What Good Are Treatment Effects without Treatment? Mental Health and the Reluctance to Use Talk Therapy*

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ABSTRACT: Mounting evidence across disciplines shows that psychotherapy is more curative than antidepressants for mild-to-moderate depression and anxiety. Yet, few patients use it. This paper develops and estimates a structural model of dynamic decision-making to analyze mental health treatment choices in the context of depression and anxiety. The model incorporates myriad costs suggested in previous work as critical impediments to psychotherapy use. We also integrate links between mental health and labor outcomes to more fully capture the benefits of mental health improvements and the costs of psychotherapy. Finally, the model addresses measurement error in widely-used mental health variables. Using the estimated model, we find that mental health improvements are valuable, both directly through increased utility and indirectly through earnings. We also show that even though psychotherapy improves mental health, counterfactual policy changes, e.g., lowering the price or removing other costs, do very little to increase uptake. We highlight two conclusions. As patient reluctance to use psychotherapy is nearly impervious to a host of *a priori* reasonable policies, we need to look elsewhere to understand it (e.g., biases in beliefs about treatment effects, stigma, or other factors that are as yet unknown). More broadly, large benefits of psychotherapy estimated in randomized trials tell only half the story. If patients do not use the treatment outside of an experimental setting—and we fail to understand why or how to get them to—estimated treatment effects cannot be leveraged to improve population mental health or social welfare.

KEYWORDS: Mental Health, Demand for Medical Care, Labor Supply, Structural Models.
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1 Introduction

Mental illness is widespread and costly. Roughly one in five adults in the US experiences mental illness in a given year, the most common being mild-to-moderate depression or anxiety.[1] Mental health problems are consistently associated with a host of costly outcomes, ranging from low labor market productivity (Frank and Gertler, 1991) to problematic parenting (Ronda, 2019). Yet, how patients choose to treat mental illness remains poorly understood. For example, below we describe a broad, cross-disciplinary literature which suggests that psychotherapy is more curative than antidepressants for mild-to-moderate depression and anxiety, yet the vast majority of individuals treating these conditions opt for the latter. In the last two decades, antidepressant use has soared. In a survey running from 2011 to 2014, about 13% of Americans over age 12 reported using an antidepressant in the past month (Pratt, Brody, and Gu, 2017). For comparison, as of 2011, just over 3% of Americans over the age of 12 reported using psychotherapy during the past *year* (see Figure 1). Failing to understand patient mental health treatment decisions impedes the development and evaluation of policies to address mental health problems.

This paper develops a structural model of dynamic decision-making to analyze mental health treatment choices in the context of depression and anxiety. The aim is to shed light on patient decisions—in particular, reluctance to use psychotherapy versus antidepressants—and to evaluate policies that could affect these choices.[2] To that end, the model incorporates several benefits of antidepressants and psychotherapy (sometimes known as talk therapy or colloquially as "therapy"). Both treatment options improve mental health, albeit to different degrees, which increases utility directly and also indirectly through higher earnings. The model also includes a host of costs that could help to explain reluctance to use psychotherapy, including out-of-pocket payments (which are a function of insurance), time costs, and disutility of psychotherapy. Moreover, the model accounts for a unique feature of the context: just under 50% of people go to two or fewer psychotherapy appointments even though

1. According to the National Alliance on Mental Illness (NAMI). Moreover, as of 2014, there were more than 222 million annual prescriptions filled for antidepressants, generating over $14 billion in revenues (Greenblatt, Harmatz, and Shader, 2018).

2. In what follows, the term "depression" is meant to include a narrow set of psychological disorders that share a common set of symptoms and treatments (e.g., major depressive disorder, generalized anxiety disorder, acute reaction to stress, etc.). Moreover, the term "antidepressants" is meant to include all prescription drugs used in the treatment of these conditions (e.g., SSRIs, SNRIs, Benzodiazapines, etc.). In Section 3.2, we show that these conditions and treatments describe the overwhelming majority of mental conditions experienced and drug treatments used by our population of study (26-55 year old Americans). Thus, while our empirical analysis allows for a broader range of conditions and treatments, it is most appropriate to interpret our findings as relating to the class of depressive conditions and treatments.

a typical course is 8-12 sessions.[3] The reason is that patients may try psychotherapy, learn they dislike either their therapist or perhaps psychotherapy in general (e.g., speaking about private matters to a stranger), and stop going after the first or second appointment.[4] We model this behavior as a post-decision "shock" that ends the course of treatment and does not improve mental health. This captures the idea that patients cannot predict an early end to psychotherapy with certainty, but are aware it is a possibility when deciding whether to incur the upfront costs of seeking psychotherapy, which could contribute to their reluctance to do so. The addition of this feature is important. As we will show, reducing the possibility of premature discontinuation is one of the few ways to increase psychotherapy usage.

The structural model also addresses problems associated with the measurement of mental health. In rich, longitudinal datasets such as the one we use, mental health is often indexed using a series of targeted questions (e.g., the Kessler 6 or SF-12) or proxied using a coarse, self-reported scale, of "excellent," "very good," "good," "fair," or "poor." While a worse report of such mental health variables is associated with a higher probability of depression diagnosis, two individuals with similar reports in a given period could have vastly different underlying mental health problems. For example, one could be having a bad day and the other could be clinically depressed.[5] One way to address this issue is to focus attention on individuals who are diagnosed as depressed, but there is selection into diagnosis.[6] An alternative is to observe individuals over time and categorize them as depressed only if they report low levels of mental health repeatedly. Our approach is related, but does not require us to make arbitrary assumptions about how to categorize individuals. In particular, we permit unobserved heterogeneity in the form of latent types that can differ in average mental health along with disutility of treatment and of work. This approach exploits repeated observations over time to identify persistent differences in mental health, which delivers a better proxy

3. Similarly high psychotherapy drop-out rates are well documented in the psychology literature (Wierzbicki and Pekarik, 1993; Swift and Greenberg, 2012).

4. An alternative explanation is that people go to a therapist once or twice in order to obtain medication. This is unlikely to explain the pattern as our data set explicitly asks whether or not the reason for a visit was to obtain psychotherapy.

5. The Kessler 6 or other more formal measures are not available in our data for all periods, which is why we rely on self-reported scales. While the more formal measures would be preferable, they are still obtained by asking people questions about how they feel and are thus prone to measurement error. For example, see Prochaska et al. (2012) for an analysis of the sensitivity and specificity of the Kessler 6 scale for identifying moderate mental distress.

6. Individuals must see a licensed health professional in order to be diagnosed with a disorder, particularly if they wish to receive treatment; thus, there is selection into diagnosis, as those willing and able to seek a diagnosis and/or treatment are more likely to be measured as ill. Using representative household surveys and interviewers trained to diagnose depression, Thornicroft et al. (2017) report that among those in the United States with depression during the past year, 74 percent "recognised that they needed treatment and 77 percent of these individuals made at least one visit to a service provider; thus, only 57 percent of those depressed during the prior year saw a provider.

of mental health, as it is less prone to measurement error arising from intermittent drops in reported mental health that are unlikely to reflect clinical depression.[7]

We estimate parameters of the dynamic choice model using moments from several data sources. First, we use the 1996-2011 cohorts of the Medical Expenditure Panel Survey (MEPS) data, which apart from mental health treatments and conditions, also contain rich data on labor supply and earnings. One unique feature of this data set is that it includes mental health information for individuals who are unemployed, which allows us to explore links between mental health conditions, employment decisions, and related outcomes. In principle, we should be able to estimate the impact of treatment on mental health using these data. However, selection problems (in part due to the coarse mental health measure discussed previously) and a lack of credible instruments make doing so difficult. Thus, our preferred estimates rely on findings from a collection of randomized controlled trials (RCT) summarized in the medical and psychology literatures. This means we can focus attention on patient dynamic decision-making in light of treatment benefits credibly estimated from more suitable data.[8]

Model estimates reveal substantial heterogeneity among observably similar individuals. In particular, there are four distinct latent types. Type 1 individuals comprise roughly 5% of the population and have perpetually poor mental health (i.e., 1.6 standard deviations below the sample mean on average). Type 2 individuals comprise roughly 16% of the population and have what might be considered moderate mental health (i.e., just 0.1 standard deviation below the sample mean on average). Types 3 and 4 comprise the remaining 79% of the population and have very good mental health. This heterogeneity is crucial in interpreting cross-sectional patterns in the data, which show higher rates of low-to-moderate mental health arising from intermittent reports of poor mental health. Indeed, if we take all reports of low-to-moderate mental health at face value (i.e., if we do not allow for unobserved heterogeneity), we over-estimate the number of individuals who could benefit from treatment and do not use it, which inflates forgone benefits, leading us to under-estimate the marginal benefit of good mental health. Regarding psychotherapy, the estimated latent type probabilities suggest that the proportion of the population that could benefit

7. A simple exercise we perform is to verify that individuals who report low subjective mental health multiple times are indeed more likely (measured as a posterior probability) to belong to latent types characterized by persistently low subjective mental health.

8. In Appendix A.III, we merge two sources of county-level data into the MEPS in order to construct instruments that allow us to estimate the impact of treatment. The instrumental variables strategy produces effect sizes that are larger than those found in most clinical or field-experimental settings. Thus, we prefer to use estimates from well-identified settings. If we instead rely on our estimates, results are qualitatively similar to the main results; however, doing so deflates the estimated disutility of poor mental health, as the model struggles to explain the underutilization of highly effective treatments.

from psychotherapy is somewhat smaller than cross-sectional reports of poor mental health would suggest. Never-the-less, type probabilities identified from persistent reports of low-to-moderate mental health mean that roughly 21% of the population could benefit from psychotherapy—and the vast majority do not use it.

Model estimates also show that individuals derive substantial utility from mental health. These effects are identified by relatively high antidepressants use along with higher treatment uptake at lower levels of mental health. We illustrate the value of mental health improvements with a simple counterfactual that supposes mental health in the population cannot fall below the sample mean. Applied to the US population as a whole, we find that citizens would value such a technology at roughly $150 billion dollars per year (in 2018 dollars), with the largest benefits accruing to Types 1 and 2. About 15 percent of this value is attributable to recouping lost earnings from poor mental health. Importantly, finding large utility gains from mental health rules out a potential explanation of low psychotherapy use: that people simply do not value mental health very much. Our findings show that mental health is valued, but that people choose not to use the most effective treatment available. Our remaining counterfactuals are designed to shed light on this puzzle.

In particular, our second set of counterfactuals assesses the impact of assignment to psychotherapy (assuming perfect compliance) in one 6-month period, then tracks behavior and outcomes over the following year and a half. As expected, the roughly 80% of individuals with high baseline levels of mental health (i.e., Types 3 and 4) experience little change to their mental health. Consistent with earlier work exploiting random assignment (Baranov et al., 2019), the remaining 20% (i.e., Types 1 and 2) experience increases in mental health in both the short and the long run, both of which are largest for the sickest individuals. This improvement in mental health is driven in part by greater antidepressant use, suggesting complementarities between the two treatments. Persistent effects are also driven by an increased willingness to go to psychotherapy once individuals have gained experience with the treatment. We do not find positive employment or wage effects. This finding runs counter to narratives suggesting that policy aimed at bettering mental health could "pay for itself" through increased employment or worker productivity (Laynard et al., 2007). It is driven by the fact that among individuals with the most to gain from psychotherapy, labor supply and wages are not very elastic to mental health.[9] These discouraging results echo

9. Both Butikofer, Cronin, and Skira (2020) and Shapiro (2020) find instances where observed increases in antidepressant use yield greater labor supply. The contrast in our findings may suggest important differences in the elasticity of labor supply with respect to mental health for marginal psychotherapy and antidepressant users or employment-specific costs associated with psychotherapy. For example, our findings suggest that the non-trivial time cost of psychotherapy makes employment difficult, which negates some of the employment-enhancing effects of improved mental health that comes with psychotherapy use.

findings in Baranov et al. (2019), who show that women suffering postpartum depression who are randomly assigned to psychotherapy exhibit substantial mental health improvements compared to women in the control group—in both the short and long run. While these mental health improvements are found to impact some choices, such as time and monetary parental investments, they do not spill over to children's outcomes or lead to statistically significant increases in employment.[10]

While random assignment to psychotherapy, either in an experimental setting or as a counterfactual policy, can shed light on the impact of psychotherapy, the population-level benefits of estimated treatment effects accrue only if people actually use psychotherapy, which they rarely do. This leads us to explore policies that focus on individual choices versus random assignment. In particular, in our third set of counterfactuals we assess responsiveness to several commonly-suggested policies that would presumably lower barriers to psychotherapy and thus increase usage, including monetary and time costs. Individuals hardly respond to lower psychotherapy prices or a reduction in time costs. A notable exception is that, among the 5% of individuals in persistently "fair" or "poor" mental health (i.e., Type 1 individuals), we see a 120 to 160 percent increase in psychotherapy use when we remove the post-decision preference shock that leads to discontinuation, which yields economically meaningful improvements in mental health.[11]

These counterfactuals are somewhat bleak. Factors widely viewed as critical barriers to psychotherapy use (e.g., time and monetary costs) explain little patient reluctance to use the treatment. The only exception is a notable rise in uptake in the 5% of individuals with more severe depression or anxiety when shocks leading to early discontinuation of psychotherapy are removed. While this does not translate to higher employment for this group, it does appreciably improve mental health and thus merits further exploration. Type 2 individuals, who represent the 16% of Americans suffering from more mild mental health issues (but who would still benefit substantially from mental health improvements), are nearly impervious to the policies we consider, even when the cost reductions are dramatic. The effort to produce meaningful improvements in population mental health thus requires that we look elsewhere.

Our estimates suggest that simple disutility from psychotherapy is the primary disincentive for use. It is entirely possible that this is correct, e.g., maybe people simply do not

10. It should be noted that baseline labor supply outside the home is low for the sample in Baranov et al. (2019), similar to Type 1 and 2 individuals in our sample, so there is little scope to detect any effects on employment. Moreover, they do report increases in financial empowerment and time spent with children. Unfortunately, we do not observe whether this is reflected in our results as neither is measured in the MEPS.

11. The other three types each experience a 70 percent increase in psychotherapy when all impediments are removed. The baseline is low, so the absolute rise is modest and has no measurable impact on population mental health. E.g., Type 3 individuals' use of psychotherapy rises from 1.4% of periods to 2.3% of periods.

want to share private issues with a stranger. In that case, policy designed to increase psychotherapy use would need to change the way psychotherapy occurs or is perceived, which is challenging. Another interpretation consistent with this finding is that low psychotherapy use is driven by stigma. An extensive literature has discussed stigma in the context of mental health treatment (see, for example, Corrigan, 2004; Bharadwaj, Pai, and Suziedelyte, 2017), and there is evidence that stigma plays an important role in various other contexts, such as social welfare programs (Moffitt, 1983) and HIV testing (Yu, 2019). Alternatively, substantial treatment disutility may proxy for mechanisms outside the scope of the model, such as heterogeneity in beliefs. For example, some individuals may not see psychotherapy as particularly helpful. These beliefs may be correct if treatment effects are heterogeneous or if outcome expectations influence treatment success, of which there is some evidence (Greenberg, Constantino, and Bruce, 2006; Sotsky et al., 2006; Constantino et al., 2011). Yet, well-designed, randomized trials with low rates of attrition have established large and positive average treatment effects (see subsection 3.4.3), so this is unlikely to be the full story. Instead, it is possible that beliefs are simply biased, i.e., that individuals incorrectly assume that psychotherapy is not effective. Given that commonly suggested impediments to psychotherapy do not appear to be binding constraints, data collection efforts should shift towards factors that could shed light on the roles that heterogeneity in treatment effects, biases in beliefs, and stigma play in deterring people from psychotherapy. We return to this point in the conclusion.

The paper proceeds as follows: Section 2 discusses related research. Section 3 introduces the data used in this project and highlights several key empirical patterns that motivate our analysis. Section 4 introduces the dynamic choice model. Section 5 discusses estimation and identification. Section 6 presents parameter estimates, model fit, and the results from counterfactual policy simulations, which use the estimated dynamic choice model. Section 7 concludes.

2 Literature

In studying mental health treatment choices, we contribute to a large literature engendered by Grossman (1972) that views medical treatment decisions as rational, dynamic decisions made under uncertainty. Within this framework, medical treatment is seen as a costly investment. Rational, forward-looking patients make medical decisions by weighing both current and future costs and benefits of different treatment options. The Grossman framework has been applied to a number of health contexts, such as chronic illness (Cronin, 2019) and infectious disease (Chan, Hamilton, and Papageorge, 2015) and extended to incorporate additional features of healthcare decisions, such as learning and uncertainty about treatment quality (Crawford and Shum, 2005; Chan and Hamilton, 2006), drug side effects (Papageorge, 2016), risky behaviors that affect illness (Arcidiacono, Sieg, and Sloan, 2007; Darden, 2017), and links between health and the labor market (Gilleskie, 1998).

A smaller, growing literature in economics also studies mental health. Generally, this literature documents that mental health is valuable and corroborates the medical literature showing that psychotherapy improves mental health. An early contribution is Ettner, Frank, and Kessler (1997) who provide evidence that psychiatric disorders significantly reduce employment, hours worked, and income. In a more recent and groundbreaking study, Baranov et al. (2019) find that random assignment to psychotherapy for women with postpartum depression yields substantial mental health benefits that extend over many years. In another recent study, Jolivet and Postel-Vinay (2020) estimate a job search model where mental health and job stress play a role in selection into employment and across types of jobs. A key finding is that negative mental health shocks are very costly, equating to roughly one-third of the cost of losing a job for an average worker. While our findings are in line with these papers, their key findings create a puzzle. If psychotherapy is highly effective, and people value mental health, then why is psychotherapy rarely used? The unique contribution of this paper is to shed light on this puzzle, which we do by incorporating various costs and benefits of different mental health treatments into a unified framework to assess treatment choices, in particular, reluctance to use psychotherapy. Doing so is an important complement to previous work on the value of mental health and the benefits of psychotherapy, which can only be harnessed if people opt for psychotherapy outside of experimental settings.

To our knowledge, we are the first to apply the Grossman framework in the form of a structural dynamic model of treatment choices and employment decisions to understand how forward-looking individuals manage their mental health.[12] This gap in the literature is itself puzzling. It likely arises from some of the econometric issues we encounter in this study, including difficulties measuring mental health and the impact of treatment due to coarse subjective mental health measures; nonrandom selection into diagnosis and into treatment; as well as limited data available to relate mental health, treatment decisions, and labor market outcomes. Another unfortunate reason for this gap in the literature is that mental health problems—perhaps due to widespread stigma or ignorance—may be seen as fundamentally different from physical health problems. The implicit suggestion is that rational choice,

12. Davis and Foster (2005) use the Grossman framework to study a parent's choice to seek mental health treatment for their children. Yet, as Currie and Stabile (2006) mention, the Grossman framework has generally not been applied to mental health investments.

applied in a wide variety of medical contexts, is somehow inappropriate for an analysis of mental healthcare. This position ignores the fact that the vast majority of mentally ill individuals manage relatively mild illnesses. Indeed, it seems especially inappropriate to begin with an assumption of irrationality for Type 2 individuals, who make up the vast majority of people who could benefit from psychotherapy, are least likely to use it and are most impervious to policy changes; and yet, most are educated, married, and a nontrivial fraction are employed.[13] Moreover, it impedes progress on the fundamentally important question of why people do not use a beneficial treatment despite widespread evidence of its effectiveness, which is the focus of this paper.

Finally, we contribute to a massive and well-developed medical and public health literature on the determinants and consequences of mental health issues, the effectiveness of mental health treatment, and predictors of mental health treatment choices. Our approach is motivated by earlier work on the substitutability of mental health treatments (Elkin et al., 1989; Berndt, Frank, and McGuire, 1997) and patient price sensitivity (Ellis, 1986; Frank and McGuire, 1986; Keeler, Manning, and Wells, 1988). In addition, we contribute to research examining how mental health, treatment, education, and the labor market interact for both adolescents (Currie and Stabile, 2006; Fletcher and Wolfe, 2008; Fletcher, 2008, 2014) and for adults (Frank and Gertler, 1991; Ettner, Frank, and Kessler, 1997; Stewart et al., 2003; Greenberg et al., 2003; Butikofer, Cronin, and Skira, 2020; Shapiro, 2020). Much of this literature focuses on more severe mental health problems, whereas we consider a representative sample, which includes individuals suffering from moderate, mild, or no mental illness at all. We are thus able to place focus on the relatively large set of individuals who are not severely ill, but who could benefit from psychotherapy and yet choose not to, even when the costs of doing so are drastically reduced. Finally, we relate to medical and psychological literature studying barriers to access and stigma as possible reasons why psychotherapy uptake is low (Corrigan, 2004). This paper marks an initial attempt to incorporate various costs and benefits of psychotherapy into a unified choice framework to understand reluctance to use a valuable treatment and assess which types of policies are worth pursuing to increase usage.

13. According to the National Survey on Drug Use and Health, in 2015 18% of U.S. adults reported mental illness in the past year, while only 4% report a *serious* mental illness. According to the National Institute of Mental Health (NIMH), *serious* mental illnesses are defined as those, "resulting in serious functional impairment, which substantially interferes with or limits one or more major life activities." Even among these individuals, inpatient treatment, much less institutionalization, is rare. In 2008, only 7.5% (NIMH) of individuals reporting a serious mental illness sought inpatient treatment.

3 Data

3.1 Data Set

Our empirical analysis uses data from the Medical Expenditure Panel Survey (MEPS), which has been collected annually since 1996 by the Agency for Healthcare Research and Quality (AHRQ). Each year, a nationally representative sample of new participants is added to the MEPS, drawn randomly from the previous year's National Health Interview Survey (NHIS) sample. Each cohort is interviewed five times over the two years that follow January 1st of the cohort year.

Several characteristics of the MEPS make it well suited for our purposes. The MEPS contains individual-level panel data on mental health, treatment, and employment, all of which are needed to estimate our dynamic model.[14] To our knowledge, no other publicly available data set offers this unique set of features.[15] Moreover, the MEPS offers several clear advantages over the large, administrative claims data that have become popular in the literature. For example, the MEPS consistently reports a mental health measure that does not require diagnosis. Such a measure is needed if one hopes to model dynamic mental health transitions as a motivation for treatment decisions. Claims data only offer treatment decisions and, possibly, mental illness diagnosis, which can only be observed if a patient endogenously chooses to visit a physician. Moreover, researchers typically acquire claims data from large, self-insuring employers; thus, all individuals are employed and insured. Critical to our study is the relationship between mental health and employment, which may be influenced by insurance status; thus, observing variation in all three measures is required.

Despite these advantages, the MEPS data have several drawbacks. First, the panel is short and individuals enter at various points in the lifecycle; thus, we need to address endogenous initial conditions with our econometric specification. Second, an important relationship we want to estimate—the impact of medical treatments on mental health transitions—is subject to selection bias and the data offer few credible instruments to help in identification. We discuss these challenges in further detail in Appendix Section A.III.1. Third, survey data are likely to contain measurement error in key variables, such as wages, medical care prices, and medical care treatment, as the data rely on accurate self-reports of events that

14. Eckstien et al. (2019) explain how dynamic models can be estimated using cross-sectional data as long as endogenous state variables are observed. However, panel data is required if one is to capture permanent unobserved heterogeneity in choice and transition probabilities. As explained in Section 6.1, such heterogeneity is critical to our findings.

15. An exception is the Health and Retirement Survey (HRS)—Medicare link, which focuses on the elderly making it less than ideal for a study of mental health and employment.

may occur several months in the past. When administrative data sets report these variables, they are likely subject to less measurement error; however, many such data sets used in this literature exclude variables such as wages and the price of specific medical treatments due to individual privacy and corporate proprietary concerns (e.g., Kowalski, 2015).

Our estimation sample is comprised of individuals 26-55 years old from the 1996-2011 MEPS cohorts. We exclude the first interview period because lags of several variables are important in our analysis. We exclude individuals who miss one or more interviews. We also exclude individuals who have an interview period that is less than three and a half months or greater than seven months. The resulting analytic sample consists of 54,989 individuals and 208,113 individual-period dyads. In Section A.I.1 of the appendix, we discuss these sample restrictions and their impact on the generalizability of our results.

3.2 Mental Health and Treatment in the MEPS

The MEPS offers several ways to measure an individual's mental health and associated treatment decisions. As we are interested in mental health broadly, the primary measure that we employ is self-reported. Specifically, survey participants are asked, "In general, would you say that your mental health is excellent (5), very good (4), good (3), fair (2), or poor (1)?" Additionally, participants are asked to report all "health problems (experienced during the current interview period) including physical conditions, accidents, or injuries that affect any part of the body as well as mental or emotional health conditions, such as feeling sad, blue, or anxious about something." The description of the illness is recorded as verbatim text, which is later coded to 5-digit ICD9-CM codes by professional coders.[16] While these condition codes can help us to understand the specific illnesses afflicting our population of interest, they are troublesome empirically, as they are prone to selection. Individuals who have been formally diagnosed with depressive disorder, for example, and/or use treatments for depression are more likely to report the disorder. This selection creates a form of non-classical measurement error in the illness reports, as those willing and able to seek a diagnosis and/or treatment are more likely to be coded as ill. This selection problem does not exist with self-reported mental health.

While variation in self-reported mental health could be generated by changes in any particular mental condition, we argue that in our sample it should be interpreted as largely relating to a narrow set of psychological disorders that share a common set of symptoms and treatments; namely, Depressive Disorders (ICD9 Codes 296 and 311), Anxiety Disorders

16. Only 3-digit codes are available in the public use files that we use in our analysis.

(ICD9 Code 300), and Stress Induced Disorders (ICD9 Codes 308 and 309). These "DAS" disorders represent an overwhelming share of all mental health conditions reported in our sample. Specifically, among all interview rounds in which a mental illness (i.e., ICD9 Codes 290-319) is reported, 93 percent contain a DAS report, while only 12 percent contain a non-DAS report.[17] Furthermore, in the section that follows, we show that self-reported mental health is highly correlated with reports of these DAS disorders.

Medical treatments are reported by the survey respondent. After reporting each medical treatment, individuals are asked about the condition being treated, which is then coded as an ICD9 code.[18] For prescription drugs, individuals also report the corresponding condition being treated, as well as the name, dose, refill information, and price of the drug.[19] Because self-reported mental health is not condition specific, the medical treatments that we model can be related to any mental health condition. Specifically, an individual is coded as using psychotherapy during an interview round if he or she (i) visited a medical office in person, (ii) the visit relates to a mental health condition (i.e., ICD9 code $\in$ [290,319]), *and* (iii) they received psychotherapy/counseling. An individual is coded as using prescription drugs during an interview round if he or she filled a prescription for the treatment of a mental health condition. Note that our choice to code prescription drug use in this way has two implications. First, off-label drug use, which represents as much as 30 percent of antidepressant use (Wong et al., 2016), is intentionally ignored in our analysis, as these treatment regimens should have no impact on mental health.[20] Second, this coding implies that we measure the use of some drugs outside the class of antidepressants (e.g., SSRIs, SNRIs, TCAs, etc.) and benzodiazepines (e.g., alprazolam, diazepam, etc.) that are commonly used to treat DAS disorders. However, given the prevalence of DAS disorders reported above, it should come as no surprise that the overwhelming majority of psychotherapeutic drug use observed in our sample relates to these conditions. Specifically, among all interview rounds where individuals report using a drug to treat a mental health condition, 91 percent take a drug to

17. Examples of other mental illnesses would include substance abuse disorders, dementia, schizophrenia, psychosis, ADHD, Autism, physical brain injuries/deformities, and mental retardation.

