transgender

[23] This is a function of our temporarily smaller sample size. Patient demographics are randomly assigned by state. The inclusion of state fixed effects controls for between-state differences in response rates, which is a significant source of variation in positive response rates. The inclusion of state fixed effects also means that we put more weight onto within-state differences by patient demographics. We have less within-state variation in patient demographics since, unlike many other audit field experiments, we do not send subjects (MHPs) more than one email.

men and women, non-binary individuals (with either masculine-coded or feminine-coded names), and cisgender men and women (where cisgender men are the comparison group). All estimates are from a regression that includes the control variables in our preferred specification (column (5) in Table 3.5).

Table 3.6: Differences in Positive Response Rates, Results by Gender Identity

	(1)	(2)	(3)	(4)
Transgender or Non-binary	.0123	…	…	…
	(.0426)			
…Binary Transgender	…	.0289	…	…
		(.0472)		
…Trans Women	…	…	.0004	.0096
			(.0624)	(.0618)
…Trans Men	…	…	-.0200	-.0158
			(.0660)	(.0665)
…Non-binary	…	-.0272	-.0607	…
		(.0690)	(.0706)	
…Non-binary female first name	…	…	…	-.0100
				(.0873)
…Non-binary male first name	…	…	…	-.0892
				(.0891)
Cisgender Women	…	…	-.1082**	-.1086**
			(.0527)	(.0534)
African American	-.1333**	-.1355**	-.1505***	-.1459**
	(.0404)	(.0399)	(.0412)	(.0422)
Hispanic	-.1302**	-.1309**	-.1183**	-.1262**
	(.0495)	(.0509)	(.0451)	(.0454)
Mean positive response rate for excluded category (cisgender white men):	.6353	.6383	.6776	.6826
N	1,000	1,000	1,000	1,000
Adjusted R^2	0.1070	0.1076	0.1096	0.1100

Notes: All regressions include the controls in column (5) of Table 5: mental health concern (depression and anxiety relative to the excluded category of stress), state fixed effects, day-of-the-week-sent fixed effects, and week-sent fixed effects. Column (1) repeats the results from column (5) in Table 5 for ease of interpretation. Standard errors, clustered at the patient level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Column (1) of Table 3.6 reports the coefficients from column (5) of Table 3.5 for comparison. Column (2) considers binary transgender and non-binary individuals separately,

column (3) further differentiates binary transgender people to consider transgender women, transgender men, and non-binary individuals separately. Column (4) further differentiates non-binary individuals by if they have feminine-coded or masculine-coded first names. Regardless of how we divide the TNB population, we do not find any differences within TNB subgroups, or between TNB subgroups and cisgender individuals. However, we do find that cisgender women are about 10.8 percentage points less likely to receive a response compared to cisgender men.

In Table 3.7, we disaggregate cisgender and TNB prospective patients by race and ethnicity to quantify any intersectional discrimination, a trend we saw in the raw data in Table 3.4. Column (1) of Table 3.7 again reports baseline estimates from our preferred specification reported in Column (5) of Table 3.5. Column (2) reports differences in response rates for specific racial and ethnic groups disaggregated by gender identity. We find that white TNB prospective patients are about 10.0 percentage points *more* likely to receive a positive response compared to white cisgender prospective patients, although this is only statistically significant at the 10 percent level. However, TNB prospective patients that are African American are 13.3 percentage points less likely to receive a positive response than white cisgender prospective patients (significant at the 5 percent level). Hispanic TNB prospective patients have a 10.3 percentage point lower response rate, but this is not statistically significant. We do not find any differences in positive response rates between cisgender African Americans, cisgender Hispanics, or cisgender whites.

