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COMMON PRACTICE:
SPILLOVERS FROM MEDICARE ON PRIVATE HEALTH CARE

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Common Practice: Spillovers from Medicare on Private Health Care
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ABSTRACT

Efforts to raise the productivity of the U.S. health care system have proceeded slowly. One potential explanation is the fragmentation of payment across insurers. Each insurer's efforts to improve care could influence how doctors practice medicine for other insurers, leading to unvalued externalities. We study these externalities by examining the unintended private insurance spillovers of a public insurer's intervention. In 2015, Medicare randomized warning letters to doctors to curtail overuse of antipsychotics. Even though the letters did not mention private insurance, they reduced prescribing to privately insured patients by 12%. The reduction to Medicare patients was 17%, and we cannot reject one-for-one spillovers. The results imply that physicians experience large costs to setting insurer-specific medical practice styles. If private insurers conducted a similar intervention with their own limited information, they would stem half as much prescribing as a social planner able and willing to better target the intervention. Our findings establish that insurers can affect health care well outside their direct purview, raising the question of how to match their private objectives with their scope of influence.

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A randomized controlled trials registry entry is available at AEARCTR-0003209

1 Introduction

With nearly 20% of GDP at stake, the U.S. health care system plays an outsize role in the performance of the economy at large. Yet in many cases, productivity-raising innovations in the health care system proceed slowly. Classic examples of delayed adoption include low-cost, evidence-based treatments such as aspirin and beta-blockers; health information technology in the form of electronic medical records; and reforms to health payment mechanisms that favor value over volume (Cutler, 2011; Lee, McCullough and Town, 2013; Skinner and Staiger, 2015). One potential root of this phenomenon is the fragmented system that pays for care. Health care providers contract with an array of government and private insurers. When insurers try to raise the performance of health care providers, their efforts may have externalities that accrue to other insurers. For example, if one insurer tries to change how physicians treat its beneficiaries, those physicians may change how they treat all patients (Baker, 2003; Glied and Graff Zivin, 2002). As a result, fragmentation may yield weak incentives for insurers to invest in improving performance relative to the socially optimal level (Glazer and McGuire, 2002; Frandsen, Powell and Rebitzer, 2019). Whether such external impacts occur in practice remains an open question.

This study provides evidence on how physicians behave when contracting with multiple health insurers. We show that one insurer's investment to improve quality of care generated large, unintended spillovers onto the health care covered by other insurers. Our research exploits a randomized controlled trial conducted by Medicare, the largest insurer in the U.S., which sent warning letters to doctors who heavily prescribed the most popular antipsychotic medication, quetiapine. We consider the external effects of these letters on private insurance patients covered by three of the five largest insurers in the U.S. Those patients were not mentioned in, nor were they the focus of, the Medicare letter.

Figure 1 shows our central result: Medicare's investment in stemming use of antipsychotics also reduced their use outside Medicare. The figure shows that following the intervention, treated doctors rapidly curtailed their prescribing of the antipsychotic in Medicare *and* private insurance compared to doctors in the control group. Treated physicians cut back by 17% in Medicare and 12% in private insurance. We cannot reject that the spillover effect was the same as the direct effect.

We next probe a potential mechanism that led Medicare's intervention to have such a large

and unintended effect on other care. We explore whether the intervention encouraged physicians to check clinical guidelines and update their beliefs about the returns to antipsychotics. Changes in beliefs could result in spillover effects if physicians draw upon these primitives when they treat all of their patients. Yet our findings are inconsistent with this mechanism in two ways. First, we look at effects on prescribing of other antipsychotics with similar costs and benefits as the one mentioned in the letter. Updated beliefs should apply to these drugs as well. However, there are no signs that physicians reduced their prescribing of these drugs. Second, we test whether physicians tailored their cutbacks to patients who were observably (to the econometrician) poor candidates for antipsychotics. If the letters prompted physicians to learn from clinical guidelines, we would expect the reductions to be concentrated here. In contrast, we find that cutbacks occurred irrespective of the patient's appropriateness.

These patterns suggest an alternative mechanism: the letters raised the implicit costs of prescribing, resulting in a blunt change in prescribing. That we further observe a spillover to private insurance suggests that physicians are unable or unwilling to distinguish insurers when determining how they treat patients. This suggests that doctors face additional costs to customizing treatment decisions to the insurer paying for the care. The result implies that when physicians develop styles of practicing medicine, those styles are not insurer-specific.

Next, we show evidence that physicians used a more clinically salient patient attribute to tailor their reductions: age. In private insurance, large prescribing cutbacks occurred for patients old enough to be eligible for Medicare within the next decade. Reductions to younger people were economically small and statistically indistinguishable from zero. If doctors use age as a proxy for Medicare coverage, they can more closely concentrate their reductions on Medicare patients while keeping their treatment approaches agnostic to insurer. Spillovers onto private insurance remain large because the privately insured recipients of antipsychotics tend to be older and Medicare recipients tend to be younger.

Rich data on patient health outcomes provides evidence on the welfare impacts of the intervention and its spillovers. We consider whether the respurce savings from reduced prescribing were offset by patient harms. We fail to detect adverse effects on patients in the form of emergency department visits and hospital stays. Doctors may have used private information to appropriately cut back prescribing to patients who appeared to be good candidates to the econometrician. Al-

ternatively, doctors could have prescribed poorly to begin with, and their cutbacks could rectify prescribing that did not benefit patients in the first place.

Finally, as a window into how the fragmented payment system discourages similar interventions with spillovers, we test what would happen if private insurers conducted one. We simulate a private insurance intervention with the same effects as the one Medicare conducted and select doctors who were outliers in private insurance data. The simulation shows that these doctors are typically not high prescribers in overall prescribing. If private insurers intervened with the same intervention size as Medicare, they would stem half as much prescribing as a social planner that was able and willing to target the overall outliers. Private insurers' limited information on outside prescribing, as well as their objectives that ignore patients on other plans, serve to reduce the social return on the intervention.

Our study builds on a theoretical and empirical literature on physician-insurer contracting. Classically, the literature views physician behavior through a principal-agent framework (see e.g. Pauly, 1980). A key variant of this model considers physicians contracting with multiple insurers at once. These physicians are called common agents of insurers. This model dates to Bernheim and Whinston (1986), who extended the principal-agent problem to contexts in which an agent contracts with several principals who do not necessarily coordinate. Glazer and McGuire (2002) and Frandsen, Powell and Rebitzer (2019) consider common agency in health care. They set out conditions under which insurer principals can free ride on each other (or fail to coordinate) in contracting with health care provider agents. The result is the under-provision of performance-improving investments.

The Medicare intervention provides a unique opportunity to study common agency in health care empirically. First, it was targeted at prescribing covered by Medicare – the letter did not mention private insurance prescribing, and Medicare did not track or have access to the outside data. Thus, changes in private insurance prescribing did not trivially derive from Medicare using its leverage to influence health care covered by other insurers. Second, the direct effects of the intervention on Medicare prescribing were large. In turn, it is *a priori* plausible that the intervention led to detectable spillovers. Large direct effects make it more plausible that a detected spillover is real and that a failure to detect a spillover reflects a true null.

To our knowledge, this study is the first to use a randomized intervention to show that one insurer's actions to change physician practice styles can affect the health care that physicians provide

to patients covered by other insurers. We review the related research on doctors below, though these studies do not exploit random variation. The randomized treatment in the current study eliminates concerns about endogeneity. This is particularly useful for evaluating spillovers. When identification is questionable, an effect on a second-order outcome like a spillover could represent a failure to address endogeneity rather than a true external impact.

Earlier literature on managed care plans generally found spillovers, but the non-random growth of managed care threatened causal inference (c.f. Baker, 2003). In the cross-section, Glied and Graff Zivin (2002) show that physicians who treat more managed care patients utilize a more judicious practice style for all of their patients. Two quasi-experimental studies have looked at cross-insurer spillovers, though not for doctors. Baicker, Chernew and Robbins (2013) find that Medicare managed care lowers the intensity of hospital treatment for the privately insured while Grabowski, Gruber and Angelelli (2008) show that nursing homes provide common levels of quality across self-pay and Medicaid patients. An adjacent literature has studied the effects of one insurer's payment changes on other insurers. Private insurers mimic Medicare's payment systems and relative prices for services (Clemens, Gottlieb and Molnár, 2015; Clemens and Gottlieb, 2017); a randomized Medicare hospital payment reform to lower costs of hip and knee replacements had similar effects on the (private) Medicare Advantage program, which was not a part of this reform (Wilcock et al., 2020; Einav et al., 2020). Appointment availability and utilization in one insurer depends on the payment rates of others, suggesting that spillovers can operate through demand channels (Garthwaite, 2012; Glied and Hong, 2018; Richards and Tello-Trillo, 2019).

Our findings speak to the nature of physician practice styles. Much of the research on practice styles derives from the health care variations literature, which has found large regional differences in care delivery. This work tends to find little or negative associations between cost and outcomes – suggestive of variations in productivity across regions (Baicker and Chandra, 2004). Regional variations in practice patterns reflect decisions of health care practitioners in the regions, highlighting the importance of studying practice styles at the physician level as well. Doctors tend to have idiosyncratic practice styles overall and for specific procedures like c-sections (Phelps and Mooney, 1993; Currie and MacLeod, 2017; Molitor, 2018; Cutler et al., 2019; Fadlon and Van Parys, 2020).

We view the Medicare intervention as a shock to physicians' practice styles with respect to psychiatric medications. Berndt et al. (2015) and Currie and MacLeod (2020) study prescribing of

psychiatric drugs and focus on variations in a physician's concentration of prescribing (i.e. favoring one drug in a class vs. distributing patients to many drugs in a class). In particular, Berndt et al. (2015) find that psychiatrists tend to concentrate their prescribing on one or a handful of antipsychotics. The extant economic literature on psychiatric prescribing has yet to show how physicians tailor their practices to insurers. We find that the intervention changed practice styles in Medicare and private insurance. The result suggests that doctors' practice styles depend on the characteristics of patients, rather than those of insurers.

The remainder of the paper proceeds as follows. In Section 2, we describe the health care context. In Section 3 we describe the intervention. Section 4 reviews our conceptual model and econometric approach. Section 5 presents our findings and Section 6 discusses them. Section 7 presents an extension looking at the targeting of the intervention under limited information available to private insurers. We conclude in Section 8.

2 Background

The complexity and uncertainty around the use of antipsychotics make these drugs excellent objects of study for understanding physician practice styles. In this section, we provide background on antipsychotics and how and why they are prescribed.

2.1 Approved Uses of Quetiapine and Other Antipsychotics

Quetiapine, the focus of this study, is the most commonly used antipsychotic medication in the U.S. (Gallini, Donohue and Huskamp, 2013). Over 9 million quetiapine prescriptions were written in 2015 (Nutt and Keating, 2018). Antipsychotics like quetiapine are widely used to treat serious mental illness, most notably schizophrenia and bipolar disorder. This use is backed by an array of randomized clinical trials demonstrating reductions, often dramatic, in psychiatric symptoms for these patients. For example, a meta-analysis of antipsychotics for schizophrenia found that they reduce the risk of relapse by 59% (Leucht et al., 2012).

Quetiapine has three FDA-approved indications: schizophrenia, bipolar disorder and, when alongside an antidepressant, major depression (Food and Drug Administration, 2020*a*,*b*). Its key alternatives are other antipsychotics, which fall into two classes. The dominant class is second-

generation or "atypical" antipsychotics, including quetiapine, risperidone (Risperdal), and aripiprazole (Abilify); these drugs displaced the earlier first-generation or "typical" medications like haloperidol (Haldol) and fluphenazine (Prolixin) (Alexander et al., 2011). Most trials comparing quetiapine with other antipsychotics have demonstrated comparable benefits across medications (Meltzer, 2013). However, there are meaningful differences in the side effects of these medications, making the match of patient to side effect profile the central task in selecting an antipsychotic. For example, a physician might prescribe quetiapine to patients with serious mental illness and insomnia because quetiapine leads to more pronounced sedation than other antipsychotics (Jibson, 2019). The potential for life-threatening movement disorders is greater in first-generation antipsychotics compared to newer atypical drugs like quetiapine, so the literature generally suggests starting patients on the newer medications (Meltzer, 2013).

2.2 Use of Antipsychotics Outside Approved Indications

A key aspect of antipsychotic prescribing practice styles is how physicians use the drugs within and outside their FDA-approved indications. The latter phenomenon is called off-label prescribing; there has been significant controversy over such prescribing of antipsychotics. The practice is legal but insurers may refuse to cover the drugs. The central concern is that it could reflect use that is, in expectation, harmful or unproductive because the manufacturer has not demonstrated clinical efficacy and safety, the FDA's standard for approval. However, off-label use could benefit patients by allowing them to access treatments based on early evidence or when alternatives are limited.

We leverage the fact that off-label prescribing of antipsychotics has been the subject of intensive study in randomized trials. We distinguish between on-label prescribing with excellent support in trials (described in the prior section), off-label prescribing that has some support, and off-label prescribing that trials suggest has little benefit or even causes harms. Two indications fall into the intermediate category with some support: generalized anxiety disorder and major depression (when used without an antidepressant). While trials show that quetiapine benefits these patients, it also causes much more severe side effects than alternative medications like antidepressants. The FDA cited these side effects when it denied adding these indications to quetiapine's labeling (Food and Drug Administration, 2009).

We next move to conditions for which quetiapine and other antipsychotics have small or even

negative expected benefits. Much of the attention around quetiapine has focused on treatment of patients with dementia, especially those in nursing homes. This use elevates the risk of death and a host of other adverse effects, prompting warnings from major medical specialty societies, the FDA, and federal oversight agencies (American Psychiatric Association, 2016; Maglione et al., 2011; American Geriatrics Society, 2019; OIG, HHS, 2011; Government Accountability Office, 2015). The sedating effects of antipsychotics have led some practitioners to call the drugs "chemical restraints" (Flamm, 2018). Yet no pharmaceutical is currently approved in the U.S. to treat behavioral symptoms of dementia. The recommended alternatives tend to involve modifying how caregivers interact with patients, which can be labor-intensive for nursing homes or involve major commitments from informal caregivers (Press and Alexander, 2018). Other common off-label uses include insomnia, alcohol use disorder, and post-traumatic stress disorder (PTSD). There is very little evidence supporting the use of quetiapine for these indications (Maglione et al., 2011). Other options typically exist to treat these conditions. Some of these do not involve medication, like cognitive behavioral therapy for insomnia. Others involve different medication classes, like antidepressants for anxiety.