18. Note that if the condition was not initially reported, an expanded set of questions is asked, so that the illness is fully documented in the MEPS Condition files. This process of "back-coding" illnesses virtually guarantees that illness reports suffer from the non-classical measurement error referenced above. Moreover, the process ensures that illness reports provide a poor measure of one's current mental health, as someone who is successfully managing their depressive symptoms with treatment (i.e., their mental health is improved because of treatment) will still be coded as "depressed", due to the use of treatment.

19. AHRQ attempts to verify the information provided by participants with the (medical) providers of the treatment, via telephone interviews and mailed survey materials. For prescriptions, AHRQ attempts to contact pharmacies regarding each fill/refill reported by the participant. Physicians are also contacted for reported office-based visits, but may be subsampled at various rates in certain years.

20. According to (Wong et al., 2016), antidepressants are commonly used to treat migraines, insomnia, pain, menopause, etc.

treat a DAS disorder (i.e., ICD9 Codes 296, 300, 308, 309, or 311). In light of this fact, and to simplify our language, in what follows we refer to this "prescription drug use for mental health conditions" as "antidepressant use."

3.3 Descriptive Statistics

The third column of Table A.I presents descriptive statistics for the analytic sample. (The first two columns report statistics for larger samples, which includes individuals with missing data). For the analytic sample, 17 percent of people report a DAS disorder at some point during their two years in the sample. Average subjective mental health is 3.9. Antidepressants are used much more frequently than psychotherapy. Only 4 percent of people ever use psychotherapy over the two year sample period, while 12 percent use antidepressants.

Tables 1, 2, and 3 further detail the relationship between demographics, mental health, and treatment choices. Table 1 shows how subjective mental health and treatment decisions differ by age. The first two columns highlight that as people age, subjective mental health worsens and the likelihood of reporting a DAS condition increases. For example, 6.0 percent of 26-30 year-olds report a DAS condition, while the same is true of 13.5 percent of individuals between 51 and 55. Similarity in these age patterns is one piece of evidence that both subjective mental health and the diagnosis of a condition capture variation in latent mental health. The correlation between these and two other measures of mental health is further discussed in Appendix Section A.I.3.

Columns 3 and 4 of Table 1 show antidepressant and psychotherapy usage, respectively, by age group. Three patterns emerge. One, use of mental health treatment rises with age. Two, an individual is about three-to-five times more likely to use antidepressants than psychotherapy in an interview period. Three, the relative popularity of antidepressants holds across age groups, and psychotherapy becomes even less popular (relative to antidepressants) as individuals age.

Table 2 presents sample means for demographic and labor market variables by treatment choice. The statistics indicate that those individuals who use psychotherapy (columns 2 and 3) are younger, less likely to be married, more likely to live in a metropolitan statistical area (MSA), and are more highly educated than those who use antidepressants alone.[21] Individuals in treatment have worse subjective mental health than those not in treatment, and those using *both* types of treatment have the worst subjective mental health, all of which suggests selection into treatment.

21. Note that here, and throughout the paper, dollar values are reported in 2018 dollars unless stated otherwise

Table 3 presents sample means by level of subjective mental health. The following are associated with worse subjective mental health: being female, older ages, living outside an MSA, being unmarried, living in the South, and being a part of a racial or ethnic minority group. Moreover, unemployment rises as mental health declines. Across levels of subjective mental health, individuals are more likely to choose antidepressants than psychotherapy. For example, individuals in the lowest subjective mental health category remain more than twice as likely to use antidepressants as they are to use psychotherapy and those in the second-to-lowest subjective mental health category are almost three times as likely to use antidepressants.

Table 3 again reveals a close relationship between mental health and reporting a condition; however, even at low levels of subjective mental health, a large proportion of individuals do not report a condition. The fact that those with very low subjective mental health do not always report a condition could result from either undiagnosed conditions or from subjective mental health being an imperfect measurement of latent mental health.

In Table 4, we group individuals by the *lowest* subjective mental health that they report over the course of the survey. Within these groups, we show the proportion that ever report a DAS disorder and the corresponding proportion using treatment. This decomposition again emphasizes that (i) subjective mental health is strongly correlated with diagnosed disorders and (ii) a large share of those who report poor or fair mental health never report a disorder. Moreover, among those who report poor or fair subjective mental health, treatment probabilities are much lower for those who do not report a disorder. For example, of those who report poor mental health at least once and also report a DAS disorder at least once, 43 percent use psychotherapy and 78 percent use antidepressants, but among those not reporting a disorder only 7 percent use psychotherapy and 9 percent use antidepressants. This is further evidence of either (i) undiagnosed and untreated disorders or (ii) subjective mental health being an imperfect measure of latent mental health.[22] That the average subjective mental health across all interviews is consistently higher for those who never report a DAS condition than for those who do, even when holding fixed the lowest level of subjective mental health, suggests that poor subjective mental health sometimes reflects a temporary dip in emotional health rather than the existence of a condition.

22. We have also calculated the statistics in Table 4 across insurance statuses and find that, regardless of insurance status, reporting a disorder is more predictive of treatment use than reporting low levels of subjective mental health.

3.4 Key Empirical Patterns

3.4.1 Treatment Usage

As shown in Tables 3 and 4, individuals are unlikely to use psychotherapy and are far more likely to use antidepressants than psychotherapy regardless of subjective mental health or reporting of a DAS disorder. Table 5 presents estimates from a multinomial logit model, where the outcome categories are no treatment, antidepressants only, psychotherapy only, and both antidepressants and psychotherapy. This exercise highlights heterogeneity in treatment use across demographics, which provides some insight into why individuals are unlikely to use psychotherapy despite its significant benefits. For example, that insured individuals are the most likely to use treatment suggests that financial costs are a barrier to receiving care. The fact that the most likely subgroup to use psychotherapy is college-educated, white women, living in a northeastern MSA suggests that stigma and access to care could be important determinants of psychotherapy use. However, even for this group, psychotherapy is unpopular relative to antidepressants—the average predicted probability of using any psychotherapy is 0.059, whereas the predicted probability of using antidepressants *alone* is 0.084 and the probability of using any antidepressants is 0.126. The highly-persistent unpopularity of psychotherapy across delineated demographic groups and mental health categories suggests that a more robust choice model is needed to understand how patients choose mental health treatments.

3.4.2 Costs of Treatment

That individuals rarely use psychotherapy could be a reflection of the high cost of attending psychotherapy. The most obvious treatment cost to consider is the monetary cost. In Appendix Table A.II, we provide summary statistics for the monetary cost associated with a single psychotherapy visit and a single antidepressant prescription fill, both across time and insurance status. Note that across all insurance types, a large fraction of psychotherapy requires no out-of-pocket payment. For the insured, this is due to cost-sharing. For the uninsured, this is likely due to charity care and public mental health clinics. Prescription drugs are the most expensive for the uninsured, followed by publicly insured individuals. The inflation-adjusted total price of both treatments has increased over time, while the share paid out of pocket has decreased.

Another cost relates to uncertainty. One striking feature of the data is that a large proportion of the individuals who attend psychotherapy do so only once or twice before stopping treatment. To show this, we define a psychotherapy *treatment episode* as a consecutive sequence of psychotherapy sessions occurring without a two-month gap in visits. Figure 3 contains a histogram of the number of psychotherapy visits within each treatment episode. Notice that roughly 50% of these treatment episodes contain only one or two visits, meaning one or two psychotherapy sessions are attended without any sessions attended in the two months preceding or following these visits. It is highly unlikely that such a course of treatment would be prescribed; rather, such behavior reflects what is called "drop-out" or "discontinuation" in the psychology literature, where similarly high rates are reported (Wierzbicki and Pekarik, 1993; Swift and Greenberg, 2012). Many of those who consume psychotherapy at some point during the two-year survey period (1,847 individuals) are observed to *only* consume these very short treatment episodes (583 individuals or 31.5 percent). Relative to other psychotherapy users, those who consume these short psychotherapy episodes are more likely to be from the South, are less educated, are less likely to live in an MSA, and have better subjective mental health.

We assume that treatment episodes containing only one or two visits represent a discontinuation of psychotherapy, which could either mean (i) that the individual is an inexperienced psychotherapy user and, upon their initial visit, learns that they dislike this type of treatment and quits or (ii) that the individual, experienced or inexperienced, visits a new therapist that happens to be a bad match, leading them to quit treatment.[23] Unfortunately, we are not able to see the identity of the therapist that an individual visits and we have limited information on an individual's history of psychotherapy use; thus, distinguishing type (i) and type (ii) individuals from those who consciously visit a therapist every 3-4 months is difficult with our data. In our main analyses, we will view such episodes as an unanticipated discontinuation of psychotherapy, which means some costs, but no benefits, of treatment are incurred.[24]

Finally, psychotherapy carries a significant time cost, as individuals must travel to and participate in individual psychotherapy sessions, which average 50-55 minutes in length.

23. The psychology literature refers to the relationship between therapist and patient as "therapeutic alliance" (Ardito and Rabellino, 2011).

24. Clinical psychologists have struggled to determine whether patients discontinuing treatment experience an improvement in mental health, as discontinuation cannot be randomized and patient outcomes are not visible once they stop attending psychotherapy sessions. Cahill et al. (2003) explores the phenomenon by having patients complete the Beck Depression Inventory (BDI) prior to each psychotherapy session, allowing them to compare last session BDI scores for those completing and not completing an agreed upon number of sessions. (There was no control group not receiving psychotherapy to which non-completers could be compared.) They find that 71 percent of completers reached a clinically significant improvement in their depression symptoms, while only 13 percent of non-completers did. Hansen, Lambert, and Forman (2002) take an alternative approach, conducting meta-analyses of studies using both clinical and naturalistic data. They find that average treatment effects are roughly 2-3 times larger in a clinical setting, where the average number of visits is 2-3 times more, due to discontinuation in the naturalistic setting.

While neither of these time costs are observable in the data, we do observe and model employment, which has a substantial effect on the available time an individual has for psychotherapy. One way to capture this cost in the structural model would be to explicitly model preferences for leisure, which could be decreasing in work hours and some fixed psychotherapy time cost (e.g., 2 hours, which would include a 50 minute session and 70 minutes of round-trip travel time). To provide some evidence that time costs are relevant in decision making, we reestimate the multinomial logit model discussed above, controlling for part- and full-time employment.[25] Consistent with these treatments having relevant time costs, we find that full-time workers are less likely than part-time workers, who are less likely than the unemployed, to use all types of treatment. Moreover, this relationship is strongest for individuals consuming both types of treatment. With that said, we do find that antidepressant use is also decreasing in employment, possibly suggesting that in addition to time costs, the well documented side-effects commonly associated with these drugs impair an individuals ability to work (Cascade, Kalali, and Kennedy, 2009). In light of these findings, we decided not to measure leisure directly in the structural model; rather, we allow preferences for treatment to vary with employment. The latter approach is more flexible in the sense that it captures time costs, but also any other employment-related motivations for not using treatment.

3.4.3 Benefits of Treatment

It is possible that psychotherapy is rarely used because it has limited benefits relative to antidepressants. To consider the benefits of treatment, we turn to the medical literature that has estimated the effects of these treatments on mental health using randomized controlled trials. Effect sizes in the medical literature are typically standardized, i.e., the mean effect is divided by the standard deviation of the outcome, making for a relatively easy comparison of the magnitude of the effects of psychotherapy and antidepressants across research studies.[26] In what follows, we focus on effect sizes estimated for depression and anxiety scales, such as the Hamilton Depression Rating Scale and Hamilton Anxiety Rating Scale.

With respect to the effect of antidepressants on depression, results are consistent across the most highly cited medical research. Turner et al. (2008) performed a meta-analyses of both published and unpublished studies submitted to the Food and Drug Administration for

25. Clearly, employment is endogenous in this simple model, as treatment could also impact the decision to work. As such, this exercise is only meant to be suggestive. These results are available upon request.

26. Different research articles use different measures of the standard deviation based on the type of analysis or meta-analysis being performed. For example, while some report a simple standardized mean difference (SMD), many meta-analyses use either Cohen's d statistic, where a pooled standard deviation is used in the denominator, or Hedges' h statistic, where a pooled, weighted standard deviation is used in the denominator.

review. Among published studies, they report a standardized effect size of 0.37 with a 95 percent confidence interval from 0.33 to 0.41. Among unpublished studies, they report an effect size of 0.15 with a 95 percent confidence interval from 0.08 to 0.22. Kirsch et al. (2008) also perform a meta-analysis and report an effect size of 0.32 with a confidence interval from 0.25 to 0.40. A more recent study by Cipriani et al. (2018) found a similar effect size at 0.3 with a 95 percent confidence interval from 0.26 to 0.34. Regarding the effects of medication on anxiety, Mitte et al. (2005) perform a meta-analysis and document that benzodiazepines and azapirones have effect sizes of 0.32 and 0.30, respectively.

For psychotherapy, in the most highly cited papers, there is a broader range of effect size estimates, but the effect sizes are consistently higher than for antidepressants.[27] Figure 2 shows effect sizes and confidence intervals for psychotherapy from the medical literature. Gloaguen et al. (1998) directly compare the effects of psychotherapy on depression with the effects of antidepressants. They find an effect size of 0.82 for cognitive psychotherapy relative to placebo and an effect size of 0.38 for cognitive psychotherapy relative to antidepressants. Ekers, Richards, and Gilbody (2008) report an effect size for behavioral psychotherapy relative to placebo of 0.70 with a 95 percent confidence interval from 0.39 to 1.00.[28] Gould et al. (1997) consider studies that estimate the effects of psychotherapy on generalized anxiety disorder and report a mean effect size of 0.7 with a 95% confidence interval from 0.57 to 0.83. Smits and Hofmann (2009) also focus on those with generalized anxiety disorders and find an effect size of 0.73 for anxiety measures and an effect size of 0.45 for depression measures (among those with anxiety). Hofmann et al. (2012) review the literature on the effects of cognitive behavioral psychotherapy (CBT) and report that papers tend to find that CBT has "small to moderate" effect sizes for depression and "moderate to large" effect sizes for anxiety.[29] A potential concern with each of these randomized controlled trials is that the treatment environment and patient characteristics may not reflect the typical patient experience. Stewart and Chambless (2009) conduct a meta-analysis of those studies that have estimated effects of psychotherapy on anxiety in *clinical* settings and find effect sizes

27. Fortunately, the hypothesis that different forms of psychotherapy all have the same effect size, commonly referred to as the Dodo Bird Conjecture, is typically not rejected (Wampold et al., 1997). Therefore, our literature search was focused on finding highly cited papers in the medical literature rather than focusing on specific forms of psychotherapy.

28. Cuijpers et al. (2010) argue that studies estimating the effects of psychotherapy on depression are flawed and that a focus on what they deem "high-quality" studies yields a much lower effect size than those reported by earlier meta-analyses such as Churchill et al. (2002) and Wampold et al. (2002), who report effect sizes above 0.5. However, a simple mean of the effect sizes for the "high-quality" studies reported in Cuijpers et al. (2010) that focus on a general adult population (Elkin et al., 1989; Jarrett et al., 1999; DeRubeis et al., 2005; Dimidjian et al., 2006) gives an average effect size above 0.4.

29. In this literature small effect sizes refer to ones from 0.2 to 0.5, moderate effect sizes refer to ones from 0.5 to 0.8, and large effect sizes are those over 0.8.

above 0.8 for almost all forms of anxiety considered.

If we simply average the estimated effect sizes across these literatures, we get an effect size near 0.6 for psychotherapy, which is within the 95 percent confidence interval of all studies (except one) reporting a confidence interval shown in Figure 2. For antidepressants, we get an effect size near 0.3. As discussed below, we use these figures as our baseline effect sizes for any antidepressant use and a complete (i.e., no discontinuation) series of psychotherapy sessions in the structural model.[30] We then discuss deviations from this effect size, as well as heterogeneity in this effect size by mental health status, in Section 6.3.[31]

Treatment improves mental health, which individuals presumably value. Moreover, it is likely that improved mental health affects labor market outcomes in a beneficial way, meaning treatment has important indirect benefits (see, e.g., Butikofer, Cronin, and Skira, 2020; Shapiro, 2020). Table 6 shows the results from ordinary least squares regressions of labor market outcomes on mental health, controlling for gender, age, race, marital status, whether or not one lives in an MSA, region, and education. The results indicate that better mental health is associated with significantly higher amounts of labor supply on both the extensive and intensive margins, and also higher hourly wages. Mental health is clearly endogenous in these regressions, which is addressed by the structural model that follows.

4 Dynamic Model

4.1 Overview

Consider an individual, $i = 1, ..., N$, who seeks to maximize expected lifetime utility in time period $t = 1, .., T$. Each period, the individual receives utility, U_{it}, from consumption of a numeraire good, C_{it}; his or her mental health health status, M_{it}; and employment and treatment decisions, d_{it}^{rce}; where $e = 0, 1, 2$ denotes no employment, part-time employment, or full-time employment, respectively; $c = 0, 1$ denotes psychotherapy (i.e., “couch”) use; and $r = 0, 1$ denotes psychiatric prescription drug (i.e., “Rx”) use.[32] The model is designed

30. According to Kazdin (1999), those discontinuing treatment in RCTs of psychotherapy are generally replaced or excluded from the study; thus, applying these effect sizes to complete treatment episodes only seems appropriate.

31. Again, we also estimate the impact of treatment within the observational MEPS data using an instrumental variables approach. These findings are reported in Appendix A.III.

32. We acknowledge the role that physicians play as advisors, and potential gatekeepers, in treatment choices. Unfortunately, unlike Dickstein (2014) our data do not allow us to separately identify the incentives faced and choices made by patients and physicians. Thus, while we describe in this section an optimization problem solved by an individual, the true data generating process is determined by a joint patient-physician optimization problem, and our estimates for treatment preferences will reflect this.

to capture the key contemporaneous and dynamic tradeoffs associated with treatment and employment alternatives. Regarding employment, key benefits are the receipt of wages, w_{it}^{e}, which allows for greater contemporaneous consumption, and the accumulation of experience, K_{it}, which may increase future wages. The primary cost of employment is reduced utility from lost leisure. Regarding treatment, the key benefit is improved future mental health, which may impact future utility through several labor and non-labor channels. Treatment is costly in that it reduces contemporaneous consumption via treatment prices, p_{it}^{x}, requires a time investment, and may have direct negative effects on utility (e.g., physical discomfort, psychological discomfort due to stigma, etc.). Psychotherapy is distinct in that it may result in a discontinuation, D_{it}, which impacts future mental health differently than non-discontinued visits. The model also allows for search costs in both treatment and employment transitions.[33]

4.2 Model Specification

4.2.1 Preferences

Let vector $\boldsymbol{d_t}$ be comprised of d_t^{rce} $\forall$ $r, c,$ and e, where $d_t^{r'c'e'} = 1$ when alternative (r', c', e') is chosen and zero otherwise. Flow utility from any decision d_t^{rce} can then be expressed as

$$U_t^{rce} = \frac{C_t^{1-\alpha_0} - 1}{1 - \alpha_0} + U(d_t^{rce}, \boldsymbol{d_{t-1}}, M_t, \boldsymbol{X_t}; \boldsymbol{\alpha}) + \mu_k(d_t^{rce}) + \epsilon_t^{rce} \tag{1}$$

where C_t measures numeraire good consumption and $\boldsymbol{X_t}$ measures a variety of exogenous individual-specific observables. We abuse notation in using $\boldsymbol{X_t}$ as a generic vector of control variables that may include different sets of controls in different equations. The function $U(\cdot)$ is linear in parameters $\boldsymbol{\alpha}$ and includes interactions. The function $\mu_k(d_t^{rce})$ captures permanent, unobserved preferences for alternative (r, c, e) among type k individuals, while ϵ_t^{rce} captures any remaining unobserved, idiosyncratic preferences.[34]

4.2.2 Budget Constraint

Total gross household income, GY_t, in period t can be written as

33. The individual i subscript will be suppressed moving forward for notational simplicity. All variables are individual-specific unless otherwise stated.

34. We assume each individual has a permanent, unobserved type, k, which allows for correlation between the unobserved determinants of choices and outcomes in the model. The estimation and identification of these types is discussed in detail in Section 5.2 and 5.3.

$$GY_t = \sum_{e=1}^{2} [d_t^{rce} * w_t^e * h_t^e] + I_t \tag{2}$$

The first term measures the individual's labor income, where w_t^e is wages from employment type e and h_t^e is the corresponding hours worked in period t, the latter of which is held fixed across all individuals of employment type e. I_t measures all other household income sources and is assumed to evolve exogenously.

Numeraire consumption, C_t, is calculated as disposable income minus treatment expenses

$$C_t = D(GY_t, \boldsymbol{X_t}) - p_t^r * d_t^{1ce} - p_t^c(D_t) * d_t^{r1e}. \tag{3}$$

The disposable income function $D(\cdot)$ adjusts gross household income for approximate total tax liability and housing expenses, as well as family size.[35] Note that the price of psychotherapy is allowed to vary by whether discontinuation occurs; thus, contemporaneous utility is affected by the discontinuation draw. We assume that the individual consumes all income in each period due to data constraints; however, the model could easily accommodate a savings decision.

Wages and prices are stochastic and vary over time. Wages in period t for part-time and full-time employment are expressed as

$$log(w_t^e) = F(M_t, K_t, \boldsymbol{X_t}; \boldsymbol{\delta^e}) + \mu_k^{w,e} + \epsilon_t^{w,e} \tag{4}$$

where $F(\cdot)$ is linear in parameters $\boldsymbol{\delta^e}$ and includes interactions (here and elsewhere), $\mu_k^{w,e}$ captures the permanent unobserved wage effects for individuals of type k, and $\epsilon_t^{w,e}$ is an idiosyncratic error.

Out-of-pocket treatment prices are somewhat complicated by the fact that insured individuals often face no out-of-pocket payments for medical care (e.g., see Appendix Section A.I.2). As such, out-of-pocket treatment prices for psychiatric prescription drugs ($x = r$) and psychotherapy ($x = c$) are written using the following latent variable structure

$$\begin{aligned} f_t^{*x} &= \boldsymbol{X_t \eta^x} + \mu_k^{f,x} + \epsilon_t^{f,x} \\ p_t^{*x} &= exp(\boldsymbol{X_t \gamma^x} + \mu_k^{p,x} + \epsilon_t^{p,x}) \end{aligned} \tag{5}$$

35. Because Equation 1 is non-linear in C_t, the marginal utility of treatment varies across the income distribution. We include adjustments for average housing costs and family size in $D(\cdot)$ to account for the fact that both reduce disposable income and, therefore, impact the marginal utility of treatment. The function $D(\cdot)$ is discussed in detail in Appendix Section A.II.1.

where

$$p_t^x = \begin{cases} p_t^{*x} & if\ f_t^{*x} > 0 \\ 0 & if\ f_t^{*x} \leq 0 \end{cases} \tag{6}$$

In other words, latent variable f_t^{*x} greater than zero indicates that the out-of-pocket treatment price p_t^x is greater than zero. As before, $(\mu_k^{f,x}, \mu_k^{p,x})$ and $(\epsilon_t^{f,x}, \epsilon_t^{p,x})$ capture permanent and idiosyncratic unobserved heterogeneity, respectively.[36]

4.2.3 State Transitions

Work experience, psychotherapy discontinuation, and mental health evolve over time as a function of individual employment and treatment decisions. Work experience, K_{t+1}, updates deterministically, beginning with an initial value of zero and increasing by one (one-half) each period that the individual decides to be employed full (part) time.

The probability of discontinuation, D_t, upon visiting a psychotherapist is determined by

$$D_t = F(\boldsymbol{d_{t-1}}, D_{t-1}, \boldsymbol{X_t}; \boldsymbol{\omega}) + \mu_k^D + \epsilon_t^D \tag{7}$$

We model discontinuation as an unforeseen shock at the time the psychotherapy decision is made. A more elaborate model would incorporate the idea that agents choose prior to each session whether or not to continue their course of psychotherapy. We abstract from intra-period decision-making since the critical consideration in modeling discontinuation is that it is unforeseen, but its probability enters agents' expectations. Therefore, it lowers the *ex ante* probability that a course of psychotherapy is completed and benefits accrue to the patient, which in turn may help to explain patient reluctance to use psychotherapy.

Self-reported mental health, M_{t+1}, takes discrete, integer values from 1 (i.e., poor) to 5 (i.e., excellent). Define M_{t+1}^* as a latent, continuous measure of mental health that can be expressed as

36. A more restrictive Tobit framework could be used to capture the occurrence of zero prices in the data; however, our preliminary analysis suggested that the two-part model was more appropriate, as some covariates decrease (increase) the probability of a positive price while increasing (decreasing) the price conditional on it being non-zero.

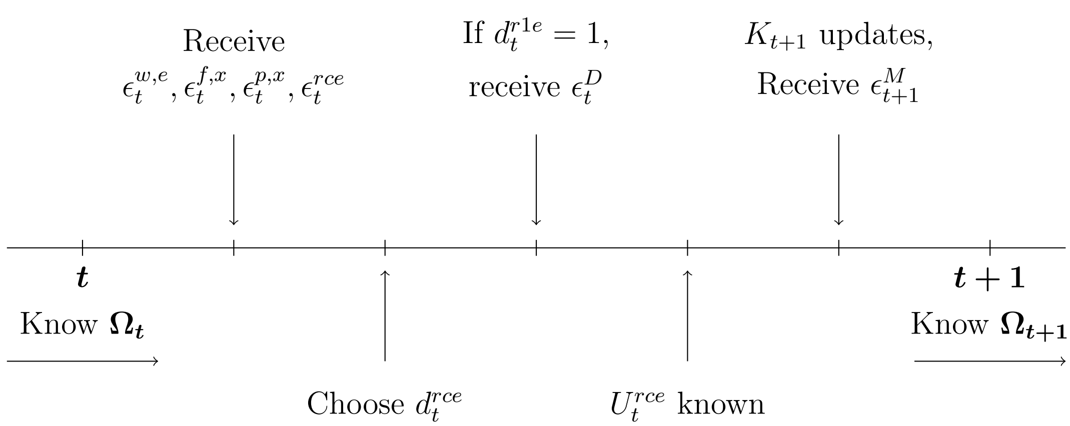
$$M_{t+1}^* = F(M_t, d_t^{rce}, D_t, \boldsymbol{X_t}; \boldsymbol{\nu}) + \mu_k^M + \epsilon_{t+1}^M$$

$$\text{where} \quad M_t = \begin{cases} 5 & if\ \nu_3 < M_t^* \\ 4 & if\ \nu_2 < M_t^* \leq \nu_3 \\ 3 & if\ \nu_1 < M_t^* \leq \nu_2 \\ 2 & if\ 0 < M_t^* \leq \nu_1 \\ 1 & if\ M_t^* \leq 0. \end{cases} \tag{8}$$

4.2.4 Model Timing

With the above described choices, transitions, and payoffs, the timing of the model is as follows: an individual enters period t knowing their state vector $\boldsymbol{\Omega_t} = (M_t, K_t, \boldsymbol{X_t}, D_{t-1}, \boldsymbol{d_{t-1}}, k)$. Upon entry, he or she receives wage ($\epsilon_t^{w,e}$), price ($\epsilon_t^{f,x}, \epsilon_t^{p,x}$), and preference ($\epsilon_t^{rce}$) draws. With this information, the individual makes treatment and employment decisions, d_t^{rce}, to maximize expected lifetime utility. If the individual decides to visit a therapist (i.e., $d_t^{r1e} = 1$), then he or she receives a discontinuation draw, ϵ_t^D, after which contemporaneous utility, U_t^{rce}, is fully determined. Following this, experience, K_{t+1}, is updated and the individual receives a mental health shock, ϵ_{t+1}^M. At this time, all elements of $\boldsymbol{\Omega_{t+1}}$ are known upon entering period $t+1$. Figure 1 summarizes the timing of the model.