Table 3.7: Differences in Positive Response Rates, Intersectional Results by Trans/Cisgender Status and Race/Ethnicity

	(1)	(2)
Transgender or Non-binary	.0123	…
	(.0426)	
…and white	…	.0998*
		(.0574)
…and African American	…	-.1333**
		(.0613)
…and Hispanic	…	-.1025
		(.0625)
Cisgender		
…and African American	…	-.0241
		(.0659)
…and Hispanic	…	-.0321
		(.0673)
All African American	-.1333**	…
	(.0404)	
All Hispanic	-.1302**	…
	(.0495)	
Mean positive response rate for excluded group (cisgender whites):	.6353	.6510
N	1,000	1,000
Adjusted R^2	0.1070	0.1100

Notes: All regressions include the controls in column (5) of Table 5: mental health concern (depression and anxiety relative to the excluded category of stress), state fixed effects, day-of-the-week-sent fixed effects, and week-sent fixed effects. Column (1) repeats the results from column (5) in Table 5 for ease of interpretation. Standard errors, clustered at the patient level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.8 disaggregates these results further by specific gender identity (transgender women, transgender men, non-binary individuals, and cisgender women, compared to the

excluded category of cisgender men), by race and ethnicity, and by their intersections. Table 3.8 shows that white transgender men (white transgender women) are 21.1 (16.9) percentage points *more* likely to receive a positive response than cisgender whites (both significant at the 5 percent level). However, Hispanic transgender women are 37.0 percentage points less likely to receive a positive response (significant at the 1 percent level). African American transgender men (African American transgender women) have positive response rates that are 12.4 (7.6) percentage points lower than cisgender whites (not statistically significant).

Table 3.8: Differences in Positive Response Rates, Intersectional Results by Gender Identity and Race/Ethnicity

	(1)
Transgender Women	
…and white	.1689**
	(.0743)
…and African American	-.0760
	(.0993)
…and Hispanic	-.3701***
	(.0936)
Transgender Men	
…and white	.2105**
	(.0962)
…and African American	-.1239
	(.0978)
…and Hispanic	-.0819
	(.1025)
Non-binary	
…and white	-.0017
	(.0906)
…and African American	-.4913***
	(.1082)
…and Hispanic	-.1380*
	(.0808)
Cisgender	
…and African American	.0167
	(.0712)
…and Hispanic	.0228.
	(.0709)
Mean positive response rate for excluded group (cisgender whites):	.7546
N	1,000
Adjusted R^2	0.1163

Notes: All regressions include the controls in column (5) of Table 5: mental health concern (depression and anxiety relative to the excluded category of stress), state fixed effects, day-of-the-week-sent fixed effects, and week-sent fixed effects. Standard errors, clustered at the patient level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

For non-binary prospective patients, there is no difference in positive response rates between non-binary white and cisgender white prospective patients. However, African American non-binary prospective patients have a 49.1 percentage point lower positive response rate, significant at the 1 percent level. Hispanic non-binary individuals have a 13.8 percentage point lower positive response rate, but this is only significant at the 10 percent level.

3.5.2 Robustness Checks

We first check if our main results are different when using a probit instead of a linear probability model. Appendix Table A5 presents the results of Table 5 using a probit instead and the results are very similar.

Then, we move to our more important robustness check which is to determine if our method of coding the categorical responses (see Table 3.1) into binary outcomes is robust to plausible alternative binary codings. In our default specification, we deem positive responses to be explicit appointment offers or call or consultation offers, the same coding as in Kugelmass (2019). However, two other possible responses are more ambiguous: screening questions and referrals and we consider an alternative binary coding of positive responses that includes these two as positive as well.

While a screening question could indicate a barrier to access (Kugelmass 2019), such as providers being differentially more concerned about insurance status for minorities, a screening question does not necessarily mean that an appointment would not be offered. Screening questions may also be more common for minorities if, for example, the MHP asks if the

concerns are trans-specific, or if the MHP asks if the prospective patient would prefer someone who specializes in trans/race issues. Thus, screening questions, while generally suggestive of barriers in access to appointments, could in some situations fail to capture responses that are positive.

Referrals are also likely to indicate a barrier to access (Kugelmass 2019), but it depends on why a referral is provided. Many referrals are essentially appointment rejections, but a way to moderate the negative implications of denying an appointment by providing the prospective patient with an alternative. The question is, then, whether this alternative provider is better for the prospective patient.

