Spousal Peer Effects in Specialty Behavioral Health Services Use:
Do Spillovers Vary by Gender, Subscriber Status and Sexual Orientation?

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ABSTRACT

This paper fills gaps in the literature on spousal peer effects in mental health outcomes by investigating spousal spillover effects in a potential mediator, health investment decisions. We relate the subject's behavioral healthcare use to her partner's lagged decision, and exploit the rich set of characteristics available (including the subject's past use and diagnoses) to control for assortative mating. Given that spillover effects may be asymmetric, we stratify analyses by subscriber's status, gender and couple type. Results show positive significant spousal peer effects in behavioural healthcare use, driven primarily by individual psychotherapy visits. Spillover effects are stronger for females than males in heterosexual couples and for males in same-sex vs. heterosexual couples. Estimates are robust to various sensitivity analyses. Spousal spillover effects in individual psychotherapy use represent up to 76% of the impact of depression for female in heterosexual couples and up 91% for men who are in same-sex couples.

Keywords: Health care use, Spillovers, Mental Health, Household behaviour.

JEL codes: D19, I12, I31, J15

1- Introduction

Poor mental health has biological and psychological factors, as well as a social component leading to spillovers that can affect family and friends[1]. A growing body of the literature is investigating different aspects of mental health spillovers, mostly focusing on documenting spillovers in mental health outcomes (Eisenberg et al., 2013; Mervin and Frijters, 2014; Fletcher 2009; Fletcher and Marksteiner, 2017). Nevertheless, there is very scant evidence on the mechanisms via which these spillover effects may occur. Particularly, the possibility that these spillovers flow through health investments remains an unexplored territory.[2] It is well known that health investments are the result of individual choices (Grossman, 1972), but these decisions are rarely made in a vacuum (Manski, 1993). Specifically, empirical evidence suggests that social interactions with friends and family play an essential role in shaping people's decisions in general and health-related choices in particular. As a result, one cannot analyze individual health-seeking choices without accounting for the social component that shapes such decisions. This paper is motivated by the burgeoning literature on mental health spillovers and the scant evidence on the pathways through which these spillovers occur. Its objective is to go beyond a black box approach to spousal mental health spillovers and uncover a potential channel through which these spillovers flow, namely, behavioral healthcare use.[3]

Compared to other health care services, the decision to use behavioral health care services is complex. The ambiguity around the need for such services, the uncertainty around their long-term beneficial effects, and the social stigma associated with their utilization decrease the incentives to use them, yet all of these factors might be influenced by peer effects. This paper therefore seeks to quantify spousal peer effects in behavioral health care use, primarily individual psychotherapy use, because it is a dimension of health investments in which spillovers are most likely to occur. The paper focuses on peer effects among spouses (including domestic partners) because spouses are close and spend a substantial time together, so spillovers between them are likely to be important.

[1] See for instance Carell et al., (2011), Clark and Etile (2011), Fortin and Yazbeck (2015), Palali and Van Ours (2017) among others.
[2] One recent exception is Golberstein et al. (2016) who looks as peer effects in health seeking behaviour for mental health related issues using randomly assigned pairs of roommates.
[3] It is important to note that in this paper we use spouses and domestic partners interchangeably

Indeed, spouses are more likely to share information about their behavioral health care use compared to friends or roommates. This information can either be shared explicitly through conversations or implicitly by observation. Thus, if there are spillover effects in behavioral health care use, they are likely to be present among partners. We therefore believe that investigating the presence of peer effects among partners is crucial.

Estimating spousal spillover in behavioral health care use is policy-relevant as it provides evidence regarding a factor that can be modified via targeted interventions. While documenting the presence of spillovers in mental health outcomes has been a crucial first step, from a policy perspective, the next step is to document spillovers in a dimension that is amenable to change. We therefore rely on the established links between behavioral health care use and mental health (see for instance Brown et al.,2005; Dimidjian et al., 2006; Lang 2013) and investigate behavioral health care use as a potential channel through which spillover effects on mental health may occur. Furthermore, it is important to emphasize that these health-seeking decisions may generate additional health benefits to a broader reference group than the one we consider in this paper: namely, to families and society in general. In impact evaluations and cost-effectiveness analysis of health interventions assume zero spillover effects; thus, the importance of the beneficial (detrimental) impact of specific health policies may be underestimated when we dismiss the presence of such spillovers.

To analyze the presence of spousal spillover effects, we will use a simple linear model in which we allow for spousal behavioral health care use to be interdependent and control for the respondent's prior use as well as prior diagnosis and a rich set of controls. The identification of spousal peer effects in behavioral health care utilization, as in the case of any other type of peer effects, raises well-known empirical challenges. Specifically, the presence of correlation may not necessarily be evidence of the presence of spillovers. Indeed, four factors may account for the presence of raw correlations in behavioral health care use between two partners: (1) assortative mating, (2) shared living environment (these two are also known as correlated effects), (3) simultaneity of decision and choices, and (4) true spillover effect from one spouse to the other.

Assortative matching arises because individuals usually choose to partner with those with whom they share commonalities (including but not limited to specific mental health disorders). This matching can occur based on observed as well as unobserved characteristics. In this paper, we account for matching based on observed characteristics using a rich set of controls including prior diagnosis. We partially account for matching on unobservable variables by including the person's prior use. To account for correlated effects related to shocks at the state or year level, we introduce state and year fixed effects. Finally, to account for the simultaneity of partners' behavioral health service use, we exploit information on a spouse's lagged specialty behavioral health services utilization and use it as an explanatory variable in a reduced-form equation. We also estimate an instrumental variable specification in which lagged behavioral health care services utilization is used as an instrument as a robustness check.

This paper's contribution to the literature is fourfold. First, it bridges the literature on mental health spillover effects within families with the literature on spillovers in health care use to examine the presence of spousal spillover effects in behavioral health care utilization. To our best knowledge, we are the first to examine spousal peer effects in health care use. Second, it contributes to the literature on spousal mental health spillovers by examining a dimension that was not examined before and uncovers a potential mechanism through which these spillover effects occur. Many papers investigated the presence of spousal spillovers in happiness, mental health, and life satisfaction, yet not one investigated the presence of spillovers in mental health investments. Third, by focusing on behavioral health care use and exploiting the richness of our claim data, this paper sidesteps all measurement error issues and biases related to self-reported health care use. More specifically, we have access to a family ID that allows us to link beneficiaries who are in the same family (including domestic partners) and therefore, construct family consumption patterns. Family ID variables are not usually available with claims databases, so our database may be unique. More typically, researchers can access service use data for families only through surveys. However, very few surveys have comprehensive information on mental health services use. Thus, the availability of this information allows us to investigate the presence of spousal spillover in behavioral health care use, a question that we normally would have not been able to answer with available survey data. Fourth, this paper is the first to provide evidence of spillover effects on sexual minorities. Generally speaking, sexual minorities have worse health outcomes (Conron et al., 2010; Perales, 2016; Booker et al., 2017; Saxby et al., 2020) and are in worse mental health than their heterosexual counterparts (Frederiksen-Goldsen et al., 2013; Lea et al., 2014). Because of their sexual orientation, these minority groups are exposed to stressors that are of a different nature (e.g., discrimination, harassment) and intensity than those facing heterosexual couples. Given the large sample size of claims data, we can conduct analyses stratifying by whether the couple is same-sex or heterosexual, whereas most survey data do not have enough sample size to power estimations on sexual minorities. Thus, with our claims data, based on family identifiers and information on relationship to subscriber, we can estimate spillover effects for same-sex couples and test whether spillover effects are different from those estimated for heterosexual couples.

Our empirical evidence suggests that there are spousal spillover effects in individual psychotherapy use. These effects are larger for females than for males and are larger for males with a same-sex partner than for males with a different-sex partner. These spillover effects are of non-negligible magnitude, between 67% and 76% as large as the impact of one's own depression on individual psychotherapy for females and between 56% and 91% for males. Although smaller in relative magnitude, these significant spillover effects are also paralleled in medication management visits and overall behavioural health care use.

The remaining of this paper is organized as follows. Section 2 provides a brief review of the related literature. Section 3 discusses the data and the empirical framework. Section 4 presents and discusses the results. Section 5 concludes.

2- Literature Review

Table 1 summarizes the literature looking at spillover effects for three types of outcomes: mental health outcomes, mental health services use, and other types of health care use. The studies can be further divided into those looking at spillover between spouses and those examining spillover between more loosely connected individuals. To our knowledge, no previous analyses have been conducted to examine spillover between spouses in behavioral health services use, which is the goal of our study.