Figure 1: Timing



4.2.5 Dynamic Programming Problem

An individual selects alternative (r, c, e) to maximize his or her expected lifetime utility, V^{rce}, which can be written recursively as the sum of contemporaneous utility and expected, discounted future utility (Bellman, 1966). To ease interpretation, we first specify the Bellman equation when psychotherapy is not selected (i.e., $d_t^{r0e} = 1$) as

$$\begin{aligned} V^{r0e}(\boldsymbol{\Omega_t}, w_t^e, p_t^x, \epsilon_t^{r0e}) = U_t^{r0e}(&\boldsymbol{\Omega_t}, w_t^e, p_t^x, \epsilon_t^{r0e}) \\ &+\beta \sum_{m=1}^{5} P(M_{t+1} = m | \boldsymbol{\Omega}_t, d_t^{r0e}) \\ &\left[\int_{R_+^6} EV(\boldsymbol{\Omega_{t+1}}, w_{t+1}, p_{t+1}) g(w_{t+1}) h(p_{t+1}) dw_{t+1} dp_{t+1} \right]. \end{aligned} \tag{9}$$

where β represents an exponential discount factor, $P(M_{t+1} = m)$ is the probability that the individual transitions to mental health state m, and $g(\cdot)$ and $h(\cdot)$ represent wage and price probability density functions. Superscripts e and x have been suppressed on future wages and prices for notational simplicity; however, note that in order to calculate $EV(\cdot)$, which is the expected future value of period t alternative $(r, 0, e)$ assuming optimal future behavior (i.e., the "Emax" function), the individual must integrate over a total of six future shocks, $(\epsilon_{t+1}^{w,1}, \epsilon_{t+1}^{w,2}, \epsilon_{t+1}^{f,r}, \epsilon_{t+1}^{f,c}, \epsilon_{t+1}^{p,r}, \epsilon_{t+1}^{p,c})$, which fully determine future wages and prices. Upon integrating over future wage and price shocks, the future value of an alternative is known only in expectation, as future preference shocks, ϵ_{t+1}^{rce}, are still unknown in period t. Thus, the Emax function is written as

$$EV(\boldsymbol{\Omega_{t+1}}, w_{t+1}, p_{t+1}) = E_t[\max_{rce} V^{rce}(\boldsymbol{\Omega_{t+1}}, w_{t+1}, p_{t+1}, \epsilon_{t+1}^{rce})] \tag{10}$$

The decision to visit a psychotherapist (i.e., $d_t^{r1e} = 1$) complicates the value function because the visit may result in discontinuation. The corresponding value function is expressed as

$$\begin{aligned} V^{r1e}(\boldsymbol{\Omega_t}, w_t^e, p_t^x, \epsilon_t^{r1e}) = \sum_{mm=0}^{1} P(D_t = mm | \boldsymbol{\Omega_t}) \Bigg[&U_t^{r1e}(\boldsymbol{\Omega_t}, w_t^e, p_t^x, \epsilon_t^{r1e}, mm) \\ &+\beta \sum_{m=1}^{5} P(M_{t+1} = m | \boldsymbol{\Omega}_t, d_t^{r1e}, mm) \\ &\left[\int_{R_+^6} EV(\boldsymbol{\Omega_{t+1}}, w_{t+1}, p_{t+1}) g(w_{t+1}) h(p_{t+1}) dw_{t+1} dp_{t+1} \right] \Bigg]. \end{aligned} \tag{11}$$

This structure reflects the fact that discontinuation impacts each piece of the value function; namely, discontinuation affects (i) contemporaneous utility through the psychotherapy price, (ii) mental health transitions through the marginal product of a psychotherapy visit, and (iii) the expected future value of an alternative, via $\boldsymbol{\Omega_{t+1}}$.

Solving individual i's dynamic programming (DP) problem thus begins in the terminal period T, where we assume that the integrated Emax function can be approximated by a non-stochastic, linear in parameters function $T(M_{T+1}, K_{T+1}, d_T^{rce}, age_{T+1}; \boldsymbol{\chi})$. Using Equations 9 and 11, one can then calculate $V^{rce}(\boldsymbol{\Omega_T})$ for every combination (r, c, e). This process is repeated backwards from period T, until $V^{rce}(\boldsymbol{\Omega_t})$ has been determined for every individual $i = 1, ..., N$, in every time period $t = 1, ..., T$, and for every combination (r, c, e).

5 Estimation and Identification

The structural parameters of the dynamic model specified in Section 4 are estimated using the data described in Section 3 and a nested fixed-point algorithm (Rust, 1987). In the inner algorithm, the DP problem described in Section 4.2.5 is solved for a given set of parameters. The outer algorithm uses the solution to calculate a likelihood function value and updates the parameter vector using the Berndt, Hall, Hall, and Hausman (1974) algorithm. In this section, we describe (i) several modeling assumptions made necessary given the features of our data, (ii) permanent unobserved heterogeneity, (iii) parameter identification, and (iv) distributional assumptions that allow us to specify a likelihood function.

5.1 Taking the Model to the Data

We describe the estimation sample in Section 3.3. Within this sample, the average interview period length is 5.4 months; thus, for simplicity, we assume that all decision periods in the model are six months in length, meaning our data allow us to model decisions over four six-month periods. The 6-month period length has several implications for estimation. First, employment-specific hours in Equation 2 are set to 1,100 for full-time workers and 650 for part-time workers, which reflects 25 weeks of 44 and 26 hours worked, respectively.[37] Second, treatment choices reflect the decision to consume any treatment during the six-month time period and prices reflect expenditure levels, the latter of which are calculated by summing over all observed out-of-pocket payments within the period. Discontinuation complicates

37. Anyone in the data working over 37.5 hours per week is categorized as full-time. Among these individuals, the average number of hours is 44, while the average for those working under 37.5 hours per week is 26.

this process for psychotherapy visits; thus, adjustments are made so that p_t^c is interpreted as the price of a successful (i.e., non-discontinuation) period of psychotherapy visits and the expenditure required upon discontinuing is a fraction of p_t^c.[38]

The fact that individuals enter the data at various ages also has implications for the model. Most notably, we do not observe experience over the entire career, which is a key determinant of wages. As such, we condition the wage distributions on the observed wage in the first period of the data and measure experience, K_t, earned since the first period. Initial wage, w_0, can then be added to a long list of endogeneous initial conditions, which are discussed in Section 5.2.

The explicit set of exogenous, non-stochastic control variables that comprise $\boldsymbol{X_t}$ are as follows: initial wage, gender, age, calendar year, lives in an MSA, has public insurance, has private insurance, high school education, college education, nonwhite (race), married, family size, problem child, and household income.

To reduce the computational burden, we estimate our model using a 20% random subsample of the estimation sample.

5.2 Permanent Unobserved Heterogeneity

We assume in estimation that each individual has a permanent, unobserved type, which allows for correlation between the unobserved determinants of choices, outcomes, and transitions in the model. The strategy decomposes all model unobservables into two additively separable components: an i.i.d. serially-uncorrelated random component, ϵ_t, and a persistent component, $\boldsymbol{\mu_k}$, that varies across individuals of $k = 1, \ldots, K$ different types. We assume that the distribution of persistent unobserved heterogeneity can be approximated by a discrete function, which is sometimes referred to as a discrete factor model (DFM) (Heckman and Singer, 1984; Mroz, 1999). Thus, the estimation procedure seeks to determine (i) the number of unobserved types in the population, K; (ii) the share of the population that is described by each type, θ_k for $k = 1, \ldots, K$ where $\sum_{k=1}^{K} \theta_k = 1$; and (iii) the impact that each unobserved type k has on all model choices, outcomes, and transitions, $\boldsymbol{\mu_k}$ for $k = 1, \ldots, K$.

The DFM offers two advantages over a popular alternative, which is to assume a joint

38. To code discontinuation periods, each *individual psychotherapy session* is grouped into a psychotherapy episode, which is then categorized as successful or discontinuation according to the rule described in Section 3.4. If a period contains both successful and discontinuation visits, the period is coded as successful. Regarding expenditure p_t^c, the average number of visits in a successful visit episode is 8.2, while the average number of visits in a discontinuation episode is 1.4; therefore, in order to interpret p_t^c as the required expenditure for a successful psychotherapy episode, we scale observed discontinuation expenditure by (8.2/1.4) in estimation. Then, upon experiencing a discontinuation in the model, the expenditure faced in the budget constraint (i.e., equation 3) is just $p_t^c/(8.2/1.4)$.

parametric distribution (e.g., multivariate normal) over the model's error terms. First, the DFM is more flexible. Mroz (1999), and more recently Guilkey and Lance (2014), uses Monte Carlo simulation in a two-equation, joint MLE setting to show that when the true error distribution is joint normal, DFM estimates are comparable to those derived using the correct distribution. However, when the true error distribution is not normal, the DFM outperforms all other tested estimation methods. Second, the DFM is almost certainly faster than assuming a joint parametric distribution, which typically requires the use of maximum simulated likelihood estimation.

In the following section, we discuss how allowing for permanent unobserved heterogeneity resolves several identification and measurement error challenges in estimation. One such challenge is the endogeneity of initial conditions. Recall, as individuals enter the data at various points in their career, it is unlikely that all initial state variables are exogenous. For example, consider someone who is observed to have poor mental health entering our data. This individual's personal history and particular life circumstances, some of which are outside the scope of our model, likely contributed to that poor health state, and also makes this individual more susceptible to bad mental health shocks moving forward. One reason that we model permanent unobserved heterogeneity is to capture the persistence of mental health shocks in an individual like this, but we must also address the endogeneity of their initial poor mental health.[39] As such, we condition type probabilities, θ_k, on the initial state vector, $\mathbf{\Omega_0}$, which includes all endogenous initial conditions, as well as exogenous variables $\boldsymbol{X_0}$. Thus, in the example above, the fact that this individual entered the data with poor mental health is allowed to influence the probability that they are of an unobserved type, k, that experiences worse mental health shocks. In using this strategy, we assume that all initial conditions are exogenous, conditional on unobserved type k. To our knowledge, this strategy was first used to address endogenous initial conditions in Keane and Wolpin (1997) and was later formalized by Wooldridge (2005).[40]

5.3 Identification

Identification follows standard arguments from Magnac and Thesmar (2002). In a dynamic discrete choice model, one needs state-specific choice probabilities and choice-and-state specific transition probabilities along with normalizations, a fixed discount factor, and distri-

39. Note that several other initial conditions pose similar endogeneity concerns, including initial wages, employment, treatment, education, and household income.

40. We prefer this strategy to modeling all endogenous initial conditions separately (e.g., as in Darden, 2017), as it (i) does not require additional exclusion restrictions to identify the initial conditions and (ii) allows type probabilities to vary by observables, which eases the post-estimation interpretation of types.

butional assumptions on error terms to identify utility parameters. Note, this relates to identification in the rank order sense, the idea being that there is a unique set of parameters that maximize the likelihood function. In the model, agents form expectations based on the transition process, which includes changes to mental health and, thus, to productivity. Given this information, agents choose between treatment alternatives, the values of which are a function of future transitions, but also of current-period payments. This identifies preferences over mental health treatments. Preferences over work are similarly identified from joint work decisions. Differences in treatment decisions across the price distributions, as well as differences in work decisions across the household income and wage distributions, identify the consumption utility parameter. Preferences over illness, which are identified in a dynamic model but not a static one, are identified by the restriction that treatment preferences are not allowed to vary across mental health levels; thus, differences in treatment choices across the mental health distribution identify preferences for good mental health.[41]

Obtaining the correct utility parameters requires that the modeled beliefs about the impacts of treatment are a correct representation of agents' beliefs. One approach would be to estimate the effect of treatment using the mental health transitions in the data, where selection concerns are addressed through distributional assumptions, as well as exclusion restrictions. In Appendix Section A.III.2, we discuss our efforts with this approach. Another approach is to simply use treatment effects established in well-identified settings, such as the clinical trials literature. We use the latter approach in this paper.[42] Still, as in all cases where no belief data is available, rational expectations are a key identifying assumption (Magnac and Thesmar, 2002). Consider an agents' reluctance to use psychotherapy; it could be because they believe it is effective, but have a distaste. Alternatively, agents may think the treatment's productivity is less than what is found in clinical trails and this biased belief affects their choice. Absent belief data, either narrative could explain the same data pattern.

41. The variation that identifies these effects is displayed in Table 3. Note that treatment is decreasing in mental health. The model does not allow this variation to be explained by heterogeneity in treatment preferences across the mental health distribution; rather, conditional on productive treatment, greater treatment in worse health states suggest that individuals dislike being in poor health and are, thus, willing to undertake the various treatment costs in order to improve their mental health.

42. In Section 3.4, we argue that the average standardized impact of prescription drugs and psychotherapy on mental health are approximately 0.3 and 0.6, respectively. Standardized effects for an outcome, Y, are measured as $(Y_1 - Y_0)/SD(Y)$, where Y_1 measures the outcome for the treated group and Y_0 for the untreated group. We can utilize these estimates within our model, despite having a different measure of mental health, by scaling the medical literature estimates by the standard deviation of the latent mental health variable in Equation 8. Note that the standard deviation of M^* is a function of model parameters and, thus, must be calculated at every iteration of the model. As such, for a given iteration of the parameter set $\mathbf{\Theta}_s$, we calculate treatment effects as $\nu_{0,1} = 0.3 * SD(M^*(\mathbf{\Theta}_s))$ and $\nu_{0,2} = 0.6 * SD(M^*(\mathbf{\Theta}_s))$. Furthermore, we assume that discontinuation visits have no impact on mental health in the following period. We explore the robustness of these assumptions in Section 6.3.

Consistent with much of the dynamic structural literature, we impose rational expectations, functionally assuming that agents have the right average treatment effects in mind when making choices. Here, one can imagine that patients (or their doctors) use evidence from the medical literature when making choices. Somewhat reassuringly, we show in Section 6.3 that the main results are not terribly sensitive to using a range of treatment effects from the literature or from our own reduced-form estimates discussed in Appendix Section A.III.1.

Another source of concern is poorly measured mental health. In particular, we are concerned that not all reports of, say, "fair" mental health reflect mental illness, as generally healthy people may offer this report on a particularly bad day. Similarly, perpetually ill individuals may report "very good" mental health on a good day. The problem with these isolated reports is that they do not represent the need to alter one's treatment course. Upon experiencing such a shock, perpetually sick or healthy individuals likely expect to return to the status quo. To address this measurement error, we allow for permanent unobserved heterogeneity in mental health. By identifying some proportion of the population as persistently well or sick, the model no longer needs to rationalize non-use of psychotherapy by people who wouldn't really benefit from it. Allowing for permanent unobserved heterogeneity also addresses a more general concern in dynamic structural models, which is the identification of parameters measuring the impact of one endogenous variable on another. For instance, the impact of period $t-1$ mental health on mental health in period t, or the impact of mental health on wages. As discussed below, permanent unobserved heterogeneity allows us to distinguish true causal relationships from (i) correlation arising via state dependence and (ii) common omitted variables.

Three attributes of the model and data help to identify the permanent unobserved type distribution. The first is repeated individual-level observations. Assume that a subset of the population is persistently healthy and that this persistence cannot be explained by observables. The estimation procedure, then, identifies a type, k', that corresponds to that subset. The larger the subset, the larger the share, $\theta_{k'}$, assigned to that type. The better their mental health, the larger the factor loading, $\mu^M_{k'}$, on that type. Second, as discussed above, unobserved type shares are estimated conditional on endogenous initial conditions. To understand how this affects, say $\partial M_t/\partial M_{t-1}$, again assume that unobserved type k' is persistently healthy. The model allows initial mental health to influence the probability that an individual is of type k'. An individual who is observed to have perfect mental health in each period, including $t=0$, then contributes little to the estimation of $\partial M_t/\partial M_{t-1}$, as their data is best explained by them being type k' with high probability. Third, non-linearities and exclusion restrictions aid in determining whether the relationship between two endogenous variables is causal or due to common unobservables. For example, note that wages and mental health are positively correlated (see Table 3). This relationship could be due to a causal effect of mental health on wages (i.e., $\partial w_t / \partial M_t > 0$) or permanent unobserved heterogeneity (i.e., for unobserved reasons, the people who are most likely to fall into poor mental health may also receive the lowest wage offers). Exclusion restrictions, such as having a problematic child and observed treatment choices, help to distinguish these competing explanations by generating unique variation in mental health that cannot be entirely explained by permanent unobserved heterogeneity. If variation in these exclusion restrictions is also associated with changes in wages, then it suggests a direct relationship between mental health and wages, and the unobserved heterogeneity parameters must adjust accordingly.

Conditional on knowing the number of unobserved types in the population, K, the estimation of $\{\theta_k, \boldsymbol{\mu_k}\}_{k=1}^{K}$ is straightforward, as these parameters are part of the likelihood function described below.[43] Determining K is less straightforward. Mroz (1999) recommends an "upwards-testing approach," where one first estimates all model parameters assuming one unobserved type, $K = 1$, which produces a log-likelihood function value, LLF_1, and a set of maximizing parameters, $\widehat{\boldsymbol{\Theta}}_{\boldsymbol{1}}$. The model is then re-estimated with two unobserved types, $K = 2$, using the previously estimated parameters, $\widehat{\boldsymbol{\Theta}}_{\boldsymbol{1}}$, as starting values, which produces a new log-likelihood function value, LLF_2, and a new set of maximizing parameters, $\widehat{\boldsymbol{\Theta}}_{\boldsymbol{2}}$. A likelihood ratio (LR) test is used to determine whether the additional unobserved type led to a significant improvement in the log-likelihood function. Mroz suggests continuing in this fashion, adding additional types so long as significant improvements are made in the value of the log-likelihood function. Using this strategy, we arrive at four types.

5.4 Likelihood Function

Observed decisions (i.e., d_t^{rce}), stochastic state transitions (i.e., M_t, D_t), and stochastic payoffs (i.e., p_t^e, w_t^x) are partly determined by a set of random variables, $\boldsymbol{\epsilon_t}$, that agents observe, but that we, the econometricians, do not. Constructing the likelihood function requires that we assume to know the distribution from which these unobservables are drawn. We begin by assuming that unobservables impacting discontinuation, ϵ_t^D, and mental health, ϵ_t^M, are drawn from a logistic distribution, making $P(D_t)$ and $P(M_t)$ logit and ordered logit probabilities, respectively. We further assume that log-wage errors are normally distributed, $\epsilon_t^{w,e} \sim N(0, \sigma_{w,e}^2)$. We assume non-zero price errors, $\epsilon_t^{f,x}$, are drawn from a logistic distribution, while log-price errors (conditional on prices being non-zero) are drawn from a normal

43. Identifying $\{\theta_k, \boldsymbol{\mu_k}\}_{k=1}^{K}$ requires several normalizations. First, if $K = 1$, then $\theta_1 = 1$ and $\boldsymbol{\mu_1}$ is not separately identified from the model's constants, so the vector is set to zero. In other words, setting $K = 1$ assumes that the model's error terms are conditionally independent. If $K > 1$, then $\theta_1 = 1 - \sum_{k=2}^{K} \theta_k$ and $\{\boldsymbol{\mu_k}\}_{k=2}^{K}$ are only identified relative to $\boldsymbol{\mu_1}$; thus, we set $\boldsymbol{\mu_1}$ to zero throughout our analysis.

distribution, $\epsilon_t^{p,x} \sim N(0, \sigma_{p,x}^2)$.[44] We assume that the unobservables impacting treatment and employment decisions, ϵ_t^{rce}, are drawn from a Type 1 Extreme Value (T1EV) distribution. This assumption is popular in the DP literature because it yields closed form expressions for both the maximal value function in Equation 10 and choice probabilities; specifically,

$$EV(\boldsymbol{\Omega_{t+1}}, w_{t+1}, p_{t+1}) = \gamma + log\left(\sum_{r=0}^{1}\sum_{c=0}^{1}\sum_{e=0}^{2} exp\Big[\overline{V}^{rce}(\boldsymbol{\Omega_{t+1}}, w_{t+1}, p_{t+1})\Big]\right) \tag{12}$$

and

$$P(d_t^{rce} = 1|\boldsymbol{\Omega_t}, w_t, p_t) = \frac{exp\Big[\overline{V}^{rce}(\boldsymbol{\Omega_t}, w_t, p_t)\Big]}{\sum_{r=0}^{1}\sum_{c=0}^{1}\sum_{e=0}^{2} exp\Big[\overline{V}^{rce}(\boldsymbol{\Omega_t}, w_t, p_t)\Big]} \tag{13}$$

where γ is Euler's constant and $\overline{V}(\cdot)$ is the deterministic part of the value function.

Let the variables $e_t = \{0, 1, 2\}$, $r_t = \{0, 1\}$, and $c_t = \{0, 1\}$ represent observed, period t employment, antidepressant use, and psychotherapy use, respectively. Under the above assumptions, an individual's contribution to the likelihood function, for a given realization of the parameter set $\boldsymbol{\Theta}$, can thus be expressed as

$$\begin{aligned} L_{i,t}(\boldsymbol{\Theta}|\boldsymbol{\Omega_t}) =& g_1(w_t^1|\boldsymbol{\Omega_t})^{\mathbb{1}[e_t=1]} g_2(w_t^2|\boldsymbol{\Omega_t})^{\mathbb{1}[e_t=2]} h_r(p_t^r|\boldsymbol{\Omega_t})^{\mathbb{1}[r_t=1]} h_c(p_t^c|\boldsymbol{\Omega_t})^{\mathbb{1}[c_t=1]} \\ & \prod_{r=0}^{1}\prod_{c=0}^{1}\prod_{e=0}^{2}\Bigg[P(d_t^{rce} = 1|\boldsymbol{\Omega_t}) \prod_{mm=0}^{1}\Big[P(D_t = mm|\boldsymbol{\Omega_t}, d_t^{rce}) \\ & \prod_{m=1}^{5} P(M_t = m|\boldsymbol{\Omega_t}, d_t^{rce}, mm)^{\mathbb{1}[M_t=m]}\Big]^{\mathbb{1}[F_t=mm]}\Bigg]^{\mathbb{1}[r_t=r, c_t=c, e_t=e]}. \end{aligned} \tag{14}$$

The first row measures wage and price contributions to the likelihood function, which exist only if the individual was employed and/or sought treatment. The second row measures the choice and discontinuation contribution.[45] The last row measures the mental health contribution, where the probability of observing health state m is allowed to vary by the

44. Under these assumptions, the price probability density function is as follows, where $\Lambda(\cdot)$ is the CDF of a standard logistic distribution and $\phi(\cdot)$ is the pdf of a standard normal distribution.

$$h_x(p_t^x|\boldsymbol{\Omega_t}) = \Big(1 - \Lambda(\boldsymbol{X_t\eta^x} + \mu_k^{f,x})\Big)^{\mathbb{1}[p_t^x=0]}\left(\Big(\Lambda(\boldsymbol{X_t\eta^x} + \mu_k^{f,x}\Big)\frac{1}{\sigma_{p,x}}\phi\Big(\frac{log(p_t^x) - \boldsymbol{X_t\gamma^x} - \mu_k^{p,x}}{\sigma_{p,x}}\Big)\right)^{\mathbb{1}[p_t^x\neq 0]}$$

45. For simplicity, choice probabilities are written unconditional on wages and prices. In practice, we only observe wages and/or prices when individuals are employed and/or consume treatment; thus, in the absence of employment/treatment, choice probabilities are calculated by integrating over wage and price distributions, $g(\cdot)$ and $h(\cdot)$. When individuals are employed and/or consume treatment, choice probabilities are calculated conditional on observed wages/prices.

observed choice vector, (r_t, c_t, e_t), and whether discontinuation occurs, D_t.

The individual likelihood contribution is conditional on $\boldsymbol{\Omega_t}$, which contains type k, which is unobserved by the econometrician. Thus, construction of the log-likelihood function below requires that $L_{i,t}(\boldsymbol{\Theta}|\boldsymbol{\Omega_t})$ is calculated for each k, then weighted appropriately.

$$\mathscr{L} = \sum_{i=1}^{N} log\left(\sum_{k=1}^{K} \theta_k(\boldsymbol{\Omega_0}) \prod_{t=1}^{T} L_{i,t}(\boldsymbol{\Theta}|\boldsymbol{\Omega_t})\right) \tag{15}$$

6 Results and Counterfactuals

6.1 Parameter Estimates and Model Fit

Parameter estimates and model fit tables can be found in Appendix Section A.V. We provide a brief overview here. Model parameters are presented in Tables A.X - A.XVI and contain estimates from the model with just one unobserved type (i.e., K=1) and four unobserved types (i.e., K=4). Table A.X contains unobserved heterogeneity parameters for the four type model. Note that Type 1 marginal effects are normalized to zero, which complicates the interpretation of some of the model's parameters. For example, $\alpha_{1,0}$ measures the disutility of psychotherapy for Type 1 individuals, while the disutility of psychotherapy for Type 2 individuals is $\alpha_{1,0} + \mu_k^{U,0}$. The interpretation of the remaining preference parameters and all model constants are similarly affected.

Utility function parameters are presented in Table A.XI. Note that the CRRA parameter, α_0, which is identified according to the discussion in Section 5.3, is held fixed in estimation. While we have estimated this parameter under several specifications of the model, including multiple unobserved heterogeneity types, estimation has consistently yielded very large estimates. For example, our baseline model with one unobserved type estimates α_0 at approximately 4. The estimate produces a utility-consumption profile that is essentially flat, which would suggest that individuals do not consider wage offers in employment decisions, nor prices in treatment decisions.[46] While large estimates of this parameter are not unheard of in the literature (see, for example De Nardi, French, and Jones, 2016), estimates between 0.8 and 1.1 are far more common (Hurd, 1989; Rust and Phelan, 1997; Blau and Gilleskie, 2008; Cronin, 2019). In Appendix Section A.IV, we detail the underlying sources of variation that contribute to our large estimate and argue that the most conservative path forward is to fix the CRRA parameter to 0.95.

46. In early iterations, we also explored allowing consumption preferences to vary by current mental health status, but ultimately removed this feature as it revealed little heterogeneity.

Parameter signs meet *apriori* expectations. We briefly describe some of these findings. First, regarding medical treatments: (i) holding future mental health constant, individuals derive disutility from antidepressants, and even greater disutility from psychotherapy; (ii) *past* psychotherapy use lowers the disutility of *current* psychotherapy use, indicating search costs; (iii) *past* drug use lowers the disutility of *current* psychotherapy use, indicating dynamic complementarities;[47] (iv) employed individuals derive greater disutility from treatment and this effect is largest for full-time employees, indicating that both treatments involve relevant time costs; and (v) employment-by-psychotherapy disutility is greater than employment-by-drug disutility, indicating psychotherapy has greater time costs than drugs. Second, regarding employment: (i) holding earnings constant, individuals derive disutility from part-time employment, but greater disutility from full-time employment, indicating the value of leisure; (ii) past employment lessens employment disutility, indicating switching costs; and (iii) the disutility from employment increases as mental health worsens.[48] Third, regarding mental health: (i) individuals derive disutility from poor mental health; (ii) poor mental health in the past lowers current mental health; and (iii) wages decrease as mental health worsens, though these effects are very small.

To assess the model's ability to explain unique features of the data, we use the model to simulate new datasets and compare key moments of the observed and simulated data. The simulated data are constructed by sampling from the joint error distribution, permanent unobserved heterogeneity distribution, and parameter covariance matrix 50 times for each individual, then forward simulating. Appendix Table A.XVII shows that the model matches the data on most key moments, including mean treatment and employment, as well as mental health, wage, and price distributions. Appendix Figures A.II-A.V show that we also match these key moments across genders and across the age distribution. Note that for all of these comparisons, the one unobserved type model does just as well as the four type model.

