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Physician Response to Prices of Other Physicians: Evidence from a Field Experiment

Scott Barkowski*†

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Abstract

Recent efforts to increase price transparency for American consumers of health care have largely failed to produce savings. Medical-field research on physician-side price transparency, however, has shown promise for savings but suffers from pervasive methodological problems. I perform a field experiment that addresses these measurement difficulties while studying an area that has received little attention: physician referrals. Working with a group of medical practices linked as an Independent Practice Association (IPA), I randomly selected primary care practices to receive a list of average costs – that is, prices – for new referrals to six ophthalmology practices that were part of the IPA's provider network. These practices handled the bulk of the IPA's ophthalmology patients and represented substitute providers. Using the IPA's administrative data on referrals, I find that during the first two months following the distribution of the price list, the treatment group primary care physicians (PCPs) increased referral share towards the least expensive ophthalmology practice by 147 percent. These referrals were allocated away from the most expensive practice and those not listed on the report. These effects were only found, however, for patients for whom the PCPs had a cost reduction incentive. The large initial effect dissipated over the following four months. For patients with a limited financial interest for the PCPs, I find little evidence of a treatment response. These contrasting results suggest the PCPs were influenced by cost reduction motives and provide more evidence of the potential for savings from physician-side price transparency.

Keywords: Physician price transparency, referrals, information
JEL categories: I11, D83

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1 Introduction

A well-known stylized fact of the American health care industry is that prices of medical services are not known by consumers and providers in the market. As the field has struggled with cost containment in recent decades, this lack of transparency has increasingly become a point of focus for members of the industry, policy makers, and researchers. Much of this focus has manifest itself in pushes for price transparency on the consumer side. Many states have pursued or passed legislation to increase such price transparency, while at the federal level multiple unpassed bills have been introduced in Congress to that end (Sinaiko and Rosenthal, 2011). Most recently, Donald Trump issued an executive order requiring hospitals to post price lists, which, after some legal challenges, went into effect at the start of 2021 (Luhby, 2019, 2021). In the private sector, employers and insurers have begun providing access for employees and plan members to price transparency tools in the hopes that these would help consumers engage in price shopping for medical services.[1] Despite high hopes, results of these efforts thus far have been discouraging. A report by the U.S. Government Accountability Office (2011) reviewing early transparency efforts noted that relevant information on prices was still difficult to find. Additionally, while some researchers have found evidence that consumers who use price transparency tools end up consuming lower cost services (Whaley et al., 2014; Desai et al., 2017), overall these tools have been found not to affect overall spending or prices because their use by consumers has been low (Tu and Lauer, 2009; Sinaiko and Rosenthal, 2016; Desai et al., 2016, 2017; Gourevitch et al., 2017).

Beyond consumers, the lack of information on prices in health care also extends to physicians. Surveys of doctors consistently suggest that they have low awareness of, and low access to, the prices for services they provide, recommend, or prescribe (Shulkin, 1988; Tierney et al., 1990; Reichert et al., 2000; Allan and Innes, 2004; Allan et al., 2007; Allan and Lexchin, 2008; Okike et al., 2014). Despite this, there has not been large scale public or private effort put into providing information on prices to physicians as there has been for consumers – an asymmetry that is surprising for two reasons. First, acting as agents on behalf of consumers, physicians heavily influence the choice of health care goods and services that are consumed. Second, there has been significant interest in physician responses to price transparency in the medical literature, and studies there have shown some promise towards achieving cost savings (Varkey et al., 2010; Goetz et al., 2015; Silvestri et al., 2016; Mummadi and Mishra, 2018). Part of the explanation for tepid interest could be the ethical tension physicians face

[1] Though price transparency is generally pursued in the hopes that it would result in lower prices, Kyle and Ridley (2007) provide one argument that transparency might not lower prices for all consumers and could have other, undesirable effects.

when they are asked to balance medical quality and cost considerations (Riggs and DeCamp, 2014; Galen, 2014), but given the potential for spending reduction, it would seem to be an idea meriting further pursuit.

In this paper, I consider the effects of price transparency for primary care physicians (PCPs) on their referrals to specialist physicians. I partnered with a group of medical practices located in a western American state – an Independent Practice Association (IPA) – to perform a field experiment where we distributed a report on prices for several ophthalmology practices to randomly selected PCP groups and measured the effect on the PCPs' referrals to those specialty practices. Measurement is based on IPA administrative data that specifically identifies when a referral is made, who sent it, and to whom it was made. I estimate effects for two different types of patients. For the first type, the PCPs have a financial incentive to refer patients to less costly specialists. For these patients, I find that the treatment group PCPs reallocated referrals towards the least expensive ophthalmology practice by 147% during the first two months after treatment, an effect that dissipated over the following four months. In contrast, I find little evidence of a response to the treatment for the second type of patients. For these, the PCPs' financial incentive to refer to cheaper specialists is largely muted because payment for their treatment by ophthalmologists is primarily on a flat-rate basis. This asymmetric result is consistent with cost reduction motivating the PCPs' responses.