The literature focusing on mental health peer effects analyzed the presence of peer effects in the setting of different reference groups (namely roommates, friends, and family members) as well as using different types of mental health measures. From a general perspective, the conceptual frameworks used for the analysis of the different reference groups are reasonably similar; however, the empirical results reflect a lack of consensus, with more evidence in favour of the presence of spillovers. While Eisenberg et al. (2013), seem to indicate that peer effects are mainly due to selection (homophily), another part of the literature (Fletcher, 2009; Mervin and Frijters, 2015; Powdthavee, 2009) seems to provide evidence that supports the presence of such peer effects among family members. One possible reason behind the discrepancy in these findings is the difference in the type of reference group considered. Specifically, the reason why Eisenberg et al. (2013) do not find evidence of the presence of peer effects may reside in the fact that they are looking at a randomized social network where students who have no affinity are randomly allocated into a room. This random allocation of social networks, and thus interactions, allows the authors to avoid spurious spillovers arising from the homophily issue. However, a randomized social network does not necessarily reflect how normal social interactions occur. Due to perceived stigma, mental health issues are typically not shared or made salient except with close friends. Thus, it may be the case that even if a roommate is having mental health issues, she will not share or show any distress signs if she does not feel close enough to her roommate. Also, it may be the case that a roommate may be incapable of reading/interpreting her roommate's mental health distress even if she has a close interaction with her and thus, will not be affected. While we acknowledge the importance of randomization of the network, it is essential to keep in mind that a randomized social network may not be able to replicate or reflect the same type of relationship in a natural social setting. Also, these randomized network settings are of limited applicability, particularly in a family setting.

The interdependence of individuals' decisions is at the root of the peer effect literature. While documenting the presence of peer effects in health outcomes is valuable, documenting the presence of peer effects in health investment decisions is crucial as it provides valuable information that can help in tailoring public health policies and interventions. As such, this paper is related to a second strand of literature that analyses the presence of peer effects in health investments or, more specifically, in our case: health care services utilization. While the mechanisms underlying the interdependence in choices to seek care are sometimes unclear (i.e., whether it is an information effect or a decrease in the stigma associated with certain types of care), its presence cannot be denied.

Most of the literature that investigated the presence of peer effects in health-seeking behaviour implicitly focused on the interdependence that flows through information effect. In their seminal paper, Aizer and Currie (2004) investigated the presence of social network effects in the use of publicly funded prenatal care among pregnant women who belong to the same ethnic group and who live in the same zip code area.[4] The authors use a linear-in-means model and identify the presence of peer effects using leave-out strategy and lagged values for prenatal care use of the reference group. Their results seem to suggest that take-up is highly correlated within groups. In the same spirit, Deri (2005) investigates the presence of peer effects in health services use among immigrants in Canada. In her paper, Deri (2005) identifies peer effects in decisions to utilize different health services among immigrants; she exploits regional variations and language group variations to implement an instrumental variable approach. She finds evidence of significant and robust social network effects, which is in line with the literature that analyzes peer effects in the context of health outcomes. Motivated by the importance of the role of information networks and their impact on health care utilization, Devillanova (2008) focused on undocumented immigrants' health utilization in Italy and assessed the presence of network effects in providing information that facilitates primary health care utilization. She defines networks as a strong social tie and considers any person referred by a strong social tie as being referred by a network. Empirical evidence suggests that there are high social network effects and that the presence of social networks decreases the time to first consultation.

In the same spirit as Aizer and Currie (2004), Deri (2005) and Davillanova (2008) but using a different network definition (i.e., a different reference group), Golberstein et al. (2016) investigates the presence of causal spillovers in mental health services use among first-year college students. Exploiting the advantage of randomized allocation of roommates and hallmates, Golberstein et al. (2016) sidestep identification issues related to endogenous network formation. Also, by focusing on first-year college students, they can overcome the issues related to a shared environment. They

[4] It is important to note that the authors also use alternative definitions of reference group.

find no evidence of spillover effects when they focus on roommates. However, when they focus on hallmates, the authors find that a peer's service use increases one's use of mental health services and that this effect is driven by those who have had prior use. While it may seem counterintuitive to find effects at the hallmate level and none at the roommate level, the authors argue that interactions with hallmates may be a closer reflection of the social interactions. Thus, a person may not be close friends with a roommate but still be friends with many hallmates. In addition, the authors raise the point that interaction with hallmates allows spillovers to arise from more than one peer, thus the spillovers are likely to be more present as the person is exposed to more than one person. In addition to the presence of peer effect at the hallmate level, the authors find evidence that this spillover effect is informational.

This paper is at the intersection of two literatures. It finds its basis in the literature on spousal mental health peer effects and health care use peer effects literature discussed above. It is closely related to the work of Golberstein et al. (2016) but remains distinct from it in two respects. First, we focus on a unit of great importance for public policy: the family. Second, unlike Golberstein et al. (2016) we focus on observational non-experimental social ties using information on family coverage from Optum®, United Health Group insurance claims data. This allows us to assess the presence of spillovers in a setting where social links of reference group are naturally formed and thus, reflect the ties observed in society. Use of claims data allows us to sidestep biases related to self-reported health care commonly present in survey data. We therefore assess spousal spillovers based on bona fide behavioral health care use. An additional originality of our work resides in our capacity of linking family members (and their use) based on unique family ID. Also, given that we have access to family ID, we can investigate whether spousal spillovers for same-sex couples are different from spousal spillovers for heterosexual couples. Indeed, it is difficult to analyse the presence of peer effects for same-sex couples because most surveys do not have enough observations to power the estimations for sexual minorities. As such, this paper bridges the literature that investigates the presence of mental health peer effects within families and the one that investigates the presence of peer effects in health services. Thus, by looking at the presence of peer effect in behavioral health services use among partners, we fill the gap in the literature on the interdependence of mental health investment choices among partners.[5] We are also the first paper that investigate whether these effects are heterogeneous by couple type.

3- Data and Empirical Framework

3.1 Sources of Data

Our study is based on three linked administrative databases from 2008-2014, provided to us by the behavioral health division of Optum®, United Health Group, which is one of the largest managed behavioral health organizations (MBHOs) in the country. Optum contracts with approximately 2500 facilities and 130,000 providers to serve 2500 customers (including UnitedHealthcare and other commercial medical insurance plans in addition to employer groups), with members distributed across all U.S. states and territories. The files include the following: (i) member eligibility files, (ii) specialty behavioral health claims, routinely collected and archived for all Optum behavioral health beneficiaries; (iii) the "Book of Business" file. Member eligibility data include age, gender, relationship to subscriber, state of residence, and eligibility information; and (iv) sociodemographic data from a commercial marketing database for a subset of enrollees. The claims provide information on the patient and provider; setting (inpatient vs. outpatient); date(s) of service; diagnosis and procedure codes; amounts billed and reimbursed; deductibles; and copayment/coinsurance amounts. Optum uses fee-for-service reimbursement for specialty behavioral healthcare, so all records have payment amounts. The "Book of Business" file has information on employer group and plan characteristics, such as funding arrangement (self-funded vs. fully insured), employer group size, type of coverage (behavioral health, EAP, work-life, etc.) and type of plan (HMO, PPO, etc.). Information on whether the employer group uses a carve-out or carve-in model (or both) came from other Optum business records.

3.2 Study Cohort and Stratification

Figure 1 provides a sample size flowchart summarizing the inclusion and exclusion criteria used to derive our study cohort of childless couples. Our initial sampling strategy included all 2008-

[5] While considering this interdependence in the context of families with children is an important question, including them in the analysis adds a layer of complexity to the analyses. This is why this paper focus on partners who do not have children.

2014 person-years from couples with only one subscriber and one spouse or domestic partner during the year. A number of inclusion criteria were imposed, leading to drops in sample size, particularly resulting from requirements related to continuous eligibility, age and lack of dependents (see Figure 1). The final overall sample size included 2,915,332 person-years, corresponding to 1,230,863 unique individuals and 615,929 couples in 11,135 plans offered by 561 employers.

For the estimation, we stratify the sample by subscriber/dependent to reflect the possibility of asymmetrical impacts. In addition, we allow for partner's impact to vary by gender and sexual orientation. While our main interest is in spousal peer effects in behavioral health services use at the global level, it is important to highlight that behavioral health services seeking behaviours are highly heterogeneous. This is why we allow for the partner's impact to vary by the recorded status of the spouse in the insurance plan (i.e., subscriber vs. spouse). The underlying rationale for the first stratification is that a partner's status in the insurance plan is the result of a joint decision process in the household. Often, this decision is related to labour force participation (Royalty and Abraham, 2006), which in turn may lead to a lower bargaining power for the dependent spouse and may lead to a differential impact on his/her mental health status and thus expenditures. While we acknowledge that this is not necessarily the best measure of bargaining power in the household, we believe that any systematic difference between respondents and dependents is worth investigating in this setting.