3 Intervention

The intervention was conducted by Centers for Medicare and Medicaid Services (CMS), the U.S. Office of Evaluation Sciences, and academic researchers. It was part of a larger program to test interventions informed by behavioral science to make prescribing safer in the Medicare program (Sacarny, Yokum and Agrawal, 2017). It built on existing efforts by CMS to stem overuse of health care in the Medicare program using letters and electronic communications. CMS communications with providers tend to focus on comparing them to peers with the aim of encouraging them to self-audit (eGlobalTech, 2017). To our knowledge, these programs had not been rigorously studied prior to these efforts.

This CMS collaboration sought to test whether augmenting a peer comparison message with language about negative consequences and penalties for prescribers could lead to improvements in the quality of prescribing in Medicare. Randomized evaluations have found that letters that emphasize penalties raise tax payment, but there is less evidence on using this language in health care (Castro and Scartascini, 2015; Fellner, Sausgruber and Traxler, 2013). CMS chose high prescribers

of quetiapine as the target following a report by a federal oversight agency that found high rates of questionable antipsychotic prescribing in Medicare as well as case reports of patients abusing and reselling these drugs (Government Accountability Office, 2015; Klein-Schwartz, Schwartz and Anderson, 2014; Cubał a and Springer, 2014).

3.1 Selection of Physicians

The study population was primary care physicians (PCPs, defined as physicians with a specialty of general practice, family practice, or internal medicine) who were persistent outliers in their prescribing of quetiapine relative to other PCPs in the same state.[1] This class of physicians was chosen for the study because they supplied a high volume of a powerful psychiatric medication yet lacked intensive psychiatric training. Moreover, AstraZeneca was fined for marketing quetiapine to PCPs, which regulators questioned because these physicians were less likely to treat patients with serious mental illness (U.S. Department of Justice, 2010).

CMS analyzed Medicare prescription drug claims in 2013 and 2014. Only PCPs were included. Those who reported additional specialization in psychiatry, as well as those with little quetiapine prescribing (fewer than 10 drug dispenses in the year), were removed. CMS identified outliers based on two measures of volume: 1) the number of quetiapine prescriptions filled at pharmacies and 2) the number of quetiapine days across the filled prescriptions. Outlier thresholds for each state-year-measure were calculated as the 75th percentile plus 0.25 times the interquartile range of physicians falling into the cell (Tukey, 1977). To be selected, a physician had to clear the thresholds for both measures in 2013 and 2014. After excluding one who had died by the time of analysis, there were N=5,055 such physicians, and they became the study population.

The outlier calculations did not adjust for differences across physicians in their patient populations and the suitability of patients for the antipsychotics. In addition, the calculations omitted prescriptions to long-term care patients. Many of the excluded prescriptions were likely dispensed to people with dementia where overuse is most severe. While these factors may have deteriorated the targeting of the intervention, a corollary is that study physicians tended to treat patients who were observably appropriate *as well as* questionable candidates for quetiapine – permitting the econometrician to study effects on both populations. We include all quetiapine prescribing in our

[1] For more details on the selection process, see the online supplement in Sacarny et al. (2018).

analyses.

3.2 Intervention

The treatment letter combined a peer comparison emphasizing social and professional norms with penalty-focused language emphasizing the adverse consequences for physicians determined to be improper prescribers (Appendix Figure A1). It stated that the physician was responsible for many more quetiapine prescriptions than other PCPs in the same state. A bar graph displayed the physician's Medicare prescribing level in red and compared it to that of her average peer. The subject line stated that the physician's quetiapine prescribing was "under review by the Center for Program Integrity" – the division of CMS responsible for stemming fraud, waste, and abuse in Medicare. The text of the letter discussed the consequences of medically unjustified prescribing, like restrictions on receiving payments from Medicare. It also indicated that physicians could expect to receive future communications from CMS. The placebo intervention was a letter and pamphlet discussing an unrelated Medicare provider enrollment regulation.

A random 50% of the 5,055 physicians were allocated to the treatment group and the remainder were allocated to the control group. The randomization procedure was pre-specified to reject allocations that were very imbalanced ($P < 0.4$ on a test of distance between the treatment and control groups on *ex ante* observed covariates); the first candidate randomization passed this test and was accepted.

CMS sent the treatment and placebo letters to the respective study groups in April 2015. The treatment group was also sent follow-up letters with updated prescribing data in August and October 2015. The control group was sent a clarification notice in June 2015 to address questions about the placebo message. Neither message to the control group mentioned quetiapine or antipsychotics.

3.3 Prior Findings from Medicare Data

Two prior clinical studies have analyzed the effects of these letters on Medicare prescribing but have not considered spillovers to patients outside Medicare. The intervention caused PCPs to become much less intensive prescribers of quetiapine in Medicare, a finding we closely replicate in this study (Sacarny et al., 2018). Tracking prior Medicare patients of the PCPs, cutbacks were larger for those who appeared to be poorer candidates for the drug; there were no detected patient harms

as measured by mortality or hospital use. A subsequent analysis failed to detect peer effects on prescribers who worked with study PCPs (Sacarny, Olenski and Barnett, 2019). In this study, we access prescribing covered by the three HCCI insurers to focus on private insurance patients who were not tracked in the earlier works.

4 Data and Methods

4.1 Analytic Framework

Our main analyses are at the level of the physician. The estimating equations all take the form:

$$y_i^n = \alpha^n + \beta^n \cdot TREAT_i + X_i^n \Gamma^n + \varepsilon_i^n \tag{1}$$

where i indexes physicians and $n \in \{P, M\}$ indexes the two sources of insurance in the data (private insurance or Medicare). y_i^n denotes an outcome of interest, $TREAT_i$ indicates the assignment of the physician to the treatment or control group, and X_i^n is a set of statistical controls. Due to the random assignment, there is no need for controls to identify the treatment effect. However, covariates can improve precision when they have explanatory power and thus reduce residual variation in the regression. Given the strong autocorrelation in prescribing volume, we pre-specified the inclusion of one control, the lagged outcome, in all analyses. We do not control for any other covariates.

For each outcome y_i^n, we estimate two sets of regressions: one for privately insured patients and one for Medicare patients. We convert our estimates of β^n to percent effects by dividing them by the mean for the control group. To assess the magnitude of spillovers, we conduct tests of equality of the percent effects between the two populations, i.e. $\beta^C / \bar{y}^{C,control} = \beta^M / \bar{y}^{M,control}$.

An additional set of analyses tracks a cohort of baseline patients who received quetiapine from study physicians in the year prior to the intervention. These analyses are at the patient level and take the form:

$$y_j = \alpha^n + \beta^n \cdot TREAT_{i(j)} + X_{i(j)}^n \Gamma^n + Z_j \Delta^n + \epsilon_j \quad j \in J^n \tag{2}$$

where j indexes patients and $i(j)$ is the physician from which patient j received quetiapine in the baseline period. We run separate regressions for privately insured patients ($j \in J^C$) and patients

insured under Medicare ($j \in J^M$). We denote physician-level statistical controls by X_i^n and patient-level controls by Z_j. We pre-specified one patient level control Z_j and one physician-level control X_i^n: the patient's lagged outcome and the physician's lagged outcome, respectively. For example, when we study receipt of quetiapine for privately insured patients, we control for the volume of quetiapine received by the patient in the baseline period as well as the volume of prescribing of the patient's physician to private insurance during that period.

In patient analyses, we cluster all standard errors at the physician level to match the level of randomization. We also convert our estimates of β^n to percent changes relative to the mean for the control group patients in J^n and test for equality in percent effects between the private insurance and Medicare populations.

4.2 Data

Our data come from the Health Care Cost Institute (HCCI), an independent, non-profit entity that collects claims from three of the five largest health insurers in the U.S.: Aetna, Humana, and UnitedHealthCare. Our analyses use data from 2013-2017. Unless otherwise stated, we measure all outcomes from the day after letters were mailed through the end of the data period, i.e. April 21, 2015 through December 31, 2017.[2]

We track prescribing using the HCCI pharmacy claims file. Each record in the file is an instance of a pharmacy filling and dispensing a prescription. Prescriptions that are never filled do not enter the data. Prescriptions with refills enter the data each time the patient fills them. We link records to the IBM Micromedex RED BOOK database to identify the active ingredient (e.g. quetiapine) and calculate the milligrams of the drug that were dispensed. Data on inpatient, outpatient, and physician encounters provides diagnosis codes to classify the appropriateness of prescribing. We also use this data to assess patients' health care utilization.

HCCI data includes claims for enrollees who receive coverage through private insurance (employer-sponsored insurance and individual insurance market products) and Medicare Advantage. We remove private insurance patients age 65 and up since they are likely covered by Medicare. In 2017, the data includes 27.2 million privately insured lives with prescription drug coverage and 7.1 million

[2]We pre-specified that the outcome period would end on December 31, 2016, but later received 2017 data and modified the outcome period to include it. The effect on prescribing was essentially identical with the shorter outcome period (Appendix Table A1).

Medicare covered lives with drug coverage. The availability of claims for both Medicare Advantage and private insurance patients allows us to capture both direct and spillover effects side-by-side.

4.3 Classifying the Appropriateness of Prescribing

To provide a sense of how physicians responded to the letter and whether these responses were likely to be welfare-improving, we consider patients' appropriateness for antipsychotics using their observable characteristics. We develop a classification methodology based on clinical literature (Maglione et al., 2011; Painter et al., 2017; American Psychiatric Association, 2016; American Geriatrics Society, 2019). We use diagnoses from the baseline and outcome periods to classify patients. Appendix Section A provides more detail on our approach.

The algorithm classifies patients into four mutually exclusive and exhaustive categories. Patients who match multiple categories are assigned to the one listed first. *Guideline-concordant* patients appear to be receiving quetiapine consistent with FDA approvals. These patients have bipolar disorder, schizophrenia, or, if they are also taking an antidepressant, major depression. *Intermediate evidence* patients have a condition that the drug may effectively treat according to the literature, though the indication is not FDA-approved: generalized anxiety disorder or, if they are not taking an antidepressant, major depression. *Low-value* candidates appear to be receiving quetiapine for unapproved indications and evidence suggests it is unlikely to help them or may be harmful. The key low-value condition in Medicare is dementia; the other conditions in this category are insomnia, PTSD, obsessive-compulsive disorder, personality disorders, eating disorders, and alcohol use disorder. *Unknown* patients have no diagnoses in the above categories or are under age 18 (given the differing guidelines for pediatric use).

4.4 Analysis Sample and Descriptive Statistics

The CMS intervention included 5,055 physicians, all of whom had large Medicare patient panels on antipsychotics by construction. The vast majority of these physicians had contact with the HCCI insurers: over 99% had at least one prescription of any drug covered during the one year pre-intervention period. Table 1 provides summary statistics on the study physicians. As expected given the randomization, we found that pre-intervention outcomes were balanced between the treated and control groups (P=0.32). The sample is 18% female; study physicians mostly specialized in family

medicine (48%) and internal medicine (48%) with a smaller share classifying themselves as general practitioners (4%). Reflecting the CMS inclusion criteria of non-psychiatrist PCPs, less than 1% of the sample had any psychiatric specialization (including secondary specializations).

While the physicians were high-volume quetiapine prescribers in the overall Medicare program by design of the study, their volumes were often less extreme in the HCCI data. In the year prior to the intervention, 42% had no quetiapine prescribing to Medicare Advantage patients and 74% had no private insurance quetiapine prescribing. The low volume for some physicians occurs partly because HCCI data does not include patients on Medicare with standalone prescription drug plans. It also reflects variations in the market share of the HCCI contributors in private insurance and Medicare Advantage, since Blue Cross and Blue Shield insurers that dominate some markets do not participate in the version we access.

Patient-level analyses use a second sample, the baseline patients of the physicians. We define this group as the patients who received quetiapine from study physicians in the one year pre-intervention period. We omit patients who were not still covered when the study began, switched between private insurance and Medicare, or received quetiapine from more than one study physician (see Appendix Section B). Appendix Table A2 provides summary statistics on the 9,364 baseline patients. As expected, pre-intervention characteristics are balanced between treatment and control (P=0.23 and 0.70 for private insurance and Medicare, respectively). The private insurance patients tend to be much younger than Medicare patients, but there is also meaningful overlap in the age distributions. Both groups have a substantial mass of patients age 45-64 – just over one-half in private insurance and just under one-third in Medicare.

We find that off-label use of quetiapine is quite common: in the privately insured population, just half of private insurance patients had diagnoses that matched FDA guidelines for prescribing (guideline-concordant). One in eight lacked a clear indication for quetiapine but had off-label diagnoses for which quetiapine could carry benefit (intermediate evidence); one in six had diagnoses for which quetiapine has little benefit or is even harmful (low-value), and one in five had no relevant diagnoses at all. Patterns were similar for Medicare patients although more patients were guideline-concordant and low-value candidates while fewer were in the intermediate and unknown categories.

4.5 Trial Registration and Analysis Plan

The original trial with CMS was registered on ClinicalTrials.gov (NCT02467933) and the AEA Social Science Registry (AEARCTR-0000729). We pre-registered these secondary analyses on the AEA Social Science Registry (AEARCTR-0003209) and archived a pre-specified analysis plan prior to viewing any HCCI data. The analysis plan can be accessed from the registry website. We note deviations from the analysis plan in the text.

The pre-specified primary outcome of the study is the amount of quetiapine supplied by the physician, as measured by the days supply on the prescription fills. The analysis plan pre-specified a host of secondary outcomes, all of which are presented in Section 5 or Appendix Section E.

5 Results

5.1 Effects on Prescribing Volume

Figure 1 presents an unadjusted time series of quetiapine prescribing by treatment and control physicians. To visualize direct and spillover effects, the upper area plots Medicare prescribing and the lower area plots private insurance prescribing (the primary outcome). The plot uses a log scale on its y-axis to display effects in percent terms to facilitate comparisons between Medicare and private insurance. Arrowheads denote when treatment letters were mailed. Relative to control physicians, treatment physicians reduced their private insurance and Medicare prescribing substantially after the letters were sent. These reductions persist through the end of the outcome period.