Table A.IV highlights a key challenge of our analysis. The table suggests that those who received treatment in period t have worse mental health in period $t+1$ than those not receiving treatment, conditional on mental health in period t. Moreover, we use treatment effect

47. The same effects exist for current drug use. That the treatments would act as dynamic complementarities is quite sensible in this setting. In most cases, in order to acquire either treatment patients must first reveal depressive symptoms to their general practitioner (GP), who serves as gatekeeper. The GP may prescribe antidepressants or refer the patient to a specialist, who can administer/prescribe either type of treatment. In this setting, using one type of treatment both reveals a willingness to treat their symptoms and opens a dialogue with the gatekeeper, each of which facilitates greater use of alternative treatments in the future.

48. Quidt and Haushofer (2016) posit that depression acts as an exogenous shock to an agent's beliefs about the returns to effort; whereby pessimistic beliefs may reduce expected productivity for a fixed amount of effort or the expected cost of effort, ultimately reducing labor supply. The latter interpretation may explain why we find the disutility from employment to be increasing as mental health worsens.

estimates from the medical literature that suggest that both types of medical treatments are productive. With these treatment effects, it is difficult for our model to explain the data pattern in Table A.IV, i.e., persistently poor mental health, despite the use of treatment. The implications of this dichotomy can be seen in Table A.XVII. For both types of treatment, when there is only one unobserved type, the model under-predicts treatment when individuals are in the lowest two mental health states. The treatment effects suggest that these individuals' health should improve, while the data suggests that their health does not improve; thus, the model cannot rationalize the treatment decision.[49] The four unobserved type model offers an improvement. By allowing permanent unobserved heterogeneity, the model rationalizes the conflict with an unobserved type that (i) received consistently poor mental health shocks and (ii) has strong preferences for treatment.

Finally, consider the four permanent unobserved types revealed by the model. In each of the simulations described above, individuals receive a permanent unobserved type draw according the estimated posterior probability distribution. The initial conditions $\boldsymbol{\Omega_0}$ and simulated outcomes of each type are described in Table 7. Each unobserved type can be characterized as follows:[50]

Type 1: These individuals represent roughly 5% of the population. They have persistently poor mental health and consume substantially more treatment than the other types. These individuals have an incredibly low employment rate, earn the lowest full-time wages conditional on employment, and the second lowest part-time wages. Given these characteristics, it is unsurprising that these individuals enter the data in poor health with high rates of unemployment and high rates of treatment. In terms of demographics, these individuals are relatively likely to be women and nonwhite; are unlikely to live in an MSA, to be married, or to have a college degree; are very likely to be publicly insured; and have the lowest household income.

Type 2: These individuals represent 16% of the population. Their mental health is notably

49. In Section 5.3 above, we explain how measurement error in mental health forces the model to rationalize a lack of treatment use when individuals may simply be having a bad day. We also explain how allowing for permanent unobserved heterogeneity corrects this problem. The implication for parameter estimates can be see in Table A.XI. Note that the marginal disutility of poor mental health (i.e., α_5, α_6) increases with unobserved heterogeneity. In other words, when mental health is mismeasured, the model underestimates the disutility associated with poor mental health in order to rationalize the lack of treatment when individuals face intermittent illness. This bias produces the under-prediction of treatment while ill in column 1 of Table A.XVII.

50. Type labels have no inherent ordinal meaning (i.e., 1, 2, 3, and 4 could just as easily be red, orange, blue, and yellow). As such, to ease interpretation, we use numerical labels and order types in a way that is consistent with the unobserved type's persistent mental health state—Type 1 having the worst mental health and Type 4 having the best.

better than Type 1, but worse than Types 3 and 4. Despite this, the group consumes less treatment than Type 3 individuals. Type 2 individuals also have employment rates that are somewhere between the very low rates of Type 1 individuals, and the higher rates of Types 3 and 4. Demographically, Type 2 individuals again find themselves somewhere between Types 1 and 3, except the high proportion of females and household income that exceeds even that of Type 3 individuals.

Type 3: These individuals represent 74% of the population. They have persistently good mental health and consume the second most treatment of the four types, which is a small fraction of Type 1 consumption. These individuals have the highest employment and wage rate. Endogenous initial conditions match this description. In terms of demographics, these individuals are relatively unlikely to be women and nonwhite and are the most (least) likely to have private (public) insurance.

Type 4: These individuals represent 5% of the population. Their outcomes are like Type 3 individuals in all ways except they consume less treatment and are less likely to work at their lower wages. Demographically, they have slightly larger household incomes and more education than Type 3 individuals.[51]

6.2 Counterfactuals

In this section, we use the estimated model to perform several counterfactuals. Our first counterfactual assesses the personal and economic value of improvements to mental health. In order to place these findings within the literature, we use our nationally representative estimation sample as the basis for analysis. After establishing the potential for economic gains, our second and third counterfactuals focus on the decision to improve one's mental health via psychotherapy. In light of the starkly different choices and outcomes observed among our four permanent unobserved types, these counterfactuals aim to highlight differences in their responses. To do so, we begin by returning to our full data file, Sample C described in Table A.I. Using the estimated posterior unobserved type probabilities, we first assign an unobserved type to each individual, based on their initial state vector. We then define a new sample of 10,000 individuals that includes 2,500 randomly selected individuals of each type. We conduct these counterfactuals using this sample in order to avoid statistical

51. Note that Type 4 individuals are incredibly unlikely to visit a therapist. As a result, psychotherapy-type specific price and discontinuation parameters are not identified for these individuals. Given the similarities between Types 3 and 4, we set these parameters at Type 3 levels, which has virtually no impact on the likelihood function, but is relevant in counterfactual analysis, where individuals are, at times, assigned to treatment.

error in simulated differences due to small samples; in particular, our concern is with Types 1 and 4, which are under-represented in the population. Simulations are conducted in the manner described in the previous section.

6.2.1 The Value of Mental Health Improvements

We begin by measuring the economic value of improvements to mental health. Specifically, we simulate behaviors and outcomes over a two-year period while assuming that an individual's mental health cannot fall below the baseline sample mean. To fix ideas, this would be like inventing a totally costless treatment (i.e., no financial cost, no time cost, etc.) guaranteed to return an individual's mental health to the population average; as such, those below the average would always take the treatment and those above never would. With the counterfactual simulation in hand, we then return to the baseline model and calculate consumer willingness to pay for the hypothetical treatment, which accounts for the aggregate utility and labor market effects yielded by the mental health improvements. The experiment serves two purposes. First, it provides a sanity check for our model, as other researchers have also estimated the impact that mental illness has on the labor market at large. Second, because mental health is *the* mechanism through which treatment can yield welfare improvements, this counterfactual establishes the potential gains of treatment inducing policy.

The counterfactual environment produces mental health improvements for nearly everyone, ranging from 78 percent on average for Type 1 individuals to 10 percent for Types 3 and 4. The increase in mental health results in full-time (part-time) employment increases of 2.8 (0.6) percent, while full-time (part-time) wages actually fall by 0.2 (0.7) percent;[52] though these aggregate changes mask important heterogeneity. For example, full-time employment increases by 103 and 18 percent for (low-employment, poor-health) Types 1 and 2 individuals, respectively, but just one and four percent for (high-employment, good-health) Types 3 and 4 individuals. A full set of results is available upon request.

In Figure 4 we summarize the average economic gains produced by the hypothetical treatment, as well as individual willingness to pay for the treatment, by unobserved heterogeneity type. The left-most (black) bar measures willingness to pay for the treatment, holding household income constant across types, while the middle (speckled) bar allows household income to vary.[53] We make both calculations to highlight that while the sickest Type 1 individuals

52. Ceteris paribus increases in mental health yield small wage gains (see parameters δ_2^e and δ_3^e in Appendix Table A.XIV). This counterfactual lowers wages slightly due to selection into employment, i.e., those selecting into employment in light of their improved mental health are low wage earners.

53. In both instances, employment decisions are simulated and influenced by prospective wages; however, in the former willingness to pay calculation, we assume individuals have an *ex-post* total annual disposable

(who have the most to gain from the treatment) are willing to pay the most, their valuation is somewhat constrained by the fact that they have little disposable income. The right-most (grey) bar measures the average earning gains for individuals of each type. The figure clearly shows that (i) willingness to pay falls as baseline mental health improves, (ii) labor market gains only account for a fraction of the total gains produced by improved mental health, and (iii) the share of the total gains attributable to the labor market is falling in baseline employment.

With the obvious caveat that all labor market changes reflect a partial equilibrium response, we can use the model predictions and supplemental data to calculate the total economic and welfare cost of below average mental health in the U.S. For example, the U.S. had approximately 101.7 million residents between the ages of 26 and 55 years old in 2002 (FRED). Our findings suggest that in this year, the U.S. population was willing to pay just over \$103 billion (in 2002 dollars) to avoid below average mental health and that just 15 percent (\$16 billion) of this value is attributable to labor market gains.[54] This valuation contrasts with Kessler et al. (2008), who argue that serious mental illness cost the U.S. \$193 billion in lost earnings in 2002, a figure commonly cited by the American Psychological Association. The difference in our findings is not surprising, as our study accounts for the endogeneity of mental illness.[55] To put these figures in perspective, Ricci and Chee (2005) estimate that obese workers cost the U.S. \$42 billion in 2002 in lost productive time annually.

6.2.2 Assignment to Psychotherapy

We have established that mental health has direct utility and indirect earnings benefits and relied on myriad findings showing that psychotherapy is the most effective mental health treatment. Our second counterfactual assigns all individuals to psychotherapy in the first period of the model. We assume perfect compliance, but allow for discontinuation. The counterfactual is meant to mimic a randomized control trial that assigns a treatment group to psychotherapy, but cannot guarantee that follow-up visits occur. We begin our analy-

household income of \$40,000.

54. The year 2002 was chosen for ease of comparison across papers. Adjusted for inflation and population growth, the total value of mental health improvement in 2018 is \$148 billion, \$22 billion of which is attributable to labor market gains. To generate the \$103 billion figure, willingness-to-pay is calculated for each individual from the baseline simulation (i.e., not fixing income), then averaged across individuals of each type, as in the middle (speckled) bar in Figure 4. We then weight each of these values by the share of the population corresponding to the type (e.g., 74 percent of the population is of Type 3) and multiply by 101.7 million. Similar steps are taken to calculate aggregate labor market gains.

55. Kessler et al. (2008) simply calculate the difference in earnings and wages for those with serious mental illnesses vs. well individuals, while controlling for age, gender, race, census region, and urbanicity. When additional controls for education, marital status, and household size are added, the figure is reduced to \$144 billion dollars.

sis by calculating the average increase in mental health over the next three periods, which represents approximately a year and a half. For the sickest Types 1 and 2, which comprise roughly 20% of the population, mental health increases by an average of 16.2 and 5.9 percent, respectively. For the healthiest Types 3 and 4, which comprise roughly 80% of the population, mental health increases by an average of 4.2 and 3.6 percent, respectively. This is a fairly unsurprising conclusion, but it is also an important point to make. Our model of heterogeneous types highlights that even when we assume that treatment effects are constant in the population, a large subset of the population benefits little from treatment because their mental health is quite high without it.[56]

We further explore the response to assigned treatment for the two sickest types in Table 8.[57] For Type 1 individuals, we find that a year and a half after assignment to treatment (i.e., column labeled $t = 4$), women (men) are 59.9 (81.8) percent more likely to go to psychotherapy, which is in part due to the alleviation of search costs captured by $\alpha_{1,1}$. Moreover, the dynamic complementarities between psychotherapy and antidepressants produces a 30.7 (50.2) percent increase in female (male) antidepressant use over the same time interval. Both treatment increases are economically meaningful, yielding a 13.1 (13.3) percent increase in mental health. These findings are qualitatively similar to Baranov et al. (2019), who find that women suffering postpartum depression who are randomly assigned to psychotherapy exhibit mental health improvements, but that these improvements diminish over time.

Our finding of positive spill-overs of psychotherapy to other choices (e.g., antidepressant use) but not secondary outcomes (e.g., wages) is also consistent with Baranov et al. (2019), who find that psychotherapy significantly increased mothers' financial empowerment and parental investments, but had no impact on child outcomes. An advantage of our structural model is that it enables us to explore the mechanisms behind these changes. Most notably, despite the increase in mental health that follows assignment for Type 1 individuals, we do not observe an increase in wages as (i) our model suggests that the impact of mental health on wages is very small (see δ_2^e and δ_3^e in Table A.XIV) and (ii) few Type 1 individuals work. Moreover, we observe economically inconsequential changes in employment for these individuals because, while employment preferences are increasing in mental health, they are decreasing in treatment; thus, the model suggests that any positive employment effects for

56. Note that the mechanism here is somewhat subtle. By assigning psychotherapy in the first period, we lower the search cost for both types of treatment in the following period. Despite these lower search costs, few Type 3 and 4 individuals choose to go to treatment, because their mental health is already quite good, meaning future mental health doesn't improve above baseline.

57. We present results for healthy Types 3 and 4 in Appendix Table A.XVIII. As the aggregate mental health effects for these types is quite small, it should come as no surprise that the dynamic treatment response is also quite minor and there are virtually no long-run employment effects.

Type 1 individuals produced by better mental health are offset by the additional time cost of treatment that generates these mental health improvements.

Type 2 individuals, despite having less than perfect mental health, are incredibly reluctant to use psychotherapy. Just a year after assignment to psychotherapy (i.e., column labeled $t = 3$), fewer than 1 percent are still using it. As a result, this group experiences much smaller improvements in their mental health and these improvements are driven mostly by (i) increases in antidepressant use, which they have stronger preferences for, and (ii) the dynamic effects of the initial improvement in mental health that was caused by psychotherapy assignment.

Assignment to psychotherapy can be viewed as a very strong one-time public policy. For Type 1 individuals, i.e., the sickest 5 percent of the population, this policy produced long-run mental health improvements, but only by generating post-assignment increases in mental health treatment. For moderately depressed Type 2 individuals, the reduction of future treatment search costs produced by assignment was not enough to spawn sustained increases in future treatment. A clear implication of these findings is that more sustained interventions are needed to reach individuals not dealing with severe mental health conditions. Moreover, more realistic policy interventions must be considered for those with more serious mental health conditions. We explore such interventions in the next subsection.

6.2.3 Lower Costs of Psychotherapy

Our third set of counterfactuals explores several potential policies designed to reduce the costs of using psychotherapy. We again focus on how our four unobserved types differ in their response. Results for the sickest Types, 1 and 2, are presented in Table 9. Results for the healthier Types, 3 and 4, are again left to the Appendix (Table A.XIX), as the previous section revealed that these individuals stand to benefit little from more treatment. The first column of these tables reports baseline sample means for the simulated data, averaged over the four interview periods. For each policy, we then present the corresponding sample mean and percentage change from baseline.

The first policy that we consider eliminates the financial cost associated with psychotherapy. Note that over the past three decades, several major US policies have attempted to encourage mental health treatment by forcing insurers to share in the monetary cost of treatment, effectively lowering the out-of-pocket price for individuals.[58] Table A.II provides some

58. Examples include state-level mental health parity laws passed throughout the 1990s and early 2000s; the (federal) Mental Health Parity Act of 1996 and Mental Health Parity and Addiction Equity Act of 2008; and the Patient Protection and Affordable Care Act of 2010, which made mental health one of 10 essential

evidence that these policies may have been effective at reducing costs as, in our data, a large share of the sample receives psychotherapy with no out-of-pocket cost and the share of the total cost of care paid out of pocket has fallen over time. Table 9 suggests that Types 1 and 2 are essentially unaffected by the policy change, as psychotherapy increases by roughly 1.5 percent.[59] The finding is somewhat surprising, particularly for Type 1 individuals, who (i) have the lowest earnings (labor and household) of the four types and (ii) are the most willing to use treatment. Low prices are likely a contributing factor to this finding. According to Table A.XVII, Type 1 individuals receive almost 70% of their psychotherapy for zero out-of-pocket cost, which is partly explained by the high rate of public insurance among these individuals.

The second policy that we consider eliminates the possibility of discontinuation. In other words, upon visiting a therapist, the policy guarantees that the patient completes a full eight sessions and, thus, reaps the productive benefits of psychotherapy.[60] This policy has a large impact on psychotherapy use for Type 1 individuals, increasing average use from 25 (14) to 55 (37) percent for women (men). This increase, plus the corresponding increase in antidepressant use, increases mental health by 18 (14) percent for women (men) on average. Much like the findings in Section 6.2.2, the policy yields no employment benefit, as the employment gains from improved mental health are offset by the time-cost of treatment. For Types 2-4, the removal of discontinuation also produces a large percentage increase in psychotherapy use (roughly 70 percent); however, baseline psychotherapy levels are so low that this large percentage increase is not economically meaningful, e.g., average mental health increases by well under 1 percent for each group.

Our third policy experiment eliminates the employment/time cost of psychotherapy. In practice, we simulate the model with $\alpha_{1,3} = \alpha_{1,4} = 0$. The counterfactual is meant to explore the value of bringing psychotherapists into the work-place and allowing employees to visit a therapist during work hours. Similar employee benefits are already provided by some large employers (McLeod, 2001). Both Type 1 and Type 2 individuals are unlikely to work; thus, it is unsurprising that Table 9 reveals little change in psychotherapy use in response to this

health benefits all individual and small-group insurance plans must cover.

59. We see similar results for Types 3 and 4 in Table A.XIX. In relation to this finding, recall that the CRRA parameter was selected in a way that would make individuals *more* sensitive to price changes than the data suggest. As such, this finding provides some confidence that fixing the CRRA parameter has a minimal impact on our findings.

60. Here, we approximate the benefit of policies designed with the intent of reducing discontinuation. Such policies could, for example, promote the medically-proven benefits of completing a full psychotherapy course (Cahill et al., 2003). Alternative policies could use machine learning to increase the likelihood that patients find a therapist best-suited for their needs and preferences. The use of machine learning to improve patient outcomes has received some attention in the psychology literature (see, for example, Imel et al. (2017)).

policy.[61] Reducing employment costs for Type 3 and Type 4 individuals produces a stronger psychotherapy response, but again, the level change in treatment is unremarkable, meaning overall employment and mental health are unaffected.

Finally, our fourth experiment implements all three policies, i.e., eliminating the financial, discontinuation, and the employment/time cost of psychotherapy. The result of this experiment is unsurprising given the findings above, but illustrates an important point: for 95% of the population, a wildly ambitious, expensive, and likely unrealistic effort to encourage use of psychotherapy would have virtually no impact on aggregate mental health and employment. This includes Type 2 individuals, who may be described as having moderate mental health. The remaining 5% of the population, who we identify as persistently ill Type 1 individuals, would realize significant mental health improvements, which would generate small improvements in employment among this group. As was previously shown, virtually all of this benefit is produced by the elimination of psychotherapy discontinuation.

These results show that factors widely viewed as critical barriers to psychotherapy use, such as time and monetary costs, explain little patient reluctance to use the treatment. Moreover, even when psychotherapy is increased, via incentives or counterfactual assignment to treatment, the resulting improvement in mental health does not yield increases in employment or wages. Independent of treatment, our first counterfactual showed that when mental health was increased artificially, employment increased as well. Though not discussed above, artificial mental health improvements produced the largest employment gains for several low employment subgroups. For example, full-time (part-time) employment increased by 3.8 (1.0) percent among women. For Type 2 individuals, full-time (part-time) employment increases by 18.3 (7.1) percent, on a base of 8.4 (9.5) percent. Recall, these individuals, who represent 15 percent of the population, exemplify the take-up issue that challenges policy makers. Type 2 individuals have persistently moderate mental health and relatively low rates of employment at baseline; however, policy does not improve their labor market outcomes due to a lack of take-up, despite the fact that mental health improvements in this population can yield meaningful labor market gains. This brings us back to the critical question: *Could new approaches to policy improve psychotherapy take-up?*

Above, we show that policies aimed at reducing discontinuation rates may be effective at increasing psychotherapy use, though gains were found to be concentrated among the sickest subset of the population, where treatment rates are already high. Policy makers may also consider addressing the stigma of mental health treatment. Such anti-stigma programs are discussed by Corrigan (2004). Unfortunately, measuring stigma is challenging; thus, as it

61. Type 1 individuals seem to increase their employment by roughly 10 percent; however, base employment is so low that this increase is quite small.

relates to our model, the impact of stigma on treatment decisions is unobserved and therefore one of the many factors contributing to the disutility of treatment. In our final set of counterfactuals, we consider the potential impact of anti-stigma programs by reducing the disutility associated with psychotherapy. Table 10 shows the change in treatment for disutility reductions between 2.5 and 30 percent.[62] While it is difficult to know how a particular antistigma policy maps to a specific percentage reduction in disutility, two things are made clear by this simulation. First, seemingly modest reductions in treatment disutility produce large increases in treatment use. For example, a 20 percent reduction in the disutility of psychotherapy more than doubles baseline psychotherapy use to 4.5%, which is the same aggregate take-up rate induced by the wildly impractical "Policy 4" that makes psychotherapy free, removes the possibility of discontinuation, *and* removes the time/employment cost of psychotherapy (see Tables 9 and A.XIX). Second, the increase in treatment is not limited to Type 1 individuals. With a 20% reduction in psychotherapy disutility, Type 2-4 individuals increase their psychotherapy use by more than 200 percent of their respective baselines (reported in Table 7); increases that are notably larger than those induced by Policy 4.

6.3 Robustness and Limitations

Modeling treatment choices requires many decisions. We have experimented, estimating models with a number of alternative assumptions. In general, our results remain robust to these assumptions. In particular, all models yield four unobserved types that can be characterized in a way that is similar to our preferred model, especially as it pertains to mental health and psychotherapy use, e.g., Type 1 is the sickest and is relatively willing to use psychotherapy, while Type 2 is moderately ill, though unwilling to use the treatment. We discuss a subset of alternative assumptions in more detail here.[63]

First, we reestimate the model allowing psychotherapy discontinuation to have some productive health effects; namely, as the average number of visits in a completed episode is 8.2, while the average number of visits in a discontinuation episode is 1.4, we scale the effect size for discontinuation episodes by 1.4/8.2.[64] Parameter estimates are virtually identical.

62. To account for heterogeneity in psychotherapy preferences, we multiply all psychotherapy utility parameters (i.e., main and interaction effects), excluding treatment-employment interactions, by 1-x, where x ranges from zero to 0.3.

63. Given the computational cost of reestimating the model, for all of the robustness tests shown we start with four unobserved types, beginning from the parameters recovered from the preferred model. We only add a fifth type if it yields a significant improvement in the likelihood function.

64. While Cahill et al. (2003) and Hansen, Lambert, and Forman (2002) highlight the benefits of completing a full course of psychotherapy sessions, they cannot rule out the possibility that effect sizes are simply proportional to the total number of visits, i.e., that discontinuation episodes are effective, but just less so

Second, there is some concern that persistently healthy individuals never consider mental health treatment and, thus, our model overstates distaste for treatment. To address this concern, we reestimate the model while excluding individuals who report "excellent" mental health in every period (12.5 percent of the sample). Parameter estimates change in predictable ways. For example, lagged mental health is found to have less of an impact on current mental health. Importantly, the characterization of the four unobserved types is the same, though the share of the population that is the healthy Type 3 falls by 1.5 percentage points and the share that is less healthy (i.e., Types 1 and 2) increases proportionally. Interestingly, as many of the "excellent" health group choose not to work, removing them from the sample leads to a stronger, negative relationship between mental health and employment preferences. Third, in an effort to validate the chosen interpretation of our results, i.e., that our results relate mainly to depressive conditions and related treatment, we reestimate the model while excluding those reporting a non-DAS mental health condition (about 2 percent of the sample), as described in Section 3.2. Predictably, the share of the population that is Type 1 falls by a small amount (0.7 percentage points), but otherwise, results are very similar.

Finally, we explore robustness to the average treatment effects that we take from the medical literature; in particular, we reestimate the model while (i) halving our preferred treatment effects, (ii) increasing our preferred treatment effects by 50 percent,[65] and (iii) allowing the antidepressant treatment effects to vary by lagged mental health status. The latter is motivated by the meta-analysis of Fournier et al. (2010), which finds that the effectiveness of antidepressants is increasing in illness severity, and Elkin et al. (1989), which finds that psychotherapy is no more effective than antidepressants for severely depressed patients.[66] These alternative specifications have a notable impact on the disutility of mental illness, but little else. When treatment effects are small (large), the disutility of poor mental health increases (decreases).[67] These differences reflect the key empirical challenge discussed

than completed episodes, because the former contain fewer visits.

65. These effects are close to the treatment effects that we estimate in Appendix Section A.III.1, using the observational MEPS data and 2SLS specification.

66. We can find no consistent evidence to suggest that psychotherapy effectiveness is either increasing or decreasing in illness severity. Fournier et al. (2010) reports effect sizes of 0.11 for mild to moderate depression, 0.17 for severe depression, and 0.47 for very severe depression; the latter has a 95 percent confidence interval from 0.22 to 0.71, making it somewhat consistent with Elkin et al. (1989). In our robustness analysis, we assume that those entering a period with self-reported mental health of "poor" are, as Fournier et al. (2010) describes, "very severely depressed", meaning antidepressants have an effect size of 0.47. Similarly, we assume treatment effects for those with fair, good, very good, and excellent mental health are 0.17, 0.11, 0.09, and 0.09, respectively.

67. When antidepressant treatment effects are heterogeneous, results look similar to when treatment effects are halved. This is because (i) antidepressants are much more popular than psychotherapy and (ii) most individuals are in relatively good mental health, meaning the alternative specification represents a decline

in Section 6.1. Namely, the raw data suggest that those with the worst mental health are the most likely to consume treatment (see Table 3), yet poor mental health is persistent, even with treatment (see Appendix Table A.XVII). This pattern is difficult to rationalize with positive treatment effects; thus, when large treatment effects are imposed on the model, persistence in poor mental health observed in the data is rationalized with low marginal utility from better mental health, producing a weaker mental health treatment gradient. Lowering treatment productivity makes it easier for the model to match persistence in poor mental health *and* high treatment use among the sick. The result is stronger preferences for mental health, which leads to higher treatment levels when sick.

Several of our assumptions cannot be tested, so our results should be interpreted with these assumptions in mind. For example, as discussed in Section 5.3, we assume that individuals have rational expectations, which implies that when making treatment decisions, they understand average treatment effects as reported in the medical literature and act accordingly. A possibility is that individuals make treatment decisions with incorrect expectations, which would bias our estimates. This hypothesis could be tested with subjective data on expected treatment effects, which we leave to future work. We also remind readers that all simulations are conducted in a partial equilibrium setting. This limitation is most notable in Section 6.2.1, where we conceive of a new medical treatment that has strong employment effects. Clearly, both medical care and labor markets are likely to respond to this counterfactual. Finally, we are careful not to simulate long-run effects. Our model is estimated using just two years of data and the unobserved types revealed in estimation are strong determinants of mental health, treatment, and labor force participation. It is certainly possible that these types are more flexible over a longer time horizon.

7 Conclusion

A variety of literatures, including economics (Baranov et al., 2019), show that randomly assigning psychotherapy improves mental health. Yet, psychotherapy is rarely used in practice. To understand why, we estimate a structural model of mental health treatment choices which we use to assess counterfactual policies that remove purported barriers to psychotherapy. Model estimates provide evidence that mental health is valuable and that roughly 20% of the population would benefit from psychotherapy. Yet, removing barriers to psychotherapy does little to increase use. An exception is that lowering the likelihood of early discontinuations of a full course of psychotherapy, which occurs frequently and merits further

in antidepressant effectiveness.

attention, increases usage among the 5% of the population with persistently "fair" or "poor" mental health. Thus, while the benefits of psychotherapy are increasingly indisputable, these benefits are difficult to leverage since people are unwilling to engage in the treatment.