Furthermore, the literature appears to suggest that there are significant gender differences in the likelihood of seeking help as well as differences in the patterns by which each gender seeks this help. To allow for this possibility, we introduce a second stratification and allow for the spousal peer effect to vary by gender. It is essential to highlight that, in the context of this paper, both partners have equal access to care (i.e., covered by the same plan), so gender differences in the use of behavioral health services are to be interpreted under the equal access to care assumption. The assumption of equal access applies to the LGBTQ stratum as well. Marginalized populations such as LGBTQ individuals face enormous psychological and social challenges at the individual level as well as at the family level. To account for this possibility and to allow for family dynamics to be different between same-sex couples and heterosexual couples, we introduce a third stratification by sexual orientation.

3.3 Study Outcomes

The main outcome of interest is an indicator of whether the respondent had any outpatient visits for individual psychotherapy during the year. Our secondary outcomes of interest are whether the respondent had any outpatient visits for psychotropic medication management and whether the respondent had any type of behavioral health care service. All study outcomes were constructed by generating an indicator function for each of these utilization measures.

3.4 Overview of identification issues

Our primary interest is to identify whether an individual's specialty behavioral care utilization is affected by his/her spouse's specialty behavioral care use. The identification of the impact of a spouse's specialty behavioral care utilization on his/her partner's utilization presents two identification challenges as correlations between their use may not necessarily reflect the presence of interdependence in their choices. First, the presence of correlated effects arising due to assortative matching and the common shocks to partners may be at the source of the presence of a positive correlation between both spouses' health care utilization. Second, the reflection problem (Manski, 1993) arising from the simultaneity of the utilization of both spouses may create spurious correlations between the respondent's specialty behavioral health care utilization and his/her spouse's.

Correlated effects may be due to two factors: assortative matching and common shock experienced by the couple. Assortative matching arises because spouses get together based on observed characteristics such as age as well as unobserved characteristics. If there is an unobserved characteristic that is common to both partners (e.g., anxiety or depression), its independent impact on each of the partners will lead to the use of behavioral health care services, which in turn will be reflected in a positive correlation between spouses' behavioral health care use. Fortunately, our administrative data allows us to control for diagnoses, which are characteristics that are usually considered to be an unobservable individual characteristic. As a result, under the assumption that selection is based on the observables, controlling for the partner's observed characteristics is sufficient to account for assortative matching. However, if assortative mating occurs based on characteristics that are unobserved in the data (e.g., both partners enjoy consuming behavioral health care services), then controlling for the observed characteristics of the partner may not be sufficient. In this case, one needs to account for selection on these unobservables. One way of dealing with selection on the unobservable is to introduce individual fixed effects. Alternatively, one can add a lagged dependent variable, which allows us to control for past behaviour including unobserved characteristics. The role of this lagged dependent variable is to control any potential bias resulting from a correlation between the error term and the spouse's specialty behavioral health care utilization that is on top of the prior diagnosis. However, under the assumption of the presence of time-invariant unobserved characteristics, this will mechanically create a correlation between the lagged dependent variable and the error term (via individual time-invariant unobservables) that cannot be evacuated by a fixed effect estimation (Nickell bias, Nickell (1981)). Given that using a lagged dependent specification when the fixed effect specification is correct produces estimates that are too small (underestimates the true effect), we use a dynamic specification as an indirect way to control for unobserved characteristics. We believe that this approach is conservative and will produce a lower bound estimate for spousal spillover effects.[6]

Common shocks experienced by the couple can occur at the household level as well as the state level. The most important common shock that a household experience is usually a financial one. Given that our data are for individuals covered by employer insurance, this type of shock may not be an issue unless the shock is occurring to a partner who works in a different industry and who is not the subscriber. It is important that subscribers who experience a job loss during the period of our analysis are not included in the data by construction. Although it is conceivable that our sample includes some former employees with COBRA coverage, such coverage is time-limited and COBRA take-up rates are exceedingly low (Rovner, 2009). As a result of using a commercially insured population, unobserved income shocks are less likely to cause problems for our estimation.

[6] We estimated a fixed effect specification and a specification with a lagged dependent variable to provide bounds for the spillover effects. Estimation results show that the coefficients of both specifications are not statistically different from each other. Given that the lagged dependent variable specification produces lower bounds, we decided to adopt this specification.

While one can argue that there may be selection issues arising from the nature of the data (claims data), it is important to note that any potential bias that may arise from this selection issue will bias the effects downwards. Specifically, if both partners lose their job, they not only eventually lose coverage but also will both have levels of stress that are higher than a couple where one (or none) has lost his job. Other types of a common shock such as cyclical shocks (e.g., same occupation) or state crime rates, state policies and time-specific shocks are considered by introducing year and state fixed effects.

The well-known reflection problem (Manski, 1993) arises because of the simultaneity in the decision of the individuals. This simultaneity hinders the identification of the causal impact of a spouse's utilization on the other, as it is impossible to disentangle the direction of the causality. In the literature, this issue has often been addressed by exploiting variation induced through an instrumental variable approach, some of which used internal instruments (e.g., lagged values of the endogenous variable), and others have used external instruments (e.g., life events affecting one partner).

In the context of this paper, since we are using claims data, the availability of instruments is very limited. We thus exploit the longitudinal aspect of the data and estimate a reduced form in which we replace the spouse's current behavioral health care use by its lagged value. In robustness checks, we also estimate an alternative specification in which we instrument the partner's current behavioral health care use with its lagged value. The following section elaborates on our empirical models.

3.5 Empirical Framework

Using the entire sample available, we first estimate baseline OLS models in which we assume that the assortative matching occurs based only on the observable characteristics. The specification is as follows:

$$H_{ist} = \rho H_{ist-1} + \beta H_{jst} + \theta X_{ist} + \delta X_{ist-1} + \gamma X_i + \lambda_s + \mu_t + \varepsilon_{it}, \quad (1)$$

where H_{ist} and H_{ist-1} are the specialty BH care utilization measures for individual i, in state s, at time t and time $t\text{-}1$ respectively and H_{jst} is the specialty BH care utilization of the spouse in the same period. To deal with selection on the observables, we control for current time-variant characteristics, X_{ist} (e.g., age) and time-invariant characteristics X_i (e.g., employer size, plan characteristics), as well as lagged time-variant characteristics of the respondent X_{ist-1} (e.g., past diagnosis). It is important to note that because we do not have socio-demographic characteristics for the entire sample, the main regressions will use a limited set of controls that is dictated by the information we have for the entire sample. However, in the sensitivity analysis described below, we will focus on the subsample for which we have a richer set of controls. Thus, we are able to test whether these results are sensitive to the inclusion of a wide set of controls. We account for the possible unobserved common shocks that may occur at the state level and the year level by including a state fixed effect λ_s and year fixed effects μ_t. We also allow for the error terms to be correlated within states and cluster our regression error terms at the state level.[7] We will estimate this baseline equation using a single-equation linear probability approach.[8]

As mentioned earlier, the inclusion of partner's utilization will lead to a simultaneity issue in spouses' utilization which hinders the identification of the direction of the impact of behavioral health care use among partners. To overcome this issue, we next exploit the timing of the spouse's behavioral health care use. In our main set of OLS analyses, we use the lagged value of the partner's behavioral health care use as the variable of interest to quantify the spillover effects, estimating the following equation:

$$H_{ist} = \rho H_{ist-1} + \beta H_{jst-1} + \delta X_{ist-1} + \gamma X_i + \lambda_s + \mu_t + \varepsilon_{it} \; (2)$$

Finally, to provide a meaningful interpretation of the magnitude of the spousal spillovers, we compare the spillover estimates from these two equations with the estimated impact of a widely accepted predictor of behavioral health services use among commercially insured populations,

[7] We believe that clustering at the state level is more conservative. We tested our results at industry level as well and results do not seem to be sensitive for the level of clustering.
[8] We also estimate the same equation using a probit, and the results are consistent with the OLS estimation. We chose to use the linear probability model to insure comparability with the results of the instrumental variable regression.

diagnosis of depression. Specifically, we compare the magnitude of the coefficient of the spillover effects to the magnitude of the depression coefficient, expressed in percentage terms. Thus, for a reported impact of 50%, we can say the marginal effect of spouse's behavioral health care use on that of the respondent is 50% of the impact of being diagnosed with depression.