Table 2 reports estimates from equation (1) for the primary outcome and related secondary outcomes. Consistent with the time series plot, the intervention caused study physicians to cut back prescribing to private insurance and Medicare patients in nearly equal measure. Specifically, the intervention caused physicians to reduce the days of quetiapine supplied to private insurance patients over the outcome period by 24.5 days on a control group mean of 209.5 days, or 11.7%. For Medicare, the reduction was 183.6 days on a control mean of 1,094.5 days, or 16.8%. We are unable to reject that these percent effects are equal – i.e., we cannot reject one-for-one spillovers (P=0.34).

These findings are highly robust to alternative measures of prescribing (Appendix Table A1).

Impacts are similar for fills (ignoring the durations of the prescriptions), costs (the charge the insurer allowed for the fill), milligrams, and counts of unique patients; effects on costs and unique patients are more imprecisely measured on the private insurance side.[3] Taken together, the results indicate that prescribing mainly fell through a reduction in the number of fills to patients, and not simply because patients filled the same number of prescriptions but with shorter durations or at lower doses. Physicians cut back the size of their patient panels on quetiapine.

Table 2 next splits prescribing into fills from new prescriptions and refills. New fills require the physician to take action by writing a new script. They could respond rapidly due to the intervention. In contrast, patients can receive refills from prior scripts until they exhaust them (unless physicians take the unusual action of revoking them). Refills could thus respond slowly. As expected, the percent reductions are greater when focusing on new fills: the decline is 17.8% for private insurance and 20.2% for Medicare. In the long run, reductions are also expected for refills – if physicians slow their new prescribing, refills on those scripts will also decline after a lag. Indeed, refills fall but the magnitudes are smaller and are only statistically significant for Medicare (7.3% for private insurance and 14.7% for Medicare).[4]

5.2 Effects on Prescribing of Other Drugs

We next consider prescribing of other drugs, including antipsychotics and other psychiatric medications (Appendix Table A3 lists all included medications). Effects on prescribing of other antipsychotics are *ex ante* ambiguous. Given the similarities between these drugs and quetiapine, prescribing could rise if physicians seek to game the metric of quetiapine announced in the letter. Prescribing could also decline if physicians glean new clinical information from the letter about antipsychotics in general. Regardless, we fail to find any meaningful signs of such a shift. The final outcome of Table 2 adds together prescribing of quetiapine with other drugs in the same class and earlier antipsychotic medications. Absolute effects are slightly larger in magnitude for this outcome, inconsistent with substituting patients to similar drugs. Confirming this result, we find no statisti-

[3]Effects on costs are large in magnitude but, for private insurance, are only significant when the outcome is logged (which was not pre-specified). This imprecision is partly the result of introducing variations in price which come from whether the drug is branded and/or extended-release.

[4]We identify new fills and refills using the refill flag on the claim. The analysis plan pre-specified using whether the fill was the first for the patient-prescriber in the last year. This approach tended to mis-classify fills because patients frequently churned off coverage. Appendix E reports those results for completeness.

cally significant changes in prescribing of other drugs in quetiapine's class nor in prescribing of the earlier class (Appendix Table A4).

Given the off-label use of quetiapine to treat conditions for which other psychiatric medications are indicated, we also study prescribing of other drugs that are approved for those conditions: benzodiazepines, which are frequently used to treat anxiety and insomnia; antidepressants, which can treat depression and a variety of other mental health issues; and non-benzodiazepine insomnia medications. Of the three drug classes across the two insurers, we detect a significant effect only on private insurance antidepressant prescribing, which increased 4.5%. While such substitution could benefit patients, the signal of this result is weakened by our failure to detect it in Medicare; we also fail to detect this substitution in the patient-level analyses (Appendix Table A10).

5.3 Effects on Prescribing to Patient Subgroups

We next study effects on prescribing to patient age and appropriateness subgroups. These outcomes help to better describe the marginal prescription curtailed by the intervention.

Age Table 3 shows effects on prescribing to varying patient age groups. Study physicians tend to prescribe to older privately insured patients. Among control arm physicians, volume monotonically increases in age band. Point estimates also become monotonically more negative with patient age. Reductions are only statistically significant to privately insured patients age 55-64, the decade of life before Medicare take-up becomes nearly universal. Cutbacks to this age range were 22.7%. Figure 2 visualizes this result. It presents estimates for three age groups (0-44, 45-54, and 55-64) that roughly divide prescribing volume into thirds. There is a clear monotone path of the point estimates, with a large and statistically significant effect only for the oldest private insurance age band. A similar path of effects holds for Medicare prescribing, though the age distribution is shifted to the right. These patterns show that spillovers onto private insurance were concentrated on patients who were closest to the Medicare age range.

Appropriateness Next, to build evidence on whether the marginal prescription was likely to have benefited the patient, we study effects on prescribing to patients of varying observable appropriateness (Table 4 and Figure 3). We find little relationship between the magnitude of the point estimate

and appropriateness. While the intervention significantly reduced private insurance prescribing to likely good (guideline-concordant) candidates for the drug, point estimates were similar for low-value candidates, though not significant. Given the smaller prescribing volumes and imprecise estimates for some of the subgroups on the privately insured side, Medicare effects help to assess whether physicians tailored their cutbacks to observably appropriate or inappropriate patients. In Medicare, the effects are nearly identical across the four appropriateness groups: reductions are all 17-18% and statistically significant. Percent effects are never statistically significantly different between private insurance and Medicare. Thus in percent terms, the intervention appears to curtail prescribing by similar magnitudes regardless of whether patients are observably indicated for quetiapine.

5.4 Effects on Patients

In order to study patient health outcomes, we now estimate equation (2) for a baseline cohort of patients who received quetiapine from study physicians in the pre-intervention period. In Table 5, we first replicate physician-level findings for these patients. These outcomes count prescriptions received from all physicians, not just the original study physicians, and so they are inclusive of any potential substitution to other prescribers. As expected, private insurance patients experienced a 9% decrease in receipt of quetiapine due to the letter; reductions are similar when we count all antipsychotics, not just quetiapine, showing that patients did not simply substitute away to other drugs. These effect estimates are statistically significant at the 10%, but not 5%, level. A subgroup analysis shows reductions for patients who met prescribing guidelines as well as likely low-value patients, but the effect was larger in magnitude and only statistically significant at the 10% level for guideline-concordant patients (Appendix Table A5).

Medicare patients experienced a significant reduction; it was smaller (5%) but not significantly different from the private insurance patients. As before, we fail to see signs of substitution to other antipsychotics for private insurance and Medicare patients: including all antipsychotics in the outcomes yields little change in the estimates.

Given the "first stage" on antipsychotic treatment, particularly for patients with serious mental illness, a key concern is that some prescriptions curtailed by the intervention may have carried benefit for patients. To explore this issue, we study patient health outcomes and correlates thereof. The subsequent rows of Table 5 report several measures of hospital utilization that proxy for adverse

outcomes. These patients have frequent interactions with the hospital: the average control private insurance patient has 0.4 inpatient hospitalizations and one emergency department visit during the outcome period. While going to the hospital might seem like a relatively extreme outcome, the presence of mental health conditions among the vast majority of baseline patients and their high rate of inpatient and emergency department encounters suggests that these outcomes might be sensitive to improper disruptions in care. It is thus notable that we detect no statistically significant changes in the number of inpatient stays or emergency department visits and that the point estimates are small in absolute and percent terms. A similar pattern prevails for Medicare patients, where the sample is larger and rates of encounters are higher.

In the final rows of Table 5 we study two measures of visits with mental health care providers that could proxy for patient benefit from the intervention. Patients enter the baseline cohort because they were prescribed an antipsychotic medication by a generalist physician. Given the frequency of mental illness in this population, these patients might benefit from a mental health specialist. The average control group private insurance patient has 1.2 visits with psychiatrists and 0.3 with psychologists – these patients are nearly as likely to have a hospital visit as they are to see a mental health care provider. We detect decreases in psychiatrist visits and increases in psychologist visits. Both effects are significant at the 10% level and they net to zero nearly exactly, suggesting that this channel is not operative in a substantial way. We also note no significant effects on these encounters in the larger Medicare cohort.

6 Discussion

6.1 Mechanisms

We now explore the channels that drive our key finding: that physicians altered their prescribing to patients who were unmonitored and not intentionally targeted by Medicare. We consider two potential mechanisms. First, the intervention could have provided information that altered physician beliefs around the clinical benefits of prescribing – the information channel. Second, the intervention could have raised the implicit costs of prescribing by highlighting the potential for a CMS review – the review channel. Since the trial involved only one intervention, there is no way to perfectly distinguish the channels, but we find that the evidence is most consistent with the review channel.

The letter contained a peer comparison designed to activate social and professional norms as well as a message emphasizing the administrative penalties CMS could levy against inappropriate prescribers. Under the information channel, these two features of the letters may have induced physicians to update their beliefs on the optimality of their prior quetiapine prescribing by, for example, prompting them to consult with peers or review clinical guidelines. In this case, we would expect a corresponding drop in prescribing to inappropriate patients irrespective of their insurer. However, the effects by patient appropriateness described in Section 5.3 and presented in Figure 3 are inconsistent with reductions targeted at patients who failed to meet clinical guidelines.

The information channel would also predict changes in prescribing of other antipsychotics because these drugs tend to share guideline recommendations with quetiapine. Yet Section 5.2 and Appendix Table A4 demonstrate no significant effects on antipsychotics in the same class as quetiapine nor effects on first-generation antipsychotics. It is hard to reconcile the broad patterns of this study with updating based on clinical evidence.

The review channel provides an alternative (though not exclusive) mechanism. By sending this letter, which stated the physician was under review and could face Medicare administrative actions, CMS broadly raised the cost of prescribing to Medicare patients. It is consistent with reductions in prescribing to Medicare patients irrespective of appropriateness, as we observe. It also aligns with the failure of the letter to reduce prescribing of medications similar to quetiapine, since the plain text of the letter left the costs of prescribing these drugs unchanged. However, this channel alone does not imply cutbacks to private insurance patients. The spillovers we observe would require physicians to find it costly to distinguish the insurer of their patients.

The effects on prescribing by patient age, presented in Section 5.3 and Figure 2, provide a potential explanation for the nature of these costs. While providing care, it may be difficult for physicians to assess a patient's insurer. Physicians may find the insurer less salient relative to the patient's clinical features. Alternatively, physicians may find it distasteful to vary practice style on the basis of attributes like insurer that are not directly relevant to care. In turn, physicians could minimize the cost of responding to the letter by developing heuristics based on other readily available patient attributes like age.

Under such a heuristic, a physician who does not directly observe the patient's insurer could predict it on the basis of age but only imperfectly. There are two principal reasons for Medicare

eligibility: old age (age 65 and up) and disability (receiving Social Security Disability Insurance for at least two years). Much of study physicians' Medicare prescribing goes to the latter category – more than one in three of their baseline Medicare patients are under age 65. A physician trying to cut back prescribing to Medicare patients but unable to observe insurer could do so effectively but imperfectly by targeting older patients. This heuristic is consistent with the pattern we see of large and statistically significant cutbacks in prescribing to the oldest privately insured patients under age 65.

6.2 Welfare Impacts

Spillovers can compound or offset the welfare impacts of an intervention on its targeted population. A prior clinical evaluation of this intervention, Sacarny et al. (2018), looked at its effects on the universe of Medicare beneficiaries without observing the private insurance spillovers. This evaluation was generally consistent with the intervention yielding welfare benefits. Specifically, the letters triggered a large reduction in prescribing of quetiapine in Medicare, reducing quetiapine costs by $979 per treated physician during the first 9 months. These cost reductions were not offset by any detected harms in outcomes as measured by hospital visits.

The prescribing landscape in private insurance differs from Medicare in welfare-relevant ways. Aside from the expected difference in age, the rate of dementia is much lower: just 2% of baseline privately insured patients had it, compared to 43% of baseline patients in Medicare. Stemming antipsychotic prescribing to patients with dementia has been a key objective of CMS. Yet this off-label use is clearly a second-order problem for private insurers. In practice, physicians in the study still prescribed to likely off-label patients in private insurance with other conditions like insomnia and PTSD. As a result, the appropriateness of prescribing was ultimately similar by insurer. Taken together, there is little evidence that the welfare effects differ when accounting for the spillover onto private insurance.

6.3 Common Agency

The common agency framework of Bernheim and Whinston (1986) has motivated two more recent studies of the failure of physician-insurer contracting to incentivize greater performance and to finance performance-raising investments. Glazer and McGuire (2002) and Frandsen, Powell and

Rebitzer (2019) argue that free-riding and coordination failures across insurers can explain low levels of investment. In the simplest model, a market failure occurs because some of the gains from one insurer implementing the optimal contract accrue to the other insurers. To date, there has been little empirical evidence on the key pattern these models assume: that efforts by one insurer to alter physician practice styles affect the care reimbursed by other insurers.

Our finding of strong spillovers onto private insurance shows that the potential for free-riding on these interventions exists. While warning letters like the ones sent by CMS can have powerful effects on the practice of medicine, depending on the motivations and information available to insurers, there is little reason to expect they would be provided optimally in equilibrium. For example, suppose that insurers are fully informed about the care that physicians deliver to all patients, even patients covered by other insurers. If insurers only seek to maximize the quality of care they reimburse, they will fail to internalize the benefits (or harms) of such interventions when determining how much of them to supply.

An additional friction comes from the incomplete information available to health insurers. In general, insurers only observe the care that they cover. As a result, even a socially-minded insurer that valued the external effects of its interventions would be hampered in its efforts. Taking the CMS algorithm as given but broadening the data on which it was applied would likely have led the algorithm to target a different set of physicians. We explore this issue in depth in the next section.

7 Targeting Simulation

We now simulate the effects of a similar intervention conducted by an insurer that does not observe, or does not care about, prescribing covered by other plans. With multiple health insurers and spillovers from one insurer's contracting onto the care covered by other insurers, we expect privately minded or information-constrained insurers to fail to intervene on the optimal population, even conditional on the number of physicians in the intervention.

The simulation studies the targeting and effects of three potential interventions: one performed by private insurers alone, one by Medicare alone, and one by a merged (social planner-like) entity. It assumes that the effects of the intervention would match those of the CMS trial. It takes the CMS outlier algorithm as the correct measure of prescribing quality; thus intervening on higher

prescribers is sufficient to improve intervention targeting. We also fix the number of physicians to match the sample size CMS used as closely as possible in each simulated intervention. We then show that insurers' limited ability to observe prescribing covered by others deteriorates targeting. In turn, it attenuates the economy-wide reductions in prescribing achieved.

