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Causal Spousal Health Spillover Effects and Implications for Program Evaluation†

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Current methods of cost effectiveness analysis implicitly assume zero spillovers among social ties. This can underestimate the benefits of health interventions and misallocate resources toward interventions with lower comprehensive effects. We discuss the implications of social spillovers for program evaluation and document the first evidence of causal spillovers of health behaviors between spouses by leveraging experimental data from the Lung Health Study (smoking) and COMBINE Study (drinking). We find large decreases in spousal substance use from treatments with a therapy component, which reduces the incremental cost effectiveness ratios of some treatments by 12 to 18 percent. (*JEL* D61, H52, I12, I18, J12)

Large literatures in public health and health economics provide evidence that health behaviors, such as tobacco use (Clark and Etilé 2006; Cobb et al. 2014; Cohen-Cole and Fletcher 2008; Falba and Sindelar 2008; Franks, Pienta, and Wray 2002) and alcohol use (Falba and Sindelar 2008; Meyler, Stimpson, and Peek 2007), are highly correlated among spouses. Additionally, several models of household decision making suggest the importance of co-determination of consumption and related decisions, including labor market outcomes (Goux, Maurin, and Petrongolo 2014; Hospido and Zamarro 2014) as well as health related behaviors (McGeary 2015).[1] Some analyses that attempt to separate causal and selection pathways suggest that selection is the main explanation for the correlation between spouses (Clark and Etilé 2006). On the other hand, experimental evidence using other social ties finds causal

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[1] Wilson 2002 discusses the mechanisms that may drive inter-spousal correlation in health status including assortative matching on health-related characteristics such as education, shared environment and lifestyle, and

spillovers, such as in roommate smoking and drinking (Eisenberg, Golberstein, and Whitlock 2014). No current research has been able to leverage experimental designs to estimate causal effects of health behaviors between spouses. We use data from two health randomized controlled trials (RCTs) to fill in this gap in the literature.

In addition to providing novel causal spillovers in health behaviors, our analysis has implications for program evaluation of RCTs in health settings. Although RCTs are widely considered to be the gold standard for detecting causal effects of treatments, the clean estimates they afford are often limited by the focus of data collection and analysis on subjects alone. Such estimates inform effectiveness analyses, which have growing impact due to the passage of the Affordable Care Act and introduction of the Patient-Centered Outcomes Research Institute. While experiments provide clean estimates of treatment effects for trial subjects, evaluations generally do not account for the full social impact of an intervention through indirect effects, which may not follow the same pattern as the subject-only analysis.

Social effects may occur through the utility or, as we show in our analysis, the behavior and outcomes of a subject's family and peers. We examine the latter case by evaluating spousal spillover effects on substance use and calculating implications for cost-effectiveness in two addiction interventions. Not surprisingly, our causal spousal spillover estimates imply that traditional cost effectiveness analysis on the RCTs we analyze undercounts the social effectiveness of the treatments. Our comprehensive cost-effectiveness analysis finds that including spillovers decreases the incremental cost effectiveness ratios of Lung Health Study treatments (relative to usual care) by 11.9 to 13.4 percent and the COMBINE study's therapy treatment by 17.7 percent. Importantly, if these treatments also have spillover effects on non-spousal social ties (e.g., friends), our estimates still undercount potential social benefits. We also present a case where the naive effectiveness ordering would flip when incorporating the comprehensive effects, suggesting the importance of estimating social spillovers both for detecting levels of treatment benefits and for relative rankings across treatments that may have different levels of indirect benefits.

Related Literature.—Although an extensive literature provides evidence of spousal correlations in health (and other) behaviors and a small but growing literature considers social spillover effects in program evaluation, there is currently no causal evidence in the former and therefore also no specific discussion of implications for program evaluation.[2] The implication of a causal relationship among socially connected individuals in health (and other) behavioral outcomes is that a treatment or

direct effects (e.g., second-hand smoke or disease transmission). He finds that controlling for related observables decreases the health correlation coefficient by one-third to one-half, signifying that co-determination of household health also has a significant unobserved behavioral component.

[2] Manski (2000) outlines at least three mechanisms whereby an action of one agent may affect another agent—constraints, expectations, and preferences. Our analysis examines the social spillovers of a treatment to reduce smoking (or problem drinking), which could affect a spouse through multiple channels. It is possible that a person's decision to quit smoking relaxes the household budget constraint and could allow a spouse to smoke more (*constraint interactions* in Manski 2000). Likewise, a person's decision to quit smoking could create observational learning opportunities that shape the expectations of the spouse (*expectation interactions* in Manski). Finally, smoking could be a joint activity for some couples, so that a spouse's decision to quit smoking could affect the other person's preference ordering over alternative choices, e.g., continue smoking versus quitting (*preference interactions* in Manksi). Our research design is not able to separate these various social mechanisms.

program that reduces, for example, tobacco use for one adult may also reduce use for other adults within her social or family network.[3] While there is no direct causal evidence of the existence and magnitude of these spillover effects within clinical treatment settings—and a current implicit spillover estimate of zero in evaluations—there are several related literatures that suggest the importance of spillover effects between spouses.

Quasi-experimental evidence in other contexts suggests that the correlation in health behaviors is in fact causal. For example, Eisenberg, Golberstein, and Whitlock (2014) utilize random college roommate assignment to estimate causal effects on risky behaviors, including substance use, and find strong spillovers in binge drinking, divergent spillovers by gender for smoking, and little evidence for effects on gambling, risky sexual behavior, or self-injury. A roommate's (precollege) binge drinking increases the probability of the other roommate binge drinking by 8.6 percentage points or a 19 percent increase relative to baseline. The same relationship holds by gender in drinking, but a female smoking decreases her roommate's probability of smoking by a marginally significant 7.8 percentage points, while a male smoking increases his roommate's smoking probability by 5.7 percentage points. Likewise, Carrell, Hoekstra, and West (2011) show that random assignment to a squadron in the Air Force Academy with highly fit peers increases health outcomes, and Babcock and Hartman (2010) find spillovers on untreated students that grow with the number of friendship links to treated students in a field experiment incentivizing gym usage.

An expanding body of work exploits experimental variation to identify spillovers when subsets of observed populations are treated using the "partial population" approach outlined by Moffitt (2001). For example, several papers analyze spillovers from the Mexican conditional cash transfer program PROGRESA onto non-recipient households, finding effects on consumption (Angelucci and De Giorgi 2009) and secondary school enrollment decisions (Bobonis and Finan 2009). Miguel and Kremer (2004) estimate treatment externalities of a deworming intervention in Kenya, and find large enough improvements in health and school attendance among untreated children to justify larger subsidies for the intervention. Studies have also uncovered large effects on program take-up among untreated peers in varied contexts, including an age-based expansion of a paternity leave policy in Norway (Dahl, Løken, and Mogstad 2014) as well as an incentive to attend a retirement program information fair that led increased subsequent utilization of the benefit even among untreated employees (Duflo and Saez 2004).