3.6 Robustness checks

3.6.1. Nickell bias

One issue that arises from the estimation of the above equations is the presence of unobserved heterogeneity arising from individual time-invariant unobserved characteristics. Since estimating a fixed effect version of this equation leads to Nickell bias (Nickell, 1981) we test whether estimating the spousal peer effects using this specification differs significantly from a version in which the lagged dependent variable is left out and where fixed effects are introduced instead.

3.6.2 Instrumental variables (IV) estimation and IV upper bounds

Given that most of the literature in spousal spillovers uses an instrumental variable approach, we explore this path by using a spouse's lagged behavioral health care use as an instrument for the current spouse's behavioral health care utilization. We thus assume that the spouse's past use has no direct effect on the respondent's current use and that all the impact of the spouse's prior use flows through the spouse's current use. A spouse's use in the previous period may affect the respondent's use in the previous period, generating a correlation between the error term and the instrument. We account for this possibility by controlling for the respondent's behavioral health use in the previous year. Still, we run the instrumental variable regression assuming that this instrument is an imperfect IV (Nevo and Rosen, 2012). Under the assumptions and conditions in Nevo and Rosen (2012), we can identify upper bounds for the spillover effects.

3.6.3 Addition of sociodemographic controls

As noted above, when using the full sample, our regression specification is parsimonious, limited to explanatory variables that can be derived from claims data alone. In sensitivity analyses, we use the subsample of observations for which sociodemographic information is available to compare the results with and without controls for race/ethnicity, income/asset, and education categories.

4- Results

4.1. Stylized facts

4.1.1 Characteristics of the study population

As shown in Table 2, the study population is about half female and tends to be older, with 49% of women and 56% of men falling into the 55- to 64-year-old age category. About one-third of women but two-thirds of men were the primary insured person (subscriber). The study population was diverse in terms of geography and employer group size, although very few individuals were in the smallest employer groups (<50 employees). Almost three-quarters of both men and women were in more managed plan types, about one-third were in carve-out plans, almost ten percent were in fully-insured plans and about four percent were in plans that were collectively bargained. About eleven percent were in plans that do not renew on a calendar-year cycle. As is commonly seen among commercially insured patients, the most common behavioral health diagnoses among both men and women using specialty services were depressive disorder, adjustment disorder and generalized anxiety disorder.

4.1.2. Descriptive statistics for the outcome measure

Overall, 4.16% of women and 2.34% of men had any individual psychotherapy visits; 3.74% of women and 2.2% of men had any psychotropic medication management visits; and 7.71% of women and 4.83% of men had any specialty behavioral healthcare expenditures (results not shown in tables). However, the annual rates of use of these services varied substantially by demographic group (Tables 3a and 3b), with women having higher use rates than men and both men and women in same-sex couples having higher use rates than their counterparts in heterosexual couples.

4.2. Main Results

4.2.1. Baseline OLS estimates of the impact of spouse's current use on subject's current use

Looking regression as laid in equation (1) we first estimate a baseline regression including all the control variables and fixed effects. From the OLS results reported in Tables 4a and 4b, we note that the spousal peer effects range from 0.0976 to 0.1640 for individual psychotherapy use; from 0.0266 to 0.0633 for medication management visits; and from 0.0866 to 0.1511 for overall behavioural health care use. The range of the magnitude of the spousal spillover effects relative to the depression effects is between 115% and 200% for individual psychotherapy visits; between 37%% and 59% for medication management, and 44% to 77% for overall behavioural health care use. All of these effects are statistically significant and reveal interesting patterns of heterogeneity across gender, subscriber status and couple type, yet they are biased because of the simultaneity of the behavioural health care use of the subject and spouse. We present these results to emphasize that in the presence of selection, the magnitude of spillover effects tend to be substantial; however, we will refrain from interpreting these effects.

4.2.2. OLS estimates of the impact of spouse's prior-year use on subject's current use

Given that the simultaneity between the subject's and spouse's behavioral health care use may be biasing the result upwards, we replace the spouse's current use by the spouse's use in the previous period and estimate our main specification as laid out in equation (2) above. These are our preferred estimates and are presented in Tables 5a and 5b.

The spillover effects from the preferred specification are smaller than those from the baseline regressions, ranging between 0.0431 and 0.0685 for individual psychotherapy visits; between .0171 and .0377 for medication management visits; and between .0421 and .0648 for any specialty behavioral health services use. The range of the magnitude of the spousal spillover effects relative to the depression effects is between 56% and 91% for individual psychotherapy visits; between 23% and 34% for medication management visits; and between 21% and 50% for any specialty behavioral healthcare use.

For individual psychotherapy, the spillover effects are higher for females than males in heterosexual couples, among both dependents and subscribers. That is, everything else held constant and controlling for prior utilization and previous diagnosis, women are more responsive than men to the individual psychotherapy use of their spouse. An increase in the spouse's use leads to a higher increase in individual psychotherapy use among female respondents. Specifically, the relative effects are higher for female subscribers than for male subscribers (respectively 75% and

57% of the impact of depression) and higher for female dependents than for male dependents (respectively 76% and 56% of the impact of depression). The relative difference between female dependents and male dependents is almost the same as the relative difference between female subscribers and male subscribers, suggesting that regardless of the gender of the subscriber (and potentially bargaining power), the spillover effects remain consistently stronger for females. The presence of higher spillovers flowing from male respondents to female respondents is compatible with masculine norms and more generally male help-seeking attitudes. Often, males have unfavourable attitudes towards behavioural health care use (Vogel et al., 2011); thus, they may be more resistant to being influenced by their spouses' attitudes. In addition, the presence of higher spillover effects among women is compatible with the literature on social psychology stating that peer effects are more likely to be higher for females (Minton and Schneider, 1980; Cross and Madson, 1997; Baumeister and Kristin, 1997). The symmetry between subscribers' and dependents' spillover effects suggests that being the main subscriber (or the dependent) does not have a differential impact on the decision to use individual psychotherapy. Indeed, in the absence of children there are fewer household responsibilities. Therefore, there is no need for traditional gender role division (homemaker and bread-earner) and we should not expect asymmetries in outcomes related to subscriber vs dependent status (proxy for bargaining power).

These spillovers patterns are slightly different among same-sex couples, with no statistical differences between females and males. The relative spillovers on females remain of similar magnitude regardless of the partner's gender but males (whether dependents or subscribers) seem to experience higher relative spillover effects with a male partner. The result that spillover effects seem to be significantly stronger for men in same-sex couples than the relative effects estimated for heterosexual men suggests that the interdependence of men's decision regarding the use of individual psychotherapy depends on the gender of the partner. In contrast, for women, the influence of the partner's decisions remains the same regardless of whether they are in a heterosexual or same-sex relationship. A possible explanation for the presence of higher spillover effects for males is the potential for higher level of assortative mating among same-sex couples. If same-sex partners are closer in their characteristics, then we should expect a higher level of spillovers. More specifically, if we follow tightly the theory on assortative mating theory (Becker, 1973), and given that partners are drawn from the same set, we should expect individuals who are in same-sex couples to have identical characteristics. Nevertheless, recent empirical evidence on same-sex couples suggest the opposite; homogamy seem to be less prevalent among same-sex couples than heterosexual couples (Jepsen and Jepsen, 2002; Andersson et al., 2006; Schwartz and Graf, 2009; Verbakel and Kalmijn, 2014). Besides, if same-sex homogamy is the main driver of these results, we should expect to observe similar patterns in spillover differences between heterosexual and same-sex couples for both male and female same-sex couples, which is not the case here. Another possible driver for the presence of differences in the relative spillover effects for same-sex couples is the underlying difference in the magnitude of the impact of depression on one's individual psychotherapy use. Nevertheless, for both dependent and subscriber, the change in the coefficient for depression remains approximately the same (see Appendices A-C for depression coefficients). Thus, these impacts are mainly driven by an increase in the absolute spillover effects of the partner and reinforce the idea that men seem to be more influenced by a male partner than a female partner.