We begin by applying the CMS outlier algorithm to HCCI Medicare and private insurance prescribing in 2013 and 2014 (see Appendix Section D for a detailed description of the methodology). We make several modifications to reflect the new data and context. We also update the outlier formula so the outlier threshold is adjustable to yield the desired number of doctors. To ensure that our findings are not driven by these changes, the section focuses on outliers in HCCI data rather than the PCPs selected by CMS in the original intervention. The simulations yield three sets of PCPs: Medicare outliers, private insurance outliers, and Medicare + private insurance outliers. These analyses were not pre-specified.

7.1 Characteristics of the Outliers

In the simulation, each insurer accounts only for the prescribing it covers when running the algorithm. Its outliers may not match up well with the outliers for the other insurer, nor with those selected when combining both insurers' data (which we use to proxy for the socially optimal targeting). A Medicare intervention may be poorly targeted in private insurance or vice versa. Figure 4 and Panels A and B of Appendix Table A6 characterize the targeting. They describe the three sets of outliers and their prescribing volume during the 2013-2014 period that the algorithm analyzed.

As expected, the outliers selected from one insurer's data have the highest prescribing volume for that insurer. They often prescribe little or nothing to patients of the other insurer. In Panel A of Figure 4, Medicare-only outliers are shown prescribing more than fivefold the quetiapine volume to Medicare patients than do outliers selected from private insurance data alone. 56% of private insurance outliers prescribe no quetiapine in Medicare at all. Panel B shows Medicare outliers tend to have low volumes covered by private insurance; they supply one-sixth as much quetiapine in private insurance as the private insurance outliers. 72% supply none in private insurance at all. The two sets of outliers had little overlap: only 9% of Medicare outliers were also private insurance outliers and vice versa.

The final panel of Figure 4 plots prescribing in Medicare and private insurance combined. By

construction, the outliers selected from the combined data prescribe the most here on average – yet outliers selected from Medicare data alone prescribe nearly as much. This finding reflects that Medicare prescribing volume is much higher than private insurance. Accordingly, 76% of the Medicare outliers were also outliers in the combined data. In contrast, targeting high total prescribing with private insurance data alone works relatively poorly. The private insurance outliers prescribe only half as much, on average, as the outliers selected from the combined data. Only 32% of them are also outliers in the combined data.

7.2 Foregone Reductions in Prescribing

The simulation has so far shown that information-constrained or privately minded insurers would fail to select doctors who prescribe heavily overall. We now explore how the difference in targeting deteriorates the effects of the intervention. Panel C of Appendix Table A6 projects the change in economy-wide primary care quetiapine prescribing from the interventions during the outcome period. We project the volume of prescribing curtailed by the intervention by assuming the intervention has effects on Medicare and private insurance given by Table 2. That projection is scaled by an estimate of the volume of primary care prescribing that would have occurred nationally absent the intervention. To avoid polluting results with the effects of the actual CMS intervention, the estimates give treated PCPs in the original trial no weight and control PCPs proportionately higher weight.

Private insurers intervening on their own would forego substantial economy-wide reductions. Their intervention on top private insurance prescribers would reduce total prescribing in Medicare and private insurance by less than half the effect of the social planner's intervention (1.8% vs. 3.9%). These findings have two key implications. First, even if private insurers had found such an intervention worthwhile and conducted it at the same intensity (and with the same effect on prescribing) as CMS, they would have achieved substantially smaller national reductions. Because private insurance prescribing and total insurance prescribing by PCPs is imperfectly correlated, even if private insurers were to intervene, they would often not act upon PCPs that a social planner would choose. Second, limited access to data on external prescribing means that even if private insurers valued the spillovers of their efforts, they would struggle to target those external effects.

Panel C also shows that a Medicare program conducting its own intervention would essentially

match the social planner's effect (3.9%) up to a rounding error. The concerns the private insurance simulation uncovered are dampened when Medicare is the actor. Though Medicare does not observe private insurance data in the simulation, its own information still provides a close approximation to the total because it covers the bulk of quetiapine prescribing (roughly three-fourths in HCCI data). Medicare is thus more able to target the doctors the social planner would pick. This result highlights that when a single insurer observes an accurate signal of aggregate behavior, it can more closely replicate the actions of the social planner. Whether the insurer would desire to replicate those actions is less clear, since it may not value the spillovers it has on care covered by others.

8 Conclusion

This paper provides new, randomized evidence documenting spillovers from Medicare on private insurance. We study a trial of Medicare letters which sought to improve the quality of prescribing of the most commonly used antipsychotic medication, quetiapine. The letter focused on the Medicare program and did not mention other insurers. Yet we find large effects on prescribing to privately insured patients and cannot rule out one-for-one spillovers. Physicians cut back to patients who were observably appropriate and questionable candidates, but did not detectably change prescribing of medications with similar guidelines. This evidence suggests that the letter acted by bluntly raising the costs of prescribing the drug. The spillovers suggest that physicians found it costly to set distinct medical practice styles on the basis of insurer.

These findings have broader implications for the causes and evolution of health care productivity in the U.S. A large literature has hypothesized that agency problems, and common agency in particular, can explain striking failures of performance-raising changes to diffuse through the health care system. The large cross-insurer spillovers following this intervention show that one insurer's efforts can lead to changes in care delivery well beyond the care the insurer covers. If insurers fail to internalize the full benefits of these interventions, those that raise productivity are likely to be under-provided in equilibrium. Thus a key open question for policymakers is how to encourage these performance-raising investments when payment is fragmented across multiple, uncoordinated entities.

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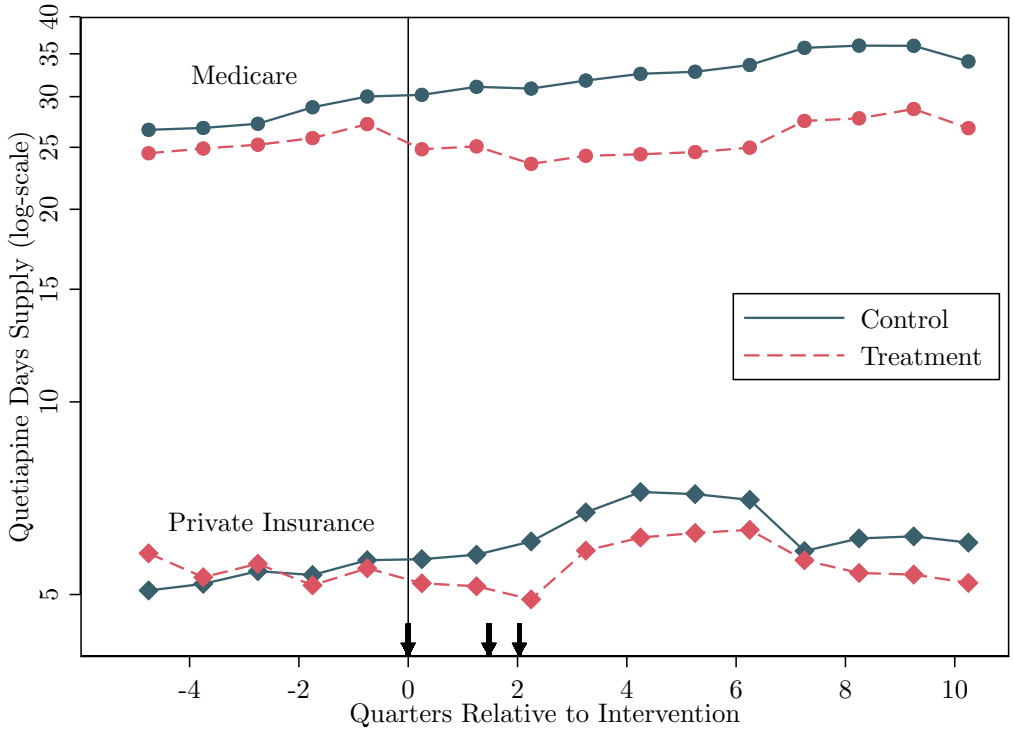
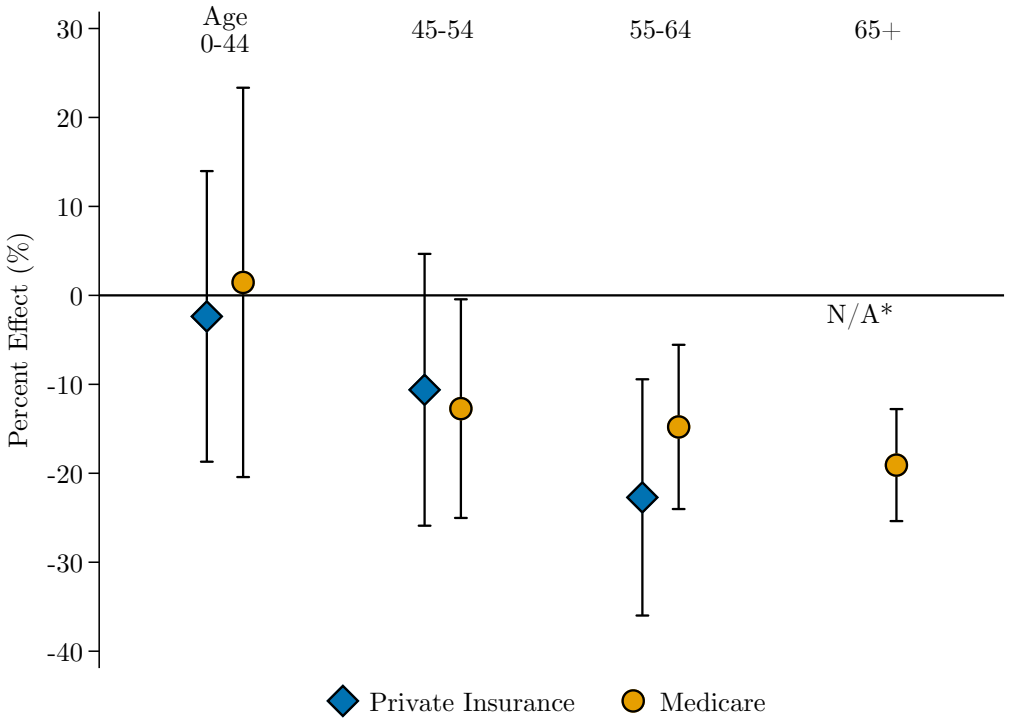
Figures

Figure 1 – Quarterly Prescribing over Sample Period



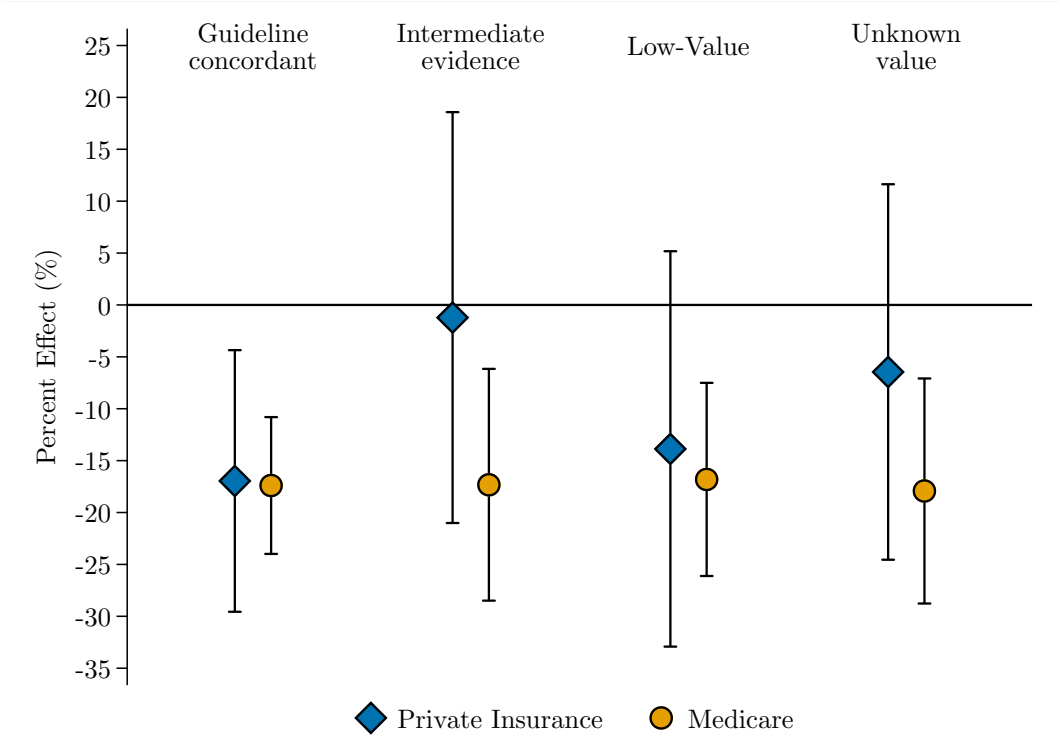
Notes: Figure plots the average days of quetiapine supplied by control (blue solid line) and treatment (red dashed line) study prescribers. The series in the upper half of the figure count prescribing in Medicare while the series in the lower half count prescribing in private insurance. The solid vertical line denotes the start of the intervention and the arrowheads indicate when the treatment letters were sent. The y-axis uses a log-scale to visualize differences in percent terms, given the large difference in prescribing volume to private insurance and Medicare patients.