We try to avoid these types of referrals by not suggesting that the common mental health concerns are trans- or race-specific as we seek to quantify discrimination in access to general mental health care for common mental health concerns. However, MHPs may still suggest alternative providers under the view that the TNB prospective patients, or racial or ethnic minority prospective patients, would do better with a specialist. While many MHPs provide these referrals in addition to appointment, call, or consultation offers, there are some who provide a referral only and a subset of these could still be considered positive outcomes.

After the next wave of data collection, we will start exploring the quality of these referrals to see if the referrals are to lower or higher quality MHPs. Determining the quality of the MHP that is referred to is possible if they also have an online profile or website, which almost all MHPs do. Until we are better able to differentiate between likely good and likely bad referrals, we test the robustness of our binary positive outcome coding by including a referral as a positive response.

Appendix Tables A2 to A8 present all of our results with this alternative positive binary coding, where positive responses are appointment, call, or consultation offers, screening questions, or referrals. Since screening questions occur 6.0 percent of the time and referrals occur 4.8 percent of the time (see Table 3.1), the alternative positive response rate is 67.1 percent on average, compared to 56.6 percent for our default rate. Our results are similar across all tables except for a few minor differences. We generally find less discrimination against non-binary African Americans using the alternative binary coding. This suggests that non-binary African Americans are relatively more likely to be asked screening questions or given referrals.

With the alternative positive coding, we also find discrimination against Hispanic transgender men when we did not find discrimination with the default positive coding. Under this alternative coding, discrimination is similar against Hispanic transgender women and men, while with our default positive coding, there was a much larger magnitude of estimated discrimination against Hispanic transgender women, with no statistically significant evidence of discrimination against Hispanic transgender men. This suggests that Hispanic transgender women (men) are differentially more likely (less likely) to be asked screening questions or given referrals. Our broader result – that discrimination is primarily against TNB African-Americans and Hispanics – is unchanged with this alternative binary coding.

3.6 Conclusion and Discussion

To summarize our results from our first wave of the experiment, we generally find no differences in positive response rates between TNB and cisgender prospective patients in our preferred specifications. This lack of a difference occurs because positive response rates may actually be *higher* for white transgender women and men. However, this is offset by African American and Hispanic TNB people (particularly Hispanic transgender women, and non-binary African American people) facing significantly lower positive response rates. Ignoring intersectionality would have obscured this discrimination against TNB people of color.

We find that African American and Hispanic prospective patients face discrimination on average. However, this average result is driven by the significant discrimination faced by intersectional groups, namely non-binary African Americans and Hispanic transgender women. We do not find differences in response rates by race or ethnicity for cisgender prospective patients. This may suggest that discrimination against African Americans and Hispanics is primarily against African Americans and Hispanics who are transgender or non-binary only. However, given our small sample size at this time we cannot rule out meaningful magnitudes of discrimination against cisgender African Americans or Hispanics, which motivates our planned data collection going forward.[24]

[24] In Table 3.7, column (2), the coefficient on cisgender African Americans is -0.0241, with a standard error of 0.0659. This imprecise estimate has a 95 percent confidence interval of -0.153 to 0.105. The confidence interval is -0.164 to 0.0998 for cisgender Hispanics. These intervals clearly include meaningful magnitudes of discrimination, which means that the most honest interpretation of our results is that it is inconclusive if cisgender African Americans or Hispanics face discrimination in access to mental health care appointments. Our power analysis filed with our pre-analysis plan discusses how many observations we would need in our final experiment to detect meaningful magnitudes of discrimination.

Interestingly, we also find that cisgender women face lower positive response rates than cisgender men. We also find that prospective patients that mention depression as their mental health concern receive higher positive response rates than prospective patients that mention stress of anxiety, although this difference is not entirely robust as it does not appear without state fixed effects, which we include in our preferred specifications.