Medication management visits are an important component of behavioral care; it accounts for around 20% of behavioral health care use. Indeed, psychotropic drugs are playing an increasing role in treating mental health disorders. Both partners may be using medication therapy, however we expect that the spillover effects for medication management visits will be smaller than for individual psychotherapy. Indeed, evidence suggests there is still a preference for psychotherapy compared to medication treatment (McHugh et al., 2014). This is particularly true for men, who are more reluctant to taking medications (Berger et. al., 2013) and more open to use individual psychotherapy. Results from Tables 5a and 5b are in line with our expectations and with the presence of modest spillovers in medication management visits. These spillovers are around almost half of the spillover effect of individual psychotherapy visits and their relative impact compared to the impact of own depression, are at most only 34%. One important pattern to note here is that similarly to individual psychotherapy, these effects are reasonably stable across subscriber status and couple type; however, unlike individual psychotherapy, the effects are similar across gender. One possible reason behind the coherent pattern across strata may reside in the difference between private (unobserved) individual psychotherapy and the physical (observable) aspect of medicating. While the beneficial effects of individual psychotherapy may not be directly attributable to psychotherapy sessions as these sessions remain mostly unobserved by the partner, taking medication is directly observable. Thus, there is a possibility of having first-hand information on the association between medicating and its beneficial impacts.

Given that behavioral health care use encompasses a wide variety of services, other than individual psychotherapy and medication management visits, we investigated whether these spillovers can be reflected on a broader range of services by considering the impact on any behavioral health care use. The spillover coefficients for this outcome are in line with our findings for individual psychotherapy, with significantly lower spillover effects for males in a heterosexual relationship. Also, while the relative magnitude of these spillovers is lower than for individual psychotherapy, differences in the absolute magnitude of the spillover effects (i.e., the coefficients) are not statistically different for individual psychotherapy and overall use (except for female dependents in heterosexual relationship). Furthermore, the coefficients for the spillover effects on any behavioral healthcare are significantly higher than spillovers in medication management visits.

To sum up, we can say that the results are in general consistent with the hypothesis that help-seeking behaviour is linked to intimate partner decisions regarding (mental) health investments and that these effects are stronger for females compared to males in heterosexual couples regardless of their subscriber status. These gender differences in help-seeking behaviour echoes results and concerns about men's help-seeking attitudes and their gendered perceptions about mental health disorders (Coen at al., 2013). Thus, while men are open to be influenced by their intimate partners, evidence seem to suggests that the spillovers flowing from females to males remains asymmetric and significantly smaller. Another notable result is the presence of higher spillovers for males when partnered with another male. This, in turn, suggests policies targeted towards redesign of mental health services should consider strategies for targeting of men based on their gendered perceptions of help seeking behaviour.

4.4 Robustness Checks

4.3.1 Nickell bias

Based on Angrist and Pischke's (2009) discussion on Nickell bias, the spousal spillover effects in the fixed effect specification (when the right specification is a dynamic specification) should be higher than the spillover from the dynamic specification (when the right specification is a fixed effect specification). Thus, the fixed effect and the dynamic specification can be used to bound the spousal spillover effect. However, when we compare estimates from both specifications, the difference between the two specifications is not statistically significant. Given that the lagged dependent variable provides a lower bound for the spousal peer effect and the estimates from the two specifications do not yield different results, we retained the dynamic model specification.

4.3.2. IV estimates of the impact of spouse's current use on subject's current use

Estimates from the instrumental variable approach (Tables 6a and 6b) are smaller than the baseline OLS regression (Tables 4a and 4b) and are all statistically significant; however, the coefficient estimates are larger than those obtained in the main OLS specification (Tables 5a and 5b). The imperfect IV upper bounds are shown in Table 7. The spillover effects estimated using an instrumental variables approach constitute an upper bound for the spillover effects and are thus less conservative than our main results.

4.3.3. Sensitivity analyses controlling for sociodemographic characteristics

It is also possible that some sociodemographic characteristics may be driving the results. For instance, it may be the case that individuals of specific ethnicities may tend to use behavioral health care more than others. If this is the case, and given that intra-ethnic marriages are more common than interethnic marriages, then it is possible that the estimated spillover effects may be overestimated. Indeed, while the use of claims data presents many advantages, it does not come without a cost: the limited information on socioeconomic characteristics. To test whether the results obtained are robust to the inclusion of the sociodemographic characteristics, we re-ran the estimation on a subsample for which we have a set of controls that include a more extensive set of socioeconomic characteristics such as race/ethnicity, education and income/assets. Comparing the results obtained with and without these controls (see Appendices D and E) suggests that there is no statistical difference. In results not shown here, the subsample with sociodemographic data does not differ from the entire sample based on observed characteristics; thus we can assume that results would not have been different if we had the full set of sociodemographic controls for the entire sample.

5- Conclusion

Assessing the presence of spousal spillovers in health care use is essential in general and is crucial in the context of behavioral health care use in particular. This is especially true because mental health issues have long-lasting effects on life trajectory and labor market outcomes. Given that fewer than 50% of individuals with mental health needs use behavioral health care services, it is important to understand the extent to which there is interdependence in the decision to seek behavioral health services.

Motivated by the importance of documenting spousal spillovers, this paper is the first to investigate the presence of spousal spillovers in behavioral health care use. While this question is of great importance, it requires longitudinal information on behavioral health care use and details regarding the diagnosis that are not readily available in surveys. Only claims data can provide such information; however, most claims databases do not allow us to construct family ties. In this paper, we exploit the richness of administrative data and extract information regarding behavioral health care use of childless couples along with detailed information about prior diagnosis and a set of individual characteristics. This allows us to assess spousal spillover effects by gender and by subscriber status (a proxy for negotiation power). Furthermore, given that we can identify the gender of each individual in the couple, we were able to separate same-sex couples and heterosexual couples. As claims data has a large number of observations, it provides us with a setting in which it is possible to investigate spousal spillovers for same-sex couples. As such, this paper is also the first paper that investigates whether spousal peer effects for same-sex and heterosexual couples are different.

Results show that there are spousal spillover effects and that these effects are sizeable and statistically significant. Also, empirical evidence shows that these effects are more substantial for women compared to men and larger for males when their partners are of the same sex. It is interesting to note the large gender differences in spillover effects in individual psychotherapy use among heterosexual couples, regardless of whether the spillovers are driven by increased information or decreased stigma. Perhaps interventions targeting stigma or information regarding the benefits of individual psychotherapy should account for the potential differences between males' and females' social interactions.

Several limitations of our data should be noted. Our findings are not based on a random sample of the population, although given the size and geographic diversity of Optum enrollees, our conclusions are likely to generalize to a fairly large swath of the population. Diagnoses are claims-based, so will not capture undiagnosed conditions. Moreover, we have sociodemographic characteristics for only a subset of our sample, although sensitivity analyses suggested that this dearth of information is unlikely to have affected our conclusions. Finally, while it is conceivable that we are missing utilization of services covered by another insurer, only 0.15% of our sample had Coordination of Benefits (COB) coverage.

While our claims database allows us to assess spousal spillovers for behavioral health services use, it does not allow us to explore the mechanism via which these spillovers occur. Thus, whether these effects occur via the informational channel or the stigma channels remains an open question. Also, given that we are using claims data, the spillover effects estimated are for a specific group of the population: couples in which at least one member is in the labor market and where both hold private health insurance. It would be interesting to measure these spillovers in a more generalizable framework where couples are insured regardless of their employment status.

While documenting the presence of health spillovers is interesting per se, it has important implications for economic evaluations of health care services. It reveals information regarding the extent to which policies and programs may have effects beyond the targeted population. Thus, if these spillovers are internalized, then this will affect the optimal funding decisions and may generate additional health benefits to society.

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Figure 1. Sample size flowchart

Include if couple had only one subscriber and one spouse/domestic partner during the year:
N= 25,042,596 person-years

Include if subscriber status stayed the same during the year:
N=24,819,698 (99.11%) person-years

Include if the enrollee had same spouse/domestic partner during the year:
N=24,816,482 (99.99%) person-years

Include if only one gender recorded during study period:
N=24,614,116 (99.18%) person-years

Include if continuously enrolled during the year: N=19,076,518 (77.50%) person-years

Include if 18 ≤ age ≤ 64: N=16,697,220 (87.53%) person-years

Include if couple has no dependents: N=5,617,932 (33.65%) person-years

Include if couple resides within the 50 U.S. states: N=5,610,170 (99.86%) person-years

Include if plan has behavioral health coverage: N=5,397,256 (96.20%) person-years

Include if standard plans (not retiree, supplemental, or indemnity):
N=5,082,910 (94.18%) person-years

Include if enrolled for at least 2 continuous years: N=4,241,004 (83.44%) person-years

Include if prior-year data are available: N=2,960,264 (69.80%) person-years

Include if no missing data: N=2,932,568 (99.06%) person-years

Include if never lived in AK, ND, WY: N=2,915,524 (99.42%) person-years

Include if employer has >1 observation in each subsample:
N=2,915,332 (99.99%) person-years

Note: Final sample size has 1,230,863 unique people, corresponding to 615,929 couples, 11,135 plans and 561 employers.