Figure 2 – Effects on Prescribing by Patient Age



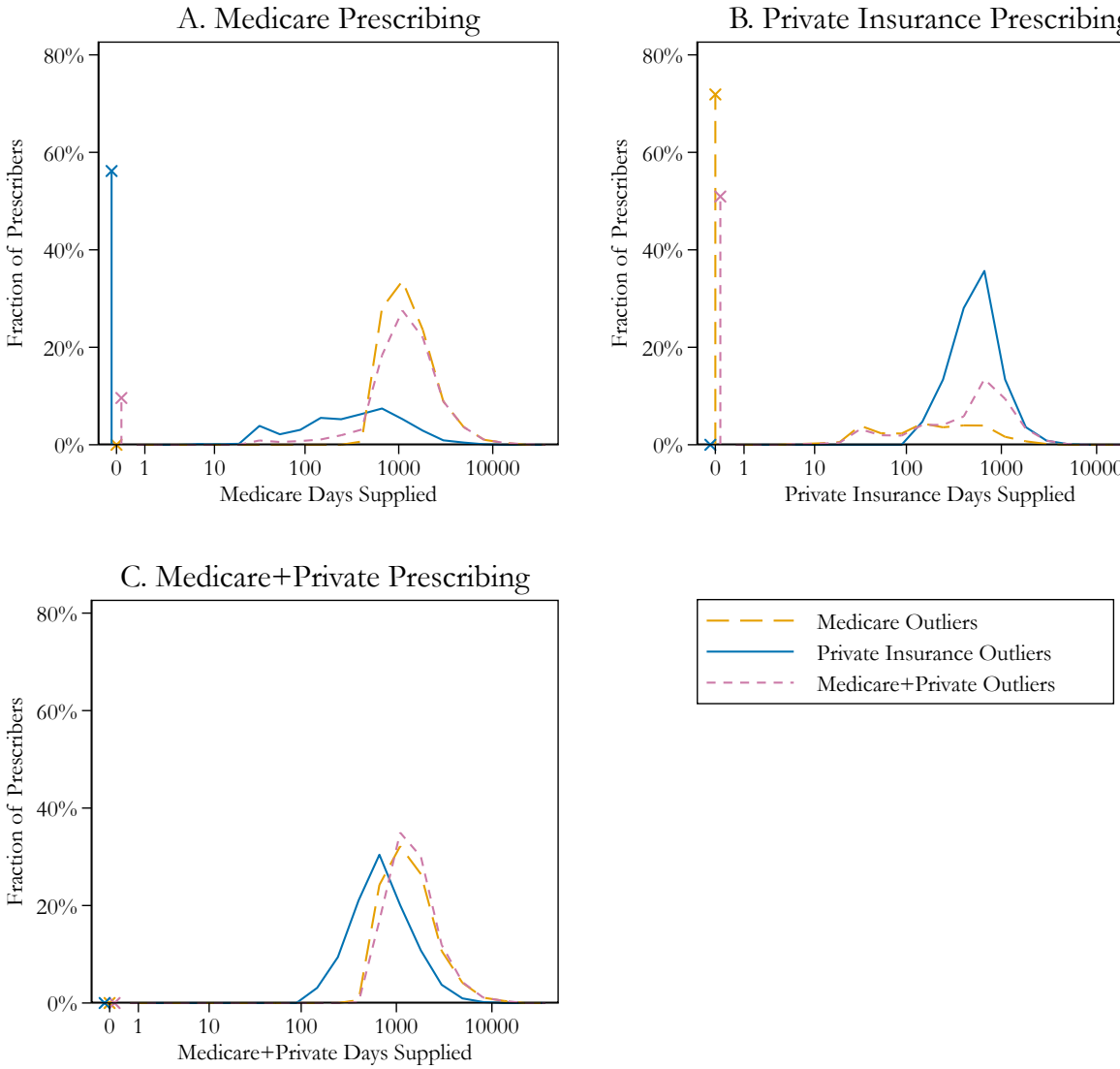
Notes: Figure plots estimates of the effect of the intervention on prescribing to patients in varying age bins. To facilitate comparisons of the effects across age bins with different baseline levels of prescribing, we present percent effects (i.e. treatment effect divided by the control mean). Error bars show 95% confidence intervals. See Table 3 for more details.
* No estimate presented because privately insured patients age 65 and up are omitted from the sample.

Figure 3 – Effects on Prescribing by Patient Observable Appropriateness



Notes: Figure plots estimates of the effect of the intervention on prescribing to patients by observable appropriateness for quetiapine according to clinical guidelines. Section 4.3 describes the algorithm we use to classify patients into appropriateness categories. To facilitate comparisons of the effects across appropriateness bins with different baseline levels of prescribing, we present percent effects (i.e. treatment effect divided by the control mean). Error bars show 95% confidence intervals. See Table 4 for more details.

Figure 4 – Quetiapine Prescribing of Outlier PCPs in Targeting Simulations



Notes: Figure plots the distribution of quetiapine prescribing volume for PCPs selected in targeting simulations. It depicts three groups of outlier PCPs: Medicare (dashed yellow line), private insurance (solid blue), and Medicare+private insurance (short dashed red). Each panel shows histograms of prescribing volume covered by the given insurer. The x-axis scale is produced by taking log(1+days supply). The spikes above 0 indicate the share of PCPs in the group with no prescribing in that insurer. Panel A plots prescribing in Medicare. Panel B plots prescribing in private insurance. Panel C plots prescribing in Medicare and private insurance combined. See Appendix Table A6 and text for more details.

Tables

Table 1 - Summary Statistics of Study Prescribers

Characteristic	(1) Control	(2) Treatment	(3) p-value
Female	17.8%	17.8%	0.936
Primary Specialty			
Family Medicine	47%	49%	0.125
General Practice	4%	5%	0.156
Internal Medicine	49%	46%	0.027
Any Psychiatric Specialty	<1%	<1%	0.999
No Quetiapine Prescribing During Baseline Period			
To Privately Insured Patients	74.2%	74.4%	0.853
To Medicare Patients	40.8%	43.1%	0.096
Days Supply of Quetiapine During Baseline Period			
To Privately Insured Patients	65.0 (184.7)	64.7 (204.6)	0.963
To Medicare Patients	338.7 (723.1)	309.4 (543.5)	0.104
P-value, omnibus test of equality			0.317
N	2,528	2,527	

Notes: This table reports summary statistics of the study prescribers. Binary variables displayed as percentages and continuous variables displayed as means (standard deviations). Data on prescriber sex and specialization come from pre-intervention NPPES files. A small number (<1%) of prescribers had a primary specialization outside the three categories in the table. Quetiapine prescribing measures only consider the baseline period (the year prior to the intervention).

Table 2 - Effects on Key Prescribing Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Insurer	Private Insurance			Medicare			P-Val, %
	Control	Treatment	Percent	Control	Treatment	Percent	Effects
Outcome	Mean	Effect	Effect	Mean	Effect	Effect	Equal
Days Supply*	209.5	-24.5	-11.7%	1,094.5	-183.6	-16.8%	[0.344]
		(10.5)	(5.0%)		(30.1)	(2.7%)	
New Fills	2.53	-0.45	-17.8%	11.24	-2.26	-20.2%	[0.754]
		(0.15)	(5.9%)		(0.58)	(5.1%)	
Refills	3.61	-0.26	-7.3%	18.99	-2.79	-14.7%	[0.268]
		(0.22)	(6.0%)		(0.66)	(3.5%)	
Days Supply	387.7	-28.2	-7.3%	2,002.1	-195.3	-9.8%	[0.563]
(All Antipsychotics)		(14.8)	(3.8%)		(49.7)	(2.5%)	

N=5,055. Notes: Table reports estimates for prescriber-level outcomes for private insurance (columns 1-3) and Medicare (columns 4-6). All measures count prescribing during the outcome period (April 21, 2015 through December 31, 2017) and include only quetiapine, except the final measure which includes all antipsychotics. See text for more detail. Columns 1 and 4 report the mean outcome for control prescribers. Columns 2 and 5 report the treatment effect estimate from equation (1). Columns 3 and 6 divide the treatment effect by the control mean to produce a percent effect. Column 7 reports the p-value from a test that the percent effects for private insurance and Medicare are equal. Robust standard errors in parentheses. P-values in brackets.

* Pre-specified primary outcome.

Table 3 - Effects on Prescribing by Patient Age

Insurer	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Private Insurance			Medicare			P-Val, %
Age Bin	Control Mean	Treatment Effect	Percent Effect	Control Mean	Treatment Effect	Percent Effect	Effects Equal
Age 0-17	2.0	0.7	37.6%				
		(1.1)	(55.8%)				
Age 18-24	9.1	0.4	4.7%				
		(1.7)	(18.5%)				
Age 25-34	19.3	0.0	0.1%	4.4	1.8	40.3%	[0.216]
		(2.6)	(13.4%)		(1.3)	(30.0%)	
Age 35-44	39.7	-1.6	-4.0%	28.8	-2.1	-7.5%	[0.820]
		(4.1)	(10.2%)		(3.4)	(11.6%)	
Age 45-54	60.9	-6.5	-10.6%	98.3	-12.5	-12.7%	[0.824]
		(4.7)	(7.8%)		(6.2)	(6.3%)	
Age 55-64	78.4	-17.8	-22.7%	209.3	-31.0	-14.8%	[0.310]
		(5.3)	(6.8%)		(9.9)	(4.7%)	
Age 65+				753.6	-143.8	-19.1%	
					(24.2)	(3.2%)	

N=5,055. Notes: Table reports estimates for prescriber-level outcomes for private insurance (columns 1-3) and Medicare (columns 4-6). Each row counts quetiapine prescribing in days supply to patients in the specified age range during the outcome period (April 21, 2015 through December 31, 2017). Commercial insurance patients age 65+ are omitted here (and throughout the study) because their status as Medicare patients is uncertain. Columns 1 and 4 report the mean outcome for control prescribers. Columns 2 and 5 report the treatment effect estimate from equation (1). Columns 3 and 6 divide the treatment effect by the control mean to produce a percent effect. Column 7 reports the p-value from a test that the percent effects for private insurance and Medicare are equal. Robust standard errors in parentheses. P-values in brackets.

Table 4 - Effects on Prescribing by Patient Appropriateness

Insurer	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Private Insurance			Medicare			P-Val, %
Appropriate-ness Group	Control Mean	Treatment Effect	Percent Effect	Control Mean	Treatment Effect	Percent Effect	Effects Equal
Guideline-Concordant	104.4	-17.7 (6.7)	-17.0% (6.4%)	638.2	-111.0 (21.5)	-17.4% (3.4%)	[0.950]
Intermediate Evidence	29.1	-0.4 (2.9)	-1.2% (10.1%)	103.4	-17.9 (5.9)	-17.3% (5.7%)	[0.149]
Low Value / Inappropriate	29.1	-4.0 (2.8)	-13.9% (9.7%)	200.5	-33.7 (9.5)	-16.8% (4.7%)	[0.773]
Unknown	46.8	-3.0 (4.3)	-6.5% (9.2%)	152.3	-27.3 (8.4)	-17.9% (5.5%)	[0.254]

N=5,055. Notes: Table reports estimates for prescriber-level outcomes for private insurance (columns 1-3) and Medicare (columns 4-6). Each row counts quetiapine prescribing in days supply to patients in the specified appropriateness group during the outcome period (April 21, 2015 through December 31, 2017). See text for descriptions of the appropriateness groups and the algorithm used to classify patients. Columns 1 and 4 report the mean outcome for control prescribers. Columns 2 and 5 report the treatment effect estimate from equation (1). Columns 3 and 6 divide the treatment effect by the control mean to produce a percent effect. Column 7 reports the p-value from a test that the percent effects for private insurance and Medicare are equal. Robust standard errors in parentheses. P-values in brackets.

Table 5 - Effects on Baseline Patients

Patient Cohort	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Private Insurance (N=1,980)			Medicare (N=7,384)			P-Val, %
Outcome	Control Mean	Treatment Effect	Percent Effect	Control Mean	Treatment Effect	Percent Effect	Effects Equal
Quetiapine Days Received	260.7	-23.2	-8.9%	441.0	-22.0	-5.0%	[0.481]
		(13.3)	(5.1%)		(10.0)	(2.3%)	
Antipsychotics Days Received	293.3	-27.3	-9.3%	496.8	-23.9	-4.8%	[0.408]
		(14.7)	(5.0%)		(10.7)	(2.1%)	
Inpatient Stays	0.39	-0.02	-4.4%	0.87	0.04	4.0%	[0.522]
		(0.05)	(12.3%)		(0.04)	(4.6%)	
ED Visits	0.99	-0.04	-4.4%	1.75	0.07	3.9%	[0.487]
		(0.10)	(9.8%)		(0.12)	(6.6%)	
Psychiatrist Visits	1.16	-0.26	-22.2%	1.14	0.07	6.4%	[0.072]
		(0.16)	(13.7%)		(0.09)	(7.7%)	
Psychologist Visits	0.33	0.24	71.2%	0.84	-0.07	-8.5%	[0.115]
		(0.13)	(39.8%)		(0.26)	(30.9%)	

Notes: Table reports estimates for outcomes for privately insured baseline patients (columns 1-3) and baseline patients on Medicare (columns 4-6). See text for more details on the construction of the baseline patient cohorts. Each measure counts health care use during the outcome period (April 21, 2015 through December 31, 2017). Columns 1 and 4 report the mean outcome for baseline patients of control prescribers. Columns 2 and 5 report the treatment effect estimate from equation (2). Columns 3 and 6 divide the treatment effect by the control mean to produce a percent effect. Column 7 reports the p-value from a test that the percent effects for the private insurance and Medicare cohorts are equal. Robust standard errors clustered at the baseline prescriber level in parentheses. P-values in brackets.

Appendix To:

Common Practice: Spillovers from Medicare on Private Health Care

Michael Barnett, Andrew Olenski, and Adam Sacarny

May 27, 2020

A Detailed Description of Patient Classification Algorithm

To develop an algorithm for classifying patients by clinical appropriateness, we studied the clinical literature and guidelines for antipsychotics (Maglione et al., 2011; Painter et al., 2017; American Psychiatric Association, 2016; American Geriatrics Society, 2019). The algorithm we ultimately elected classifies each patient into one of four mutually exclusive and exhaustive categories using diagnosis codes from the baseline and outcome periods (April 20, 2013 through December 31, 2017). When patients fit in multiple categories, they are assigned in cascading order to the highest-value one.

To make the approach as parsimonious as possible, the final algorithm was based on FDA approvals and an evidence summary table in Maglione et al. (2011), a systematic review of off-label prescribing of antipsychotics. Table A of that study displays the quality of evidence for each of a multitude of off-label uses. In the resulting algorithm, one category contains FDA approved uses and the remainder map to standards of evidence in Table A:

1. **Guideline-concordant** patients have a serious mental illness – bipolar disorder, schizophrenia, or major depression – for which quetiapine is approved by the FDA. If a patient has major depression but not bipolar disorder or schizophrenia, quetiapine is FDA approved for use alongside an antidepressant (called adjunctive therapy). For these patients, to match FDA approvals, the prescribing must overlap with an antidepressant. In the systematic review, these conditions are not listed because they are on-label, or in the case of major depression are listed as "moderate or high evidence of efficacy" with FDA approval.

2. **Intermediate evidence** patients have a condition for which the clinical evidence is mixed but has some support. We include patients with generalized anxiety disorder as well as those with major depression who are not concurrently receiving an antidepressant (called quetiapine monotherapy). In the systematic review, these conditions are listed as "moderate or high evidence of efficacy" without FDA approval.