Moreover, a frequently endorsed narrative is that policies designed to encourage mental health treatment are likely to "pay for themselves" (Laynard et al., 2007). Such arguments typically rely on the premises that treatment has been shown to be clinically effective and individuals with mental health issues are (i) unlikely to be employed and (ii) have large medical costs when untreated. Easing access to treatment would then necessarily increase treatment use, reducing medical costs and increasing employment (and thus tax revenue) in a way that compensates for any new government expenditures. Our analysis, which allows for rich individual-level heterogeneity, suggests that this is unlikely to be the case. Our counterfactuals show that while the sickest individuals are the most responsive to policy, they also have strong preferences for leisure and low earning potential; thus, when policy yields mental health improvements for this group, there is no earnings gain. Moreover, those in our sample with higher earning potential also have consistently higher rates of mental health, meaning they benefit little from additional treatment. As such, when treatment is incentivized, they simply do not respond.

Improving population mental health thus requires that we look beyond commonly suggested impediments, such as time and monetary costs. It is possible that individuals simply dislike psychotherapy and that the utility costs we estimate should be taken at face value. Stigma, i.e., that individuals feel ashamed that they need professional help to process their emotions, may explain some of this distaste. There is some evidence that increased information about mental health can work to overcome stigma and encourage people to seek effective treatment (Corrigan, 2004). Another possibility is that biases in beliefs about the effectiveness of psychotherapy drive use patterns. For example, individuals may find that taking a pill is a concrete step that makes them feel better, but that talking about their private issues with a professional is an absurd form of treatment that is unlikely to work. This could also be corrected with information. A caveat, however, is that there may be actual heterogeneity in treatment effects that require further study.

As a concrete step to explore reluctance to use psychotherapy, it would be useful to collect information on stigma and beliefs. Ideally, such information would be collected as a module in an existing data set (such as the MEPS) so that it could be analyzed alongside treatment choices, mental health, employment and other sources of heterogeneity (including unobserved heterogeneity). As such an effort would be a costly, if worthwhile, undertaking, initial data collection efforts could be in the form of surveys designed to assess not only the roles of beliefs or stigma, but whether there are other barriers to psychotherapy that merit exploration in future work. These could eventually be incorporated into the type of model estimated here to evaluate counterfactual policies designed improve population mental health.

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Tables and Figures

Table 1: Mental Health and Treatment Decisions By Age

	Subjective MH	DAS Disorder	Antidepressants	Psychotherapy
Ages 26-30	4.114	0.060	0.040	0.013
Ages 31-35	4.052	0.077	0.054	0.016
Ages 36-40	3.982	0.091	0.066	0.019
Ages 41-45	3.911	0.107	0.078	0.022
Ages 46-50	3.844	0.121	0.093	0.024
Ages 51-55	3.800	0.135	0.105	0.024

Notes: An observation is an interview period; thus, sample statistics are calculated across all 208,113 observations in the estimation sample (54,989 individuals). Subjective MH is the respondent's subjective assessment of own mental health and ranges from 1 (poor) to 5 (excellent). Depression and anxiety indicators are based on the ICD-9 codes associated with reported diagnoses.

Table 2: Sample Means By Treatment Choice

	Antidepressants N=12,091	Psychotherapy N=878	Both N=3,284	Neither N=191,860
Demographics				
Male	0.296	0.339	0.307	0.470
Age	43.412	41.54	42.780	40.818
Live in M.S.A.	0.777	0.877	0.834	0.826
Married	0.560	0.448	0.385	0.660
Family Size	2.909	2.653	2.505	3.434
White (race)	0.856	0.798	0.799	0.766
Public Insurance	0.303	0.351	0.499	0.123
Private Insurance	0.629	0.580	0.463	0.661
Non-Labor HH Income	28473	26593	24178	27254
Schooling and Employment				
High School Grad.	0.576	0.483	0.542	0.529
College Grad.	0.226	0.344	0.244	0.255
Employed	0.591	0.607	0.414	0.783
Hourly Wage	23.302	27.609	24.779	23.421
Mental Health				
Subjective	3.121	2.899	2.467	4.026
DAS disorder	0.936	0.908	0.932	0.029
Any disorder	0.999	1.000	1.000	0.032

Notes: An observation is an interview period; thus, sample statistics are calculated across all 208,113 observations in the estimation sample (54,989 individuals). The mean hourly wage excludes the unemployed. Subjective MH is the respondent's subjective assessment of own mental health and ranges from 1 (poor) to 5 (excellent). Depression and anxiety indicators are based on the ICD-9 codes associated with reported diagnoses.

Table 3: Sample Means By Subjective Mental Health

	MH=5 N=78,512	MH=4 N=63,258	MH=3 N=51,540	MH=2 N=11,877	MH=1 N=2,926
Demographics					
Male	0.484	0.458	0.432	0.393	0.393
Age	40.188	40.957	41.718	42.851	43.759
Live in M.S.A.	0.844	0.828	0.801	0.787	0.746
Married	0.699	0.673	0.611	0.445	0.328
Family Size	3.404	3.423	3.434	3.026	2.725
Problem Child	0.444	0.571	0.668	0.968	1.150
White (race)	0.766	0.793	0.769	0.722	0.616
Public Insurance	0.087	0.102	0.178	0.415	0.616
Private Insurance	0.734	0.699	0.568	0.386	0.245
Non-Labor HH Income	30942	28227	23079	18781	16592
Schooling & Employment					
High School Grad.	0.521	0.542	0.539	0.525	0.523
College Grad.	0.327	0.271	0.162	0.111	0.073
Employed	0.832	0.809	0.712	0.460	0.220
Hourly Wage	25.623	23.516	20.247	18.589	19.400
Treatment Decisions					
Psychotherapy	0.003	0.009	0.025	0.111	0.227
Antidepressants	0.023	0.053	0.103	0.299	0.476
Mental Health					
DAS disorder	0.034	0.071	0.139	0.393	0.599
Any Condition	0.038	0.077	0.147	0.418	0.646

Notes: An observation is an interview period; thus, sample statistics are calculated across all 208,113 observations in the estimation sample (54,989 individuals). The mean hourly wage excludes the unemployed. Subjective MH is the respondent's subjective assessment of own mental health and ranges from 1 (poor) to 5 (excellent). Depression and anxiety indicators are based on the ICD-9 codes associated with reported diagnoses.

Table 4: DAS Reporting and Treatment by Subjective Mental Health

	Minimum Subjective Mental Health Observed				
	Poor (3.4%)	Fair (11.2%)	Good (40.6%)	Very Good (29.6%)	Excellent (15.2%)
DAS disorder ever (Yes)	0.693	0.455	0.153	0.072	0.037
Psychotherapy ever	0.434	0.305	0.168	0.116	0.063
Antidepressants ever	0.784	0.691	0.645	0.582	0.516
Average Subj. MH	1.892	2.751	3.547	4.358	5.000
DAS disorder ever (No)	0.307	0.545	0.847	0.928	0.963
Psychotherapy ever	0.069	0.012	0.002	0.001	0.000
Antidepressants ever	0.094	0.023	0.007	0.006	0.004
Average Subj. MH	2.160	2.930	3.682	4.420	5.000

Notes: An observation is an individual; thus, statistics are calculated across 54,989 observations. Table columns group individuals by their minimum subjective mental health report during the 5 survey rounds.

Table 5: A Multinomial Logit for Treatment Choices

	Antidepressants		Psychotherapy		Both	
	Coef.	SE	Coef.	SE	Coef.	SE
Constant	-2.386	0.086	-3.554	0.275	-2.079	0.144
Male	-0.718	0.021	-0.451	0.072	-0.601	0.040
Age	0.032	0.001	0.002	0.004	0.016	0.002
MSA	-0.092	0.025	0.397	0.106	0.221	0.051
Married	-0.265	0.021	-0.591	0.074	-0.607	0.041
Mental Health						
Excellent	-3.097	0.055	-3.663	0.165	-5.003	0.098
Very Good	-2.304	0.052	-2.701	0.147	-3.897	0.077
Good	-1.637	0.050	-1.956	0.138	-2.649	0.064
Fair	-0.544	0.052	–0.709	0.140	-0.976	0.062
Region						
Midwest	0.306	0.033	-0.194	0.099	-0.000	0.056
South	0.226	0.030	-0.604	0.097	-0.338	0.053
West	-0.133	0.033	-0.424	0.098	-0.367	0.056
Race						
Black	-1.061	0.034	-0.617	0.103	-0.856	0.055
Other (non-white)	-0.673	0.048	-0.560	0.150	-0.665	0.086
Education						
High School	0.457	0.027	0.518	0.099	0.621	0.049
College	0.598	0.034	1.374	0.113	1.312	0.062
Insurance						
Public	1.112	0.029	1.174	0.098	1.499	0.051
Private	0.603	0.028	0.426	0.097	0.491	0.053

The model is estimated on the full estimation sample and the base outcome is no treatment. All models control for mental health, sex, age, race, marital status, MSA, region, education, and insurance status. The excluded mental health category is poor, the excluded region is the north, the excluded race is white, the excluded education level is less than high school, and the excluded insurance status is uninsured.

Table 6: Mental Health and Labor Market Outcomes

	Employment		ln(Wage)		Hours	
	Coef.	SE	Coef.	SE	Coef.	SE
Mental Health						
Excellent	0.536	0.007	0.123	0.020	3.822	0.432
Very Good	0.525	0.007	0.081	0.020	3.420	0.433
Good	0.459	0.007	0.022	0.020	3.035	0.434
Fair	0.230	0.008	-0.026	0.021	1.777	0.454
Observations	N=208,113		N=159,284		N=159,284	

The excluded mental health group is "poor". All models control for sex, age, race, marital status, MSA, region, year effects, and education. Models for hours and hourly wage are estimated on those who are working.

Table 7: Model Predictions by Unobserved Type

	Type 1		Type 2		Type 3		Type 4	
Variable	Mean	S.E.	Mean	S.E	Mean	S.E	Mean	S.E
Treatment								
Psychotherapy Ever	0.375	0.004	0.016	0.001	0.036	0.000	0.004	0.001
Any Psych. per. t	0.212	0.003	0.004	0.000	0.010	0.000	0.001	0.000
Share with $p_t^c = 0$	0.685	0.004	0.496	0.021	0.270	0.006	0.325	0.057
$p_t^c = 0 \vert p_t^c > 0$	316.943	7.368	272.117	24.526	385.197	6.965	285.006	43.51
$D_t \vert M_t = 1$	0.392	0.006	0.424	0.021	0.480	0.006	0.546	0.059
Rx Ever	0.641	0.003	0.100	0.002	0.113	0.001	0.075	0.003
Any Rx period t	0.516	0.003	0.044	0.001	0.051	0.000	0.029	0.001
Share with $p_t^r = 0$	0.212	0.002	0.125	0.004	0.036	0.001	0.073	0.007
$p_t^c = 0 \vert p_t^r > 0$	233.815	2.344	152.451	2.853	183.208	1.601	131.101	7.027
Employment								
PT	0.026	0.001	0.095	0.001	0.187	0.001	0.193	0.003
Mean: W_t^1	18.281	0.244	9.983	0.031	21.522	0.047	20.124	0.149
SD: W_t^1	9.331	0.286	3.849	0.055	17.653	0.076	11.951	0.163
FT	0.009	0.001	0.084	0.001	0.762	0.001	0.543	0.004
Mean: W_t^0	6.624	0.196	10.663	0.051	24.769	0.018	19.641	0.129
SD: W_t^0	3.449	0.257	4.494	0.110	16.431	0.025	12.008	0.119
Mental Health								
$MH_t = 5$	0.024	0.001	0.311	0.001	0.414	0.000	0.422	0.003
$MH_t = 4$	0.069	0.001	0.301	0.001	0.311	0.000	0.307	0.001
$MH_t = 3$	0.309	0.002	0.313	0.001	0.234	0.000	0.230	0.002
$MH_t = 2$	0.390	0.002	0.067	0.001	0.037	0.000	0.036	0.001
$MH_t = 1$	0.207	0.002	0.009	0.000	0.004	0.000	0.005	0.000
$\mathbf{\Omega_0}$								
log(Initial wage)	3.714	0.111	2.674	0.046	22.881	0.014	18.997	0.204
PT_0	0.056	0.002	0.023	0.001	0.199	0.000	0.133	0.003
FT_0	0.036	0.002	0.035	0.001	0.786	0.000	0.436	0.004
$MH_0 = 5$	0.030	0.001	0.333	0.001	0.449	0.000	0.448	0.005
$MH_0 = 4$	0.078	0.002	0.278	0.001	0.307	0.000	0.299	0.003
$MH_0 = 3$	0.317	0.002	0.303	0.001	0.209	0.000	0.211	0.003
$MH_0 = 2$	0.381	0.002	0.074	0.001	0.032	0.000	0.036	0.001
$MH_0 = 1$	0.193	0.002	0.012	0.000	0.003	0.000	0.006	0.000
c_0	0.184	0.002	0.009	0.000	0.010	0.000	0.000	0.000
r_0	0.469	0.003	0.031	0.001	0.045	0.000	0.032	0.002
female	0.585	0.003	0.792	0.001	0.495	0.000	0.522	0.006
age_0	43.598	0.047	39.570	0.025	40.321	0.004	39.828	0.064
year_0	8.523	0.032	7.917	0.012	7.702	0.002	8.185	0.035
ave. msa	0.702	0.003	0.803	0.001	0.820	0.000	0.837	0.004
ave. pub. ins.	0.742	0.004	0.321	0.001	0.065	0.000	0.106	0.003
ave. priv. ins.	0.179	0.002	0.331	0.002	0.783	0.000	0.621	0.005
hs education	0.504	0.003	0.483	0.001	0.541	0.000	0.494	0.003
college education	0.103	0.002	0.110	0.001	0.287	0.000	0.338	0.004
nonwhite	0.340	0.003	0.227	0.001	0.202	0.000	0.220	0.004
ave. married	0.346	0.003	0.641	0.002	0.668	0.000	0.690	0.005
ave. hh income /100	241.337	1.759	393.918	1.290	354.390	0.223	451.708	3.323
Share of Population	0.049	0.000	0.162	0.001	0.742	0.000	0.047	0.001

Notes: The simulated data are constructed by sampling from the joint error distribution, permanent unobserved heterogeneity distribution, and estimated parameter covariance matrix 50 times for each individual, then forward simulating four periods from initial conditions. All moments are calculated over all four simulation periods.

Table 8: Assigned Psychotherapy in First Period, Sick Types

	t=1			t=2			t=3			t=4		
	Base	Sim	% Δ	Base	Sim	% Δ	Base	Sim	% Δ	Base	Sim	% Δ
Type 1, Women												
Psychotherapy	0.235	1.000	3.254	0.265	0.663	1.500	0.270	0.524	0.944	0.223	0.357	0.599
Medication	0.587	0.738	0.256	0.618	0.842	0.362	0.626	0.833	0.332	0.575	0.751	0.307
Avg MH	2.355	2.355	0.000	2.317	2.620	0.130	2.307	2.649	0.148	2.301	2.603	0.131
Working PT	0.034	0.027	-0.194	0.028	0.026	-0.093	0.029	0.027	-0.069	0.021	0.021	-0.032
Working FT	0.007	0.005	-0.266	0.007	0.006	-0.076	0.008	0.008	0.004	0.007	0.007	0.024
Avg Wage	15.609	16.467	0.055	16.563	16.660	0.006	16.103	16.161	0.004	15.118	15.323	0.014
Type 1, Men												
Psychotherapy	0.146	1.000	5.868	0.151	0.497	2.288	0.151	0.358	1.377	0.128	0.233	0.818
Medication	0.395	0.542	0.373	0.407	0.657	0.615	0.412	0.645	0.567	0.378	0.568	0.502
Avg MH	2.394	2.394	0.000	2.304	2.629	0.141	2.250	2.588	0.150	2.220	2.516	0.133
Working PT	0.035	0.030	-0.152	0.031	0.027	-0.101	0.029	0.027	-0.076	0.021	0.021	-0.008
Working FT	0.013	0.010	-0.176	0.011	0.009	-0.160	0.010	0.011	0.060	0.009	0.010	0.109
Avg Wage	15.516	16.373	0.055	16.314	17.093	0.048	16.122	15.802	-0.020	15.178	14.954	-0.015
Type 2, Women												
Psychotherapy	0.005	1.000	187.932	0.004	0.047	9.840	0.004	0.007	1.031	0.004	0.005	0.092
Medication	0.046	0.068	0.461	0.049	0.118	1.379	0.051	0.090	0.769	0.052	0.072	0.383
Avg MH	3.915	3.915	0.000	3.872	4.166	0.076	3.849	4.036	0.049	3.833	3.947	0.030
Working PT	0.077	0.064	-0.163	0.107	0.100	-0.060	0.111	0.108	-0.027	0.082	0.081	-0.006
Working FT	0.050	0.043	-0.147	0.071	0.069	-0.032	0.081	0.080	-0.009	0.064	0.064	-0.004
Avg Wage	10.084	10.123	0.004	10.048	10.122	0.007	10.062	10.085	0.002	10.041	10.067	0.003
Type 2, Men												
Psychotherapy	0.004	1.000	267.932	0.002	0.033	15.123	0.002	0.004	0.661	0.003	0.003	0.058
Medication	0.025	0.035	0.416	0.026	0.068	1.572	0.028	0.047	0.645	0.033	0.042	0.260
Avg MH	3.751	3.751	0.000	3.804	4.090	0.075	3.844	4.015	0.044	3.851	3.949	0.025
Working PT	0.080	0.068	-0.149	0.111	0.107	-0.031	0.115	0.114	-0.014	0.084	0.087	0.028
Working FT	0.123	0.104	-0.151	0.163	0.154	-0.056	0.173	0.168	-0.031	0.137	0.135	-0.013
Avg Wage	10.607	10.768	0.015	10.584	10.658	0.007	10.471	10.473	0.000	10.506	10.555	0.005

Notes: We construct the simulated data by first using the estimated posterior unobserved type probabilities to simulate a type for each individual in Sample C referenced in Table A.I. We then randomly select 2,500 individuals of each type. For this sample of 10,000 individuals, we sample from the joint error distribution and estimated parameter covariance matrix 50 times for each individual. We then forward simulate four periods from the observed initial conditions. In this table, we compare choices and outcomes in each of the four simulation periods for Types 1 and 2, using the baseline model and an alternative model where individuals are assigned to psychotherapy in period $t = 1$.

Table 9: Counterfactual Policy Simulations, Sick Types

	Base	Policy 1		Policy 2		Policy 3		Policy 4	
	Level	Level	% Δ	Level	% Δ	Level	% Δ	Level	%Δ
Type 1, Women									
Psychotherapy	0.248	0.252	0.015	0.554	1.232	0.250	0.009	0.565	1.275
Medication	0.602	0.603	0.002	0.752	0.250	0.602	0.001	0.757	0.259
Avg MH	2.299	2.302	0.001	2.719	0.182	2.301	0.001	2.730	0.188
Working PT	0.028	0.028	-0.002	0.025	-0.092	0.030	0.072	0.031	0.104
Working FT	0.007	0.007	-0.004	0.007	-0.070	0.008	0.075	0.008	0.115
Avg Wage	15.856	15.859	0.000	15.964	0.007	15.803	-0.003	15.786	-0.004
Type 1, Men									
Psychotherapy	0.144	0.146	0.015	0.374	1.595	0.145	0.010	0.383	1.661
Medication	0.398	0.399	0.003	0.537	0.350	0.399	0.002	0.543	0.364
Avg MH	2.241	2.242	0.001	2.554	0.140	2.242	0.001	2.565	0.145
Working PT	0.029	0.029	0.002	0.028	-0.044	0.030	0.046	0.032	0.097
Working FT	0.011	0.011	0.000	0.010	-0.027	0.011	0.049	0.012	0.107
Avg Wage	15.780	15.777	0.000	15.809	0.002	15.738	-0.003	15.665	-0.007
Type 2, Women									
Psychotherapy	0.004	0.004	0.016	0.008	0.729	0.005	0.052	0.008	0.868
Medication	0.050	0.050	0.001	0.051	0.020	0.050	0.001	0.051	0.024
Avg MH	3.843	3.843	0.000	3.849	0.001	3.844	0.000	3.849	0.001
Working PT	0.094	0.094	0.000	0.094	-0.001	0.094	0.001	0.094	0.002
Working FT	0.067	0.067	0.000	0.067	-0.001	0.067	0.002	0.067	0.002
Avg Wage	10.058	10.058	0.000	10.060	0.000	10.058	0.000	10.060	0.000
Type 1, Men									
Psychotherapy	0.003	0.003	0.013	0.005	0.782	0.003	0.068	0.005	0.958
Medication	0.028	0.028	0.000	0.028	0.010	0.028	0.001	0.028	0.009
Avg MH	3.837	3.837	0.000	3.841	0.001	3.837	0.000	3.841	0.001
Working PT	0.098	0.098	0.000	0.098	-0.001	0.098	0.001	0.098	0.001
Working FT	0.149	0.149	0.000	0.149	-0.002	0.149	0.002	0.149	0.001
Avg Wage	10.539	10.539	0.000	10.539	0.000	10.536	0.000	10.538	0.000

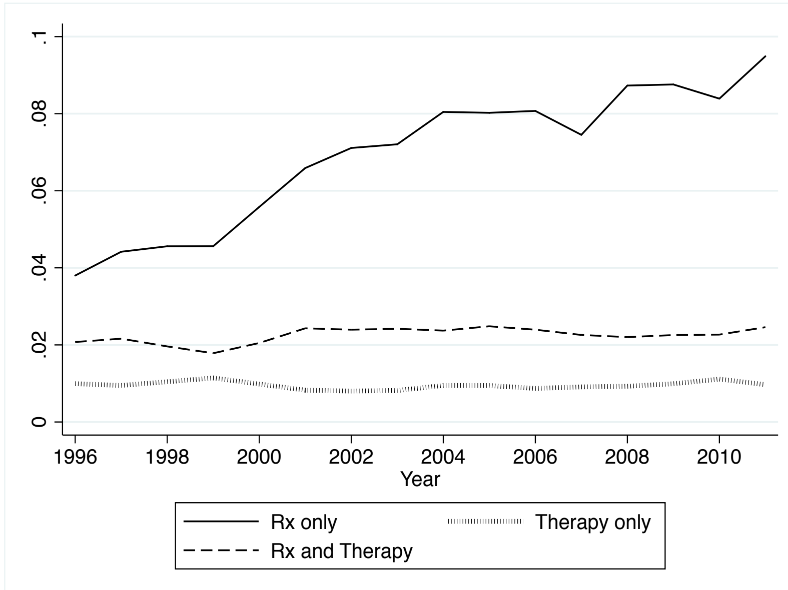
Notes: We construct the simulated data by first using the estimated posterior unobserved type probabilities to simulate a type for each individual in Sample C referenced in Table A.I. We then randomly select 2,500 individuals of each type. For this sample of 10,000 individuals, we sample from the joint error distribution and estimated parameter covariance matrix 50 times for each individual. We then forward simulate four periods from the observed initial conditions. In this table, we compare choices and outcomes for Type 1 and 2 individuals using the baseline model and five alternative models representing the following policy interventions. Policy 1: Remove the financial cost of psychotherapy. Policy 2: Remove the possibility of discontinuation. Policy 3: Remove the employment/time cost of psychotherapy (i.e., $\alpha_{1,3} = \alpha_{1,4} = 0$). Policy 4: Remove all non-utility psychotherapy costs (i.e., combine Policies 1, 2, and 3). Sample moments are aggregated across all four simulated periods.

Table 10: Reducing the Disutility of Psychotherapy

Disutility Reduction	Psychotherapy Use	Antidepressants Use
0%	0.019	0.071
2.5%	0.020	0.071
5%	0.023	0.071
7.5%	0.027	0.073
10%	0.028	0.075
12.5%	0.031	0.076
15%	0.036	0.076
17.5%	0.043	0.078
20%	0.045	0.080
22.5%	0.053	0.080
25%	0.057	0.079
27.5%	0.065	0.083
30%	0.072	0.085
For 20% Reduction		
Type 1	0.410	0.606
Type 2	0.015	0.045
Type 3	0.030	0.056
Type 4	0.023	0.037

Notes: This table shows the proportion of the population using treatment in a survey period, for different percentage reductions in the disutility of psychotherapy. The simulated data are constructed by sampling from the joint error distribution and permanent unobserved heterogeneity distribution, then forward simulating four periods from initial conditions. Sample moments are calculated over all four simulation periods. Each row of the table represents a separate simulation with a different level of psychotherapy disutility.

Figure 1: Mental Health Treatment Choices Over Time



Notes: Calculated using 1996-2011 MEPS cohorts. Individuals are grouped by cohort year and are categorized according to whether they ever received psychotherapy/counseling (i.e., *psychoth* from the office-based visits file) or ever consumed prescription drugs for depression (i.e., ICD9 codes 296 or 311), anxiety (300), or stress-related ailments (308 or 309) during the first three survey rounds.