Table 1. Literature review summary

	Outcome	Peer Group	Empirical Strategy	Results
Fletcher, (2009)	Mental Health Status	Spouses	Instrumental variable: spouse's job stress	Evidence of spousal peer effect in mental health status.
Powdthavee, (2009)	Happiness	Spouses	System GMM: Lagged levels & variation in spouse mental health	Evidence of spousal peer effect in life satisfaction.
Eisenberg et al. (2013)	Mental Health	Randomized Roommates (Freshman)	Randomization Lagged peer outcomes	No effects except Anxiety & depression
Mervin & Frijters (2014)	Mental Health Summary MCS	Spouses	Instrumental variable: spouse's life events	Evidence of spousal peer effect in mental health outcomes.
Deri (2005)	Health care use	Immigrant groups	Instrumental variable: Language variation and regional variation	Presence of peer effects in health care use.
Devillanova (2008)	Primary health care use	Undocumented Immigrants social ties		Presence of peer effects.
Aizer and Currie (2011)	Antinatal care use	Ethnic Groups	Instrumental variable: average use of reference group with leave out strategy	Evidence of peer effects in care use.
Eisenberg et al. (2016)	Mental health care service use	Randomized Roommates/Hallmates (Freshman)	Randomization	No effects for roommates, Some effects on Hallmates.

Table 2. Characteristics of the study population, stratified by gender

	Women (N=1,450,805)		Men (N=1,464,527)	
	N	%	n	%
Age group				
18-24 years	8,814	0.61	3,349	0.23
25-34	151,790	10.46	126,719	8.65
35-44	153,065	10.55	162,127	11.07
45-54	424,583	29.27	352,822	24.09
55-64	712,553	49.11	819,510	55.96
Primary insured person (vs. dependent)	484,899	33.42	972,767	66.42
Census Division				
Northeast: New England	87,730	6.05	87,901	6.00
Northeast: Middle Atlantic	130,691	9.01	133,020	9.08
Midwest: East North Central	248,859	17.15	249,976	17.07
Midwest: West North Central	120,358	8.30	120,845	8.25
South: South Atlantic	258,477	17.82	262,557	17.93
South: East South Central	71,204	4.91	71,423	4.88
South: West South Central	195,178	13.45	196,092	13.39
West: Mountain	133,614	9.21	133,943	9.15
West: Pacific	204,694	14.11	208,770	14.26
Employer group size				
>40K enrolled employees	552,150	38.06	558,040	38.10
>10K & ≤ 40K	540,863	37.28	545,895	37.27
5,000-10,000	194,984	13.44	196,340	13.41
50-5,000	162,742	11.22	164,184	11.21
<50	66	0.00	68	0
Plan type is more managed (e.g., HMO) vs.	1,054,762	72.70	1,066,722	72.84

less managed (e.g., PPO)				
Carve-out plans	477,182	32.89	479,916	32.77
Fully insured	143,840	9.91	143,662	9.81
Plan is collectively bargained	57,360	3.95	58,268	3.98
Plan does not renew on a calendar year basis	160,728	11.08	161,162	11.00
Calendar year				
2009	261,174	18.00	263,228	17.97
2010	285,698	19.69	287,736	19.65
2011	270,988	18.68	273,132	18.65
2012	229,171	15.8	231,339	15.80
2013	211,457	14.58	213,979	14.61
2014	192,317	13.26	195,113	13.32
Among people with any specialty behavioral health service use:				
Any adjustment disorder	14,538	12.97	8,778	12.27
Any post-traumatic stress disorder	4,414	3.94	1,985	2.77
Any generalized anxiety	16,557	14.77	9,115	12.74
Any obsessive-compulsive disorder	1,804	1.61	1,383	1.93
Any panic disorder	5,282	4.71	2,656	3.71
Any phobia	852	0.76	609	0.85
Any bipolar disorder	10,602	9.46	5,134	7.18
Any depressive disorder	40,892	36.49	21,416	29.93
Any psychotic disorder	1,994	1.78	950	1.33
Any alcohol use disorder	2,035	1.82	2,612	3.65
Any drug use disorder	1,474	1.32	1,571	2.20
Any other psychiatric disorder	6,174	5.51	3,422	4.78

Table 3a. Outcomes for study population, stratified by gender and subscriber status, heterosexual couples

Subject	Spouse	N	Any individual psychotherapy visits		Any medication management visits		Any behavioral healthcare expenditure	
			n	%	n	%	n	%
Female dependent	Male subscriber	954,809	39,783	4.17	39,760	4.16	75,930	7.95
Male dependent	Female subscriber	473,802	11,145	2.35	12,873	2.72	25,086	5.29
Male subscriber	Female dependent	954,809	21,103	2.21	17,801	1.86	42,227	4.42
Female subscriber	Male dependent	473,802	18,699	3.95	13,168	2.78	32,861	6.94

Table 3b. Outcomes for study population, stratified by gender and subscriber status, same-sex couples

Subject	Spouse	N	Individual psychotherapy		Medication management		Total expenditure	
			n	%	N	N	n	%
Female dependent	Female subscriber	11,097	1,009	9.09	765	6.89	1,658	14.94
Male dependent	Male subscriber	17,958	1,228	6.84	1,128	6.28	2,209	12.30
Female subscriber	Female dependent	11,097	1,043	9.40	611	5.51	1,615	14.55
Male subscriber	Male dependent	17,958	1,244	6.93	838	4.67	2,026	11.28

Table 4a. OLS estimates of the impact of whether spouse has service use in current year on whether subject has service use in the current year, controlling for whether subject had service use in prior year, heterosexual couples.

Subject	*Female Dependent*	*Male Dependent*	*Male Subscriber*	*Female Subscriber*
Spouse	*Male Subscriber*	*Female Subscriber*	*Female Dependent*	*Male Dependent*
N	*954,809*	*473,802*	*954,809*	*473,802*
Spillover Effects of Any Individual Psychotherapy Visits	*.1640 (.0046)*	*.0988 (.0032)*	*.0976 (.0027)*	*.1614 (.0052)*
As Percent of Impact of Own Depression	*200%*	*131%*	*115%*	*184%*
Spillover Effects of Any Medication Management Visits	*.0466 (.0022)*	*.0389 (.0022)*	*.0266 (.0014)*	*.0402 (.0025)*
As Percent of Impact of Own Depression	*59%*	*49%*	*37%*	*55%*
Spillover Effects of Any Behavioral Health Care Expenditure	*.1345 (.0047)*	*.0965 (.0024)*	*.0866 (.0030)*	*.1277 (.0031)*
As Percent of Impact of Own Depression	*77%*	*53%*	*44%*	*63%*

Notes: Standard errors shown in parentheses. All regressions also controlled for lagged own utilization, prior-year diagnoses, age group, state and year fixed effects, employer size category, whether carve-out or carve-in, whether fully- or self-insured plan, more vs. less managed plan, whether collective bargaining plan, and whether plan renews on a calendar-year basis. All estimates shown in table and all lagged own utilization terms are significant at $p \leq .001$.

Table 4b. OLS estimates of the impact of whether spouse has service use in current year on whether subject has service use in the current year, controlling for whether subject had service use in prior year, same-sex couples.

Subject	*Female Dependent*	*Male Dependent*	*Female Subscriber*	*Male Subscriber*
Spouse	*Female Subscriber*	*Male Subscriber*	*Female Dependent*	*Male Dependent*
N	*11,097*	*17,958*	*11,097*	*17.958*
Spillover Effects of Any Individual Psychotherapy Visits	*.1549 (.0164)*	*.1311 (.0120)*	*.1556 (.0171)*	*.1337 (.0117)*
As Percent of Impact of Own Depression	*181%*	*187%*	*183%*	*148%*
Spillover Effects of Any Medication Management Visits	*.0633 (.0132)*	*.0593 (.0108)*	*.0421 (.0089)*	*.0445 (.0077)*
As Percent of Impact of Own Depression	*62%*	*66%*	*65%*	*66%*
Spillover Effects of Any Behavioral Health Care Expenditure	*.1511 (.0127)*	*.1204 (.0113)*	*.1468 (.0132)*	*.1173 (.0095)*
As Percent of Impact of Own Depression	*97%*	*80%*	*125%*	*73%*

Notes: Standard errors shown in parentheses. All regressions also controlled for lagged own utilization, prior-year diagnoses, age group, state and year fixed effects, employer size category, whether carve-out or carve-in, whether fully- or self-insured plan, more vs. less managed plan, whether collective bargaining plan, and whether plan renews on a calendar-year basis. All estimates shown in table and all lagged own utilization terms are significant at $p \leq .001$.