3. **Low-value** candidates have conditions for which the evidence suggests that quetiapine has limited benefit or is even harmful. The most well known low-value condition is dementia, reflecting the guidelines which strongly discourage the use of antipsychotics in this popula-

tion. We also include insomnia, post-traumatic stress disorder, obsessive-compulsive disorder, personality disorders, eating disorders, and alcohol use disorder. The systematic review states these conditions as having "low or very low evidence of efficacy," "mixed results," or "low or very low evidence of inefficacy."

4. **Unknown** patients have no relevant diagnoses. We also include the small number of patients under age 18 in this category because pediatric guidelines for antipsychotics are distinct and study physicians rarely treat children and teenagers.

Appendix Table A7 provides a list of the ICD-9 and ICD-10 codes for each of these conditions.

For patients with major depression but not bipolar disorder or schizophrenia, the presence of antidepressants is pivotal for classification. In the prescriber-level analyses, we consider a quetiapine prescription to a major depression patient guideline-concordant if it overlapped with an antidepressant at the time it was dispensed and intermediate otherwise. In the patient-level analyses, we consider patients with major depression guideline-concordant if at least one of their quetiapine fills during the baseline period overlapped with an antidepressant on the day of dispense and classify them as intermediate otherwise. Overlap is determined using the date of service and days supply of the prescription fill.

While we pre-specified a classification algorithm, in practice we amended it in two ways to produce the above approach. First, the original algorithm did not consistently map between the systematic reviews and the guideline classifications. As a result, it mis-classified some indications: for example, prescribing to patients with obsessive compulsive disorder was erroneously considered to have "intermediate" support in the literature.[5] The updated algorithm uses a consistent classification. Second, we anticipated only using diagnosis codes from the baseline period in case diagnosis coding responded to the intervention. However, we found that most private insurance prescribing could not be classified with this approach due to short pre-intervention coverage durations and a lack of relevant diagnosis codes. We thus opted to include diagnosis codes from the outcome period. Despite both of these changes, our results are robust to the pre-specified approach (Appendix Table A8).

[5]Specifically, the pre-specified algorithm uses the following classification. Guideline-concordant: bipolar disorder, schizophrenia, major depression (irrespective of whether taken with antidepressant). Intermediate evidence: generalized anxiety disorder, depression (excluding major depression), obsessive-compulsive disorder, and personality disorder. Low-value: insomnia, PTSD, eating disorder, alcohol use disorder, and dementia.

B Construction of Baseline Patient Cohort

The baseline patient cohort consists of patients who received at least one quetiapine prescription from a study physician in the one year pre-intervention period (April 21, 2014 through April 20, 2015). Our initial dataset includes 12,418 patients meeting this criteria. The sample has three key restrictions. First, since patients periodically churn out of HCCI coverage and become unobserved in the data, they must still be enrolled in the month immediately prior to the intervention start, March 2015 (this excludes 2,546 patients). Second, we omit patients whose insurance type changes during the sample (e.g., private insurance to Medicare) or who maintain private insurance after age 64, since these patients are likely covered by both private insurance and Medicare (491 patients). Third, to ensure treatment status is clear, we exclude any patients who received a quetiapine prescription from more than one study prescriber during the year prior to the intervention (150 patients). These restrictions leave us with N=1,980 private insurance patients and N=7,384 Medicare patients.

C Measurement of Health Care Utilization

In addition to studying quetiapine prescribing to the baseline patient cohort, we also measure health care utilization. We define several measures of utilization relevant to this patient population, using three HCCI claims files: inpatient, outpatient, and physician. The inpatient and outpatient files contain institutional billing in their respective settings, while individual provider billing is contained in the physician file. We process claims from all three sources by reducing the these files to patient-provider-day level observations. Each patient-provider-day is considered one visit, so if a patient has multiple claims with the same provider on the same day, we only count these records as one encounter. Note that in HCCI data, claims and claim lines are already merged together.

We construct counts of inpatient, emergency department (ED), psychiatrist, and psychologist visits for each baseline patient in the post-intervention period (to use as outcomes) and pre-intervention period (to use as statistical controls). Our methodology for processing the data is as follows:

- **Inpatient stays**. We identify inpatient stays using the inpatient file and limiting to records with a hospital type-of-bill code (codes beginning with 1 or 85). Further, we drop all records

with zero allowed charges, missing DRGs, missing discharge status, or continuation discharge status (i.e. discharge status code 30). In the case that after removing these records the discharge date is not constant within a claim, we assign all records in the claim to the latest discharge date among them; in the extremely rare case that scrambled provider NPI is not constant within-claim, we pick the NPI in the first claim record. Finally, to remove duplicate claims and/or multiple encounters on the same day, we collapse together any records with the same patient, scrambled provider NPI, and discharge date. Each remaining record is considered to be one inpatient stay. We use this data to produce counts of the number of inpatient stays for each patient.

- **ED visits**. We identify ED visits using the outpatient file. We restrict to claims (i.e. claim IDs) that have at least one record with emergency department revenue centers (revenue center codes 450-459 or 981). Then we restrict to records with hospital or freestanding ED type-of-bill codes (codes beginning with 1, 85, or 78) and we drop records with zero allowed charges. In cases where the last date or scrambled NPI varies among records in the same claim, we mimic the approach used for inpatient stays and use the latest last date and first NPI among those records. Then, as with inpatient stays, we remove duplicate claims and/or multiple same-day encounters by collapsing together records with the same patient, scrambled provider NPI, and last date. Each remaining record is taken as one ED visit, and we use this data to generate the counts.

- **Psychiatrist or psychologist visits**. Visits with psychiatrists or psychologists are defined using the physician file. Only records with provider category 81 (psychiatrist) or 14 (psychologist) are loaded from this file. We exclude records with inpatient or ED place of service codes (codes 21, 23, 51, or 61), zero allowed charges, or missing scrambled NPI. To avoid double-counting visits that involve multiple claim lines or visits that are billed with multiple claims, we collapse together records with the same patient, scrambled provider NPI, and last date. If among records with the same patient-provider-date triple the provider is categorized as both a psychiatrist and a psychologist, we consider the provider a psychiatrist for all of those records. Each remaining record is taken as a visit with a psychiatrist or psychologist, and we use these records to count encounters for the patients.

D Detailed Description of Targeting Simulation

Here we describe in more detail the methodology of Section 7 in which we implement the original Medicare selection algorithm using HCCI data on Medicare and private insurance patients. For consistency with the original intervention, we closely match the algorithm that was originally run in Medicare, though in practice and by necessity our approach differs slightly. To match prior analyses in this manuscript, we omit prescribing to patients with no valid age and prescribing to privately insured patients 65 and up. We made four additional changes. First, if a patient filled multiple quetiapine scripts from the same doctor on the same day, CMS only included the fill with the longest duration, while we include all the fills; CMS previously found the two approaches were highly correlated ($\geq$95%). Second, CMS omitted long-term care pharmacies and patients but we include them because HCCI data does not identify them in its data during the analysis period. Third, CMS restricted its universe to PCPs with $\geq$10 quetiapine fills in a year but we relax the restriction to $\geq$1 fills due to lower prescribing volume in HCCI. Fourth, CMS excluded PCPs with a secondary specialty of psychiatry; we do not make this restriction because we only observe primary specialty.

We seek to identify 5,055 prescribers for each insurer (Medicare alone, private insurance alone, Medicare + private insurance). To ensure that when the algorithm is run it identifies the correct number of prescribers, we modify the outlier method described in the main text so that we can manipulate the threshold for outliers. Specifically, the new outlier threshold formula is:

$$T_{s,t,m} = Q^{75}_{s,t,m} + \kappa(Q^{75}_{s,t,m} - Q^{25}_{s,t,m}).$$

Where s indexes states, t indexes years, m indexes measures (quetiapine fills or days), $T_{s,t,m}$ is the threshold, and $Q^{p}_{s,t,m}$ is the pth percentile of measure m among prescribers in state s and year t. κ can be manipulated to raise or lower the threshold and thus the number of prescribers selected. In the original intervention, CMS searched κ to produce a sample of roughly 5,000 PCPs, picking $\kappa = 0.25$ (the method noted in the main text) which yielded a sufficiently close sample of $N = 5,055$. In practice, we search κ seeking to select 5,055 physicians. If there exists no value of κ that returns exactly 5,055 physicians, we choose the value that minimizes the absolute deviation from 5,055.

As in the CMS approach, to be selected, PCPs must be outliers relative to other prescribers in their state and year on four measures of quetiapine prescribing as given by the above formula: days supplied in 2013, days supplied in 2014, fills in 2013, and fills in 2014. The algorithm is run on just Medicare prescribing data, just private insurance prescribing data, and the combined Medicare and private insurance data. It yields three groups:

1. Outlier Medicare prescribers
2. Outlier private insurance prescribers
3. Outliers in combined Medicare and private insurance prescribing

In Figure 4 and Panels A and B of Appendix Table A6 we analyze and plot the distribution of quetiapine days prescribed by physicians in each of the three groups during the period 2013-2014. We compute the total days supplied to Medicare patients, private insurance patients, and Medicare+private insurance patients and compare the distributions across the providers in each of the groups. The main text also reports the overlap between the three groups as the share of prescribers in group g that are also in group g'.

Finally, using the three groups of providers and our point estimates of the percent effect of the intervention given in Table 2, we project the effect of intervening on each group of PCPs on national primary care quetiapine prescribing, reporting the results in Panel C of Appendix Table A6. Specifically, we estimate the reduction in quetiapine days supplied in the post-intervention period (April 21, 2015 – December 31, 2017) if an intervention were conducted in the given group of PCPs and divide it by the national volume of PCP prescribing that would have prevailed absent the intervention. Because we are analyzing prescribing that occurred after CMS actually intervened, PCPs who were treated in the original CMS study have lower volume in this period than they would absent CMS's efforts. Many of these PCPs enter the numerator and denominator of the estimates, biasing each downward relative to the counterfactual in which no intervention had truly occurred. Thus we reweight any PCPs who were in the original study so that treated PCPs get no weight and control PCPs are proportionately upweighted.

The projected reductions are given by the following formula:

$$r_g^n = \frac{\hat{\rho}^n \sum_{i \in P_g} \omega_i^g y_i^n}{\sum_{i \in P_*} \omega_i^* y_i^n}, \; n \in \{\text{Private}, \text{Medicare}\}$$

Where n indexes insurers, i indexes PCPs, and g indexes the three outlier groups. In the numerator, $\hat{\rho}^n$ is the estimated percent effect of the intervention in insurer n, P_g is the set of PCPs in outlier group g, ω_i^g is the PCP's numerator weight, and y_i^n is the PCP's prescribing in the outcome period. In the denominator, P_* is the set of all PCP prescribers of quetiapine nationally and ω_i^* is the denominator weight. The weights are given by the following formulas:

$$\omega_i^g = \begin{cases} 1 & \text{if not in CMS study} \\ 0 & \text{if treated in CMS study} \\ \left(N_g^T + N_g^C\right)/N_g^C & \text{if control in CMS study} \end{cases} \quad \omega_i^* = \begin{cases} 1 & \text{if not in CMS study} \\ 0 & \text{if treated in CMS study} \\ \left(N_*^T + N_*^C\right)/N_*^C & \text{if control in CMS study} \end{cases}$$

Where N_g^T and N_g^C are the number of PCPs in group g who were in the treatment and control group respectively in the CMS study; N_*^T and N_*^C are the number of PCPs with any HCCI quetiapine prescribing who were in the treatment and control group in the CMS study. Given the randomization, the weights for control PCPs are approximately 2 in the numerator (ω_i^g) and denominator (ω_i^*).

These calculations yield projections for Medicare and private insurance. The projections for Medicare + private insurance combined are produced by adding the private and Medicare numerators, adding the private and Medicare denominators, and taking the ratio of the two sums.

E Additional Results from Analysis Plan

We pre-specified several additional analyses that we do not report in the main text. For completeness, we present and discuss them here.

Appendix Table A9 reports additional outcomes for prescribers. First we report effects on new fills and refills using an alternative approach to the one used in the main text. In the approach here, a new fill is the first fill by a patient from the prescriber using a one year lookback period, and a refill is all other fills. While we pre-specified this approach, we found that churn in and out of

private insurance coverage meant that many patients had incomplete lookback periods, leading to misclassification of refills as new fills. In the main text we take a different approach that uses the refill flag in the prescription dispense, which is reported by the pharmacy on the claim and is not subject to misclassification if the patient has an incomplete lookback period. Consistent with churn causing misclassification, we find smaller reductions in new fills here for private insurance than we do with the approach in the main text. Next, to get a sense of effects on the typical daily dose prescribed, we report effects on milligrams per day supply, dividing the former by the latter. This outcome is only defined for PCPs who prescribed some quetiapine in the outcome period (N=1,895 in private insurance and N=3,512 in Medicare). We do not detect an effect in private insurance but note a positive and significant effect in Medicare, consistent with prescribers curtailing relatively low-dose prescriptions due to the letter.

The remaining rows of Appendix Table A9 report treatment effect estimates by quartiles of *ex ante* quetiapine prescribing volume (defined as the total of private insurance and Medicare prescribing). We counted prescribing during the 1 year pre-intervention period, a post-hoc modification from the analysis plan, which anticipated 9 months, because we sought to match our other analyses which generally used a one year pre-intervention period. Because a large number of study PCPs did not prescribe any quetiapine in HCCI data in the baseline period, quartile 1 contains all of them and is larger than one-fourth of the sample; these PCPs are missing from quartile 2, which is smaller than one-fourth. Across the quartiles absolute effect estimates are always negative and they expand in magnitude at higher quartiles for both private insurance and Medicare prescribing. Effects are only statistically significant for Medicare prescribing for quartiles 3 and 4. Percent effect estimates peak at quartile 2 for private insurance and quartile 3 for Medicare.

Appendix Table A10 reports the remaining pre-specified patient outcomes. The first three outcomes are alternative definitions of quetiapine receipt. As expected, the fills measure is similar in percent terms to the days measure reported in the main text; fills differs only because it ignores the days supply on fills, counting those with a short or long supply of medication equally. Effects on quetiapine cost are noisily measured, a pattern we also observed at the prescriber level. While effects on this outcome were not statistically significant, the confidence intervals on the percent effects easily include the point estimates of the effects on days supply. A similar pattern occurs for quetiapine milligrams where effects are negative, insignificant, and more noisily measured than

effects on days supply.