We contribute to the literature in several ways. First, we provide estimates of significant treatment spillovers in clinical experiments regarding smoking and alcohol consumption. Such spillovers are of policy relevance in determining returns to treatments, but to our knowledge have not been previously estimated. We also utilize the trial's randomized allocation of treatments as an instrument for estimating causal behavioral spillovers between spouses, an element of the overall treatment

[3] This sort of spillover is separate from spillovers on the *utility* of a treated subject's spouse or social network. For a discussion of spillovers and utility, see Basu and Meltzer (2005), which explores how treatment choices may affect the utility of untreated family members, concluding that utility spillovers are important but the policy implications of the phenomenon raise equity concerns.

spillover. Finally, we employ our treatment spillover estimates in a cost effectiveness reanalysis of the interventions to show how accounting for spousal spillovers may affect cost effectiveness conclusions.

I. Model

Define the recipient of a randomized treatment as the "subject" and the recipient's husband or wife as the "spouse." A simultaneous equations environment where each individual's behavior affects the other characterizes a couple's jointly determined substance use. For couple i, let the subject's substance use be determined as in the first equation below: by previous use, contemporaneous spousal use, and an effect due to treatment. Spousal substance use is symmetric except that since she is not assigned to treatment, her partner's treatment assignment is referenced:

$$\textit{subject use}_{it} = \alpha_1 + \alpha_2 T_i + \alpha_3 \textit{spouse use}_{it} + \alpha_4 \textit{subject use}_{it-1} + \varepsilon_{it}$$

$$\textit{spouse use}_{it} = \beta_1 + \beta_2 T_i + \beta_3 \textit{subject use}_{it} + \beta_4 \textit{spouse use}_{it-1} + \eta_{it}.$$

Then α_2 is the direct treatment effect from the intervention while α_3 and β_3 are endogenous spousal peer effects. Endogenous peer effects may operate through several mechanisms including preferences for joint consumption or conformity, or via agents' budget constraints if the cost of obtaining the substance changes when one member of the pair discontinues use. Direct treatment spillovers, represented by β_2, are effects of the intervention on a subject's spouse that do not occur through changes in the subject's substance use outcome. For example, if a subject shares a helpful booklet about quitting methods, we consider this a direct treatment spillover, but the extent to which those methods change spousal use through changes in the subject's use is not (this portion operates through the α_3 term). Broadly, what we consider to be direct effects are likely a product of information transmission, which may change the agents' individual cost benefit calculus. Effects from lagged outcomes, parameters α_4 and β_4, account for persistence in substance use. With two endogenous variables (subject and spousal use) and only one instrument (treatment, due to random assignment), we cannot immediately estimate the full model to recover all structural parameters.

To characterize the total effect of the treatment on a subject's spouse, we show the reduced-form equation for spousal use.[4] The estimating equation for spousal substance use is

$$\textit{spouse use}_{it} = \frac{\beta_1 + \beta_3\alpha_1}{1 - \beta_3\alpha_3} + \frac{\beta_2 + \beta_3\alpha_2}{1 - \beta_3\alpha_3} T_i + \frac{\beta_3\alpha_4}{1 - \beta_3\alpha_3} \textit{subject use}_{it-1}$$

$$+ \frac{\beta_4}{1 - \beta_3\alpha_3} \textit{spouse use}_{it-1} + \frac{\beta_3\varepsilon_{it} + \eta_{it}}{1 - \beta_3\alpha_3}.$$

[4]Note that prior subject use is ignored in our subsequent regressions because it will be "on" for all subjects by virtue of the study selection process, but we include it here for completeness.

Then the term $\frac{\beta_2 + \beta_3\alpha_2}{1 - \beta_3\alpha_3}$ gives the total effect of being the spouse of a subject who is randomized to treatment T, which we call the treatment spillover effect. Although it does not directly estimate any structural parameters, this is of interest as it captures previously unmeasured benefits of the treatment. If behavioral spillovers from the subject to the spouse do not exist ($\beta_3 = 0$), the term reduces to the direct treatment spillover effect, β_2. Finally, since treatment is randomly assigned in our interventions, we assume the composite error term is uncorrelated with T_i and lagged use, so we can estimate the reduced-form equation without further complications.

II. Data

We analyze data from two separate substance abuse studies, the Lung Health Study and the COMBINE Study. The studies have different strengths for our purposes. First, spillover effects may vary across behaviors and intervention types. The existing spillovers literature finds stronger spousal spillovers in smoking than in alcohol use (Meyler, Stimpson, and Peek 2007), so it is useful to examine effects in both contexts. Furthermore, the Lung Health Study benefits from a large sample size and long follow-up period, while COMBINE's primary strength is its diversity of types of treatments, which permits investigation of how spillovers vary across pharmaceutical and behavioral interventions.

A. *Lung Health Study*

The Lung Health Study (LHS) was designed to test for effects of two interventions on lung function among adults at high risk of Chronic Obstructive Pulmonary Disease aged 35–59 in the late 1980s. The study's 5,887 participants were randomized into three groups: usual care (control), an "intensive state-of-the-art smoking cessation program" plus an inhaled bronchodilator (SIA), or the same smoking cessation therapy program along with a placebo bronchodilator (SIP) (Anthonisen 2004). Therapy interventions included personalized, physician-led evaluation and discussion of health implications, an orientation session with family and friends, which revisited the physician discussion and treatment procedures, and improved access to nicotine gum. Additional group therapy meetings, which spouses could attend, covering quitting strategies and stress management were held four times during an intensive "quit week" period and nearly weekly over the following 2.5 months (O'Hara et al. 1993). Both the SIA and SIP interventions incorporated a behavioral/therapy component, so all spouses of subjects not assigned to the control group may have benefitted from direct exposure to the treatments' behavioral components. However, subjects were asked whether their spouse (or "other person who supports your efforts to quit smoking") was present at the special intervention orientation visit, which we use to condition our analysis. The first stage of the study followed participants annually for five years, though later iterations extended that period.

A key aspect of the data is that respondents are asked their marital status at baseline and the smoking status of other members of their household at each survey

wave. We restrict our sample to the 3,874 subjects who reported being married at baseline, and give extra attention to the 1,391 married subjects with a smoker spouse at the study's onset. Some individuals reported a spousal smoking status despite not being married. Of these respondents, 5 were never married and 243 were widowed, separated, or divorced.[5] We assume that marital status does not differentially change from baseline to infer spousal nonsmoking rates in posttreatment periods.

Group summary statistics for the full LHS sample as well as the married and married-to-smoker at baseline subsamples are presented in Table 1. The average LHS respondent is 48.5 years old, has 1.6 years of postsecondary education, and is slightly overweight according to BMI. Only 37 percent of respondents are women, but two-thirds are married and 36 percent of spouses smoke. Almost the entire subject sample smoked at baseline (those who did not were recently smokers) and subjects smoked nearly 30 cigarettes per day on average. Although *t*-tests find statistical differences between the full sample and the married or married-to-smoker subsamples, the differences are practically small; the main qualitative differences are that married subjects are less likely to be female, married-to-smoker subjects smoke more cigarettes per day, and married subgroups are slightly less educated than the full sample.

The right-most column of Table 1 displays characteristics for a particular group of interest: married subjects with smoker spouses who did not attend the therapy intervention orientation. Spouses of this group experience a particularly "pure" spillover in that they are unlikely to be directly present for the therapy intervention sessions, so effects on these spouses come at no extra cost to the experimenter. Clearly membership in this group is not randomly assigned, and furthermore we do not observe whether control group spouses would have attended the session if they had been treated. As such, we are particularly interested in how selection into this group may be driven by spousal support for the subject's smoking cessation objectives. We report the share of subjects who answered "Yes" or "No" to the question, "Would your spouse (or closest friend, if not married) like you to give up smoking?" Subjects attached to nonattender baseline smoker spouses expect only slightly lower levels of cessation support. The full distribution of support expectations by subsample and treatment status is reported in Appendix Table A1. In subsequent tables, columns that refer to the "Nonattender" (NA) group include all control subjects/spouses as well as treated subjects/spouses with spouses not present at the intervention orientation.