Table 5a. OLS estimates of the impact of whether spouse had service use in prior year on whether subject has service use in the current year, controlling for whether subject had service use in prior year, heterosexual couples.

Subject	*Female Dependent*	*Male Dependent*	*Male Subscriber*	*Female Subscriber*
Spouse	*Male Subscriber*	*Female Subscriber*	*Female Dependent*	*Male Dependent*
N	*954,809*	*473,802*	*954,809*	*473,802*
Spillover Effects of Any Individual Psychotherapy Visits	*.0628 (.0031)*	*.0431 (.0025)*	*.0465 (.0018)*	*.0685 (.0040)*
As Percent of Impact of Own Depression	*76%*	*56%*	*57%*	*75%*
Spillover Effects of Any Medication Management Visits	*.0235 (.0019)*	*.0221 (.0018)*	*.0171 (.0013)*	*.0226 (.0022)*
As Percent of Impact of Own Depression	*30%*	*27%*	*23%*	*30%*
Spillover Effects of Any Behavioral Health Care Expenditure	*.0523 (.0019)*	*.0424 (.0017)*	*.0421 (.0017)*	*.0595 (.0024)*
As Percent of Impact of Own Depression	*30%*	*23%*	*21%*	*29%*

Notes: Standard errors shown in parentheses. All regressions also controlled for lagged own utilization, prior-year diagnoses, age group, state and year fixed effects, employer size category, whether carve-out or carve-in, whether fully- or self-insured plan, more vs. less managed plan, whether collective bargaining plan, and whether plan renews on a calendar-year basis. All estimates shown in table and all lagged own utilization terms are significant at $p \leq .001$.

Table 5b. OLS estimates of the impact of whether spouse had service use in prior year on whether subject has service use in the current year, controlling for whether subject had service use in prior year, same-sex couples

Subject	*Female Dependent*	*Male Dependent*	*Female Subscriber*	*Male Subscriber*
Spouse	*Female Subscriber*	*Male Subscriber*	*Female Dependent*	*Male Dependent*
N	*11,097*	*17,958*	*11,097*	*17.958*
Spillover Effects of Any Individual Psychotherapy Visits	*.0678 (.0153)*	*.0667 (.0099)*	*.0603 (0130)*	*.0673 (.0089)*
As Percent of Impact of Own Depression	*75%*	*91%*	*67%*	*72%*
Spillover Effects of Any Medication Management Visits	*.0377 (.0130)*	*.0265 (.0115)*	*.0216 (.0082)*	*.0207 (.0019)*
As Percent of Impact of Own Depression	*34%*	*30%*	*32%*	*26%*
Spillover Effects of Any Behavioral Health Care Expenditure	*.0648 (.0111)*	*.0563 (.0078)*	*.0603 (.0091)*	*.0605 (.0073)*
As Percent of Impact of Own Depression	*40%*	*37%*	*50%*	*37%*

Notes: Standard errors shown in parentheses. All regressions also controlled for lagged own utilization, prior-year diagnoses, age group, state and year fixed effects, employer size category, whether carve-out or carve-in, whether fully- or self-insured plan, more vs. less managed plan, whether collective bargaining plan, and whether plan renews on a calendar-year basis. All estimates shown in table and all lagged own utilization terms are significant at $p \leq .001$.

Table 6a. IV estimates of the impact of whether spouse has service use in current year on whether subject has service use in the current year, controlling for whether subject had service use in prior year, heterosexual couples.

Subject	*Female Dependent*	*Male Dependent*	*Male Subscriber*	*Female Subscriber*
Spouse	*Male Subscriber*	*Female Subscriber*	*Female Dependent*	*Male Dependent*
N	*954,809*	*473,802*	*954,809*	*473,802*
Spillover Effects of Any Individual Psychotherapy Visits	*.1326 (.0063)*	*.0853 (.0049)*	*.0865 (.0033)*	*.1373 (.0078)*
As Percent of Impact of Own Depression	163%	113%	110%	142%
Spillover Effects of Any Medication Management Visits	*.0329 (.0029)*	*.0309 (.0026)*	*.0227 (.0018)*	*.0311 (.0030)*
As Percent of Impact of Own Depression	41%	40%	31%	42%
Spillover Effects of Any Behavioral Health Care Expenditure	.0952 (.0034)	.0764 (.0032)	.0676 (.0027)	.1018 (.0039)
As Percent of Impact of Own Depression	55%	51%	34%	50%

Notes: Standard errors shown in parentheses. All regressions also controlled for lagged own utilization, prior-year diagnoses, age group, state and year fixed effects, employer size category, whether carve-out or carve-in, whether fully- or self-insured plan, more vs. less managed plan, whether collective bargaining plan, and whether plan renews on a calendar-year basis. All estimates shown in table and all lagged own utilization terms are significant at $p \leq .001$.

Table 6b. IV estimates of the impact of whether spouse has service use in current year on whether subject has service use in the current year, controlling for whether subject had service use in prior year, same-sex couples.

Subject	*Female Dependent*	*Male Dependent*	*Female Subscriber*	*Male Subscriber*
Spouse	*Female Subscriber*	*Male Subscriber*	*Female Dependent*	*Male Dependent*
N	*11,097*	*17,958*	*11,097*	*17.958*
Spillover Effects of Any Individual Psychotherapy Visits	*.1228 (.0272)*	*.1215 (.0178)*	*.1094 (.0237)*	*.1233 (.0154)*
As Percent of Impact of Own Depression	141%	162%	125%	136%
Spillover Effects of Any Medication Management Visits	*.0541 (.0180)*	*.0364 (.0158)*	*.0319 (.0120)*	*.0285 (.0109)*
As Percent of Impact of Own Depression	50%	40%	49%	42%
Spillover Effects of Any Behavioral Health Care Expenditure	*.1106 (.0181)*	*.0924 (.0121)*	*.1010 (.0152)*	*.0970 (.0115)*
As Percent of Impact of Own Depression	74%	61%	84%	60%

Notes: Standard errors shown in parentheses. All regressions also controlled for lagged own utilization, prior-year diagnoses, age group, state and year fixed effects, employer size category, whether carve-out or carve-in, whether fully- or self-insured plan, more vs. less managed plan, whether collective bargaining plan, and whether plan renews on a calendar-year basis. All estimates shown in table and all lagged own utilization terms are significant at $p \leq .001$.

Table 7. Imperfect IV upper bounds (Nevo and Rosen, 2012)

Subject	***Spouse***	***Any Individual Psychotherapy Visits***	***Any Medication Management Visits***	***Any Specialty Behavioral Healthcare Expenditures***
Female dependent	Male subscriber	.1325 [UL=.1375]	.0328 [UL= .0357]	.0952 [UL=.0990]
Male dependent	Female subscriber	.0853 [UL=.0898]	.0309 [UL= .0340]	.0764 [UL=.0802]
Male subscriber	Female dependent	.0864 [UL=.0889]	.0226 [UL= .0241]	.0675 [UL=.0696]
Female subscriber	Male dependent	.1373 [UL=.1437]	.0311 [UL .0311]	1018 [UL=.1064]
Female dependent	Female subscriber	.1189 [UL=.1370]	.0541 [UL .0790]	.1105 [UL=.1400]
Male dependent	Male subscriber	.0776 [UL=.0974]	.0363 [UL= .0544]	.0924 [UL=.1150]
Female subscriber	Female dependent	.1228 [UL=.1544]	.0319 [UL= .0520]	.1010 [UL=.1289]
Male subscriber	Male dependent	.1215 [UL=.1447]	.0285 [UL= .0424]	.0970 [UL=.1172]

Note: First number in each cell is the IV upper bound. Second number in parentheses is the upper confidence limit for the IV upper bound.

Appendix A- OLS estimates of impact of own depression on whether subject has any (1) individual psychotherapy visits, (2) medication management visits, and (3) specialty behavioral healthcare expenditures, controlling for spouse's current use and lagged own use.