Figure 2: Estimated Effects of Psychotherapy on Mental Health

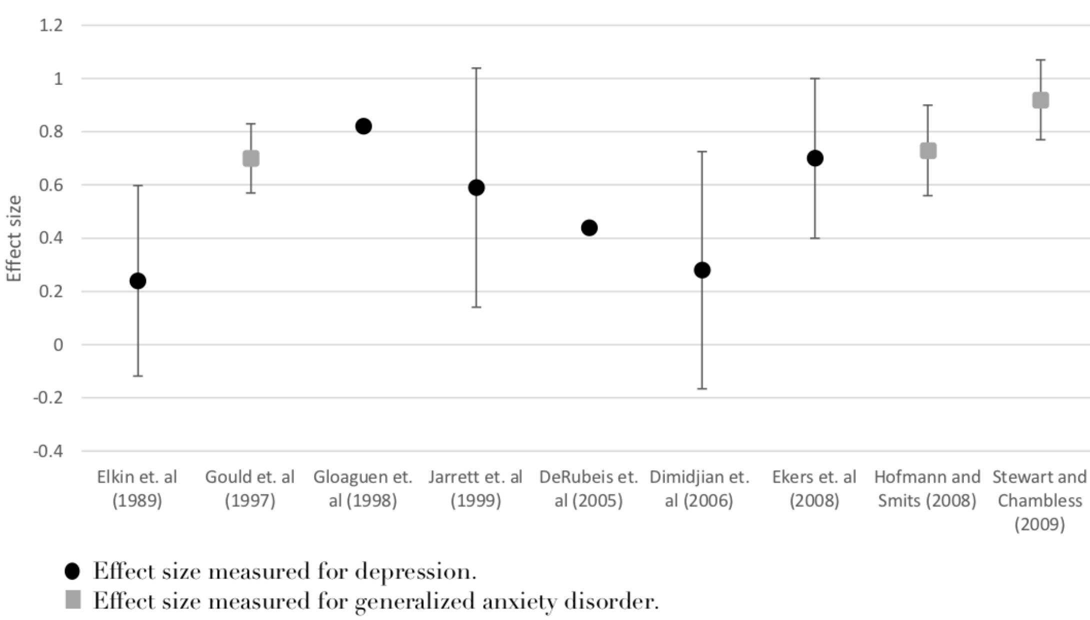
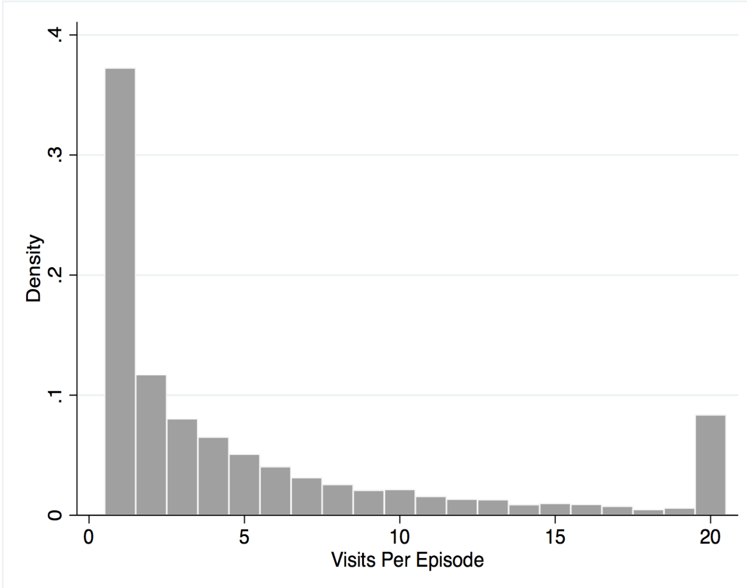
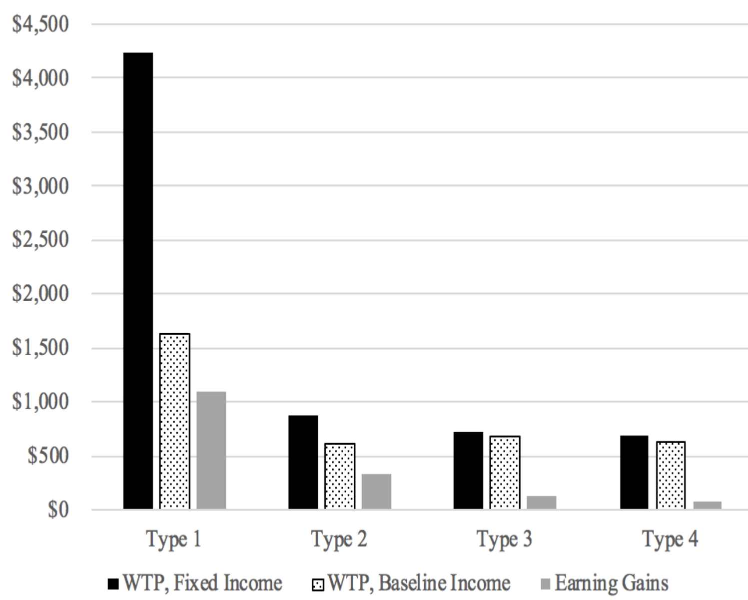


Figure 3: Therapist Discontinuation



Notes: This figure is produced using Sample C referenced in Table A.I. Sequences of psychotherapy visits are grouped into episodes according to a two-month gap rule - i.e., if a gap of two months or longer is observed between two visits, the visits are grouped into different episodes. The figure then displays the number of individual psychotherapy visits in each episode.

Figure 4: The Value of Mental Health Improvements



Notes: This figure measures the value of a hypothetical treatment that ensures one's mental health never drops below the baseline sample mean. The left-most (black) bar measures willingness to pay for the treatment, holding household disposable income at $40,000 annually, while the middle (speckled) bar allows household income to vary. The right-most (grey) bar measures the average earning gains for individuals of each type. All values are measured in 2018 dollars.

A.I Data

A.I.1 Estimation Sample Construction

We begin with individuals from the 1996-2011 MEPS cohorts. We then restrict the sample to those between the ages of 26 and 55 in order to focus on individuals for whom education is unlikely to change and who are making non-retirement employment decisions. We also remove round one observations, as lags of several variables are used as controls in our econometric specification. Demographic information for this subsample (Sample A) is provided below in Table A.I. We then limit this sample to those who complete each of the five possible interview rounds over a two year period; Sample B.

In each round, MEPS participants answer questions related to behaviors and outcomes occurring since the most recent interview. These interview periods vary in length—on average, they are about 5.2 months long and approximately 85% are between 3.5 and 7 months long. Figure A.I shows the distribution of period lengths, rounded to the nearest half-month intervals. Period length was randomly allocated as a part of the survey design. The estimation of our structural model requires that each interview period covers an approximately equal amount of time; thus, we eliminate observations where the length of time between interviews is less than 3.5 months or greater than 7 months. To avoid needing to integrate over missing time periods in the estimation of the structural model, we use the following process to eliminate individuals and observations from the data: (i) drop any observation where length is less than 3.5 months; (ii) drop any observation where length is greater than 7 months; and (iii) drop any individual whose 2nd, 3rd, or 4th interview is dropped in (i) or (ii). The resulting Sample C is used in estimation.

A.I.2 Treatment Prices across Time and Insurance Status

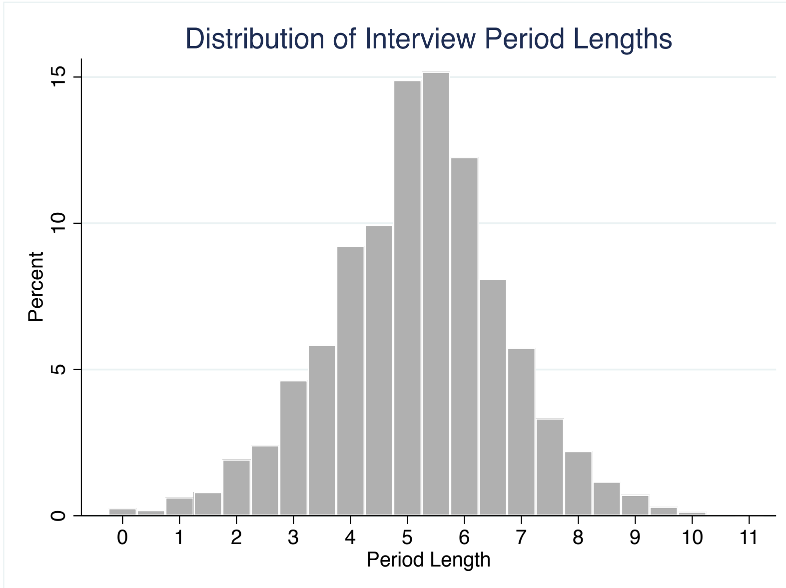
Table A.II shows how inflation-adjusted prices for individual psychotherapy sessions and individual antidepressant prescription fills have changed over the sample period. The growth in total expenditures from all sources is shown in columns 3 and 6. This growth is consistent with medical prices in general rising faster than inflation in the sample period (Peter G. Peterson Foundation, 2020). Columns 1 and 4 show how the proportion of individuals paying nothing out of pocket has grown over time, while columns 2 and 5 show that average out-of-pocket expenditures, conditional on spending anything, have fallen. Both patterns are consistent with third-party payers, i.e., government and insurers, paying a larger share of the ever growing price of treatment over time.

Table A.I: Sample Statistics Across Limiting Samples

	Sample A	Sample B	Sample C
Demographics			
Male	0.467	0.456	0.457
Age	40.572	40.937	41.002
Live in M.S.A.	0.833	0.828	0.824
Married	0.630	0.658	0.649
Family Size	3.418	3.432	3.386
White (race)	0.760	0.772	0.772
Problem Child		0.566	0.574
Health Insurance			
Public Insurance		0.130	0.140
Private Insurance		0.659	0.656
Schooling & Employment			
High School Grad.		0.538	0.532
College Grad.		0.258	0.253
Employed		0.769	0.765
Hourly Wage		23.693	23.441
Non-Labor HH Income		27964	27274
Treatment Decisions			
Psychotherapy (round)		0.019	0.020
Psychotherapy (ever)		0.041	0.043
Antidepressants (round)		0.070	0.074
Antidepressants (ever)		0.113	0.121
Mental Health			
Subjective		3.954	3.944
DAS (round)		0.095	0.100
DAS (ever)		0.162	0.169
Individuals	103,239	87,021	54,989
Observations	451,616	348,084	208,113

Notes: Sample A indicates MEPS participants from 1996-2011 between the ages of 25 and 55, excluding Round 1. Sample B eliminates from Sample A all individuals not completing all five interviews. Sample C eliminates from Sample B all individuals who have at least one interview period with a length less than three and a half months or greater than seven months, as well as any individual with an excluded month in rounds two, three, or four. Problem child is measured in rounds two and four as the average response to 13 questions regarding problems with a child in the house. Examples are "(child has) problem getting along with Mom" and "(child has) problem behavior in school." A larger value indicates more problems and we measure the most problematic child in the household. Non-Labor Household Income is the weighted sum of an individual's own non-labor income and total income (labor and non-labor) of household members. Spousal income is given full weight, while non-spousal household income is weighted at a third of its full value. The mean hourly wage excludes the unemployed. Subjective MH is the respondent's subjective assessment of own mental health and ranges from 1 (poor) to 5 (excellent). The depression, anxiety, stress (DAS) indicator is based on the ICD-9 codes associated with reported disorders.

Figure A.I: The Distribution of Period Lengths in MEPS



Notes: Sample B interview periods contribute to this distribution (see Appendix Section A.I.1). Period 1 is excluded for all individuals; thus, there are 348,084 observations in total.

The second half of the table displays treatment prices by insurance status. Publicly insured individuals are found to be the most likely to pay nothing out-of-pocket for antidepressants, while facing the highest total price. These findings are consistent with both the generosity of Medicaid, as well as the federal governments inability to negotiate for drug prices, which influences Medicaid drug prices. That the uninsured face the lowest total antidepressant prices likely reflects selection into generic medication, while the relatively high out-of-pocket prices reflects the fact that there are few opportunities for reduced-price drugs. For psychotherapy, the most generous type of coverage is public insurance, which is a somewhat misleading indicator that psychotherapy is affordable and attainable for all publicly insured individuals. In reality many therapist simply do not accept Medicaid patients, which can make it difficult for patients to receive psychotherapy. Finally, a somewhat surprising finding is how little the uninsured pay for psychotherapy. Several factors contribute to this finding. First, these figures suggest selection into treatment, i.e., those facing the lowest prices for care are the most likely to select it; thus, the low prices observed among the uninsured population partly reflects the fact that only those who can find lower prices choose treatment. Second, it is widely known that many psychologists use "sliding scale" pricing, meaning low-income and/or uninsured patients are charged less, or nothing at all for treatment.

Finally, while single session and refill prices are presented in the table, prices enter the model as per-round expenditure levels (see Section 5.1). The average number of psychotherapy sessions attended per round (for someone attending at all) is 6.3, meaning out-of-pocket expenditure per-period averages about \$175. The average antidepressant user has 5.7 refills per-period, meaning expenditure per-period is similar to that for psychotherapy. That said, the combination of high discontinuation rates and a relatively large proportion of individuals receiving free psychotherapy masks how much more psychotherapy is for some. For example, an individual completing their course of psychotherapy attends 8.2 sessions per-period on average; thus, if this individual does not receive any free care and pays just the average (non-zero) out-of-pocket price observed in the data, the individual pays over \$400 per-period for psychotherapy.

Table A.II: Treatment Prices

	Antidepressants			Psychotherapy		
	% zero paid OOP	Ave. OOP Paid (cond. non-zero)	Ave. Paid (all sources)	% zero paid OOP	Ave. OOP Paid (cond. non-zero)	Ave. Paid (all sources)
Sample Mean						
Year						
1997	0.092	37.366	95.054	0.437	50.031	99.922
1998	0.073	37.767	105.418	0.455	38.759	100.090
1999	0.075	38.875	99.419	0.363	86.188	141.166
2000	0.047	42.963	108.810	0.508	40.061	103.671
2001	0.086	42.861	114.676	0.468	45.954	175.505
2002	0.079	35.672	105.674	0.459	47.452	108.116
2003	0.062	48.699	109.472	0.522	45.745	117.211
2004	0.116	36.687	115.898	0.475	57.641	119.556
2005	0.108	39.437	113.600	0.495	48.749	131.805
2006	0.073	32.263	106.361	0.507	46.183	116.929
2007	0.087	30.022	112.816	0.443	45.119	118.165
2008	0.089	31.001	115.995	0.531	51.214	119.845
2009	0.132	32.448	110.969	0.517	35.993	170.334
2010	0.165	30.190	115.132	0.556	39.208	133.321
2011	0.145	19.414	114.975	0.515	44.646	129.248
Insurance Status						
Privately Insured	0.025	32.457	105.99	0.206	50.16	130.15
Publically Insured	0.246	28.275	121.747	0.800	37.16	130.91
Uninsured	0.050	75.441	91.267	0.485	58.17	98.10

Notes: Table reports average inflation-adjusted price per psychotherapy visit and average inflation-adjusted price per prescription. All prices are in 2018 dollars. Calculations use Sample C referenced in Table A.I

A.I.3 Alternative Measures of Mental Health

It is noted that there are various ways to measure mental health. We use subjective mental health as the primary measure throughout. There are three other potential measures of mental health in the MEPS data, but each as a significant downside that prevents us from using it as our primary measure. First, in every round, an individual is able to report depression as a medical condition, which is then coded (via ICD9) by professional coders. As discussed in Section 3.2 this report likely suffers from non-classical measurement error, as it is likely to be influenced by past, unobserved interactions with medical professional. We also have information on two indices used to measure mental health via survey questions: the Kessler 6 index (K-6) and the the Mental Component Summary (MCS). The K-6 is a commonly used mental health scale that is calculated from responses to six questions of the form: "During the past 30 days, how often did you feel . . . [nervous, hopeless, restless or fidgety, so depressed that nothing could cheer you up, that everything was an effort, worthless]?" For each question, a value of 0, 1, 2, 3, or 4 is assigned to the answers "none of the time," "a little of the time," "some of the time," "most of the time," or "all of the time," respectively. The K6 is calculated by summing the scores from each of the six questions, generating a 0-24 scale, with higher scores indicating a greater tendency towards mental illness. The MCS is calculated from the standardized SF-12 health screening questions, where mental health questions (6-9) are weighted more heavily. The index range is 1-78, where higher scores indicated better mental health. Unfortunately, these two measures are only collected in rounds 2 and 4, the K-6 has only been collected since 2005, and the MCS has only been collected since 2001.

In Table A.III, we show that the subjective mental health scores capture significant amounts of variation in reporting of depression and in the two mental health indices.

Table A.III: Association between Subjective Mental Health and Other Measures

	MH=5	MH=4	MH=3	MH=2	MH=1
Mental Health					
Depression/Anxiety	0.022	0.054	0.120	0.393	0.613
Kessler-6	1.975	2.955	4.613	10.057	15.016
Mental Component Summary	54.084	51.363	47.572	37.577	29.896

Notes: An observation is an interview period in round 2 or 4 as these are the only rounds in which the K-6 and MCS can be observed. Subjective MH is the respondent's subjective assessment of own mental health and ranges from 1 (poor) to 5 (excellent). Depression and Anxiety shares are based on the ICD-9 codes associated with reported diagnoses. K-6 ranges from 0-24, while MCS ranges from 1-78. A higher (lower) score indicates greater mental distress for the K-6 (MCS) measure. The K-6 was first collected in 2005, while the MCS was first collected in 2001.

A.II Model

A.II.1 Disposable Income

The disposable income function $D(\cdot)$ in equation 3 adjusts gross household income, GY_t, for approximate total tax liability, housing expenses, and family size. To calculate $D(\cdot)$, we first separate households into income quintiles. We then calculate disposable income as

$$DY_t = GY_t * (1 - Tr_q) * (1 - Hr_q) * \left(1 - \left(1 - \sqrt{\frac{2}{(1 + FS)}}\right)\right)$$

where Tr_q and Hr_q approximate the average total (federal, state, and local) tax rate and housing cost rate by income quintile. For the total tax rate, Wamhoff and Gardner (2019) estimate the following rates for the lowest to highest income quintiles: (20.7, 23.2, 26.5, 28.9, 32.0). Calculations are made prior to the 2017 Tax Cuts and Jobs Act (see Table 2, p. 5). We calculate the following after-tax housing cost rates, again for the lowest to highest income quintiles, using the American Community Survey micro-data: (50.14, 33.24. 22.98, 17.48, 13.04).[68]

We then adjust for family size. Similar to Eckstein, Keane, and Lifshitz (2019), the fraction of income that is spent on other family members is calculated as $1 - \sqrt{2/(1 + FS)}$, where FS is family size; thus, a single person has $FS = 1$ and consumes 100 percent of their disposable income, a married couple with one child has $FS = 3$ and consumes 69.7 percent of their disposable income, etc.

A.III Mental Health Treatment Effects

The dynamic model presented in Section 4 assumes that individuals make treatment decisions based on their beliefs about the productivity of treatment. Moreover, model simulations require estimates of the impact of treatment on mental health outcomes in order to determine the value of counterfactual policies. We assume that individuals have rational expectations, meaning they make treatment decisions based on the true impact of treatment on mental health, or in our case, the average treatment effect.

68. To calculate housing costs as a percent of household income, we use the 2011 1-year PUMS from the American Community Survey. Housing costs are based on the reported first mortgage payment for those households that own a home and on the gross rent payment for those who rent a home. We exclude from our calculation the 3.8 percent of households in the survey for whom total housing costs (based on 12 months of the mortgage or rent payment) are greater than household income.

A separate challenge is estimating average treatment effects. The strategy we use is to take estimates from the robust, well-identified clinical trial literature. We discuss this literature in Section 3.4.3. An alternative approach is to estimate treatment effects internally, using our observational data. Estimating causal effects from observation data is challenging for a number of reasons. In this instance, endogenous selection into treatment is the primary issue, i.e., the sickest individuals select into treatment. In this section, we explore using both structural and reduced form techniques for dealing with this selection problem.

A.III.1 Reduced-Form Approach to Estimating Treatment Effects

Table 2 clearly shows that the sickest individuals select into both types of treatment; thus, the initial identification challenge amounts to what is essentially an omitted variable problem, where *mental health at the time of medical care consumption* is the key omitted variable. The simplest solution, then, is to estimate the impact of treatment on mental health while conditioning on lagged mental health. This *solution* does not produce positive treatment effects in our setting (see Table A.V, column 1).[69] Table A.IV provides insight into the scope of the selection problem and why conditioning on lagged mental health is a poor solution. The table shows that conditional on mental health in period $t-1$, mental health in period t is *worse*, on average, for those receiving treatment than those not receiving treatment. This general relationship holds for both types of treatment and across the mental health distribution. These findings suggest that lagged mental health is an imperfect proxy for mental health at the time of medical care consumption. In other words, conditional on lagged mental health, individuals receive mental health shocks over the course of an interview period that influence both mental health transitions (from M_{t-1} to M_t) and treatment decisions, which must be accounted for in order to identify causal effects

We attempt to solve this selection problem using an instrumental variables approach. The instrumental variables strategy requires a minimum of two instruments that (i) alter mental

69. Column 1 contains parameter estimates from a linear model where self-reported mental health status is regressed on treatment, lagged mental health, demographic characteristics, and county and time fixed effects. Throughout this analysis, we further restrict the estimation sample discussed in Section A.I.1 to include only individuals with private insurance. This restriction strengthens the first stage effect of one of our instruments (i.e., number of psychiatrists per capita) and, thus, the precision of our 2SLS estimate. A separate analysis of publicly insured and uninsured individuals reveals that their treatment decisions are not responsive to changes in the instrument—these results are available upon request. Many private practice psychiatrists do not accept Medicaid patients (Taube, Goldman, and Salkever, 1990), which comprises nearly all of the publicly insured individuals in our estimation sample. Furthermore, according to our data, the uninsured are simply very unlikely to consume any mental health treatment, making the supply of psychiatrists mostly irrelevant for them. In light of the county fixed effect, we also drop counties in the bottom 10th percentile of total observations.

health treatment decisions (i.e., instruments are not weak) and (ii) have no *direct* effect on mental health (i.e., instruments are exogenous). The first instrument that we consider is the number of psychiatrists per capita in an individual's county of residence. This information can be found in the Area Health Resource File (AHRF), which is collected annually by the US Department of Health and Human Services, for every year between 1995 and 2016, except for 2008. There is substantial variation in the number of psychiatrists per capita across the sample—nearly 10% of individuals live in a county without any psychiatrists, the average individual lives in a county with 1.3 psychiatrists per 10,000 people, and the individual at the 90^{th} percentile lives in a county with 2.5 psychiatrists per 10,000 people. Unsurprisingly, this variable is highly persistent over time—regressing the variable on county fixed effects produces an R-squared of 0.97, suggesting that just 3% of the overall variation in psychiatrists per capita is due to within county variation. Because county fixed effects are included in our 2SLS specification, identification will come from these within-county changes in the number of psychiatrists per capita, which we argue is conditionally random.

A second instrument we consider is an indicator for whether the individual's county of residence has a Walmart with a pharmacy *and* the survey period ends in 2007 or later.[70] On September 21, 2006, Walmart began offering almost 300 generic prescriptions at a price of $4 for a monthly supply at it's stores in Tampa Florida.[71] Initially, Walmart planned to expand the offering to all Florida stores in January of the following year; however, by November 27, 2006, Walmart had expanded the policy to all of its US stores. In a 2006 company newsletter, (then) Executive VP of Professional Services, Bill Simon, explained that, "many customers have greatly benefited from the savings and consumer demand has been a significant factor in the program's expansion." According to the AARP, the average *annual* retail cost of prescription medication psychotherapy for a basket of 280 popular generics in 2006 was $391 (i.e., roughly $33 for a monthly prescription). This suggests that Walmart's offering of $4 monthly prescriptions could represent significant cost savings for individuals and, thus, increase the quantity of antidepressants demanded. 90% of our sample lives in a county with a Walmart and, therefore, had access to these low cost medications.

Our first stage results can be found in Table A.VI. All models control for county and year fixed effects as well as lagged mental health and a robust set of demographic controls. Column 1 displays the relationship between our instruments and whether an individual consumes any psychotherapy. The estimates reveal that the number of psychiatrists per

70. We purchased data from AggData containing information on the 4,618 Walmart stores operating in the US in 2016, including opening dates and whether a store has a pharmacy. These data do not contain information an Walmart closures.

71. On this list are roughly 28 medications used in the treatment of mental health, including Fluoxetine (Prozac), Citalopram (Celexa), and Paroxetine (Paxil), all popular antidepressants.

capita significantly increases psychotherapy use, while having access to low cost generic prescriptions via Walmart has no significant effect. Column 2 displays the relationship between our instruments and whether an individual consumes any prescription medications for mental illness. The estimates reveal that both psychiatrists per capital and low cost generic prescriptions through Walmart significantly increase prescription medication use.

Weak instruments can produce biased, inconsistent 2SLS estimates (Bound, Jaeger, and Baker, 1995). In the standard one-instrument, one-endogenous variable setting, it is generally accepted that the instrument is adequately strong if it's F-statistic is greater than 10, which corresponds to a bias in the 2SLS estimate that is less than (approximately) 10% of the bias in the OLS estimate. With multiple instruments and endogenous variables, the magnitude of the joint F-statistic, calculated from the instrument set in each first stage equation, cannot be interpreted in the same fashion. To see why, consider a two-instrument, two-endogenous variable setting, it is possible that only one instrument explains variation in the two endogenous variables, which can generate large F-statistics, but an underidentified model. Kleibergen and Paap (2006) develop a Lagrange Multiplier (LM) statistic for this scenario, which allows for a test of the null hypothesis that the rank of the instrument set is greater than the number of endogenous variables minus one (i.e., that the model is underidentified).[72] Moreover, Sanderson and Windmeijer (2016) develop an F-statistic for weak instruments with multiple endogenous variables that has the same interpretation as a traditional F-statistic in the typical single endogenous variable model, i.e., the bias of the IV estimate relative to the OLS estimate is approximately $1/F$.[73] Table A.VI provides Kleibergen-Paap LM and Sanderson-Windmeijer F-statistics for each of the models presented.

While the instruments presented in our first specification (Columns 1 and 2) significantly alter treatment decisions, the instruments set is weak. Moreover, with just two instruments and two endogenous variables, we cannot test of the exogeneity of our instruments. In Columns 3 and 4 of Table A.VI, we present our preferred instrument set, which contains interactions of original instruments with several demographic variables. In Column 3, the presence of psychiatrists significantly increases the use of psychotherapy for previously married and white individuals. Access to low cost medications through Walmart decreases psychotherapy use, presumably as individuals substitute psychotherapy for prescription medications. In Column 4, the presence of psychiatrists increases prescription medication use,

72. The Kleibergen-Paap LM statistic is a cluster-robust alternative to a similar statistic provided in Cragg and Donald (1993), which is only valid with homoskedastic errors.

73. Sanderson and Windmeijer (2016) F-statistic is also is only valid assuming homoskedastic errors; however, a valid weak instrument test that relaxes this assumption with multiple endogenous variables has yet to be developed.

but for males only, as does the presence of a Walmart after the generic medication price drop. The Kleibergen-Paap LM statistic allows us to reject the null of underidentification at a 7% significance level, while the Sanderson-Windmeijer F-statistic suggest that our instruments are not weak, as the approximate relative bias is well under 10%.

Column 2 of Table A.V contains parameter estimates from our 2SLS specification. The first two rows show that our identification strategy has the desired effect. Both antidepressants and psychotherapy are found to be effective in improving an individual's mental health. Moreover, consistent with the medical literature previously cited, psychotherapy is found to have a larger positive effect than antidepressants. Because our model is over-identified, we are also able to conduct a Hausman J test, which tests the assumption that our instruments are exogenous. This test statistic, which is Chi-squared with 2 degrees of freedom, is 2.924 (p-value 0.233). Thus, we fail to reject the null hypothesis that the instruments are exogenous, which supports our identifying assumptions.

In Column 3 of Table A.V, we repeat our analysis after coding discontinuation psychotherapy visits as if psychotherapy was not attended (i.e., *Any Psychotherapy*=0). These estimates lead to an increase in the effect of psychotherapy on mental health, suggesting that discontinuation visits are less efficacious.[74] As such, we believe that these sessions generally represent costly, unproductive medical care consumption that occurs primarily due to a lack of information.

Table A.IV: Mental Health Transitions by Treatment Choice

	Mental Health at t			
	Antidep. Use		Psychotherapy Use	
	No	Yes	No	Yes
$MH_{t-1} = 5$	4.533	4.055	4.524	3.380
$MH_{t-1} = 4$	4.004	3.589	3.987	3.320
$MH_{t-1} = 3$	3.479	3.009	3.441	2.829
$MH_{t-1} = 2$	2.781	2.333	2.686	2.200
$MH_{t-1} = 1$	2.087	1.683	1.975	1.535

Notes: Subjective MH is the respondent's subjective assessment of own mental health and ranges from 1 (poor) to 5 (excellent). Psychotherapy use in this table excludes discontinuation episodes.

74. Note also that the impact of psychotherapy on mental health is more significant in this specification, 2SLS-B. First stage results are presented in Column 5 of Table A.VI.