The next two measures are indicators for discontinuation in 2016Q4, i.e. the patient had no dispenses during this time, and dose reduction in 2016Q4, i.e. the patient received a lower dose in milligrams per day during this quarter as compared to the quarter before the intervention. The rate of dose reduction is lower than discontinuation because many patients already did not receive quetiapine during the last quarter before the intervention and so their dose could not be further reduced. Point estimates on these outcomes are all positive indicating less quetiapine receipt, but only reach statistical significance for dose reduction for Medicare patients.

We further pre-specified tests of whether patients were substituted to quetiapine alternatives. The next three outcomes report these tests for benzodiazepines, non-benzodiazepine insomnia drugs, and antidepressants, and do not detect any changes.

The subsequent four outcomes measure hospital encounters for substance use disorder (defined as visits with a principal diagnosis in AHRQ Clinical Classification Software categories 660 or 661) and for mental health reasons (principal diagnosis in Clinical Classification Software categories 650-652, 655-659, 662, 663, or 670), looking separately at ED visits and inpatient stays. Of the 8 estimates, we only detect a statistically significant effect (a reduction) on ED visits for mental health reasons for Medicare patients.

Next, given that differential disenrollment between treatment and control would lead to potentially spurious findings of treatment effects, we conducted a simple test of whether treatment or control patients remained enrolled in coverage at the same rate. By December 2016, only about half of private insurance patients and two-thirds are Medicare patients remained covered. We did not detect a difference in enrollment rates between treatment and control patients in either insurer group, however.

The final rows of the table report effects dividing the outcome into three mutually exclusive and exhaustive sources: the patient's baseline prescriber to whom they were attributed, other prescribers who did not have psychiatric specialization, and other prescribers who had psychiatric specialization. The effects on receipt from the three sources sum to approximately the days supply treatment effect in Table 5, but do not exactly sum to it because the baseline control is different in each regression (the control is the patient's quetiapine receipt from the given source during the baseline period). The results show that in an accounting sense, for both private insurance and Medicare patients, the

bulk of the cutback comes from the baseline prescriber with some compounding reductions from other prescribers. None of these effects is significant at the 5% level, and only the reduction from the baseline prescriber for Medicare patients is significant at the 10% level.

Appendix Figures

Department of Health & Human Services
7500 Security Boulevard, Mail Stop AR-18-50
Baltimore, Maryland 21244-1850

CMS
CENTERS FOR MEDICARE & MEDICAID SERVICES
CENTER FOR PROGRAM INTEGRITY

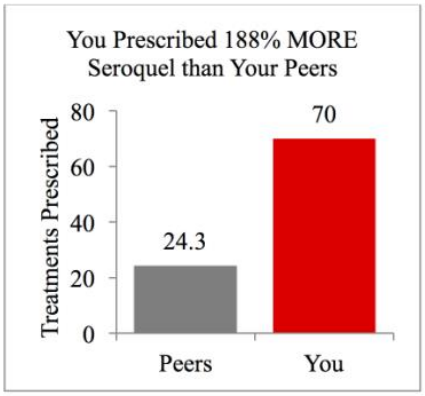
April 20, 2015

Pat Q. Provider MD
1234 Main St
Columbia, MD 21045
NPI: 1234567890 / Specialty: General Care Practitioner

Re: Your Seroquel prescribing is under review by the Center for Program Integrity.

Dear Dr. Provider,

The figure to the right displays your prescribing of Seroquel treatments (Seroquel, Seroquel XR, or generic quetiapine) compared to other general care practitioners in Maryland.



As can be seen, **you prescribed far more treatments – 188% more – than similar prescribers within your state.** In turn, you have been flagged as a markedly unusual prescriber, subject to review by the Center for Program Integrity.

We recognize that some flagged practitioners have appropriate reasons for this pattern. However, we have seen that other practitioners may drift into prescribing patterns that would be considered medically unjustified or abusive. Abusive prescribing can lead to extensive audits and even revocation of Medicare billing privileges.

We hope that you will use this information to see if your high prescribing level is consistent with the latest standards of care. To assist in your monitoring efforts, CMS will periodically send you letters with our most recent information about your Seroquel prescribing. **We may contact you at a later date to ask what steps, if any, you have taken in response to our communications.**

Read on for more information about the methodology used to analyze your prescribing behavior and to learn what actions to take next.

Sincerely,

[redacted]
Investigations and Audit Group

Figure A1 – Sample Intervention Letter

Appendix Tables

Table A1

Effects on Additional Prescribing Volume Measures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Insurer	Private Insurance			Medicare			P-Val, %
	Control	Treatment	Percent	Control	Treatment	Percent	Effects
Outcome	Mean	Effect	Effect	Mean	Effect	Effect	Equal
Fills	6.1	-0.7	-11.6%	30.2	-4.9	-16.2%	[0.438]
		(0.3)	(5.3%)		(1.0)	(3.2%)	
Cost	403.1	-44.9	-11.1%	1,417.8	-188.2	-13.3%	[0.859]
		(44.0)	(10.9%)		(80.1)	(5.7%)	
ln(Cost+1)*		-0.217			-0.189		[0.031]
		(0.058)			(0.065)		
Total	35,199.4	-4,995.3	-14.2%	140,263.2	-14,330.0	-10.2%	[0.547]
Milligrams		(2,095.0)	(6.0%)		(5,019.0)	(3.6%)	
Days (2015-2016)	135.3	-16.9	-12.5%	658.2	-108.1	-16.4%	[0.451]
		(6.5)	(4.8%)		(16.5)	(2.5%)	
Unique Patients (2015)	0.23	-0.03	-13.2%	0.66	-0.20	-30.0%	[0.073]
		(0.02)	(8.3%)		(0.03)	(4.4%)	
Unique Patients (2016)	0.42	-0.04	-8.3%	1.16	-0.31	-26.7%	[0.029]
		(0.03)	(7.7%)		(0.05)	(4.0%)	
Unique Patients (2017)	0.34	-0.02	-7.3%	1.21	-0.21	-17.0%	[0.333]
		(0.03)	(9.1%)		(0.06)	(4.7%)	

N=5,055. Notes: Table reports estimates for prescriber-level outcomes for private insurance (columns 1-3) and Medicare (columns 4-6). Each row presents an alternative measure of quetiapine prescribing volume during the outcome period (April 21, 2015 through December 31, 2017). See text for more detail. Columns 1 and 4 report the mean outcome for control prescribers. Columns 2 and 5 report the treatment effect estimate from equation (1). Columns 3 and 6 divide the treatment effect by the control mean to produce a percent effect. Column 7 reports the p-value from a test that the percent effects for private insurance and Medicare are equal. Robust standard errors in parentheses. P-values in brackets.
* Because the outcome is logged, these treatment effect estimates can be multiplied by 100 and interpreted as the log-point effect of the intervention on the cost of quetiapine covered.

Table A2

Summary Statistics of Baseline Patients

	(1)	(2)	(3)	(4)
Patient Cohort	Private Insurance		Medicare	
Characteristic	Control	Treatment	Control	Treatment
Age Band (Years)				
0-17	1.6%	1.9%		
18-25	9.5%	7.5%		
25-34	12.5%	15.9%	0.6%	0.9%
35-44	20.9%	21.4%	3.2%	3.0%
45-54	27.1%	28.0%	10.2%	11.6%
55-64	28.2%	25.3%	18.7%	18.4%
65-74			25.6%	24.5%
75-84			24.3%	23.0%
85+			17.4%	18.3%
Dual (Medicare-Medicaid) Eligible*	N/A	N/A	25.0%	27.0%
Female	58.1%	57.3%	62.7%	64.0%
Days of Quetiapine, Baseline Period	164.2 (141.1)	152.4 (135.1)	233.3 (161.9)	234.8 (159.4)
Appropriateness for Quetiapine				
Guideline-Concordant	51.1%	50.0%	57.6%	57.6%
Intermediate Evidence	11.9%	11.8%	7.5%	7.5%
Low-Value	16.0%	17.0%	20.8%	20.7%
Unknown	20.9%	21.2%	14.0%	14.2%
Enrolled December 2015	76.0%	74.0%	84.1%	83.8%
Enrolled December 2016	48.9%	51.5%	65.3%	66.0%
Months Enrolled, Outcome Period	18.8 (11.8)	18.9 (11.9)	23.4 (11.6)	23.6 (11.6)
N	974	1,006	3,837	3,547
P-value, omnibus test of equality	0.230		0.697	

Notes: This table reports summary statistics of the baseline patients of the study prescribers. Columns 1 and 2 consider baseline patients covered by private insurance who received quetiapine from one control arm prescriber and one treatment arm prescriber, respectively. Columns 3 and 4 consider baseline patients on Medicare Advantage. Binary variables displayed as percentages and continuous variables displayed as means (standard deviations). See text for more details on how patients are classified into appropriateness categories.

* Among the 69.5% of Medicare patients for whom dual status was observed.

Table A3

List of Drugs Included in Each Category

Drug Category	Drugs Included
Quetiapine	Quetiapine
Atypical Antipsychotics (Excluding Quetiapine)	Aripiprazole, Asenapine, Brexiprazole, Cariprazine, Clozapine, Iloperidone, Lurasidone, Olanzapine, Paliperidone, Pimavanserin, Risperidone, Ziprasidone
First-generation Antipsychotics	Chlorpromazine, Fluphenazine, Haloperidol, Loxapine, Molindone, Perphenazine, Pimozide, Thioridazine, Thiothixene, Trifluoperazine
Antidepressants	Amitriptyline, Amoxapine, Bupropion, Citalopram, Clomipramine, Desipramine, Desvenlafaxine, Doxepin, Duloxetine, Escitalopram, Fluoxetine, Fluvoxamine, Imipramine, Isocarboxazid, Maprotiline, Milnacipran, Mirtazapine, Nefazodone, Nortriptyline, Paroxetine, Phenelzine, Protriptyline, Selegiline, Sertraline, Tranylcypromine, Trazodone, Trimipramine, Venlafaxine, Vilazodone
Benzodiazepines	Alprazolam, Chlordiazepoxide, Clobazam, Clonazepam, Clorazepate, Diazepam, Estazolam, Flunitrazepam, Flurazepam, Halazepam, Lorazepam, Midazolam, Oxazepam, Prazepam, Quazepam, Temazepam, Triazolam
Insomnia (Excluding Benzodiazepines)	Doxepin, Eszopiclone, Ramelteon, Suvorexant, Tasimelteon, Zaleplon, Zolpidem

We used the following sources:
Antipsychotics: all included in 2016 CMS data, https://www.cms.gov/Research-Statistics-Data-and-Systems/Statistics-Trends-and-Reports/Medicare-Provider-Charge-Data/Part-D-Prescriber.html
Antidepressants: https://www.cms.gov/Medicare-Medicaid-Coordination/Fraud-Prevention/Medicaid-Integrity-Education/Pharmacy-Education-Materials/Downloads/ad-adult-dosingchart.pdf
Benzodiazepines: https://www.cdc.gov/drugoverdose/resources/data.html
Insomnia: Non-benzodiazepine, non-barbituate prescription sleep aids according to https://www.fda.gov/Drugs/DrugSafety/PostmarketDrugSafetyInformationforPatientsandProviders/ucm101557.htm

Table A4

Effects on Prescribing of Substitute or Alternative Medications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Insurer	Private Insurance			Medicare			P-Val, %
	Control	Treatment	Percent	Control	Treatment	Percent	Effects
Medication	Mean	Effect	Effect	Mean	Effect	Effect	Equal
Other Atypical	171.3	-4.0	-2.3%	782.8	-18.4	-2.3%	[0.995]
Antipsychotics		(8.7)	(5.1%)		(25.7)	(3.3%)	
First-Gen	6.9	0.6	8.3%	124.9	-10.3	-8.2%	[0.404]
Antipsychotics		(1.3)	(19.2%)		(7.3)	(5.8%)	
Benzodiazepines	2,367.6	28.6	1.2%	5,881.6	41.9	0.7%	[0.851]
		(54.8)	(2.3%)		(131.4)	(2.2%)	
Antidepressants	6,011.7	270.3	4.5%	13,883.3	232.2	1.7%	[0.181]
		(107.4)	(1.8%)		(243.1)	(1.8%)	
Insomnia (excl.	1,073.2	17.2	1.6%	1,128.3	-2.0	-0.2%	[0.577]
Benzo.)		(26.6)	(2.5%)		(28.9)	(2.6%)	

N=5,055. Notes: Table reports estimates for prescriber-level outcomes for private insurance (columns 1-3) and Medicare (columns 4-6). Each row presents prescribing of a potential substitute or alternative drug class during the outcome period (April 21, 2015 through December 31, 2017). See text for more detail. Columns 1 and 4 report the mean outcome for control prescribers. Columns 2 and 5 report the treatment effect estimate from equation (1). Columns 3 and 6 divide the treatment effect by the control mean to produce a percent effect. Column 7 reports the p-value from a test that the percent effects for private insurance and Medicare are equal. Robust standard errors in parentheses. P-values in brackets.

Table A5

Effects on Baseline Patients' Receipt of Quetiapine, by Appropriateness Group

Patient Cohort	(1)	(2)	(3)		(4)	(5)	(6)		(7)
	Private Insurance				Medicare				P-Val, %
Appropriate-ness Group	Control Mean	Treatment Effect	Percent Effect	N	Control Mean	Treatment Effect	Percent Effect	N	Effects Equal
Guideline-Concordant	317.3	-39.0	-12.3%	1,001	484.9	-22.7	-4.7%	4,255	[0.284]
		(20.9)	(6.6%)			(13.0)	(2.7%)		
Intermediate Evidence	213.2	-2.5	-1.2%	235	433.3	-15.9	-3.7%	552	[0.883]
		(32.8)	(15.4%)			(31.5)	(7.3%)		
Low Value / Inappropriate	228.5	-15.1	-6.6%	327	361.6	-38.4	-10.6%	1,536	[0.766]
		(28.4)	(12.4%)			(19.2)	(5.3%)		
Unknown	174.1	1.6	0.9%	417	383.1	0.2	0.1%	1,041	[0.949]
		(22.0)	(12.6%)			(23.6)	(6.2%)		

Notes: Table reports estimates for outcomes for different appropriateness groups of privately insured baseline patients (columns 1-3) and baseline patients on Medicare (columns 4-6). See text for more details on the construction of the baseline patient cohorts and the approach to assigning patients to groups. Each measure counts the days of quetiapine received by patients in the specified appropriateness group during the outcome period (April 21, 2015 through December 31, 2017). Columns 1 and 4 report the mean outcome for baseline patients of control prescribers. Columns 2 and 5 report the treatment effect estimate from equation (2). Columns 3 and 6 divide the treatment effect by the control mean to produce a percent effect. Column 7 reports the p-value from a test that the percent effects for the private insurance and Medicare cohorts are equal. Robust standard errors clustered at the baseline prescriber level in parentheses. P-values in brackets.