Balance on observables across treatments for the married sample and the married-to-smoker sample is tested in Online Table A2. Although we would ideally verify balance on a variety of spousal characteristics, we only observe smoking behavior (and partially support for quitting and attendance at therapy interventions for non-control subjects). Instead, we focus on those outcomes that are available (which includes a baseline measure of the main outcome of interest) and rely on

[5] Due to a skip pattern in the survey, respondents only answer whether they have a smoker spouse or not after treatment if they report that there are any other smokers in their household. To address this, we assume that individuals who were married at baseline stay married thereafter, so we impose that a spouse is not smoking for a wave whenever a baseline married subject says there are no other smokers in the household. Additionally, in the raw LHS data, some unmarried individuals responded with a spousal smoking status—these individuals are not included in the spousal subsamples.

TABLE 1—LUNG HEALTH STUDY BASELINE SUBJECT CHARACTERISTICS

	All lung health study participants			
		Married		
			Smoker spouse	
				Nonattender
Age	48.47	48.5	48.31	48.11
	(6.83)	(6.76)	(6.56)	(6.57)
Years of education	13.61	13.49	13.31	13.53
	(2.82)	(2.81)	(2.82)	(2.88)
Body mass index	25.56	25.78	25.57	25.59
	(3.93)	(3.82)	(3.78)	(3.86)
Cigarettes/day	29.54	29.49	31.26	30.81
	(13.93)	(13.82)	(14.2)	(14.3)
Female	0.37	0.32	0.36	0.35
Buddy support "Yes"	0.898	0.916	0.845	0.828
Buddy support "No"	0.017	0.011	0.022	0.027
Married	0.66	1	1	1
Spouse smokes	—	0.36	1	1
Subjects	5,887	3,874	1,391	857

Notes: Standard deviations are in parentheses. "Nonattender" group includes all control subjects as well as treated subjects whose spouse did not accompany the subject to the therapy intervention orientation. "Buddy support" responses are to a question asked of all subjects: "Would your spouse (or closest friend, if not married) like you to give up smoking? Options were: yes, unsure, doesn't care, no, or N/A.

spousal concordance in other measures to link subject characteristic balance to spousal balance. The leftmost column within each group gives means for the control subjects of each subsample, while the next columns give the deviations for all treated subjects and treated subjects with a nonattender spouse, respectively. Subject characteristics for these subsamples are balanced, as differences between groups are small and few are statistically significant at the 5 percent level. In the full married sample, the nonattender treatment subgroup has a lower baseline spousal smoking rate and higher share of subjects reporting a lack of spousal support for cessation, motivating controls for baseline smoking status and spousal support. There was no variation in retention rates among subjects in the married sample.

COMBINE Study.—The COMBINE Study was designed to test for interactions in the effectiveness of several alcoholism treatments. Participants were assigned to treatments where they received 16 weeks of any combination of a placebo, Naltrexone or Acamprosate (pharmaceutical treatments), and a combined behavioral intervention (CBI). The CBI treatment consisted of "up to twenty 50-minute sessions integrat[ing] aspects of cognitive behavioral therapy, 12-step facilitation, motivational interviewing, and support system involvement," which could include spouses (Anton et al. 2006). Although the follow-up period continued for more than a year, information about the 1,375 subjects' peers was only collected 16 and 26 weeks after the intervention began (Anton et al. 2006). Subject peer information includes the relationship description, drinking behavior, attitudes toward drinking and the intervention, as well as intensity of contact for up to ten relations.

TABLE 2—COMBINE BASELINE SUBJECT CHARACTERISTICS

	Full sample		Married sample		Drinker spouses	
	Mean	Standard deviation	Mean	Standard deviation	Mean	Standard deviation
Age	44.84	10.26	47.87	9.40	48.46	9.39
Education	14.59	2.69	15.24	2.55	15.56	2.54
Female	0.31	–	0.32	–	0.58	–
White	0.77	–	0.85	–	0.91	–
Attends AA	0.21	–	0.15	–	0.09	–
Drinks/day	9.08	6.48	8.01	5.09	7.87	5.29
Pct. heavy drinking days	65.49	28.27	66.49	27.85	72.14	25.6
Observations	1,340		426		113	

Note: Samples are restricted to respondents with full information to match regression sample.

Unlike LHS, which primarily targeted improved lung health but acknowledged smoking cessation as a means to that end, COMBINE expressly sought reductions in problem drinking. Anton et al. (2006) find that treatments including Naltrexone, Therapy/CBI, or both significantly reduced drinking as measured by their primary measures, percent days abstinent and time to first heavy drinking day, while Acamprosate did not improve drinking outcomes. Table 2 indicates some differences in observables between the full sample, married subjects, and subjects married to spouses who were reported as moderate or heavy drinkers at baseline (hereafter, "drinker spouses").[6] Married subjects, just under one-third of the full sample, were 3 years older than the full sample average of 44.8 years old, had three-quarters of a year more education than the full sample mean of 14.59 years, were more likely to be white, and less likely to attend Alcoholics Anonymous (AA). Subjects married to drinker spouses were almost twice as likely to be female, were much less likely to attend AA, and drank fewer drinks per day, but with a higher incidence of heavy drinking days. Subjects with a heavy drinking spouse at baseline accounted for about a tenth of the married subsample and almost 40 percent of the drinker spouse sample.

Spousal drinking outcomes were collected as part of a survey in which a subject could indicate that his or her spouse was a recovering alcoholic, abstained from drinking, or was a light, moderate, or heavy drinker. Spouses listed as recovering alcoholics at baseline only comprised about 1 percent of spouses and were excluded from our analysis because of the response's ambiguous implications for use. Baseline drinking intensity was balanced by treatment for both spouses and subjects. Each row of Appendix Table A3 provides the control mean as well as treatment differences and the F-statistic from an OLS regression on the dichotomous-specified drinking outcome. Subject treatment assignment explains little of the variation in baseline spousal outcomes and even less in subject outcomes. As with the LHS data, we are only able to test for spousal balance on baseline substance use information for our main outcome. One potential issue is that spouses of subjects in therapy treatments were 9 percentage points less likely to be light drinkers and 7 percentage points

[6] Married and drinker samples restricted to subjects with baseline spousal drinking information.

more likely to be abstainers at baseline. As a result, we control for baseline spousal drinking behavior and (when noted) restrict the sample to the pool of moderate and heavy drinkers at baseline. Ordered logit regressions of the categorical spousal drinking measures also yield no significant differences.

Effect of Treatments on Subjects.—Before examining treatment spillovers, it is useful to contextualize the first-order effects of the treatments on subjects. Table A4 presents treatment effects on smoking status among treatment subjects in LHS for different subgroups and specifications. From left to right the sample is restricted to unmarried subjects with no smokers in their households, married subjects, then subjects married to smoker spouses. Within each group we then restrict to subjects without a buddy/spouse present at the therapy intervention orientation. For each group we present treatment effects conditioning on wave dummies, demographics, a baseline smoking intensity measure (number of cigarettes per day), and spousal support. Estimates pool across all five posttreatment years. Not shown are limited dependent variable models, which yield nearly identical average marginal effects. For the LHS analyses we generally combine SIA and SIP interventions into a single treatment variable because effects on cessation vary little across the two treatment arms.