Table A.V: Mental Health Production Function, OLS and 2SLS

	(1) OLS		(2) 2SLS - A		(3) 2SLS - B	
	Coef.	SE	Coef.	SE	Coef.	SE
Any Antidepressants	-0.355	(0.009)	0.741	(0.346)	0.711	(0.337)
Any Psychotherapy	-0.414	(0.016)	1.265	(0.804)	1.524	(0.902)
Lagged MH						
Fair	0.510	(0.035)	0.894	(0.120)	0.883	(0.113)
Good	1.111	(0.037)	1.829	(0.198)	1.806	(0.179)
Very Good	1.565	(0.035)	2.384	(0.222)	2.357	(0.199)
Excellent	2.055	(0.037)	2.920	(0.233)	2.891	(0.209)
Age	-0.005	(0.000)	-0.005	(0.001)	-0.005	(0.000)
Male	0.017	(0.003)	0.079	(0.017)	0.076	(0.0161)
Nonwhite	-0.020	(0.007)	0.058	(0.021)	0.055	(0.019)
Marriage Status						
Never Married	-0.041	(0.007)	-0.044	(0.008)	-0.045	(0.008)
Previously Married	-0.035	(0.007)	-0.052	(0.011)	-0.051	(0.019)
Family Size	0.007	(0.002)	0.017	(0.003)	0.017	(0.003)
Education						
High school Grad.	0.106	(0.008)	0.065	(0.015)	0.067	(0.014)
College Grad.	0.082	(0.005)	0.053	(0.010)	0.053	(0.009)
Income						
Second Quartile	0.030	(0.008)	0.030	(0.007)	0.031	(0.007)
Third Quartile	0.071	(0.007)	0.078	(0.006)	0.078	(0.006)
Fourth Quartile	0.126	(0.006)	0.130	(0.008)	0.129	(0.008)
County & Time FE	X		X		X	
R-Squared	0.333		0.137		0.146	
Hansen J Stat $\rightarrow \chi^2(2)$			2.924		2.957	
(P-value)			(0.233)		(0.228)	

Notes: The sample includes all 22-62 year olds from MEPS cohorts between 1996 and 2011 who are privately insured. Further, we remove counties in the lowest 10th percentile of total observations. There are a total of 179,259 observations. Standard errors are clustered at the state level. In 2SLS - A(B), discontinuations are coded as *Any Psychotherapy*=1(0). All models also include the number of psychiatrists per capital as a control variable. The Hansen J Statistic is distributed χ^2 with degrees of freedom equal to the number of instruments minus the number of endogenous variables. The statistic enables a test of the joint null hypothesis that the instruments as uncorrelated with the second stage error term.

Table A.VI: Mental Health Production Function: First Stage Linear Probability Models

	Specification 1				Specification 2A				Specification 2B‡	
	(1) Any Psychotherapy		(2) Any Rx		(3) Any Psychotherapy		(4) Any Rx		(5) Any Psychotherapy	
	Coef.	SE	Coef.	SE	Coef.	SE	Coef.	SE	Coef.	SE
Instruments										
Psychiatrists per cap.†	0.041	(0.020)	0.079	(0.035)	0.042	(0.022)	0.046	(0.033)	0.040	(0.020)
Psych. per cap. * Nonwhite	*	*	*	*	-0.025	(0.013)	0.010	(0.012)	-0.023	(0.010)
Psych. per cap. * Prev. Married	*	*	*	*	0.031	(0.008)	0.023	(0.013)	0.028	(0.008)
Psych. per cap. * Male	*	*	*	*	-0.003	(0.008)	0.059	(0.009)	-0.001	(0.007)
Walmart * (Year>2006)	-0.005	(0.003)	0.019	(0.006)	-0.005	(0.003)	0.018	(0.005)	-0.004	(0.002)
Lagged MH										
Fair	-0.100	(0.024)	-0.199	(0.019)	-0.100	(0.024)	-0.199	(0.019)	-0.080	(0.022)
Good	-0.179	(0.024)	-0.381	(0.019)	-0.179	(0.024)	-0.381	(0.019)	-0.141	(0.023)
Very Good	-0.199	(0.025)	-0.442	(0.021)	-0.199	(0.025)	-0.442	(0.021)	-0.156	(0.024)
Excellent	-0.206	(0.025)	-0.474	(0.022)	-0.206	(0.025)	-0.473	(0.022)	-0.162	(0.024)
Age	-0.000	(0.000)	0.001	(0.000)	-0.000	(0.000)	0.001	(0.000)	-0.000	(0.000)
Male	-0.008	(0.001)	-0.043	(0.003)	-0.008	(0.001)	-0.051	(0.003)	-0.006	(0.001)
Nonwhite	-0.011	(0.001)	-0.054	(0.004)	-0.007	(0.002)	-0.055	(0.005)	-0.005	(0.001)
Marriage Status										
Never Married	0.003	(0.001)	-0.002	(0.003)	0.003	(0.001)	-0.001	(0.003)	0.003	(0.001)
Previously Married	0.006	(0.001)	0.006	(0.002)	0.002	(0.002)	0.003	(0.003)	0.001	(0.002)
Family Size	-0.002	(0.000)	-0.007	(0.001)	-0.002	(0.000)	-0.007	(0.001)	-0.002	(0.000)
Education										
High school Grad.	0.008	(0.002)	0.027	(0.003)	0.008	(0.002)	0.027	(0.003)	0.006	(0.001)
College Grad.	0.009	(0.001)	0.012	(0.002)	0.009	(0.001)	0.012	(0.002)	0.001	(0.002)
Income										
Second Quartile	-0.001	(0.001)	0.001	(0.003)	-0.001	(0.001)	0.001	(0.003)	-0.001	(0.001)
Third Quartile	-0.001	(0.002)	-0.004	(0.004)	-0.001	(0.002)	-0.004	(0.004)	-0.001	(0.001)
Fourth Quartile	-0.001	(0.002)	-0.002	(0.005)	-0.000	(0.002)	-0.002	(0.005)	-0.000	(0.001)
County & Time FE	X		X		X		X		X	
Sanderson-Windmeijer F-Stat.	6.65		17.52		24.29		16.29		24.12	
(P-value)	(0.01)		(0.00)		(0.00)		(0.00)		(0.00)	
Kleibergen-Paap rk LM Stat.		2.36				6.91			7.72	
(P-value)		(0.13)				(0.07)			(0.05)	

Notes: The sample includes all 22-62 year olds from MEPS cohorts between 1996 and 2011 who are privately insured. Further, we remove counties in the lowest 10th percentile of total observations. There are a total of 179,259 observations. Standard errors are clustered at the state level.
† Psychiatrists per capita is part of the instrument set for Specification 1, but is part of the control variables for Specifications 2A and 2B as the instrument set is weakened by it's inclusion.
‡ In Specification 2B, discontinuations are coded as *Any Psychotherapy*=0.

A.III.2 Structural Approach to Estimating Treatment Effects

Within our structural model, we attempt to estimate the impact of treatment on mental health by allowing for permanent correlation between unobserved preference and mental health shocks. Such correlation can rationalized the patterns seen in Table A.IV if unhealthy types, who regularly receive poor health shocks also have strong preferences for treatment. Unfortunately, our structural model consistently yields negative treatment effects. This finding is robust to numerous model specifications, including (i) allowing for heterogeneous treatment effects by lagged mental health status and/or unobserved type, (ii) adjusting the number of unobserved types, and (iii) expanding the set of exclusion restrictions (i.e., variables that impact mental health only through treatment choices) from insurance status, initial wages, household income, and employment choices to also include number of psychiatrists and Walmarts per county (i.e., the instruments used in Section A.III.1 above).

The negative treatment effects may be in part to due to a lack of flexibility in the error correlation we allow. The permanent unobserved heterogeneity described in Section 5.2 allows some individuals to *always* receive worse mental health draws *and* have stronger preferences for treatment. This error structure cannot account for temporary correlation in ϵ_t^M and ϵ_t^{rce} that would result from a one-time intra-interview period health shock. Such a shock would be captured if the model allowed for time-varying unobserved heterogeneity.

Given the complexity of the full structural model and the computational resources required to estimate it, we decided to test the importance of time-varying unobserved heterogeneity by estimating a simpler, but closely related system of equations. The auxiliary model has the following features: (i) psychotherapy and prescription drug "probit" equations are specified with exclusion restrictions; (ii) mental health is determined via ordered probit and is written as a function of lagged treatment and lagged mental health; (iii) permanent unobserved heterogeneity common to the three equations is allowed; (iv) initial mental health is allowed to impact the probability of permanent unobserved types, much like in the dynamic structural model, accounting for the endogeneity if this initial condition (Wooldridge, 2005); and (v) estimation is conducted via joint MLE. Importantly, to test whether accounting for time-varying heterogeneity yields positive treatment effects, we allow for time-varying correlation between each equation's unobservables. With this model, we again find negative treatment effects both with and without time-varying unobserved heterogeneity; however, the size of the (negative) treatment effect is roughly halved when we allow for time-varying unobserved heterogeneity.

A.IV CRRA Parameter

When estimated using the procedure described in Section 5, the CRRA parameter, α_0, grows to a large, positive value (e.g., a magnitude of roughly 4). The value corresponds to a flat utility-consumption profile, which suggests that individuals do not consider wage offers in employment decisions, nor prices in treatment decisions. This finding is robust to the number of unobserved types assumed in estimation. Our most basic economic priors, as well as most empirical research, suggest that this relationship is unlikely. Rather, utility should be increasing in numeraire consumption (i.e., individuals prefer more money), albeit at a decreasing rate (i.e., the marginal utility of an extra dollar diminishes as income grows).

Within the context of our model, there are three potential behavioral patterns in the data that would be consistent with, and therefore potentially identify, positive but decreasing marginal returns to consumption. After consistently finding negligible marginal returns to consumption, we looked for evidence of these behaviors in the raw data. A discussion of our findings is below. Some of our analysis is omitted for brevity, but anything not provided is available upon request.

We first ask, *is there any evidence that those with higher household income are less likely to work?*[75] Such a finding would suggest decreasing marginal utility of a dollar. In Table A.VII below, we show household income (in thousands of dollars) by employment status. The first three rows show that mean household income is falling as work intensity increases; however, the pattern does not hold at the median. More importantly, recall two features of the structural model: (i) lagged employment shifts the marginal utility of current employment and (ii) initial employment and household income impact one's permanent unobserved type. As a result of these two features, period t employment decisions are largely explained by lagged employment and one's unobserved type. Only the remaining variation in employment decisions, and the extent to which this variation correlates with household income, is available to aid in the identification of the CRRA parameter. As such, in rows 4–6 we attempt to isolate this variation by showing household income across employment status, only for those changing their employment status. Here, there is no clear, consistent negative relationship between household income and work intensity.

Second, we ask *are those with higher expected wages more likely to work?* Such a finding would suggest positive marginal returns to consumption. Answering this question is complicated by the fact that we only observe wages for those who choose to work. Thus, we estimate simple linear models of log part-time and full-time wages and use the estimated

75. Household income is defined in Table A.I. Note that it excludes an individuals labor income.

Table A.VII: Relationship Between Employment and Household Income

	Mean	Median
Unemployed (N=48,829)	39.4	23.8
Part-Time (N=35,052)	38.5	25.2
Full-Time (N=124,232)	34.6	23.4
Unemployed | Not Unemp. last period (N=6,518)	31.1	19.4
Part-Time | Not PT last period (N=3,871)	33.0	20.3
Full-Time | Not FT last period (N=4,874)	29.2	18.4

Notes: An observation is an interview period; thus, sample statistics are calculated across all 208,113 observations in the estimation sample (54,989 individuals). Household Income is measured in thousands of dollars.

parameters to predict wages independent of employment status.[76] In Table A.VIII below, we show average predicted hourly wages by employment status. The first three rows display the exact wage-employment pattern we expect, i.e., employed individuals have higher expected wages than unemployed individuals. However, again, employment decisions in the structural model are made conditional on lagged employment and one's permanent unobserved type; thus, the CRRA parameter is identified by the relationship between expected wages and employment, *conditional* on these factors. Rows 4-6 show that once we limit our analysis to periods where individuals switch their employment status, there is no longer a relationship between expected wages and employment.[77]

Third, we ask *is there any evidence that those with higher incomes are more likely to purchase medical treatments?* Such a finding would suggest decreasing marginal utility of a dollar. To answer this question, we first group individuals by quantiles of disposable income (see Appendix Section A.II.1) and then examine treatment rates across these groups. We find that the individuals with the lowest disposable incomes consume the most psychotherapy and prescription drugs, i.e., the opposite of the relationship needed for decreasing marginal utility of a dollar. That said, there are many confounding factors in this relationship, includ-

76. The statistical specification of these models exactly matches the wage equations in the structural model without unobserved heterogeneity, though we ignore endogenous selection into employment. Detailed results are available upon request.

77. Note that the expected relationships between (i) household income and employment and (ii) expected wages and employment vanish once we condition on lagged employment. Interestingly, in early versions of the model, we did not allow lagged employment to impact the marginal utility of employment. With lagged employment excluded and no permanent unobserved heterogeneity, the CRRA parameter was estimated to be roughly 0.8. Then, upon adding mass points to the unobserved heterogeneity distribution, the CRRA parameter slowly increased, until it became very large. Ultimately, we realized that the main unobserved factor captured by the permanent unobserved type was stickiness in employment, which lead us include lagged employment as a preference shifter; however, as is shown above, this inclusion complicates the estimation of the CRRA parameter.

Table A.VIII: Relationship Between Employment and Expected Wages

	Part-Time		Full-Time	
	Mean	Median	Mean	Median
Unemployed (N=48,829)	13.8	12.6	15.3	14.6
Part-Time (N=35,052)	19.4	14.3	20.1	14.8
Full-Time (N=124,232)	22.9	19.2	23.5	19.3
Unemployed | Not Unemp. last period (N=6,518)	17.1	13.2	17.7	14.4
Part-Time | Not PT last period (N=3,871)	15.0	12.8	16.1	14.6
Full-Time | Not FT last period (N=4,874)	16.1	13.0	17.1	14.7

Notes: An observation is an interview period; thus, sample statistics are calculated across all 208,113 observations in the estimation sample (54,989 individuals). Wages are predicted from a linear regression of log part-time or full-time wages on mental health, mental health squared, log initial wage, log initial wage squared, high school graduate indicator, college graduate indicator, nonwhite, female, age, age squared, experience, and experience interacted with age.

ing mental health and employment; thus, we also examine the relationship using a regression based approach. Table A.IX reports results from a regression of any psychotherapeutic treatment (i.e., psychotherapy or drugs) on disposable income quintile indicators and a number of controls. Column 1 shows a clear positive relationship between disposable income and treatment. However, much like the discussion above regarding employment, treatment decisions in the structural model are made conditional on lagged treatment and one's permanent unobserved type; thus, the CRRA parameter is identified by the relationship between disposable income and treatment, *conditional* on these factors. In an attempt to isolate this variation, in column 2 we add lagged treatment as a control variable; note that the relationship between disposable income and treatment becomes notably weaker. Finally, in column 3 we add an individual fixed effect, which the permanent unobserved heterogeneity in the structural model is meant to approximate, and the relationship between disposable income and treatment goes away entirely.

In summary, we consider three potential data patterns that would identify positive, decreasing marginal utility for numeraire consumption. We cannot find strong evidence of any of the three. Despite additional structure imposed by the model and controlling for numerous confounding factors, this lack of identifying variation yields large CRRA parameter estimates. Taken at face value, these estimates would suggest that individuals are not sensitive to changes in numeraire good consumption.

Assume for a moment that we accept this result and proceed with our counterfactual analysis. Recall that one important objective of our analysis is to determine which economic costs prevent individuals from using psychotherapy. We perform this analysis in Section 6.2.3 by removing each economic cost and simulating the increase in psychotherapy use.

Table A.IX: Regression of Any Treatment on Disposable Income and Controls

	(1)		(2)		(3)	
	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.
Lagged Any Treatment			0.671	(0.004)		
Income Quintile						
2	0.125	(0.009)	0.040	(0.004)	-0.003	(0.008)
3	0.135	(0.010)	0.044	(0.004)	-0.002	(0.009)
4	0.146	(0.010)	0.048	(0.004)	0.001	(0.009)
5	0.150	(0.010)	0.049	(0.004)	0.006	(0.009)
Lagged Mental Health						
Fair	-0.167	(0.012)	-0.047	(0.007)	-0.007	(0.009)
Good	-0.359	(0.012)	-0.103	(0.007)	-0.017	(0.009)
Very Good	-0.412	(0.012)	-0.119	(0.007)	-0.019	(0.009)
Excellent	-0.442	(0.012)	-0.128	(0.007)	-0.020	(0.009)
Employment						
Part-Time	-0.156	(0.009)	-0.054	(0.004)	-0.013	(0.008)
Full Time	-0.171	(0.009)	-0.059	(0.004)	-0.008	(0.008)
Individual Fixed Effects	N		N		Y	
Observations	208,113		208,113		208,113	

Notes: An observation is an interview period; thus, sample statistics are calculated across all 208,113 observations in the estimation sample (54,989 individuals). The table reports results from a linear regression of any treatment (binary) on the listed controls, as well as sex, race, age, age squared, and education. We report robust standard errors clustered at the individual level.

Importantly, in a model where $\alpha_0 \approx 4$, individual treatment decisions are totally unaffected by a reduction in treatment prices. Moreover, assume wages are found to be increasing in mental health; a policy that encourages mental health treatment could then, in theory, lead to increases in employment by improving mental health, which yields higher wages, which yields selection into employment. However, in a model where $\alpha_0 \approx 4$, such a finding is not possible because marginally higher wages do not elicit greater employment, because people simply do not care about increasing numeraire consumption.

As discussed in Section 5.3, our CRRA parameter estimate is well outside what has been estimated elsewhere in the literature. As a result, we elected to fix the parameter at the more conservative level of 0.95. Note that this value suggests that individuals are *more* responsive to changes in numeraire good consumption (and therefore prices and wage) than our data would suggest. We then estimated our model and conducted counterfactual analysis. This counterfactual analysis suggests that individuals are surprisingly unresponsive to changes in the price of psychotherapy, despite our correction. Moreover, mental health is found to have incredibly small effects on wages; thus, any impact that treatment policies are found to have on employment operate through channels other than wages, meaning our assumption about the CRRA parameter is inconsequential. Given these findings it seems that our adjustment of the CRRA parameter has few substantive effects on our analysis.

A.V Parameter Estimates and Model Fit

Table A.X: Unobserved Heterogeneity Parameter Estimates

		Type 1	Type 2		Type 3		Type 4	
Equation	Param.	Est.	Est.	S.E	Est.	S.E	Est.	S.E
Utility								
therapy	$\mu_k^{U,0}$	0.000	-1.044	0.356	0.103	0.212	-2.126	0.767
rx	$\mu_k^{U,1}$	0.000	-0.461	0.120	-0.246	0.122	-0.573	0.210
pt emp.	$\mu_k^{U,2}$	0.000	1.222	0.149	2.564	0.148	1.849	0.164
ft emp.	$\mu_k^{U,3}$	0.000	0.827	0.215	2.617	0.213	1.873	0.222
Mental health	μ_k^{M}	0.000	2.358	0.064	2.450	0.060	2.498	0.083
Discontinuation	μ_k^{D}	0.000	0.239	0.588	0.530	0.200	0.530	***
PT wage	$\mu_k^{w,1}$	0.000	-0.507	0.031	0.833	0.030	0.246	0.032
FT wage	$\mu_k^{w,2}$	0.000	0.580	0.031	1.578	0.031	1.075	0.031
Psychotherapy cost (any)	$\mu_k^{f,c}$	0.000	0.272	0.552	0.463	0.229	0.463	***
Psychotherapy cost	$\mu_k^{p,c}$	0.000	-0.260	0.406	-0.209	0.199	-0.209	***
Rx cost (any)	$\mu_k^{f,r}$	0.000	0.101	0.227	0.373	0.188	-0.189	0.777
Rx cost	$\mu_k^{p,r}$	0.000	-0.735	0.107	-0.601	0.074	-1.034	0.284
Type prob. param.								
Constant	θ_k^0	0.000	0.627	0.677	-3.793	0.757	-2.312	0.788
Initial wage	θ_k^1	0.000	-0.405	0.342	-2.016	0.295	-0.835	0.318
Initial PT emp.	θ_k^2	0.000	-0.554	1.076	11.000	0.932	3.502	1.023
Initial FT emp.	θ_k^3	0.000	0.335	1.202	12.151	1.030	4.450	1.101
Initial mh	θ_k^4	0.000	1.097	0.091	1.257	0.103	1.190	0.114
Initial psychotherapy	θ_k^5	0.000	-1.139	0.452	-0.234	0.448	-10.000	***
Initial rx	θ_k^6	0.000	-2.561	0.283	-1.476	0.328	-2.026	0.436
female	θ_k^7	0.000	0.900	0.197	-0.024	0.213	-0.090	0.229
Initial age	θ_k^8	0.000	-0.489	0.124	-0.491	0.135	-0.557	0.143
Initial year	θ_k^9	0.000	-0.030	0.022	-0.040	0.024	-0.021	0.026
Mean msa status	θ_k^{10}	0.000	0.634	0.224	1.067	0.251	0.857	0.276
Mean pub. ins. status	θ_k^{11}	0.000	-1.381	0.232	-2.240	0.273	-2.371	0.326
Mean priv. ins. status	θ_k^{12}	0.000	0.387	0.323	2.458	0.334	1.059	0.345
Mean edu	θ_k^{13}	0.000	-0.459	0.153	0.225	0.163	0.216	0.176
Nonwhite	θ_k^{14}	0.000	-0.492	0.211	-0.632	0.240	-0.470	0.255
Mean marriage status	θ_k^{15}	0.000	0.457	0.228	0.401	0.250	0.467	0.265
Mean log(hh inc.)/10	θ_k^{16}	0.000	-0.882	0.536	-1.834	0.549	-1.207	0.575
Mean problem child	θ_k^{17}	0.000	0.240	0.158	0.184	0.177	0.312	0.195
logit probabilities		0.049	0.164		0.739		0.048	

Notes: Permanent unobserved heterogeneity parameters are discussed in Section 5.2. We use a 20% random subsample of Sample C from Appendix Table A.I to estimate the structural model. All Type 1 parameters are normalized to zero. Logit probabilities are calculated as $\frac{exp(\theta_k \Omega_0)}{\sum_{k'=1}^{4} exp(\theta_{k'} \Omega_0)}$. Type 4 individuals are found to be (i) *very* unlikely to be this type conditional on visiting a therapist in $t = 0$ (see θ_4^5, restricted for computational reasons) and (ii) *very* unlikely to visit a therapist in later periods (see $\mu_4^{U,0}$). As a result, several psychotherapy-specific parameters are not identified ($\mu_4^D, \mu_4^{f,c}, \mu_4^{p,c}$). Given that Types 3 and 4 are very similar on other dimensions, we set these Type 4 parameters equal to those of Type 3 in estimation.

Table A.XI: Utility Function Parameter Estimates

		K=1		K=4	
Variable	Parameter	Est.	S.E	Est.	S.E
CRRA	α_0	0.950	***	0.950	***
Any Psychotherapy	$\alpha_{1,0}$	-5.722	0.220	-6.157	0.266
$\times$ c_{t-1}	$\alpha_{1,1}$	2.797	0.093	2.641	0.099
$\times$ r_{t-1}	$\alpha_{1,2}$	1.664	0.106	1.428	0.111
$\times$ PT$_t$	$\alpha_{1,3}$	-0.505	0.122	-0.275	0.179
$\times$ FT$_t$	$\alpha_{1,4}$	-0.488	0.087	-0.312	0.165
$\times$ female	$\alpha_{1,5}$	-0.074	0.093	0.055	0.111
$\times$ age$_t$	$\alpha_{1,6}$	-0.191	0.076	-0.191	0.082
$\times$ msa$_t$	$\alpha_{1,7}$	0.216	0.112	0.227	0.135
Any Rx	$\alpha_{2,0}$	-5.172	0.122	-5.351	0.152
$\times$ c_{t-1}	$\alpha_{2,1}$	1.226	0.102	1.126	0.105
$\times$ r_{t-1}	$\alpha_{2,2}$	4.127	0.047	4.003	0.048
$\times$ PT$_t$	$\alpha_{2,3}$	-0.226	0.047	-0.017	0.073
$\times$ FT$_t$	$\alpha_{2,4}$	-0.349	0.039	-0.131	0.071
$\times$ female	$\alpha_{2,5}$	0.214	0.043	0.288	0.043
$\times$ age$_t$	$\alpha_{2,6}$	0.093	0.046	0.087	0.044
$\times$ msa$_t$	$\alpha_{2,7}$	-0.116	0.043	-0.083	0.045
PT$_t$	$\alpha_{3,0}$	-10.154	0.266	-8.341	0.277
$\times$ PT_{t-1}	$\alpha_{3,1}$	4.268	0.047	3.801	0.054
$\times$ $(5-M_t)$	$\alpha_{3,2}$	-0.088	0.056	-0.105	0.064
$\times$ $(5-M_t)^2$	$\alpha_{3,3}$	-0.052	0.018	0.005	0.022
$\times$ female	$\alpha_{3,4}$	-0.102	0.097	-0.066	0.112
$\times$ age$_t$	$\alpha_{3,5}$	0.031	0.303	-0.032	0.328
$\times$ age$_t$ $\times$ female$_t$	$\alpha_{3,6}$	0.146	0.152	0.082	0.179
$\times$ age$_t^2$	$\alpha_{3,7}$	0.852	0.102	0.547	0.116
$\times$ age$_t^2$ $\times$ female$_t$	$\alpha_{3,8}$	-0.011	0.050	-0.005	0.060
FT$_t$	$\alpha_{4,0}$	-17.284	0.517	-10.969	0.499
$\times$ FT_{t-1}	$\alpha_{4,1}$	5.917	0.043	3.885	0.053
$\times$ $(5-M_t)$	$\alpha_{4,2}$	0.008	0.051	0.032	0.060
$\times$ $(5-M_t)^2$	$\alpha_{4,3}$	-0.118	0.018	-0.069	0.022
$\times$ female	$\alpha_{4,4}$	-0.361	0.079	-0.249	0.097
$\times$ age$_t$	$\alpha_{4,5}$	0.086	0.553	-0.232	0.585
$\times$ age$_t$ $\times$ female$_t$	$\alpha_{4,6}$	0.083	0.121	0.022	0.156
$\times$ age$_t^2$	$\alpha_{4,7}$	1.665	0.183	1.102	0.206
$\times$ age$_t^2$ $\times$ female$_t$	$\alpha_{4,8}$	0.024	0.040	0.018	0.052
$(5-M_t)$	α_5	-0.684	0.290	-1.364	0.319
$(5-M_t)^2$	α_6	-0.017	0.061	-0.106	0.076

Notes: Utility parameters are discussed in Section 4.2.1. We use a 20% random subsample of Sample C from Appendix Table A.I to estimate the structural model. The CRRA parameter is not estimated, which is discussed in Appendix Section A.IV.

Table A.XII: Mental Health Parameter Estimates

		K=1		K=4	
Variable	Parameter	Est.	S.E	Est.	S.E
Constant	$\nu_{0,0}$	7.053	0.070	5.039	0.082
Any Rx	$\nu_{0,1}$	0.712	***	0.726	***
Any Psychotherapy	$\nu_{0,2}$	1.424	***	1.452	***
$(5 - M_{t-1})$	$\nu_{0,3}$	-1.128	0.021	-1.317	0.022
$(5 - M_{t-1})^2$	$\nu_{0,4}$	-0.114	0.007	0.013	0.008
problem child$_t$	$\nu_{0,5}$	-0.259	0.017	-0.299	0.017
female	$\nu_{0,6}$	-0.217	0.064	-0.183	0.063
age$_t$	$\nu_{0,7}$	-0.422	0.074	-0.364	0.074
age$_t^2$	$\nu_{0,8}$	0.077	0.024	0.070	0.024
female $\times$ age$_t$	$\nu_{0,9}$	0.077	0.097	0.027	0.096
female $\times$ age$_t^2$	$\nu_{0,10}$	-0.012	0.032	0.002	0.032
nonwhite	$\nu_{0,11}$	0.071	0.022	0.142	0.024
married$_t$	$\nu_{0,12}$	0.293	0.019	0.189	0.020
high degree, high school$_t$	$\nu_{0,13}$	0.256	0.023	0.210	0.024
high degree, college$_t$	$\nu_{0,14}$	0.533	0.027	0.475	0.029
cut$_1$	ν_1	2.070	0.041	2.380	0.046
cut$_2$	ν_2	4.568	0.046	5.025	0.051
cut$_3$	ν_3	6.307	0.048	6.776	0.052

Notes: Mental health transition parameters are discussed in Section 4.2.3. We use a 20% random subsample of Sample C from Appendix Table A.I to estimate the structural model. The calculation of treatment effect parameters is discussed in footnote 42.