Table A6

Prescribing of Outlier PCPs in Targeting Simulations and Projected Effects

Prescriber Group	(1) Medicare Outliers	(2) Private Ins. Outliers	(3) Medicare + Private Outliers
A. No Quetiapine Prescribing, 2013-2014, %			
in Medicare	0.0	56.2	9.6
in Private Insurance	71.9	0.0	50.9
in Medicare+Private Combined	0.0	0.0	0.0
B. Quetiapine Days Supplied, 2013-2014, average			
in Medicare	1,580.8	286.6	1,441.0
in Private Insurance	105.2	640.0	334.3
in Medicare+Private Combined	1,686.0	926.6	1,775.3
C. Projected National Change in Primary Care Quetiapine Days from Intervening on Outliers, %			
in Medicare	-4.67	-1.39	-4.42
in Private Insurance	-1.13	-3.43	-2.24
in Medicare+Private Combined	-3.88	-1.84	-3.93
N	5,076	5,054	5,075

Notes: Table reports statistics or projections for physicians in each group. Groups are defined as physicians who are outliers in prescribing to Medicare patients (column 1), to privately insured patients (column 2), and to Medicare and private insurance combined (column 3). Panel A reports the percent of prescribers with no prescribing in the given insurer in 2013-2014, the period used by the algorithm to identify outliers. Panel B reports the average level of quetiapine days supplied during the 2013-2014 period. Finally, Panel C reports the projected national percent reduction in quetiapine days supplied by all PCPs in the given insurer during the outcome period (April 2015 to end-2017) if the entire outlier population were treated with letters. All calculations done using HCCI data only. See text for more details.

Table A7

List of Diagnosis Codes by Condition

Condition	ICD-9	ICD-10
Guideline-Concordant Conditions		
Bipolar Disorder	Multi-Level CCS Code 5.8.1	F30.10-F30.13, F30.2, F30.3, F30.4, F30.8, F30.9, F31.0, F31.10-F31.13, F31.2, F31.30, F31.31, F31.32, F31.4, F31.5, F31.60-F31.64, F31.70-F31.78, F31.81, F31.89, F31.9, F33.8, F34.81, F34.89, F34.9, F39
Schizophrenia	Multi-Level CCS Code 5.10	Multi-Level CCS Code 5.10
Guideline-Concordant or Intermediate Value Depending on Presence of Antidepressant		
Major Depression	293.83, 296.2X, 296.3X	F06.30, F32.9, F32.0, F32.1, F32.2, F32.3, F32.4, F32.5, F33.9, F33.0, F33.1, F33.2, F33.3, F33.41, F33.42
Intermediate Value Condition		
Generalized Anxiety Disorder	300.02	F41.1
Low-Value Conditions		
Dementia / Alzheimer's	331.0, 331.1, 331.11, 331.19, 331.2, 331.7, 290.0, 290.10-290.12, 290.20, 290.21, 290.3, 290.40-290.43, 294.0, 294.1, 294.10, 294.11, 294.20, 294.21, 294.8, 797, 290.13	F01.50, F01.51, F02.80, F02.81, F03.90, F03.91, F04, F05, F06.1, F06.8, G13.8, G30.0, G30.1, G30.8, G30.9, G31.1, G31.2, G31.01, G31.09, G94, R41.81, R54
Insomnia	327.0, 327.01, 327.02, 327.09, 307.41, 307.42, 291.82, 292.85, 780.51, 780.52	F10.182, F10.282, F10.982, F11.182, F11.282, F11.982, F13.182, F13.282, F13.982, F14.182, F14.282, F14.982, F15.182, F15.282, F15.982, F19.182, F19.282, F19.982, F51.02, F51.09, F51.01, F51.03, G47.01, F51.04, F51.05, G47.30, G47.00
PTSD	309.81	F43.10, F43.12
Obsessive-Compulsive Disorder	300.3, 301.4	F42.2, F42.3, F42.8, F42.9, F60.5
Personality Disorder	301.X, 301.XX	F21, F34.0, F34.1, F60.0-F60.7, F60.9, F60.81, F60.89, F68.10, F68.11, F68.12, F68.13, F69
Eating Disorder	Multi-Level CCS Code 5.15.2	F50.00, F50.9, F50.2, F98.3, F98.21, F50.89, F50.81, F50.82, F50.89, F98.29
Alcohol Use Disorder	Multi-Level CCS Code 5.11	Multi-Level CCS Code 5.11
Additional Intermediate Value Condition (only used in pre-specified algorithm)		
Depression (Ex. Major)	311, 300.4, 309.0, 309.1. 309.28, 298.0	F34.1, F43.21, F43.23, F32.9

Notes: When possible, we deferred to AHRQ Clinical Classification Software (CCS) groups. When the appropriate CCS group was a level 2 category, we used the ICD-9 and 10 codes from that group. Because level 3 categories are not yet available for ICD-10, when the group was a level 3 category (bipolar disorder, eating disorders), we used the given ICD-9 codes and found the relevant ICD-10 codes using equivalency mapping tables. ICD-9 and 10 codes for Dementia/Alzheimer's and Personality Disorder were taken from the Chronic Conditions Warehouse. For the other conditions, we sought out relevant academic literature and performed internet searches; this process typically identified ICD-9 codes which we then mapped to ICD-10 codes using equivalency tables.

Table A8

Effects on Prescribing by Patient Appropriateness (Pre-Specified Approach)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Insurer	Private Insurance			Medicare			P-Val, %
Appropriate-ness Group	Control Mean	Treatment Effect	Percent Effect	Control Mean	Treatment Effect	Percent Effect	Effects Equal
Guideline-Concordant	39.2	-7.4	-18.9%	297.9	-51.4	-17.2%	[0.858]
		(3.4)	(8.7%)		(11.3)	(3.8%)	
Intermediate Evidence	29.7	-4.8	-16.1%	145.4	-11.2	-7.7%	[0.454]
		(3.0)	(10.1%)		(7.4)	(5.1%)	
Low Value / Inappropriate	14.0	-0.5	-3.5%	126.3	-19.4	-15.4%	[0.453]
		(2.1)	(14.9%)		(7.2)	(5.7%)	
Unknown	126.6	-8.1	-6.4%	524.9	-117.9	-22.5%	[0.039]
		(8.8)	(6.9%)		(24.4)	(4.7%)	

N=5,055. Notes: This table repeats Table 4 but uses the pre-specified approach to assign patients to appropriateness groups rather than the preferred (post-hoc) approach. See appendix for more details on how the approaches differ.

The table reports estimates for prescriber-level outcomes for private insurance (columns 1-3) and Medicare (columns 4-6). Each row counts quetiapine prescribing in days supply to patients in the specified appropriateness group during the outcome period (April 21, 2015 through December 31, 2017). The text provides more details on the preferred approach to assigning patients to groups and footnote 1 describes how the approach used here differs from it. Columns 1 and 4 report the mean outcome for control prescribers. Columns 2 and 5 report the treatment effect estimate from equation (1). Columns 3 and 6 divide the treatment effect by the control mean to produce a percent effect. Column 7 reports the p-value from a test that the percent effects for private insurance and Medicare are equal. Robust standard errors in parentheses. P-values in brackets.

Table A9

Additional Prescriber-Level Outcomes from Analysis Plan

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Insurer	Private Insurance			Medicare			P-Val, %
	Control	Treatment	Percent	Control	Treatment	Percent	Effects
Outcome	Mean	Effect	Effect	Mean	Effect	Effect	Equal
New Fills	1.0	-0.1	-8.6%	3.2	-0.7	-22.4%	[0.071]
(Lookback)		(0.1)	(7.0%)		(0.1)	(3.5%)	
Refills	5.1	-0.6	-12.2%	27.1	-4.3	-15.9%	[0.564]
(Lookback)		(0.3)	(5.7%)		(0.9)	(3.4%)	
MG /	145.5	-0.6	-0.4%	125.3	11.5	9.2%	[0.031]
Days Supply*		(5.3)	(3.6%)		(3.6)	(2.9%)	
Quetiapine Days by Quartiles of Ex Ante (1-Year Baseline Period) Private + Medicare Prescribing							
Quartile 1	38.2	-4.3	-11.4%	159.7	-25.1	-15.7%	[0.819]
(N=1,778)		(6.4)	(16.8%)		(17.5)	(10.9%)	
Quartile 2	115.7	-19.8	-17.1%	500.1	-69.7	-13.9%	[0.847]
(N=782)		(17.8)	(15.4%)		(42.4)	(8.5%)	
Quartile 3	187.3	-25.0	-13.3%	960.8	-202.6	-21.1%	[0.438]
(N=1,245)		(17.2)	(9.2%)		(50.8)	(5.3%)	
Quartile 4	521.3	-52.9	-10.2%	2,859.8	-457.3	-16.0%	[0.421]
(N=1,250)		(35.8)	(6.9%)		(104.0)	(3.6%)	

N=5,055. Notes: Table reports estimates for prescriber-level outcomes for private insurance (columns 1-3) and Medicare (columns 4-6) that were defined in the analysis plan but were not otherwise reported in the main text. Each row presents prescribing of a different quetiapine measure during the outcome period (April 21, 2015 through December 31, 2017). See appendix and analysis plan for more details. Columns 1 and 4 report the mean outcome for control prescribers. Columns 2 and 5 report the treatment effect estimate from equation (1). Columns 3 and 6 divide the treatment effect by the control mean to produce a percent effect. Column 7 reports the p-value from a test that the percent effects for private insurance and Medicare are equal. Robust standard errors in parentheses. P-values in brackets.

* N=1,895 for private insurance and N=3,512 for Medicare because this outcome is only defined for physicians with quetiapine prescribing in the outcome period.

Table A10

Additional Patient-Level Outcomes from Analysis Plan

Patient Group	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Private Insurance (N=1,980)			Medicare (N=7,384)			P-Val, %
Outcome	Control Mean	Treatment Effect	Percent Effect	Control Mean	Treatment Effect	Percent Effect	Effects Equal
Quetiapine Fills	7.3	-0.7	-9.6%	11.7	-0.8	-6.9%	[0.665]
		(0.4)	(5.5%)		(0.3)	(2.6%)	
Quetiapine Cost	794.9	-88.7	-11.2%	711.8	-1.2	-0.2%	[0.423]
		(93.5)	(11.8%)		(48.8)	(6.9%)	
Quetiapine MG	54,019.6	-5,936.7	-11.0%	68,052.3	-1,044.5	-1.5%	[0.206]
		(3,662.3)	(6.8%)		(2,225.3)	(3.3%)	
Indicator for Discontinued 2016Q4	0.76	0.00	0.1%	0.62	0.02	3.2%	[0.316]
		(0.02)	(2.5%)		(0.01)	(1.9%)	
Indicator for Dose Reduced 2016Q4	0.50	0.02	4.0%	0.50	0.03	5.8%	[0.731]
		(0.02)	(4.6%)		(0.01)	(2.6%)	
Benzodiazepine Days	162.5	0.8	0.5%	239.4	1.4	0.6%	[0.992]
		(10.3)	(6.3%)		(6.9)	(2.9%)	
Non-Benzodiazepine Insomnia Drug Days	62.8	-0.8	-1.3%	42.3	3.2	7.7%	[0.449]
		(6.0)	(9.6%)		(2.9)	(6.9%)	
Antidepressants Days	318.4	2.6	0.8%	522.6	4.8	0.9%	[0.987]
		(16.9)	(5.3%)		(11.8)	(2.3%)	
ED Visits for Substance Use Disorder	0.04	0.01	22.2%	0.03	-0.01	-19.8%	[0.369]
		(0.01)	(36.3%)		(0.01)	(29.7%)	
ED Visits for Mental Health Reasons	0.04	0.01	26.5%	0.07	-0.05	-78.3%	[0.038]
		(0.01)	(35.2%)		(0.02)	(36.2%)	
Inpatient Stays for Substance Use Disorder	0.07	0.02	33.7%	0.02	0.00	6.1%	[0.528]
		(0.03)	(34.6%)		(0.00)	(26.2%)	
Inpatient Stays for Mental Health Reasons	0.04	0.01	14.6%	0.07	0.00	2.6%	[0.727]
		(0.01)	(31.1%)		(0.01)	(15.4%)	
Enrolled December 2016*	0.49	0.03		0.65	0.01		
		p=0.264			p=0.595		
Quetiapine Days by Source of Receipt							
Baseline Prescriber	188.5	-17.4	-9.2%	296.5	-17.0	-5.7%	[0.599]
		(11.3)	(6.0%)		(8.9)	(3.0%)	
Non-Psych Prescribers (ex Baseline)	56.4	-4.5	-8.1%	120.2	-3.0	-2.5%	[0.662]
		(6.7)	(11.8%)		(6.1)	(5.1%)	
Psych Prescribers (ex Baseline)	15.7	-1.9	-12.1%	24.3	-2.1	-8.7%	[0.892]
		(3.7)	(23.5%)		(2.6)	(10.7%)	

Notes: Table reports estimates for outcomes for privately insured baseline patients (columns 1-3) and baseline patients on Medicare (columns 4-6) that were defined in the analysis plan but were not otherwise reported in the main text. See text for more details on the construction of the baseline patient cohorts. See appendix and analysis plan for more details on the outcomes. Each measure counts health care use during the outcome period (April 21, 2015 through December 31, 2017) unless otherwise stated. Columns 1 and 4 report the mean outcome for baseline patients of control prescribers. Columns 2 and 5 report the treatment effect estimate from equation (2). Columns 3 and 6 divide the treatment effect by the control mean to produce a percent effect. Column 7 reports the p-value from a test that the percent effects for the private insurance and Medicare cohorts are equal. Robust standard errors clustered at the baseline prescriber level in parentheses. P-values in brackets.

* Reports simple difference in means and p-value of test of equality of means between treatment and control, p-value of test clustered at baseline prescriber level.