Treatment effects range from a 20 to 26 percentage point decrease in smoking rates due to the interventions. Effects were smallest for unmarried subjects without any smokers in the household, and effect sizes varied little between the full married sample and the subsample married to a smoker at baseline. Effects were slightly smaller for subjects without an attending buddy. Covariate choices have little impact on effect sizes in all cases, reflecting the balance across treatment status.

Treatment effects on subjects were smaller in the COMBINE study for the margin we analyze. Table A5 gives the effect of the inclusion of each of the treatments, possibly as part of a multi-pronged approach. Effects listed are percentage point changes in the probability of heavy drinking pooled across 16 and 26 weeks after baseline.[7] For each subsample of subjects, effects are shown for the simple conditional mean adjusting only for an indicator at week 26 as well as a full set of covariates including baseline percent heavy drinking days and demographics. Consistent with prior work, treatments involving Naltrexone or Therapy generally induced larger reductions in drinking across groups. Naltrexone was the most effective at reducing heavy drinking among unmarried subjects, but married subjects experienced larger reductions from therapy (though the effects are not statistically different from each other). The addition of covariates again has little effect on estimates.

III. Treatment Spillovers

The Lung Health Study reports spousal smoking status as a binary variable, so we consider linear and logit models of smoking status. As above, we combine the SIA

[7] To maximize the spousal sample size and maintain a consistent sample across spousal and subject results, we define our spousal heavy drinking variable as greater than two drinks per day. Spouses were asked directly about their own heavy drinking status, but the question has a higher missing rate than the drinks per day question. The constructed method with the cutoff at two is highly correlated with non-missing survey responses, and estimates vary little with this choice.

and SIP interventions into one treatment for simplicity because the treatments have very similar effects. The COMBINE data report categories of intensity of substance use, so we also test for outcomes on the margin of heavy drinking and across all categories using ordered logit models. Since both studies have spousal outcomes for multiple posttreatment periods, our main specifications pool across waves but include wave dummies and cluster standard errors at the individual level.

A. *Effect of Having a Spouse Selected for Treatment: LHS*

To test the reduced-form treatment effect on smoking for spouses of subjects assigned to the interventions, we estimate the equation below. The baseline model (columns 1 and 4) includes only treatment indicators, wave dummies (λ_t), demographics (subject age, sex, education, and BMI), and a baseline smoking status indicator (when not redundant given the sample). Columns 2 and 5 then condition the sample to nonattender spouses, and columns 3 and 6 control for subjects' assessment of the spouse's support for cessation, which is presumably a driving force behind attendance. The tables report estimates of γ_1, the spillover effects of treatment assignment on spousal smoking among spouses. Recall that these are not structural parameters, but rather the (reduced form) policy-relevant causal effects of the treatment on spousal behavior:

$$spouse\ smokes_{it} = \gamma_1(SIA \vee SIP) + \lambda_t + X_{j(i)}\beta_3 + \varepsilon_{it}.$$

Table 3 presents the main OLS estimates of spousal spillovers for the specifications described above. Across all married couples, assignment to any treatment (either SIA (behavioral intervention + bronchodilator) or SIP (behavioral intervention + placebo)) decreased a spouse's smoking probability by 5.5 percentage points. Estimates were larger among baseline smoker spouses, for whom we estimate a 12.3 percentage point decrease from treatment. Accounting for the baseline smoking rates across samples, both sets of unconditional estimates amount to a 15 percent decrease in smoking rates. Conditioning the sample on nonattendance at the therapy intervention reduces treatment spillovers by almost half, to a 3 percentage point or 6.9 percentage point effect for all spouses and smoker spouses, respectively. Treated spouses in this (nonattender) sample are a selected group of spouses who may be less invested in their spouse quitting, so these estimates are likely a lower bound of what spillovers we would expect to see when including the willing-to-attend spouses in a counterfactual world where attendance was not possible. Adding dummies for the subject's assessment of spousal support response categories slightly increases these estimates, suggesting that these variables account for some of the differential selection into this sample among the treated group. Marginal effects from analogous logit models are nearly identical to those in Table 3.[8]

Separate estimates by subgroups are presented in Appendix Table A6. Treatment effects for the pooled treatments are estimated using the base model separately for

[8]Not shown—available upon request.

TABLE 3—TREATMENT EFFECTS ON SPOUSAL SMOKING (LHS)

	All spouses			Baseline smoker spouses		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	−0.055	−0.030	−0.032	−0.123	−0.069	−0.072
	(0.009)	(0.010)	(0.010)	(0.020)	(0.024)	(0.024)
Baseline smoker	x	x	x	Sample	Sample	Sample
Nonattender		Sample	Sample		Sample	Sample
Spousal support			x			x
Observations	18,856	12,880	12,880	6,751	4,154	4,154
Individuals	3,874	2,646	2,646	1,391	857	857

Notes: Standard errors are in parentheses. Outcome equals 1 if subject smoked at year *t*. Each estimate gives the effect (in percentage points) of treatment (SIA or SIP) on the probability of being a smoker for spouses of subjects in the Lung Health Study. Estimates are from linear regressions that pool across years 1–5 posttreatment and control for wave dummies, demographics (age, sex, education, and BMI), and variables marked with "x." "Spousal support" control adds dummies for categorical responses to a question asked of all subjects: "Would your spouse (or closest friend, if not married) like you to give up smoking?" Columns with a "Sample" marking are restricted to the group noted by that row. "Nonattender" group includes all control spouses as well as treated spouses who did not accompany the subject to the therapy intervention orientation. "Baseline smoker" indicator/sample restriction refers to the spouse's smoking status at baseline.

wives (subject is male), husbands (subject is female), and spouses of subjects who are high school educated or less, or more than high school educated.[9] For each subgroup we also present results for the full married sample, baseline smoker spouses, and baseline non-smoker (N.S.) spouses. We only observe meaningful heterogeneity in effects by education, where effect sizes are about 50 percent larger for spouses of subjects with greater than a high school degree, though the difference is not statistically significant. The lack of a differential between husbands and wives is a slight departure from the results mentioned above in Eisenberg, Golberstein, and Whitlock (2014), though their data were college students in same-sex roommate pairs rather than married couples. Patterns between columns vary little by baseline smoking status.[10]

B. *Effect of Having a Spouse Selected for Treatment: COMBINE*

The COMBINE study also induced treatment spillovers on spouses. Table 4 presents the effects of a subject's assignment to a treatment including the listed interventions on his spouse's (dichotomous) heavy drinking status. The effects are robust to specification choice and consistent as percentage changes from baseline across samples. Among all spouses, only therapy had a statistically significant spillover

[9] We also investigated heterogeneity by age, but did not find any evidence of differences.

[10] One additional dimension of heterogeneity is that free access to nicotine gum was extended to spouses for six months, indicating some possible direct treatment of spouses. However, spouses were required to have a prescription from their own doctor for the gum (O'Hara et al. 1993). To assess the extent to which the possible direct treatment of nicotine gum availability affects our spillover estimates, we examined heterogeneity by spouses' interest in trying nicotine gum (Table A7). Spillovers were smaller for spouses not named the subject's "buddy" (a prerequisite for the question on gum interest), but spouses interested in gum had only slightly larger spillovers than to those not interested in gum in the full sample and had smaller spillovers in the baseline smoker sample.