The results of this study add to the body of knowledge in two related literatures. The first, pursued in the medical field, is on effects of physician-level price transparency, and includes Cummings et al. (1982), Marton et al. (1985), Tierney et al. (1990), Frazier et al. (1991), Bates et al. (1997), Hampers et al. (1999), Feldman et al. (2013), Durand et al. (2013), Horn et al. (2014), Brotman et al. (2017), Chien et al. (2017), Melendez-Rosado et al. (2017), Schiavoni et al. (2017), Schmidt et al. (2017), Sedrak et al. (2017), Riley et al. (2018), Silvestri et al. (2018), Kozak et al. (2019), and Monsen et al. (2019). Reviews are provided by Varkey et al. (2010), Goetz et al. (2015), Silvestri et al. (2016), and Mummadi and Mishra (2018). The second one, in the economics field, studies the effect of prices on physician referrals and includes only Ho and Pakes (2014).

The medical price-transparency literature has tended to find that doctors respond to prices by recommending lower cost services, but interpretation is not straightforward since these studies usually suffer from at least one of three problems. First, though researchers have often used rising health care costs to motivate price transparency investigations, they seldom discuss why price information might affect costs or whether there are relevant incentives. This leaves ambiguous the underlying process these interventions are supposed to be testing. When effects are found, it is unclear whether they could be extrapolated or further built

upon. When not, there is little understanding as to why. Second, the prices made available to physicians are almost always not for sets of services that are substitutes. Typically physicians are provided prices for a set of commonly ordered services or tests, but the most common services tend to be so frequently used because they are hard to substitute for, at least in part. Once physicians learn prices in these studies, they might not have price information for available alternatives or even *have* alternative treatments from which to choose.[2]

Third, among randomized price-transparency trials, in most cases randomization has not been applied along the appropriate dimension. It is usually implemented at the service level, and in one case at the patient level. In service-level randomization, physicians are provided prices for one set of treatment-arm services but not for a set of control-arm ones. An immediate problem is that the control services might not be good comparisons for the treatment ones, particularly if the treatment-arm is comprised of the most commonly ordered services. More subtly, though, is there may be cross-price effects across different services. If a service in the treatment arm has a substitute or complement in the control arm, any changes induced by prices for the treatment group service would also cause corresponding changes in the control group. With patient-level randomization, as in Bates et al. (1997), control-group contamination is likely since doctors could remember prices they see for treatment group patients when making care decisions for control group ones.

In this study, each of the above issues is addressed in the research design. The physician subjects have a financial incentive to reduce costs with respect to referrals that is stronger for some patients and not others, and the intervention is designed to test the response to those incentives. Additionally, the prices that the IPA provided the treated PCPs with were for a set of ophthalmology practices that provide largely similar services, giving the PCPs a set of substitute choices with different prices from which to choose. Finally, I randomized the treatment at the PCP-practice level, so that the treatment and control groups are comparable on average and there is less danger of cross-group contamination than in many previous studies.

Aside from addressing the methodological issues above, this paper's focus on referrals to specialists fills a gap in the medical price-transparency literature, which has exclusively studied effects for laboratory tests, imaging, procedures, or prescriptions. Moreover, it is referrals that also links this paper to the work of Ho and Pakes (2014), which retrospectively studied how prices of hospitals affected the allocation of women giving birth across those hospitals. Ho and Pakes estimated a negative effect for price on the likelihood a woman

[2]One type of study that usually avoids this critique is of price transparency for drugs (e.g., Frazier et al., 1991; Monsen et al., 2019). Generic and name-brand drugs provide direct substitutes with different costs and serve as a natural strategy for a price transparency intervention.

would deliver her baby at a given hospital, and that this negative effect grew in magnitude with the probability that the women's physicians' compensation was affected by hospital prices. This implied that doctors responded to prices in referring their patients to hospitals.

While Ho and Pakes (2014) does not suffer from the problems described above that affect the medical literature, it does have three weaknesses relative to price-transparency studies: they did not *directly* observe distribution of price information to physicians nor their actions in response (that is, their referrals). Both are features that are characteristic of the medical-field studies. Further, unlike some of the medical literature, Ho and Pakes did not randomly assign prices across doctors. In advancing their work, I address each of these issues: working with the IPA I observe the distribution of price information to randomly assigned PCPs and use the IPA's administrative data to directly observe their referrals in response. Observing the distribution of prices is a particularly important improvement in the face of the physician surveys I noted above that report doctors being unaware of prices. Their study does not provide a clear answer as to how physicians would obtain a detailed understanding of hospital prices at the patient insurance-plan level. Nevertheless, despite these differences, I report results that are quite consistent with theirs. The combined findings of both studies, therefore, provide strong evidence that physicians will respond to prices in making referrals when they know them and incentives align for them to do so.

I organize the rest of this paper as follows. In the next section I present relevant background and the context of the experiment. In Section 3 I describe the experiment procedures and the treatment, and then discuss econometric methods and my referrals data in Section 4. I present results and then conclude in Sections 5 and 6, respectively.