Subject	***Spouse***	***Any Individual Psychotherapy Visits***	***Any Medication Management Visits***	***Any Specialty Behavioral Healthcare Expenditures***
Female dependent	Male subscriber	*.0807 (.0028)*	*.0785 (.0035)*	*.1739 (.0046)*
Male dependent	Female subscriber	*.0751 (.0042)*	*.0789 (.0046)*	*.1846 (.0062)*
Male subscriber	Female dependent	*.0786 (.0049)*	*.0723 (.0057)*	*.1967 (.0063)*
Female subscriber	Male dependent	*.0879 (.0039)*	*.0731 (.0039)*	*.2021 (.0061)*
Female dependent	Female subscriber	*.0701 (.0129)*	*.1081 (.0164)*	*.1542 (.0237)*
Male dependent	Male subscriber	*.0855 (.0179)*	*.0901 (.0160)*	*.1495 (.0200)*
Female subscriber	Female dependent	*.0851 (.0192)*	*.0651 (.0166)*	*.1178 (.0273)*
Male subscriber	Male dependent	*.0906 (.0166)*	*.0674 (.0159)*	*.1612 (.0229)*

Notes: Standard errors shown in parentheses. All regressions also controlled for lagged own utilization, prior-year diagnoses, age group, state and year fixed effects, employer size category, whether carve-out or carve-in, whether fully- or self-insured plan, more vs. less managed plan, whether collective bargaining plan, and whether plan renews on a calendar-year basis. All estimates shown in table and all lagged own utilization terms are significant at $p \leq .001$.

Appendix B- OLS estimates of impact of own depression on whether subject has any (1) individual psychotherapy visits, (2) medication management visits, and (3) specialty behavioral healthcare expenditures, controlling for spouse's lagged use and lagged own use.

Subject	***Spouse***	***Any Individual Psychotherapy Visits***	***Any Medication Management Visits***	***Any Specialty Behavioral Healthcare Expenditures***
Female dependent	Male subscriber	*.0826 (.0028)*	*.0789 (.0035)*	*.1760 (.0046)*
Male dependent	Female subscriber	*.0774 (.0043)*	*.0794 (.0046)*	*.1876 (.0062)*
Male subscriber	Female dependent	*.0815 (.0050)*	*.0728 (.0057)*	*.2002 (.0065)*
Female subscriber	Male dependent	*.0904 (.0039)*	*.0736 (.0039)*	*.2051 (.0061)*
Female dependent	Female subscriber	*.0902 (.0182)*	*.1088 (.0165)*	*.1609 (.0240)*
Male dependent	Male subscriber	*.0732 (.0132)*	*.0913 (.0160)*	*.1532 (.0198)*
Female subscriber	Female dependent	*.0907 (.0192)*	*.0663 (.0166)*	*.1203 (.0283)*
Male subscriber	Male dependent	*.0937 (.0174)*	*.0681 (.0160)*	*.1648 (.0229)*

Notes: Standard errors shown in parentheses. All regressions also controlled for lagged own utilization, prior-year diagnoses, age group, state and year fixed effects, employer size category, whether carve-out or carve-in, whether fully- or self-insured plan, more vs. less managed plan, whether collective bargaining plan, and whether plan renews on a calendar-year basis. All estimates shown in table and all lagged own utilization terms are significant at $p \leq .001$.

Appendix C- IV estimates of impact of own depression on whether subject has any (1) individual psychotherapy visits, (2) medication management visits, and (3) specialty behavioral healthcare expenditures, instrumenting spouse's current use and controlling for lagged own use.

Subject	***Spouse***	***Any Individual Psychotherapy Visits***	***Any Medication Management Visits***	***Any Specialty Behavioral Healthcare Expenditures***
Female dependent	Male subscriber	*.0813 (.0028)*	*.0788 (.0035)*	*.1748 (.0046)*
Male dependent	Female subscriber	*.0757 (.0042)*	*.0791 (.0046)*	*.1857 (.0062)*
Male subscriber	Female dependent	*.0792 (.0049)*	*.0726 (.0056)*	*.1980 (.0064)*
Female subscriber	Male dependent	*.0886 (.0039)*	*.0735 (.0039)*	*.2032 (.0061)*
Female dependent	Female subscriber	*.0871 (.0178)*	*.1085 (.0164)*	*.1573 (.0234)*
Male dependent	Male subscriber	*.0706 (.0129)*	*.0909 (.0158)*	*.1507 (.0198)*
Female subscriber	Female dependent	*.0870 (.0190)*	*.0656 (.0165)*	*.1196 (.0274)*
Male subscriber	Male dependent	*.0910 (.0166)*	*.0678 (.0159)*	*.1619 (.0226)*

Notes: Standard errors shown in parentheses. All regressions also controlled for lagged own utilization, prior-year diagnoses, age group, state and year fixed effects, employer size category, whether carve-out or carve-in, whether fully- or self-insured plan, more vs. less managed plan, whether collective bargaining plan, and whether plan renews on a calendar-year basis. All estimates shown in table and all lagged own utilization terms are significant at $p \leq .001$.

Appendix D. Impact of whether spouse has any specialty behavioral healthcare expenditures on whether subject has any individual psychotherapy (1), medication management (2) and specialty behavioral healthcare expenditures (3), controlling for whether subject had any specialty behavioral healthcare expenditures in prior year *with SES controls* for the subsample with SES information.

Subject	***Spouse***	***Any Individual Psychotherapy Visits***	***Any Medication Management Visits***	***Any Specialty Behavioral Healthcare Expenditures***
Female dependent	Male subscriber	*.1772 (.0054)*	*.0498 (.0036)*	*.1451 (.0040)*
Male dependent	Female subscriber	*.1009 (.0044)*	*.0400 (.0032)*	*.1044 (.0035)*
Male subscriber	Female dependent	*.1012 (.0042)*	*.0267 (.0016)*	*.0886 (.0034)*
Female subscriber	Male dependent	*.1610 (.0066)*	*.0438 (.0039)*	*.1306 (.0048)*
Female dependent	Female subscriber	*.1408 (.0246)*	*.0846 (.0224)*	*.1702 (.0198)*
Male dependent	Male subscriber	*.1270 (.0180)*	*.0645 (.0195)*	*.1173 (.0184)*
Female subscriber	Female dependent	*.1473 (.0268)*	*.0668 (.0174)*	*.1640 (.0234)*
Male subscriber	Male dependent	*.1260 (.0185)*	*.0509 (.0114)*	*.1207 (.0150)*

Notes: Standard errors shown in parentheses. All regressions also controlled for lagged own utilization, prior-year diagnoses, age group, ethnicity, education, income, assets, state and year fixed effects, employer size category, whether carve-out or carve-in, whether fully- or self-insured plan, more vs. less managed plan, whether collective bargaining plan, and whether plan renews on a calendar-year basis. All estimates shown in table and all lagged own utilization terms are significant at $p \leq .001$.

Appendix E. Impact of whether spouse has any specialty behavioral healthcare expenditures on whether subject has any individual psychotherapy (1), medication management (2), specialty behavioral healthcare expenditures (3), controlling for whether subject had any specialty behavioral healthcare expenditures in prior year *without SES controls* for the subsample with SES information.

Subject	***Spouse***	***Any Individual Psychotherapy Visits***	***Any Medication Management Visits***	***Any Specialty Behavioral Healthcare Expenditures***
Female dependent	Male subscriber	*.1783 (.0054)*	*.0500 (.0036)*	*.1463 (.0041)*
Male dependent	Female subscriber	*.1013 (.0044)*	*.0402 (.0033)*	*.1050 (.0035)*
Male subscriber	Female dependent	*.1018 (.0042)*	*.0269 (.0016)*	*.0893 (.0034)*
Female subscriber	Male dependent	*.1618 (.0067)*	*.0439 (.0039)*	*.1315 (.0049)*
Female dependent	Female subscriber	*.1426 (.0247)*	*.0855 (.0223)*	*.1702 (.0202)*
Male dependent	Male subscriber	*.1271 (.0186)*	*.0644 (.0196)*	*.1170 (.0186)*
Female subscriber	Female dependent	*.1448 (.0273)*	*.0663 (.0177)*	*.1627 (.0229)*
Male subscriber	Male dependent	*.1270 (.0186)*	*.0507 (.0114)*	*.1218 (.0151)*

Notes: Standard errors shown in parentheses. All regressions also controlled for lagged own utilization, prior-year diagnoses, age group, state and year fixed effects, employer size category, whether carve-out or carve-in, whether fully- or self-insured plan, more vs. less managed plan, whether collective bargaining plan, and whether plan renews on a calendar-year basis. All estimates shown in table and all lagged own utilization terms are significant at $p \leq .001$.