These preliminary results motivate our continued data collection and data analysis to better understand the ways and reasons that discrimination occurs in access to mental health care. We hope to better understand, for example, why white binary transgender women and men may experience higher appointment offer rates and why cisgender women may experience lower positive response rates. Statistical discrimination based on insurance status could perhaps explain the discrimination against cisgender women.[25] The source of the preference for white transgender women and men is less clear, although a simple explanation is that MHPs simply want to work with or care about the welfare of white binary transgender women and men. We will explore if this preference appears correlated with attitudes on transgender issues or implicit bias against transgender people to see if this preference appears driven by taste-based discrimination in favor of (white) transgender people.

In the final section below, we detail the next steps in this research, which includes conducting secondary analysis to explore the sources and moderators of discrimination, and also conducting "spin off" studies to quantify discrimination based on other factors not explored in this first wave of data collection.

[25] MHPs could assume that cisgender women have worse insurance on-average, given that women face lower wages and lower rates of private health insurance coverage.

3.6.1 Next Steps

We will expand this research to investigate the mechanisms behind discrimination. We will study where and why discrimination occurs by taking equation [1] and adding interactions between our minority status variables and moderators of discrimination. Table 9 presents these interaction variables, what they test for, and our hypotheses. For example, we will study if MHP race or ethnicity predicts discrimination, if transgender rights laws affect discrimination, and to what extent discrimination may be taste-based, statistical, or based on implicit bias. We will also explore how COVID-19 and related policies, such as shelter-in-place ordinances, have affected access to mental health care, and discrimination in access to mental health care.

In addition to exploring these moderators and sources of discrimination, we will extend this study in future waves by adding additional experimental arms as follows:

1. We will add prospective patients with Chinese names to quantify discrimination against that them;
2. We will randomize mention of different insurance statuses and methods of payment to quantify if prospective patients with Medicaid face reduced access to mental health care; and
3. We will add prospective patients who vary in sexual orientation to quantify sexual orientation discrimination.

We detail these plans below and welcome any feedback.

Insurance Status, Access to Mental Health Care, and Statistical Discrimination

In the next wave, we will randomly disclose insurance status or preferred payment method. We will randomly assign insurance status so that an MHP has a 10 percent probability

of receiving an inquiry in which insurance is not mentioned, a 16 percent probability of receiving an email in which self-pay with no reference to a sliding scale is mentioned, a 14 percent probability of receiving an email in which self-pay with a reference to a sliding scale is mentioned, a 30 percent probability of receiving an inquiry in which Medicaid is mentioned, and a 30 percent probability of receiving an email in which private insurance is referenced.[26]

There is some research on how insurance or ability to pay affects access to health care. Several audit field experiments quantify how access to primary care varies by insurance status (Bisgaier and Rhodes 2011; Leech, Irby-Shasanmi, and Mitchell 2019; Olin et al. 2016; Polsky et al. 2015; Rhodes et al. 2014; Sharma, Mitra, and Stano 2015; Sharma et al. 2018), however only Olin et al. (2016) quantifies access to *mental* health care (although for adolescents in in the state of New York only). These audit studies general find that those with Medicaid face reduced access to health care. We expect that those with Medicaid will face similar barriers in access to mental health care as they do for access to primary care.

In addition to randomizing insurance status to our prospective patients to quantify access to mental health care appointments, we can also use this to study statistical discrimination, as detailed in Table 9. To summarize, MHPs could statistically discriminate against minorities by assuming that they have worse insurance or worse ability to pay. We can quantify this statistical discrimination by testing if minorities face more discrimination when insurance status is not revealed than when it is revealed. If MHPs assume that minorities have worse ability to pay, then revealing ability to pay (e.g., private insurance) will differentially boost positive response rates more for minorities.

[26] The first wave of the experiment, detailed in this paper, did not include any mentions of insurance status.

Discrimination Against Chinese Prospective Patients

In the next wave of our experiment, we include names that signal the prospective patient is Chinese American. No study has examined if Asian Americans face discrimination in access to mental health care services. Chinese American status will be disclosed with frequent first and last names in the U.S. Chinese community. Half the time, our Chinese American prospective patients will have Chinese first names and last names. The other half of the time, our Chinese American prospective patients will have gender-specific English first names and Chinese last names.