Table A.XIII: Discontinuation Parameter Estimates

		K=1		K=4	
Variable	Parameter	Est.	S.E	Est.	S.E
constant	ω_0	-0.467	0.366	-0.716	0.360
female	ω_1	-0.212	0.190	-0.135	0.191
age$_t$	ω_2	0.004	0.121	0.021	0.123
nonwhite	ω_3	-0.294	0.191	-0.072	0.171
high degree, high school$_t$	ω_4	-0.430	0.189	-0.443	0.169
high degree, college$_t$	ω_5	-0.868	0.245	-0.962	0.236
msa$_t$	ω_6	0.194	0.230	0.083	0.246
D_{t-1}	ω_7	2.354	0.350	2.605	0.351
c_{t-1}	ω_8	-1.033	0.203	-1.010	0.207
year$_t$	ω_9	0.077	0.019	0.075	0.017

Notes: Discontinuation parameters are discussed in Section 4.2.3. We use a 20% random subsample of Sample C from Appendix Table A.I to estimate the structural model.

Table A.XIV: Log Wage Parameter Estimates

		K=1				K=4			
		PT		FT		PT		FT	
Variable	Parameter	Est.	S.E	Est.	S.E	Est.	S.E	Est.	S.E
constant	δ_1^e	0.477	0.048	0.633	0.019	-0.197	0.059	-0.837	0.035
$(5-M_t)$	δ_2^e	-0.032	0.009	-0.002	0.004	-0.028	0.008	-0.008	0.004
$(5-M_t)^2$	δ_3^e	0.003	0.003	-0.002	0.002	0.005	0.003	0.001	0.001
$log(W_0)$	δ_4^e	0.755	0.023	0.653	0.011	0.621	0.035	0.580	0.015
$log(W_0)^2$	δ_5^e	0.011	0.004	0.036	0.002	0.046	0.006	0.053	0.003
$\mathbb{1}_{W_0=0}$	δ_6^e	1.958	0.038	1.902	0.018	2.893	0.050	2.522	0.022
married$_t$	δ_7^e	0.023	0.006	0.018	0.002	0.007	0.006	0.009	0.002
high degree, high school$_t$	δ_8^e	0.053	0.009	0.058	0.003	0.029	0.008	0.044	0.003
high degree, college$_t$	δ_9^e	0.197	0.009	0.129	0.003	0.105	0.008	0.094	0.003
nonwhite	δ_{10}^e	-0.012	0.007	0.000	0.002	-0.008	0.008	0.003	0.002
female	δ_{11}^e	-0.038	0.007	-0.024	0.002	-0.016	0.006	-0.024	0.002
age$_t$	δ_{12}^e	0.043	0.015	0.004	0.006	0.041	0.018	-0.003	0.006
age$_t^2$	δ_{13}^e	0.000	0.005	0.000	0.002	-0.002	0.005	0.002	0.002
female $\times$ age$_t$	δ_{14}^e	0.000	1.000	0.000	1.000	0.000	1.000	0.000	1.000
K_t	δ_{15}^e	0.044	0.010	0.003	0.002	0.031	0.009	0.001	0.002
$K_t\times$ age$_t$	δ_{16}^e	-0.025	0.005	-0.002	0.001	-0.023	0.005	-0.002	0.001
variance	$\sigma^{w,e}$	0.350	0.001	0.234	0.000	0.282	0.001	0.197	0.000

Notes: Wage parameters are discussed in Section 4.2.1. We use a 20% random subsample of Sample C from Appendix Table A.I to estimate the structural model.

Table A.XV: Price Parameter Estimates

		K=1				K=4			
		Rx		Psychotherapy		Rx		Psychotherapy	
Variable	Parameter	Est.	S.E	Est.	S.E	Est.	S.E	Est.	S.E
Non-zero Price constant	η_1^x	4.062	0.709	-0.680	0.591	3.519	0.829	-0.803	0.422
female	η_1^x	-0.052	0.109	0.165	0.162	-0.061	0.112	0.161	0.168
age_t	η_1^x	-0.069	0.076	-0.117	0.114	-0.080	0.083	-0.055	0.096
nonwhite	η_1^x	-0.462	0.115	-0.209	0.184	-0.442	0.119	-0.208	0.204
high degree, high school_t	η_1^x	0.217	0.111	0.372	0.211	0.205	0.113	0.365	0.216
high degree, college_t	η_1^x	0.948	0.219	1.051	0.254	0.952	0.221	0.992	0.266
msa_t	η_1^x	-0.165	0.128	0.360	0.208	-0.218	0.133	0.375	0.215
year_t	η_1^x	-0.119	0.051	0.027	0.050	-0.088	0.058	0.008	0.042
pub. ins._t	η_1^x	-2.542	0.685	-0.714	0.491	-1.960	0.791	-0.739	0.461
priv. ins._t	η_1^x	0.047	0.700	0.952	0.491	0.344	0.785	0.622	0.481
pub. $\text{ins.}_t \times \text{year}_t$	η_1^x	0.097	0.051	-0.069	0.053	0.065	0.058	-0.049	0.046
priv. $\text{ins.}_t \times \text{year}_t$	η_1^x	0.103	0.056	0.026	0.055	0.077	0.062	0.044	0.051
Log(Price)									
constant	γ_1^x	5.204	0.193	4.537	0.700	5.620	0.200	4.594	0.722
female	γ_1^x	-0.059	0.049	0.020	0.141	-0.034	0.049	0.042	0.142
age_t	γ_1^x	-0.016	0.030	-0.112	0.093	-0.051	0.031	-0.114	0.095
nonwhite	γ_1^x	-0.099	0.064	-0.218	0.192	-0.177	0.065	-0.213	0.195
high degree, high school_t	γ_1^x	0.116	0.058	0.075	0.182	0.127	0.058	0.123	0.192
high degree, college_t	γ_1^x	0.199	0.076	0.434	0.209	0.200	0.075	0.467	0.220
msa_t	γ_1^x	0.043	0.051	0.514	0.176	0.083	0.051	0.530	0.183
year_t	γ_1^x	-0.055	0.016	0.002	0.068	-0.050	0.016	0.001	0.069
pub. ins._t	γ_1^x	0.080	0.187	-0.865	0.607	-0.138	0.189	-0.826	0.630
priv. ins._t	γ_1^x	-0.900	0.187	0.379	0.612	-0.723	0.187	0.434	0.625
pub. $\text{ins.}_t \times \text{year}_t$	γ_1^x	-0.078	0.017	0.061	0.068	-0.085	0.017	0.052	0.070
priv. $\text{ins.}_t \times \text{year}_t$	γ_1^x	0.048	0.017	-0.016	0.068	0.042	0.017	-0.015	0.070
variance	$\sigma^{p,x}$	1.445	0.018	1.243	0.048	1.423	0.018	1.239	0.048

Notes: Price parameters are discussed in Section 4.2.1. We use a 20% random subsample of Sample C from Appendix Table A.I to estimate the structural model.

Table A.XVI: Terminal Value Function Parameter Estimates

		K=1		K=4	
Variable	Parameter	Est.	S.E	Est.	S.E
$(5 - M_{T+1})$	$\chi_{0,0}$	-0.194	1.385	-1.636	1.269
$\times$ age$_t$	$\chi_{0,1}$	-0.227	1.657	1.023	1.465
$\times$ age$_t^2$	$\chi_{0,2}$	0.064	0.486	-0.309	0.435
$(5 - M_{T+1})^2$	$\chi_{1,0}$	-0.515	0.427	-0.555	0.370
$\times$ age$_t$	$\chi_{1,1}$	-0.269	0.545	-0.562	0.453
$\times$ age$_t^2$	$\chi_{1,1}$	0.055	0.158	0.143	0.132
K_{T+1}	$\chi_{2,0}$	12.733	0.557	5.665	0.488
$\times$ age$_t$	$\chi_{2,1}$	0.439	0.608	0.810	0.646
$\times$ age$_t^2$	$\chi_{2,2}$	-1.909	0.197	-1.316	0.221
c_T	χ_3	0.892	0.129	0.853	0.147
r_T	χ_4	0.476	0.200	0.360	0.225

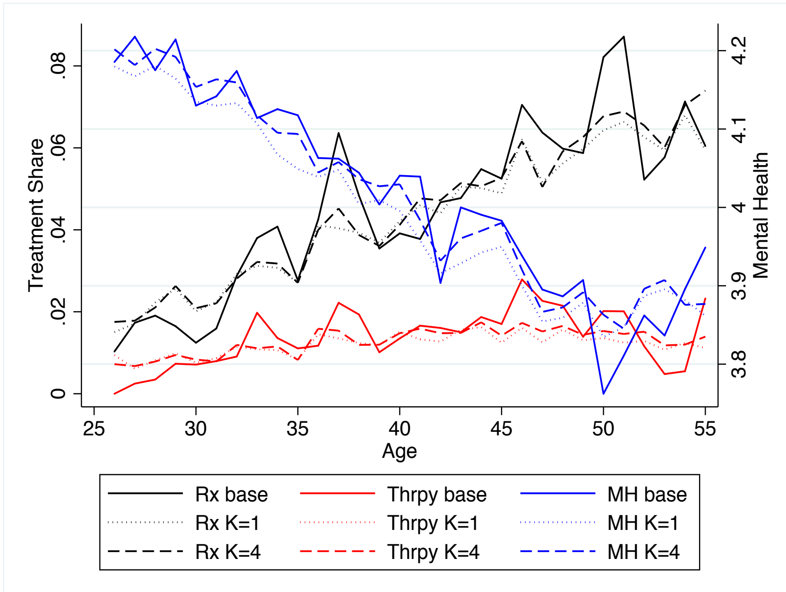
Notes: The terminal value function is discussed in Section 4.2.5. We use a 20% random subsample of Sample C from Appendix Table A.I to estimate the structural model.

Table A.XVII: Model Fit

Variable	Est. Sample Mean	Sim, K=1 Mean	Sim, K=1 S.E.	Sim, K=4 Mean	Sim, K=4 S.E
Treatment					
Any Psychotherapy	0.020	0.017	0.000	0.019	0.000
if $c_{t-1} = 1$	0.519	0.428	0.003	0.477	0.003
if $r_{t-1} = 1$	0.192	0.157	0.001	0.177	0.002
if $M_{t-1} = 5$	0.003	0.008	0.000	0.006	0.000
if $M_{t-1} = 4$	0.009	0.013	0.000	0.012	0.000
if $M_{t-1} = 3$	0.026	0.023	0.000	0.027	0.000
if $M_{t-1} = 2$	0.110	0.052	0.001	0.079	0.001
if $M_{t-1} = 1$	0.197	0.098	0.001	0.122	0.003
Share with $p_t^c = 0$	0.509	0.475	0.003	0.509	0.004
$p_t^c = 0 \mid p_t^c > 0$	346.384	346.691	4.999	357.789	6.087
$D_t \mid M_t = 1$	0.348	0.432	0.005	0.428	0.004
Any Rx	0.073	0.071	0.000	0.072	0.000
if $c_{t-1} = 1$	0.699	0.641	0.004	0.664	0.003
if $r_{t-1} = 1$	0.729	0.691	0.001	0.705	0.002
if $M_{t-1} = 5$	0.023	0.046	0.000	0.037	0.000
if $M_{t-1} = 4$	0.050	0.061	0.000	0.056	0.000
if $M_{t-1} = 3$	0.104	0.089	0.001	0.092	0.001
if $M_{t-1} = 2$	0.296	0.159	0.001	0.221	0.002
if $M_{t-1} = 1$	0.467	0.225	0.003	0.322	0.003
Share with $p_t^r = 0$	0.105	0.096	0.001	0.108	0.001
$p_t^c = 0 \mid p_t^r > 0$	190.077	193.235	1.394	195.062	1.380
Employment					
PT	0.163	0.165	0.000	0.165	0.001
if $PT_{t-1} = 1$	0.890	0.845	0.001	0.860	0.001
if $FT_{t-1} = 1$	0.008	0.006	0.000	0.012	0.000
if $M_{t-1} = 5$	0.160	0.167	0.001	0.168	0.001
if $M_{t-1} = 4$	0.168	0.166	0.001	0.168	0.001
if $M_{t-1} = 3$	0.169	0.168	0.001	0.169	0.001
if $M_{t-1} = 2$	0.147	0.153	0.001	0.135	0.001
if $M_{t-1} = 1$	0.105	0.112	0.002	0.076	0.002
Mean: W_t^1	20.436	20.109	0.032	20.344	0.044
SD: W_t^1	16.994	16.511	0.065	16.862	0.072
FT	0.602	0.594	0.001	0.605	0.001
if $PT_{t-1} = 1$	0.040	0.017	0.000	0.048	0.000
if $FT_{t-1} = 1$	0.959	0.934	0.000	0.944	0.000
if $M_{t-1} = 5$	0.672	0.648	0.001	0.671	0.001
if $M_{t-1} = 4$	0.644	0.620	0.001	0.637	0.001
if $M_{t-1} = 3$	0.540	0.546	0.001	0.553	0.001
if $M_{t-1} = 2$	0.302	0.414	0.002	0.350	0.002
if $M_{t-1} = 1$	0.139	0.279	0.003	0.156	0.002
Mean: W_t^0	24.163	23.963	0.014	24.221	0.016
SD: W_t^0	15.513	16.205	0.017	16.262	0.024
Mental Health					
$MH_t = 5$	0.376	0.379	0.000	0.378	0.000
$MH_t = 4$	0.302	0.295	0.000	0.297	0.000
$MH_t = 3$	0.249	0.249	0.000	0.250	0.000
$MH_t = 2$	0.059	0.061	0.000	0.059	0.000
$MH_t = 1$	0.014	0.017	0.000	0.015	0.000

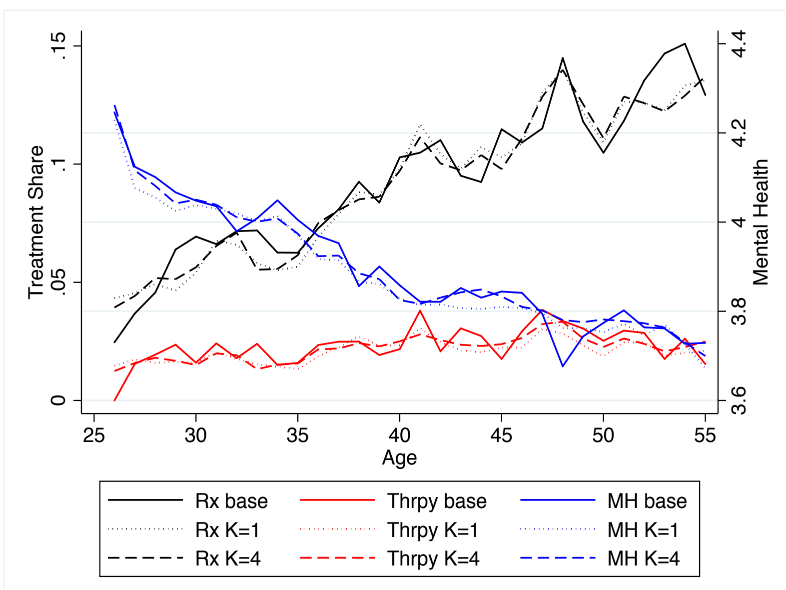
Notes: The simulated data are constructed by sampling from the joint error distribution, permanent unobserved heterogeneity distribution, and estimated parameter covariance matrix 50 times for each individual, then forward simulating four periods from initial conditions. All moments are calculated over all four simulation periods.

Figure A.II: Treatment and Mental Health Fit Over Female Lifecycle



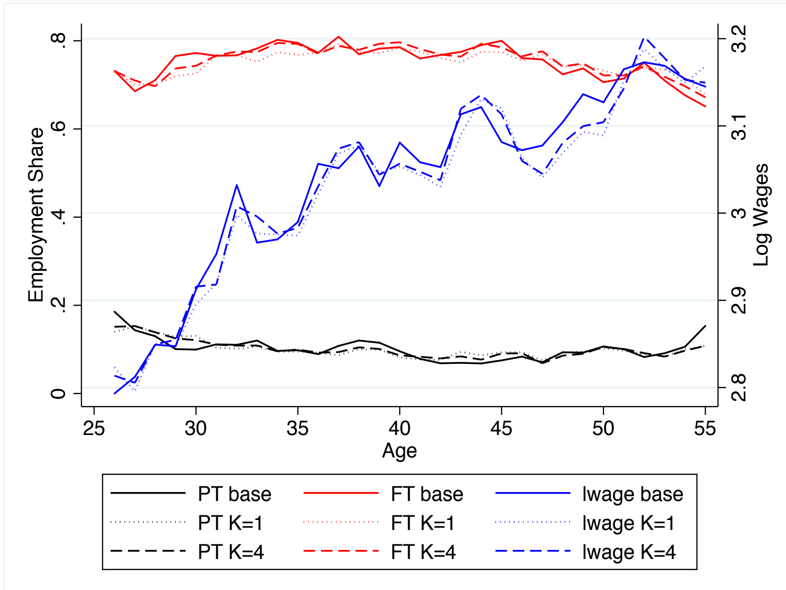
Notes: The simulated data are constructed by sampling from the joint error distribution, permanent unobserved heterogeneity distribution, and estimated parameter covariance matrix 50 times for each individual, then forward simulating four periods from initial conditions. All moments are calculated over all four simulation periods.

Figure A.III: Treatment and Mental Health Fit Over Male Lifecycle



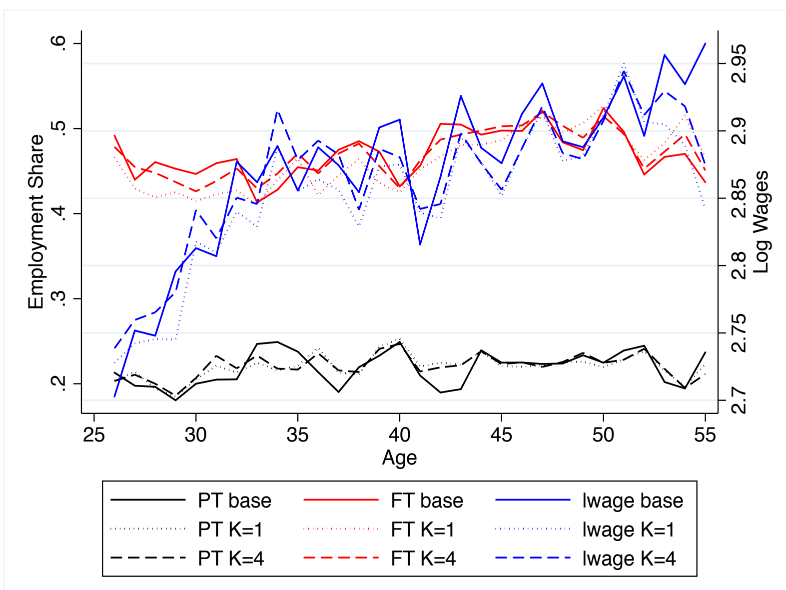
Notes: The simulated data are constructed by sampling from the joint error distribution, permanent unobserved heterogeneity distribution, and estimated parameter covariance matrix 50 times for each individual, then forward simulating four periods from initial conditions. All moments are calculated over all four simulation periods.

Figure A.IV: Employment and Wages over Female Lifecycle



Notes: The simulated data are constructed by sampling from the joint error distribution, permanent unobserved heterogeneity distribution, and estimated parameter covariance matrix 50 times for each individual, then forward simulating four periods from initial conditions. All moments are calculated over all four simulation periods.

Figure A.V: Employment and Wages over Male Lifecycle



Notes: The simulated data are constructed by sampling from the joint error distribution, permanent unobserved heterogeneity distribution, and estimated parameter covariance matrix 50 times for each individual, then forward simulating four periods from initial conditions. All moments are calculated over all four simulation periods.

Table A.XVIII: Assigned Psychotherapy in First Period, Healthy Types

	t=1			t=2			t=3			t=4		
	Base	Sim	% Δ	Base	Sim	% Δ	Base	Sim	% Δ	Base	Sim	% Δ
Type 3, Women												
Psychotherapy	0.018	1.000	55.943	0.014	0.113	7.079	0.012	0.030	1.497	0.011	0.014	0.307
Medication	0.073	0.103	0.405	0.074	0.159	1.157	0.071	0.129	0.816	0.066	0.098	0.488
Avg MH	4.115	4.115	0.000	4.065	4.318	0.062	4.028	4.205	0.044	4.003	4.116	0.028
Working PT	0.335	0.332	-0.009	0.325	0.325	0.001	0.309	0.311	0.006	0.265	0.266	0.004
Working FT	0.627	0.619	-0.013	0.649	0.644	-0.007	0.648	0.646	-0.004	0.588	0.586	-0.003
Avg Wage	22.030	22.051	0.001	21.954	22.011	0.003	21.964	22.001	0.002	22.126	22.147	0.001
Type 3, Men												
Psychotherapy	0.007	1.000	139.440	0.007	0.075	9.674	0.006	0.014	1.235	0.007	0.008	0.146
Medication	0.029	0.042	0.473	0.030	0.074	1.480	0.031	0.055	0.795	0.033	0.045	0.353
Avg MH	4.174	4.174	0.000	4.123	4.353	0.056	4.090	4.235	0.035	4.073	4.156	0.020
Working PT	0.098	0.098	0.009	0.094	0.096	0.021	0.093	0.095	0.014	0.088	0.089	0.016
Working FT	0.877	0.870	-0.008	0.892	0.890	-0.003	0.885	0.883	-0.002	0.816	0.815	-0.002
Avg Wage	25.591	25.640	0.002	25.440	25.492	0.002	25.393	25.420	0.001	25.612	25.607	0.000
Type 4, Women												
Psychotherapy	0.001	1.000	841.667	0.001	0.015	12.634	0.001	0.002	0.494	0.001	0.001	0.057
Medication	0.042	0.053	0.284	0.036	0.084	1.305	0.035	0.057	0.653	0.037	0.048	0.281
Avg MH	4.173	4.173	0.000	4.107	4.345	0.058	4.075	4.216	0.035	4.050	4.132	0.020
Working PT	0.254	0.238	-0.060	0.279	0.274	-0.019	0.266	0.266	0.000	0.205	0.206	0.004
Working FT	0.370	0.348	-0.060	0.424	0.416	-0.020	0.430	0.425	-0.012	0.356	0.354	-0.005
Avg Wage	18.339	18.382	0.002	18.365	18.437	0.004	18.442	18.474	0.002	18.605	18.629	0.001
Type 4, Men												
Psychotherapy	0.001	1.000	1029.000	0.001	0.011	10.714	0.001	0.001	0.268	0.001	0.001	-0.091
Medication	0.015	0.019	0.307	0.015	0.039	1.625	0.016	0.025	0.515	0.021	0.024	0.135
Avg MH	4.152	4.152	0.000	4.126	4.348	0.054	4.111	4.235	0.030	4.103	4.172	0.017
Working PT	0.143	0.137	-0.047	0.151	0.148	-0.018	0.145	0.143	-0.009	0.118	0.117	-0.004
Working FT	0.681	0.657	-0.035	0.729	0.720	-0.012	0.718	0.714	-0.006	0.612	0.609	-0.004
Avg Wage	21.174	21.292	0.006	20.948	21.006	0.003	20.915	20.935	0.001	21.122	21.124	0.000

Notes: We construct the simulated data by first using the estimated posterior unobserved type probabilities to simulate a type for each individual in Sample C referenced in Table A.I. We then randomly select 2,500 individuals of each type. For this sample of 10,000 individuals, we sample from the joint error distribution and estimated parameter covariance matrix 50 times for each individual. We then forward simulate four periods from the observed initial conditions. In this table, we compare choices and outcomes in each of the four simulation periods for Types 2 and 3, using the baseline model and an alternative model where individuals are assigned to psychotherapy in period $t = 1$.

Table A.XIX: Counterfactual Policy Simulations, Healthy Types

	Base	Policy 1		Policy 2		Policy 3		Policy 4	
	Level	Level	% Δ	Level	% Δ	Level	% Δ	Level	%Δ
Type 3, Women									
Psychotherapy	0.014	0.014	0.023	0.023	0.701	0.018	0.357	0.034	1.477
Medication	0.071	0.071	0.001	0.074	0.050	0.072	0.021	0.079	0.108
Avg MH	4.022	4.022	0.000	4.037	0.004	4.024	0.001	4.047	0.006
Working PT	0.308	0.308	0.000	0.309	0.001	0.308	0.000	0.309	0.001
Working FT	0.628	0.628	0.000	0.628	0.000	0.628	0.001	0.628	0.000
Avg Wage	22.015	22.015	0.000	22.019	0.000	22.017	0.000	22.021	0.000
Type 3, Men									
Psychotherapy	0.007	0.007	0.022	0.013	0.867	0.009	0.381	0.019	1.725
Medication	0.031	0.031	0.002	0.032	0.031	0.031	0.014	0.033	0.063
Avg MH	4.087	4.087	0.000	4.095	0.002	4.088	0.000	4.101	0.003
Working PT	0.093	0.093	0.000	0.093	-0.001	0.093	0.000	0.093	0.000
Working FT	0.868	0.868	0.000	0.868	0.000	0.868	0.000	0.868	0.000
Avg Wage	25.507	25.507	0.000	25.508	0.000	25.506	0.000	25.508	0.000
Type 4, Women									
Psychotherapy	0.001	0.001	0.016	0.002	0.621	0.002	0.228	0.003	1.093
Medication	0.038	0.038	0.000	0.038	0.006	0.038	0.002	0.038	0.008
Avg MH	4.065	4.065	0.000	4.066	0.000	4.065	0.000	4.067	0.000
Working PT	0.251	0.251	0.000	0.251	0.000	0.251	0.000	0.251	0.001
Working FT	0.395	0.395	0.000	0.395	0.000	0.395	0.000	0.395	0.000
Avg Wage	18.432	18.432	0.000	18.432	0.000	18.432	0.000	18.433	0.000
Type 4, Men									
Psychotherapy	0.001	0.001	0.009	0.001	0.689	0.001	0.302	0.002	1.316
Medication	0.017	0.017	0.000	0.017	0.004	0.017	0.002	0.017	0.008
Avg MH	4.110	4.110	0.000	4.111	0.000	4.110	0.000	4.112	0.000
Working PT	0.139	0.139	0.000	0.139	0.000	0.139	0.000	0.139	0.000
Working FT	0.685	0.685	0.000	0.685	0.000	0.685	0.000	0.685	0.000
Avg Wage	21.034	21.034	0.000	21.035	0.000	21.035	0.000	21.035	0.000

Notes: We construct the simulated data by first using the estimated posterior unobserved type probabilities to simulate a type for each individual in Sample C referenced in Table A.I. We then randomly select 2,500 individuals of each type. For this sample of 10,000 individuals, we sample from the joint error distribution and estimated parameter covariance matrix 50 times for each individual. We then forward simulate four periods from the observed initial conditions. In this table, we compare choices and outcomes for Type 2 and 3 individuals using the baseline model and five alternative models representing the following policy interventions. Policy 1: Remove the financial cost of psychotherapy. Policy 2: Remove the possibility of discontinuation. Policy 3: Remove the employment/time cost of psychotherapy (i.e., $\alpha_{1,3} = \alpha_{1,4} = 0$). Policy 4: Remove all non-utility psychotherapy costs (i.e., combine Policies 1, 2, and 3). Sample moments are aggregated across all four simulated periods.

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