TABLE 4—TREATMENT SPILLOVERS ON SPOUSAL HEAVY DRINKING (COMBINE)

	All spouses		Baseline drinker spouses	
	(1)	(2)	(3)	(4)
Acamprosate	−0.007	−0.007	−0.01	0.003
	(0.018)	(0.018)	(0.064)	(0.067)
Naltrexone	−0.015	−0.013	−0.043	−0.056
	(0.019)	(0.019)	(0.067)	(0.071)
Therapy	−0.036	−0.033	−0.130	−0.142
	(0.017)	(0.017)	(0.062)	(0.061)
Week 26 dummy	x	x	x	x
BL drinker dummy	x	x	x	x
Demographics		x		x
Observations	770	770	209	209
Clusters	426	426	113	113

Notes: Standard errors are in parentheses. Outcome equals 1 if subject was a heavy drinker in week *t*. "Baseline drinker" spouses were either moderate or heavy drinkers at baseline. Each estimate gives the effect (in percentage points) of a treatment on the probability of being a heavy drinker for spouses of subjects in the COMBINE Study. Estimates are from linear regressions that pool across weeks 16 and 26 posttreatment and control for the variables marked with "x" by column.

effect—a 3.3 percentage point decrease in spousal heavy drinking. For the sample of moderate and heavy drinking spouses at baseline, therapy induced a larger 13 to 14 percentage point decrease in heavy drinking rates, and both effects represent a 30 percent reduction in baseline heavy drinking rates. In this sample, Naltrexone also has a practically significant impact estimate of about 5 percentage points, though it is imprecisely estimated.

These results contrast the subject impacts, where Naltrexone dominated in cessation effects. If spouse-to-spouse behavioral spillovers dominated the reduced-form spillover effect, then we would expect Naltrexone to have the largest spillovers on spouses. Since therapy interventions have larger spousal effects and are more likely to involve direct spillovers to spouses, these results suggest that behavioral spillovers between spouses may not be as important as direct treatment spillovers in higher alcohol-use couples.

Since the spousal drinking measure provides more detail than just a dichotomous heavy drinking outcome, we examine how treatment affects drinking behavior across the spectrum of drinking intensity. Although ordered logit is a natural statistical choice here, for ease of interpretation we convert categories to counts of drinks per week and present OLS estimates in Appendix Table A8. Our conversion is as follows: 0 drinks per week for abstainers, 3 for light drinking, 9 for moderate drinking, and 18 for heavy drinking. Therapy was the only intervention to have a statistical effect on spousal drinks per week, with an effect of 0.7 fewer drinks per week for the full spousal sample and just under 2 fewer drinks per week for the baseline drinker spouse sample. Ordered logit results follow the same pattern of effect sizes and statistical significance as the OLS results.[11]

[11] Not shown—available upon request.

The COMBINE study lacks the long posttreatment history of the LHS, but it is possible to examine spillovers separately in weeks 16 and 26. Results in Appendix Table A9 suggest that the therapy interventions did not have their full impact by the end of the 16-week therapy treatment. Week 26 reductions in heavy drinking were about twice the size of week 16 effects, yielding 5 and almost 19 percentage point heavy drinking reductions for the full married and baseline drinker spouse populations, respectively. This stands in contrast to the weekly results for subjects (not shown), which changed little between weeks 16 and 26 and again saw larger effects for Naltrexone than therapy across the timeframe. Furthermore, while the relatively short follow-up period of the COMBINE study is a limitation, the fact that we observe an increase in effects over the two follow-up periods available suggests that the estimated spillovers were not merely a short-lived phenomenon. Subgroup inference is limited by the smaller sample size, but results by gender suggest that effects were larger for males than for females.[12]

IV. Behavioral Spillovers

To estimate the effect of subject substance use on spousal use we instead use treatment assignment as an instrumental variable. Since a subject and spouse's substance use are endogenously co-determined, OLS regression of one spouse's substance use on the other's use will give a biased estimate of the causal behavioral spillover. To remedy this we use random treatment assignment as an instrument for subject behavior. The validity of the IV results depends on the exclusion restriction $\beta_2 = 0$ from the spouse's structural equation; that is, the treatment must only affect spouses via changes in the subjects' substance use. We present results assuming this assumption holds and show how sensitive the estimates are to violations of the exclusion restriction.

We estimate the following two-stage relationship for each couple i and using treatments $k \in K$ as instruments:

$$\textit{subject smokes}_{it} = \rho_1 + \sum_k \rho_k T_k + \lambda_{1t} + \varepsilon_{it}$$

$$\textit{spouse smokes}_{it} = \delta_1 + \delta_2 \textit{subject smokes}_{it} + \lambda_{2t} + \upsilon_{it}.$$

We focus on dichotomous measures (subject and spouse smoking status), but also inspect specifications with somewhat continuous measures (subject and spouse's average number of cigarettes smoked per day). To formally account for the possibility that the exclusion restriction does not hold, we employ the methods described by Conley, Hansen, and Rossi (2012) and estimate the peer effect under various priors for the direct treatment spillover. Specifically, we assume the treatment

[12] Not shown—available upon request. This dimension of heterogeneity helps explain why COMBINE spousal spillover estimates are larger than subject effects for therapy. The reduction in heavy drinking due to therapy in the subject sample comes through females, who comprise 1/3 of the sample. This means about 2/3 of the spousal sample is female and therefore the average effect is larger. When interacting therapy treatment with female, effects are about twice as large for subjects.

TABLE 5—EFFECT OF SUBJECT SMOKING ON SPOUSAL SMOKING

	All nonattender spouses		Baseline smoker nonattender spouses	
Estimates	OLS	2SLS	OLS	2SLS
Subject smoking	0.119	0.130	0.292	0.308
	(0.010)	(0.038)	(0.027)	(0.095)
First-stage *F*-stat		282.36		82.43
Observations	12,880	12,880	4,154	4,154
Clusters	2,646	2,646	857	857
Robustness check–2SLS confidence intervals				
Violation size	Direct ($\tilde{\delta}$)	Union of C.I.	Direct	Union of C.I.
0 percent of reduced-form	0.0	[0.05, 0.199]	0.0	[0.124, 0.493]
20 percent of reduced-form	−0.006	[0.026, 0.199]	−0.014	[0.064, 0.493]
40 percent of reduced-form	−0.012	[0.002, 0.199]	−0.028	[0.004, 0.493]
50 percent of reduced-form	−0.015	[−0.01, 0.199]	−0.035	[−0.027, 0.493]

Notes: Standard errors are in parentheses. Each estimate gives the effect (in percentage points) of subject smoking on the probability of being a smoker for spouses of subjects in the Lung Health Study. Pools across years 1–5 posttreatment and controls for wave dummies, demographics (age, sex, education, and BMI), baseline smoking status (full sample regressions only), and dummies for categorical responses to a question asked of all subjects: "Would your spouse (or closest friend, if not married) like you to give up smoking?" Sample restricted to "Nonattender" group, which includes all control spouses as well as treated spouses who did not accompany the subject to the therapy intervention orientation (and are therefore more likely to satisfy the exclusion restriction). "Baseline smoker" indicator/sample restriction refers to the spouse's smoking status at baseline. Lower section provides union of 95 percent confidence intervals in brackets based on $U(-\tilde{\delta}, 0)$ prior for direct treatment effect. That is, each confidence interval allows exclusion restriction violations as large as $\tilde{\delta}$, which corresponds to the stated percentage of the group's reduced-form effect. Methodology is described in Conley, Hansen, and Rossi (2012). Specification follows that of the corresponding IV results in the upper portion of the table.

directly decreases spousal smoking by at most $\tilde{\beta}_2$ (determining a lower bound on the treatment effect) and calculate 95 percent confidence intervals for several levels of $\tilde{\beta}_2$. We fix the upper bound of $\tilde{\beta}_2$ at 0 (no effect), so the upper range of confidence intervals remains fixed within each column.