Sexual Orientation Discrimination

In a future wave of the experiment, we will include signals for sexual orientation, likely using a similar approach to how we signal TNB status. That is, lesbian, gay, or bisexual prospective patients would include a statement like "I am gay/lesbian/bisexual and am looking for a gay-friendly therapist." This extension to study sexual orientation discrimination would be the first audit field experiment of discrimination in access to health care for sexual minorities.

COVID-19 and Access to Mental Health Care

Our first wave of data collection (between January 28, 2020, and May 15, 2020) occurred during the first wave of the COVID-19 pandemic and we plan to continue further data collection. As of writing, cases have reached an all-time high and there is no expectation of the pandemic ending any time soon. This provides us with a natural experiment to explore how access to mental health care varies before, during, and after the pandemic and with the severity of the pandemic.

Why should access to MHPs change during a pandemic? Similarly to other social crises, the intensity of the COVID-19 pandemic—as proxied by infection and mortality rates as well as by shelter-in-place ordinances—increases depression and suicidal ideation (Killgore et al. 2020; McIntyre and Lee 2020; Pfefferbaum and North 2020; Torales et al. 2020). MHPs can help treat these conditions, but they are likely to face increased demand for appointments. Access could also change through an inability to meet in person under normal circumstances, and the movement of MHPs towards greater use of telehealth systems (Madigan et al. 2020; Reay et al 2020).

We will test three hypotheses: (i) the increase in the COVID-19 intensity, measured as either cases or deaths, reduces access to therapy appointment; (ii) shelter-in-place ordinances reduce access to therapy appointments; and (iii) discrimination against minorities increases with increased COVID-19 intensity and with shelter-in-place ordinances. We hypothesize that discrimination could increase in these cases since prior research links (but not conclusively) increased discrimination to shortages (Baert et al. 2015; Carlsson, Fumarco, and Rooth 2018; Dahl and Knepper 2020; Kroft, Notowidigdo, and Lange 2013).

The integration of COVID-19 data will help us explore in greater detail discrimination against Chinese Americans. Based on surveys early during the COVID-19 pandemic, there was an increase in anti-Asian and anti-Chinese views and events (Litam 2020; Ruiz, Horowitz, and Tamir 2020). We will examine if MHPs are more or less responsive to Chinese Americans over the course of the COVID-19 pandemic. We will exploit both geographical and temporal variation in the pandemic's severity to examine how this severity correlates with MHP behavior towards Chinese Americans.

3.7 Appendix

Table A1: Robustness Test--Differences in Positive Response Rates, Results for Aggregated Groups and by Mental Health Concern (Probit Model Marginal Effects)

	Linear Probability Model (1)	Probit Average Marginal Effects (2)
Transgender or Non-binary	.0123	.0112
	(.0426)	(.0442)
African American	-.1333**	-.1417**
	(.0404)	(.0404)
Hispanic	-.1302**	-.1280**
	(.0495)	(.0485)
Depression	.1459**	.1515**
	(.0576)	(.0572)
Anxiety	.0111	.0169
	(.0527)	(.0515)
State fixed effects:	X	X
Week-sent fixed effects:	X	X
Day-of-the-week-sent fixed effects:	X	X
Mean positive response rate for excluded category (cisgender whites w/ stress):	.6473	
N	1,000	1,000
Adjusted R^2	0.0986	0.0822

Notes. Regression estimates based on equation (1). Standard errors, clustered at the patient level and average marginal effects standard errors calculated via delta method, both in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A2. Alternative Positive Response Rates by Gender Identity