Table 5 presents estimates of behavioral spillovers of subject smoking (instrumented by randomized treatment) on spousal smoking. We control for spousal interest in subject smoking cessation and restrict the sample to pairs with nonattender spouses to more plausibly satisfy the exclusion restriction. As a result, we obtain a local average treatment effect (LATE) for behavioral spillovers from subjects who were induced to quit by the treatment, and our results are not generalizable to the sample of spouses who *do* attend sessions, which may have weaker social ties.

The biased OLS regression suggests that among baseline smoker couples, a subject's smoking makes his or her spouse in the full nonattender spousal sample 11.9 percentage points more likely to smoke, and almost 30 percentage points more likely to smoke in the baseline smoker spouse sample. In both samples, IV estimates of behavioral spillovers are similar to OLS estimates, 13 percentage points for the full married sample and 30 percentage points for the baseline smoker sample.[13] If

[13] Marginal effects from bivariate probit or IV probit regressions are similar to the 2SLS estimates, though somewhat smaller in the latter case. Using a continuous endogenous variable (daily cigarettes scaled to the mean amount of 30) in place of the dichotomous smoking indicator has little effect on the results, further suggesting that the use of binary variables is not problematic in this case.

OLS estimates are biased away from zero, this indicates that behavioral spillovers among the "compliers" (subjects induced to quit smoking by the intervention) that identify the IV local average treatment effect may be considerably larger than those in the non-complier pool. That is, heterogeneity in the true spillover effect is correlated with subject compliance with the instrument at the spousal pair level.

Each row in the lower portion of Table 5 gives the lower and upper bounds of the union of 95 percent confidence intervals around the 2SLS estimates for each sample assuming direct treatment effects bounded above by zero and below by $\tilde{\delta}$. We contextualize the restrictions on $\tilde{\delta}$ by their percentage of the reduced-form treatment spillover, which is 3 percentage points for all spouses and 7 percentage points for baseline smoker spouses. Small magnitude $\tilde{\delta}$ floors give confidence intervals that do not include zero but do include fairly large spillover effects, similar to those implied by the asymptotic standard errors above. In fact, for direct treatment spillover bounds up to 40 percent the size of the total reduced-form estimate, the union of confidence intervals does not include zero, indicating that the behavioral spillover result is robust to small violations of the exclusion restriction. Confidence intervals become fairly large as the violation gets larger than half the size of the group reduced form effect.

This section has taken two approaches to estimate novel causal effects of spousal health behavior spillovers. Assuming that treatment spillovers on spouses only operate through the treated subjects suggests large causal effects of health behaviors between spouses. We might have expected the IV estimates, which are meant to eliminate the impacts of self-selection of spouses and other spurious factors that would lead to large spousal correlations in smoking and drinking, to push the OLS estimates toward zero. However, a LATE interpretation of the finding that the IV results are on par with the OLS is that the effects of health behavior decisions on spouses are particularly large when induced by these medical treatments. We next examine the implications of our results for treatment evaluation.

V. Implications for Cost-Effectiveness

The implications of treatment spillovers for cost-effectiveness analyses are potentially large. Suppose, for example, that therapy-based treatments typically have greater spillovers than pharmaceutical interventions. Then even if a pharmaceutical intervention is more cost-effective among subjects, a therapy treatment may be more socially cost effective. Spouses provide a useful starting point for social spillovers in cost-effectiveness because marital pairs are well-defined and likely have sizable individual effects, but more comprehensive assessments of networks may find more important spillovers overall as effects cascade across peer groups.

We calculate costs per additional cessation relative to usual care, also known as incremental cost-effectiveness ratios (ICERs), for LHS and COMBINE. The incremental cost-effectiveness with spillovers for treatment y relative to z with mean subject effectiveness E, spillover population share m, and mean spillover effect size S is given by

$$ICER_{yz} = \frac{c_y - c_z}{(E_y + mS_y) - (E_z + mS_z)}.$$

TABLE 6—COST-EFFECTIVENESS RECALCULATIONS

Intervention	Additional cost/subj.	Subject effect	$/quit (ICER 1)	Spillover effect	$/quit (ICER 2)
Lung health study					
UC	Ref.	Ref.	Ref.	Ref.	Ref.
SIA	2,500	−0.19	13,158	−0.125	11,390
SIP	2,000	−0.21	9,524	−0.12	8,392
COMBINE Study					
Management/placebo	Ref.	Ref.	Ref.	Ref.	Ref.
Acamprosate	338.6	−0.028	12,093	0.003	12,211
Naltrexone	261.91	−0.054	4,850	−0.056	4,436
CBI (therapy)	143.34	−0.059	2,429	−0.142	1,997

Notes: Baseline smoker spouse share = 23.6 percent, baseline drinker spouse share = 9.2 percent. LHS costs are rough estimates from Anthonisen et al. (2005). Costs of nicotine gum for some spouses not included. UC = usual care, SIA = therapy + bronchodilator, SIP = therapy + placebo bronchodilator. COMBINE costs from Zarkin et al. (2008). See text for description of incremental cost-effectiveness ratio (ICER) inputs.

Anthonisen et al. (2005) estimate that "a unit price of $2,000 would probably cover the LHS smoking intervention including intensive initial counseling, nicotine replacement therapy, and the long-term maintenance program." Although we do not have precise cost information, for an illustrative example we add an extra $500 for medication in the non-placebo bronchodilator intervention SIA and set LHS usual care costs to $0. The shorter term COMBINE study's Medical Management and Placebo reference group cost $409, while the Acamprosate, Naltrexone, and Combined Behavioral Intervention/Therapy treatments incurred an additional $339, $262, and $143, respectively.

Departing from the combined intervention analyses presented above for LHS, we present estimates for the two treatments separately in Appenidx Table A10 to serve as a point of comparison for the cost-effectiveness analysis. We also do not restrict the samples in this table to nonattenders as the goal here is to include all previously unaccounted benefits of treatment from an intent-to-treat perspective. The effects from the two treatments are of similar magnitudes, 5 to 6 percentage points for all spouses and 12 to 12.5 percentage points for baseline smoker spouses for SIP and SIA, respectively. This indicates that the pharmaceutical element of the bronchodilator does not add much (less than a percentage point) to the full spillover effect. This is perhaps not surprising if the primary intention of the medicine is to improve lung health rather than to change behavior.