Response Rates by Trans/Cis Status:	Positive	Negative	Total		
Cisgender	71.3% (342)	28.7% (138)	480		
Transgender or Non-binary	63.1% (328)	36.9% (192)	520		
Total	67.0% (670)	33.0% (330)	1,000		
Test of independence, p-value	0.006				
Response Rates by Gender Identity:					
Cisgender men	71.9% (223)	28.1% (87)	310		
Cisgender women	70.0% (119)	30.0% (51)	170		
Transgender men	58.6% (82)	41.4% (58)	140		
Transgender women	67.1% (114)	32.9% (56)	170		
Non-binary	62.9% (132)	37.1% (78)	210		
Tests of independence, p-values	Cis men	Cis women	Trans men	Trans women	Non-binary
Cisgender men	…				
Cisgender women	0.655	…			
Transgender men	0.005	0.036	…		
Transgender women	0.265	0.561	0.124	…	
Non-binary	0.029	0.145	0.422	0.395	…

Notes: Our alternative positive response rate codes responses as positive if the MHP's response was an appointment offer, call or consultation offer, screening questions, or referral. P-values come from a t-test (two-sided).

Table A3. Alternative Positive Response Rates by Race or Ethnicity

	Positive	Negative	Total
White	68.6% (343)	31.4% (157)	500
African American	67.8% (183)	32.2% (87)	270
Hispanic	62.6% (144)	37.4% (86)	230
Total	67.0% (670)	33.0% (330)	1,000
Tests of independence, p-values	White	African American	Hispanic
White	…	…	…
African American	0.815	…	…
Hispanic	0.111	0.227	…

Notes: Our alternative positive response rate codes responses as positive if the MHP's response was an appointment offer, call or consultation offer, screening questions, or referral. P-values come from a t-test (two-sided).

Table A4. Alternative Positive Response by Race or Ethnicity, for Cisgender and Transgender or Non-Binary Patients Separately

Response rates for cisgender only:	Positive	Negative	Total
White	72.7% (189)	27.3% (71)	260
African American	72.9% (102)	27.1% (38)	140
Hispanic	63.8% (51)	36.2% (29)	80
Total	67.0% (670)	33.0% (330)	480
Test of independence, p-values	White	African American	Hispanic
White	…	…	…
African American	0.972	…	…
Hispanic	0.126	0.159	…
Response rates for transgender or non-binary only:			
White	64.2% (154)	35.8% (86)	240
African American	62.3% (81)	37.7% (49)	130
Hispanic	62.0% (93)	38.0% (57)	150
Total			520
Test of independence, p-values	White	African American	Hispanic
White	…	…	…
African American	0.724	…	…
Hispanic	0.667	0.958	…

Transgender or non-binary vs. Cisgender - Tests of independence, p-values			
	Cisgender White	Cisgender African American	Cisgender Hispanic
Transgender or non-binary White	0.040	…	…
Transgender or non-binary African American	0.036	0.064	…
Transgender or non-binary Hispanic	0.024	…	0.795

Notes: Our alternative positive response rate codes responses as positive if the MHP's response was an appointment offer, call or consultation offer, screening questions, or referral. P-values come from a t-test (two-sided).

Table A5: Differences in Alternative Positive Response Rates, Results for Aggregated Groups and by Mental Health Concern

	(1)	(2)	(3)	(4)	(5)
Transgender or Non-binary	-.0764**	-.0656*	-.0378	-.0328	-.0178
	(.0370)	(.0357)	(.0386)	(.0406)	(.0428)
African American	-.0081	-.0107	-.0617*	-.0847**	-.0753**
	(.0408)	(.0404)	(.0363)	(.0366)	(.0374)
Hispanic	-.0467	-.0565	-.0851	-.1086*	-.1345**
	(.0477)	(.0459)	(.0543)	(.0577)	(.0623)
Depression	…	.0266	. 0695	.1679**	.1688**
		(.0382)	(.0487)	(.0620)	(.0636)
Anxiety	…	-.0585	.0123	. 0557	.0494
		(.0530)	(.0570)	(.0605)	(.0604)
State fixed effects:			X	X	X
Week-sent fixed effects:				X	X
Day-of-the-week-sent fixed effects:					X
Mean positive response rate for excluded category (cisgender whites w/ stress):	. 7226	.7204	.6748	.7739	.7617
N	1,000	1,000	1,000	1,000	1,000
Adjusted R^2	0.0091	0.0143	0.0777	0.0900	0.1070