As seen in Table 6, the SIP intervention (counseling and nicotine gum access, with a placebo bronchodilator) was the most cost-effective for subjects, with an estimated cost of $9,524 per additional cessation, or 0.105 additional smoking quits per $1,000 dollars spent on treatment. Subjects receiving the non-placebo bronchodilator observed around three-quarters as many quits per thousand dollars, totaling $13,158 per quit. Including spousal smoking cessation for the 23.6 percent of the sample with a smoker spouse at baseline does not change the ordering of the treatments' cost-effectiveness, but it reduces the cost per quit of SIA by almost $2,000 (a drop of 13.4 percent) and SIP by $1,132 (11.9 percent). Costs per quit decrease by

similar percentages across treatment in this case because the SIP and SIA interventions had nearly identical effects on cessation for subjects and spouses.

The COMBINE study's variation in treatment effectiveness on subjects relative to spouses highlights the potential for misleading conclusions in cost-effectiveness analysis. Ignoring cost, Naltrexone and CBI/Therapy are similarly effective in the full sample (yielding 5.4 and 5.9 percentage point reductions in smoking rates, respectively). In contrast, CBI decreased heavy drinking among baseline drinker spouses by 8.6 percentage points more than Naltrexone. As such, for plausible ranges of costs for the two treatments and shares of subjects who were drinkers at baseline, the most cost-effective treatment would flip from Naltrexone to CBI when switching from a subject-only to social cost-effectiveness analysis. For example, with the observed 9 percent share of drinker spouses, if CBI cost an additional $150, the effectiveness ordering would flip when moving to the social analysis. In this particular experiment, however, the extra cost of Naltrexone was enough so that CBI was the most cost-effective therapy with or without spousal consideration at $2,429 or $1,997 per quit, respectively. This implies a decrease in dollars per quit for CBI of $432, or 17.7 percent from the social perspective.

The cases above emphasize three intuitive factors that determine the extent to which cost-effectiveness ranking will change with a broadened social frame. First and foremost, treatments must have differently ranked effectiveness on subjects and on subjects' peers (ranking of the E and S varies). Second, the importance of differential spillovers increases with the proportion of spouses or other relations (m) who share the condition or behavior treated by the intervention and, implicitly, the share of married subjects (or other peers). Finally, greater differentials in cost and cost-effectiveness ($C_y - C_z$) will naturally require more distinction in the factors above to induce a switch in effectiveness rankings.

VI. Conclusion

This paper leverages two randomized controlled trials that abated unhealthy behaviors (tobacco use and alcohol use) to produce the first estimates in the literature of causal spillovers on untreated spouses in health behaviors. Our estimates suggest large spillover effects. In the standard instrumental variable case, the effect of one spouse's cessation on the other is almost a 30 percentage point decrease in smoking rates. The effects are consistent with the literature showing spillover effects of alternative social ties (e.g., college roommates in Eisenberg, Golberstein, and Whitlock 2014) and are at odds with some conclusions of research that was unable to leverage experimental data that argue spousal correlations in health behaviors are driven entirely by selection effects (assortative mating) (e.g., Clark and Etilé 2006).

We then discuss the implications of these spillovers for measuring the comprehensive benefits of health RCTs as well as comparative effectiveness among alternative treatment regimes. Our results suggest that the typical (implicit) assumption that health treatments and interventions only affect the treated and have no other benefit are incorrect, with the implication that typical measures of treatment benefits and effectiveness are too small for some treatments—in our example by upward of 17 percent. However, this effect is the "tip of the iceberg" because we are unable to

assess spillovers on other social ties (friends, neighbors, co-workers, children, etc). These results imply that properly calculating comprehensive benefits of treatments and interventions could reverse prior decisions that some interventions would not pass traditional cost benefit (or effectiveness) thresholds for implementation. A second implication of our results for comparative effectiveness calculations is the possibility that some treatments are dominated by alternatives when estimating direct effectiveness (i.e., only on the treated) but dominate when estimating total social effectiveness (i.e., direct + indirect effects). These are important considerations for future efforts toward more regular comparative effectiveness analysis between potential treatment regimes.

APPENDIX

TABLE A1—BUDDY SUPPORT RESPONSE SHARES BY SAMPLE AND TREATMENT

	All married		BL smoker spouse	
	Control	Treated	Control	Treated
Yes	91.36	91.77	84.91	84.37
Not sure	2.83	2.08	5.63	4.75
Probably doesn't care	3.53	3.69	7.43	8.13
No	0.71	1.31	1.8	2.32
Not applicable	1.57	1.15	0.23	0.42
Count	1,273	2,601	444	947

Notes: Question: "Would your spouse (or closest friend, if not married) like you to give up smoking?" Columns sum to 100 percent.

TABLE A2—LUNG HEALTH STUDY TREATMENT BALANCE ON SUBJECT CHARACTERISTICS

	All married subjects			Married to BL smoker spouses		
	Control	Diff.	Nonattender difference	Control	Diff.	Nonattender difference
Age	48.404	0.147	0.045	48.036	0.398	0.146
Female	0.295	0.035	0.005	0.351	0.019	−0.01
Education	13.6	−0.168	0.191	13.468	−0.235	0.134
BMI	25.775	0.014	0.009	25.577	−0.012	0.027
Cigarettes/day	29.222	0.392	0.263	30.288	1.434	1.089
Spouse smokes	0.349	0.015	−0.048	0.971	0.009	0.003
Buddy support "yes"	0.914	0.004	−0.003	0.849	−0.005	−0.043
Buddy support "no"	0.007	0.006	0.008	0.018	0.005	0.018
Observations	1,273	2,601	1,373	444	947	413

Notes: "Diff." column compares all treated subjects to control subject characteristics in the noted group, while "Nonattender difference" gives the differential for subjects whose spouse (or other close friend) did not attend the therapy intervention orientation with the subject.

TABLE A3—COMBINE TREATMENT BALANCE FOR MARRIED SAMPLE

Outcome	Control	Differences			*F*-stat
		Acamprosate	Naltrexone	Therapy	
Spouse					
Abstainer	0.15	0.05	0.06	0.07	1.78
Light drinker	0.56	0.01	−0.04	−0.09	1.22
Mod. drinker	0.22	−0.04	−0.04	−0.02	0.96
Heavy drinker	0.07	−0.03	0.03	0.04	1.01
Subject					
Drinks/day	7.81	−0.21	0.25	0.30	0.21
Pct. heavy days	65.82	−0.19	0.53	0.88	0.04
Group count	48	181	190	252	–

Note: Group counts sum to more than the married sample total because a subject could be in more than one treatment arm.

TABLE A4—TREATMENT EFFECTS ON SUBJECT SMOKING (LHS)

	Unmarried, no smoker in household		Married		Married to smoker	
Treated	−0.204	−0.213	−0.263	−0.242	−0.254	−0.223
	(0.020)	(0.022)	(0.012)	(0.014)	(0.020)	(0.025)
Spouse/buddy support	x	x	x	x	x	x
Non-attending buddy		Sample		Sample		Sample
Observations	6,698	5,382	19,370	13,230	6,955	4,285
Clusters	1,378	1,108	3,874	2,646	1,391	857

Notes: Standard errors are in parentheses. Outcome equals 1 if subject smoked at year *t*. Treated group receives therapy plus a medicated bronchodilator (or placebo bronchodilator). Each estimate gives the effect (in percentage points) of treatment on the probability of being a smoker for subjects in the Lung Health Study. Estimates are from linear regressions that pool across years 1–5 posttreatment and control for wave dummies, demographics (age, sex, education, and BMI), and variables marked with "x." "Spouse/buddy support" includes dummies for categorical responses to a question asked of all subjects: "Would your spouse (or closest friend, if not married) like you to give up smoking?" Columns with a "Sample" marking are restricted to the group noted by that row. "Nonattending buddy" group includes all control subjects as well as treated subjects whose spouse did not accompany the subject to the therapy intervention orientation.