Notes: Our alternative positive response rate codes responses as positive if the MHP's response was an appointment offer, call or consultation offer, screening questions, or referral. Regression estimates based on the linear probability model in equation (1). Standard errors, clustered at the patient level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Differences in Alternative Positive Response Rates, Results by Gender Identity

	(1)	(2)	(3)	(4)
Transgender or Non-binary	-.0178	…	…	…
	(.0428)			
…Binary Transgender	…	-.0140	…	…
		(.0450)		
…Trans Women	…	…	.0381	.0485
			(.0697)	(.0712)
…Trans Men	…	…	-.0797	-.0751
			(.0610)	(.0625)
…Non-binary	…	-.0267	-.0285	…
		(.0812)	(.0814)	
…Non-binary female first name	…	…	…	-.0594
				(.0982)
…Non-binary male first name	…	…	…	.0266
				(.1137)
Cisgender Women	…	…	-.0157	-.0186
			(.0640)	(.0534)
African American	-.0753**	-.0758**	-.0608	-.0559
	(.0374)	(.0368)	(.0423)	(.0435)
Hispanic	-.1345**	-.1347**	-.1046*	-.1132**
	(.0623)	(.0626)	(.0565)	(.0568)
Mean positive response rate for excluded category (cisgender white men):	.7617	. 7622	.7507	.7558
N	1,000	1,000	1,000	1,000
Adjusted R^2	0.1076	0.0944	0.0964	0.0969

Notes: Our alternative positive response rate codes responses as positive if the MHP's response was an appointment offer, call or consultation offer, screening questions, or referral. All regressions include the controls in column (5) of Table 5: mental health concern (depression, anxiety, stress), state fixed effects, day of the week sent fixed effects, and week sent fixed effects. Column (1) repeats the results from column (5) in Table 5a for ease of interpretation. Standard errors, clustered at the patient level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Differences in Alternative Positive Response Rates, Intersectional Results by Trans/Cisgender Status and Race/Ethnicity

	(1)	(2)
Transgender or Non-binary	-.0178	…
	(.0428)	
…and white	…	. 0840
		(.0668)
…and African American	…	-.0983*
		(.0570)
…Hispanic	…	-.1500**
		(.0748)
Cisgender		
…and African American	…	.0401
		(.0706)
…Hispanic	…	.0007
		(.0781)
All African American	-.0753**	…
	(.0374)	
All Hispanic	-.1345**	…
	(.0623)	
Mean positive response rate for excluded group (cisgender whites):	.7617	.7771
N	1,000	1,000
Adjusted R^2	0.1076	0.0986

Notes: Our alternative positive response rate codes responses as positive if the MHP's response was an appointment offer, call or consultation offer, screening questions, or referral. All regressions include the controls in column (5) of Table 5a: mental health concern (depression, anxiety, stress), state fixed effects, day of the week sent fixed effects, and week sent fixed effects. Column (1) repeats the results from column (5) in Table 5 for ease of interpretation. Standard errors, clustered at the patient level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Differences in Alternative Positive Response Rates, Intersectional Results by Gender Identity and Race/Ethnicity

	(1)
Transgender Women	
…and white	.1796**
	(.0802)
…and African American	-.0901
	(.1228)
…Hispanic	-.2362*
	(.1259)
Transgender Men	
…and white	.0806
	(.1267)
…and African American	-.0802
	(.0668)
…Hispanic	-.2360**
	(.1184)
Non-binary	
…and white	-.0203
	(.1031)
…and African American	-.3426**
	(.1324)
…Hispanic	-.1058
	(.1051)
Cisgender	
…and African American	.0551
	(.0788)
…Hispanic	.0414
	(.0803)
Mean positive response rate for excluded group (cisgender whites):	.8302
N	1,000
Adjusted R^2	0.1047

Notes: See notes to Table A6. Standard errors, clustered at the patient level, in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

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Vita

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