TABLE A5—TREATMENT EFFECTS ON SUBJECT HEAVY DRINKING (COMBINE)

	Unmarried subjects		Married subjects		Married to baseline drinker	
Acamprosate	−0.008	−0.009	−0.052	−0.053	0.037	0.047
	(0.033)	(0.033)	(0.039)	(0.039)	(0.078)	(0.076)
Naltrexone	−0.067	−0.059	−0.047	−0.046	−0.022	−0.080
	(0.033)	(0.033)	(0.039)	(0.040)	(0.077)	(0.083)
Therapy	−0.053	−0.054	−0.085	−0.086	−0.102	−0.107
	(0.033)	(0.033)	(0.040)	(0.040)	(0.082)	(0.083)
Week 26 dummy	x	x	x	x	x	x
Demographics		x		x		x
Observations	1,453	1,453	770	770	209	209
Clusters	731	731	426	426	113	113

Notes: Standard errors are in parentheses. Outcome equals 1 if subject was a heavy drinker in week *t*. Each estimate gives the effect (in percentage points) of treatment on the probability of being a heavy drinker for subjects in the COMBINE Study. Estimates are from linear regressions that pool across weeks 16 and 26 posttreatment and control for the variables marked with "x" by column.

TABLE A6—TREATMENT SPILLOVER EFFECT HETEROGENEITY (LHS)

	Wives	Husbands	Low educ.	High educ.
All spouses	−0.052	−0.065	−0.040	−0.069
	(0.010)	(0.017)	(0.013)	(0.012)
Count	2,642	1,232	1,722	2,152
Smoker spouses	−0.131	−0.117	−0.092	−0.154
	(0.025)	(0.033)	(0.030)	(0.027)
Count	884	507	648	743
Nonsmoker spouses	−0.014	−0.028	−0.010	−0.025
	(0.009)	(0.015)	(0.011)	(0.010)
Count	1,758	725	1,074	1,409

Notes: Standard errors are in parentheses. Outcome equals 1 if spouse smoked at year *t*. Each estimate gives the effect (in percentage points) of treatment on the probability of being a smoker for spouses of subjects in the Lung Health Study. Estimates are from linear regressions that pool across years 1–5 posttreatment and control for wave dummies and demographics. Spouse gender assumes opposite-sex couples. Low education group includes HS graduates (or equivalent) or less.

TABLE A7—GUM INTEREST HETEROGENEITY

			Spouse buddy		
	Overall	Non-spouse buddy	Interested	Not interested	Interest unknown
All spouses	−0.056	−0.032	−0.067	−0.057	−0.050
	(0.009)	(0.011)	(0.010)	(0.009)	(0.009)
Count	3,874	2,004	2,587	2,342	2,460
Smoker spouses	−0.124	−0.075	−0.173	−0.428	−0.135
	(0.020)	(0.032)	(0.030)	(0.074)	(0.037)
Count	1,391	622	711	474	584
N.S. spouses	−0.018	−0.015	−0.029	−0.034	−0.029
	(0.008)	(0.010)	(0.008)	(0.007)	(0.008)
Count	2,483	1,382	1,876	1,868	1,876

Notes: Standard errors are in parentheses. Each estimate gives the effect (in percentage points) of treatment on the probability of being a smoker for spouses of subjects in the Lung Health Study. Estimates are from linear regressions that pool across years 1–5 posttreatment and control for wave dummies and demographics. Spouse buddy interest levels regard access to nicotine gum.

TABLE A8—TREATMENT SPILLOVERS ON SPOUSAL DRINKS PER WEEK (COMBINE)

	All spouses		Moderate/heavy drinker spouse at baseline	
	(1)	(2)	(3)	(4)
Acamprosate	0.023	0.012	−0.644	−0.547
	(0.263)	(0.273)	(0.869)	(0.920)
Naltrexone	−0.239	−0.225	−0.354	−0.439
	(0.269)	(0.276)	(0.876)	(0.938)
Therapy	−0.724	−0.691	−1.836	−1.954
	(0.254)	(0.252)	(0.835)	(0.845)
Week 26 dummy	x	x	x	x
BL heavy drinker dummy	x	x	x	x
Demographics		x		x
Observations	770	770	209	209
Clusters	426	426	113	113

Notes: Standard errors are in parentheses. Outcome equals the imputed number of drinks consumed by a spouse in week t. "Baseline drinker" spouses were either moderate or heavy drinkers at baseline (BL). Each estimate gives the effect of a treatment on the (imputed) number of drinks consumed by spouses of subjects in the COMBINE Study. Estimates are from linear regressions that pool across weeks 16 and 26 posttreatment and control for the variables marked with "x" by column. Follows specification in Table 4.

TABLE A9—SPILLOVERS ON SPOUSAL HEAVY DRINKING BY WEEK (COMBINE)

	Week 16		Week 26	
	All spouses	BL drinkers	All spouses	BL drinkers
Acamprosate	−0.004	0.002	−0.009	−0.024
	(0.020)	(0.075)	(0.020)	(0.072)
Naltrexone	0.001	0.016	−0.031	−0.107
	(0.021)	(0.078)	(0.020)	(0.074)
Therapy	−0.022	−0.080	−0.051	−0.187
	(0.019)	(0.072)	(0.020)	(0.070)
Observations	396	108	374	101

Notes: Standard errors are in parentheses. Outcome equals 1 if subject was a heavy drinker in the stated week. "Baseline drinker" spouses were either moderate or heavy drinkers at baseline (BL). Each estimate gives the effect of a treatment on the probability of being a heavy drinker spouses of subjects in the COMBINE Study. Estimates are from linear regressions that include a dummy for baseline drinker spouses when the sample is not restricted to that group.

TABLE A10—SEPARATED TREATMENT INTENT-TO-TREAT SPILLOVERS

	All spouses		Baseline smoker spouses	
SIA	−0.060	−0.061	−0.125	−0.128
	(0.010)	(0.010)	(0.024)	(0.024)
SIP	−0.051	−0.050	−0.121	−0.120
	(0.010)	(0.010)	(0.023)	(0.023)
Baseline smoker	x	x	Sample	Sample
Spouse support		x		x
Observations	18,856	18,856	6,751	6,751
Clusters	3,874	3,874	1,391	1,391

Notes: Standard errors are in parentheses. Outcome equals 1 if subject smoked at year *t*. SIA = therapy + bronchodilator, SIP = therapy + placebo bronchodilator. Each estimate gives the effect (in percentage points) of the individual treatment on the probability of being a smoker for spouses of subjects in the Lung Health Study. Estimates are from linear regressions that pool across years 1–5 posttreatment and control for wave dummies, demographics (age, sex, education, and BMI), and variables marked with "x." "Spousal support" control adds dummies for categorical responses to a question asked of all subjects: "Would your spouse (or closest friend, if not married) like you to give up smoking?" Columns with a "Sample" marking are restricted to the group noted by that row. "Baseline smoker" indicator/sample restriction refers to the spouses smoking status at baseline. Sample not restricted to nonattender spouses.

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