2 Background

The association of medical practices as an IPA happens for several reasons, but for this study the most important is that it creates a network of medical resources that can provide HMO services. The associated physicians can then market their network to insurance companies that offer HMO health insurance plans, but do not have vertically integrated medical facilities.[3] The IPA contracts with such insurance companies, whose customers that have HMO plans can choose the IPA to be their provider.[4] The IPA's key selling point to these customers

[3]Other important reasons for the existence of the IPA include the facilitation of contracting with health insurance companies, which can be time and resource consuming for individual practices, and the improvement of those practices' bargaining positions within the contracting process, helping to increase negotiated rates they receive.

[4]The IPA does have a small population of patients that do not have HMO insurance (and who are mainly covered under Point-of-Service plans), but since more than 91% of the IPA's claims are generated by patients with HMO coverage, I focus on those patients, and all analyses herein are limited to those with HMO plans.

is its network of services, which patients know will be covered by their health plans with relatively little out-of-pocket cost. The IPA has been successful in pursuing this strategy, and at the time this experiment was taking place, it had attracted roughly eighty-thousand patient members.

To manage these patients, the IPA had relationships with approximately 150 PCPs and 350 specialists. Physician associates are not employees of the IPA (they are employees of their respective medical practices), but they are contracted with the IPA to provide services to the patients of the IPA. Informally speaking, the IPA serves as a middleman, receiving payment from patient insurance companies for providing health care services to the patients, then turning around and paying the physicians, who are the ones actually providing the medical services.

As a provider of HMO services, the IPA assigns each patient to a PCP who is responsible for the patient's overall health and who manages the care the patient receives through the IPA. The PCPs' roles as care managers are key to the financial success of the IPA, since they potentially keep costs down by limiting the use of unnecessary services. Most importantly for the purposes of this project, this role includes managing the services of specialist physicians (including those of ophthalmologists) which are almost always only covered under patient insurance when patients are referred by their PCP.[5] The PCPs have a direct financial incentive to perform their role as gatekeepers to costly services, as bonuses paid to the PCPs are based in part on the financial results of the IPA. So if PCPs reduce the cost of their patients' care by, for example, referring to less expensive specialists, then they could see larger bonuses.

While it is clear that the PCPs indeed had a financial interest in helping to keep costs down, it is less clear how strong that interest is, and what their awareness of it was. Since the IPA was associated with many PCPs, the relative impact of any single PCP's actions on costs are certain to be small relative to the overall costs of the IPA, and so an individual who works to save money for the IPA may go unrewarded if the rest of the group's actions end up negating those savings. Moreover, IPA management believed that PCP awareness of the potential for cost savings to affect bonuses was not high, since bonuses were also affected by several types of care quality measures that were the primary focus of the PCPs' attention. On the other hand, in recent years, the IPA had been making efforts to communicate to the PCPs the importance of being mindful of costs. For example, in 2011, the IPA had provided the PCPs a list of the per patient average costs for gastroenterologists, and a report of each PCP's own patient costs. This report of PCP costs included a breakdown by specialty,

[5]Referrals from one specialist to another were possible, but the IPA is designed so that access to the specialist network in the first place goes through the PCP.

making clear what share each contributed. Moreover, in July 2013, the IPA provided all PCPs a report on PCP costs that listed each physician's name and per-patient cost, rather than ID numbers that kept true identities private, hence making each PCP's cost known to all other PCPs. Thus, while the financial incentive's salience is not clear, it is certain that the PCPs were being encouraged by the IPA to be mindful of costs.

Physicians associated with the IPA receive compensation through two different payment systems: capitation and fee-for-service (FFS). Under the capitation system, physicians receive a fixed payment per-patient, per-month that covers a set of agreed-upon services. For example, for a PCP, standard office visits are capitated, meaning they are not reimbursed by the IPA as they occur, but are included under the regular capitation payment the PCP practice receives. This is true for a number of common services provided by PCPs. More generally, the extent of services which are paid via capitation vary by specialty and patient insurance. For services that are not capitated, physicians are paid FFS, meaning on a per-service basis. The FFS payments, therefore, represent true marginal costs for these services for the IPA.

Both PCPs and specialists can have services that are paid on a capitated basis, but within this paper, except for the paragraph above, any time capitation is discussed, it is in reference to services provided by ophthalmologists. That is, any time I reference services being capitated, or physicians being paid on a capitated basis, I am referring to payments by the IPA to ophthalmology practices – not PCPs.

The IPA categorizes its patients into two broad categories for operational reasons. About three-quarters are standard, non-Medicare patients, which I call "HMO patients", or "HMOs", while the remaining quarter is comprised of Medicare Advantage patients, which I refer to as "SrHMO patients", or "SrHMOs".[6] Despite that HMO patients outnumber SrHMO ones by approximately three-to-one, SrHMO patients are responsible for more than 45% of all IPA claims. In ophthalmology, the distinction between these two types is particularly important: HMO patients are all paid on a FFS basis, but for SrHMOs, about three-quarters of services are capitated.[7] For example, when an HMO patient is referred to ophthalmology, the IPA pays for every service performed. In contrast, for SrHMO patients, the IPA makes a monthly flat payment to the ophthalmologist to cover the cost of a number of common services. If the ophthalmologist increases the intensity of treatment by increasing the use of capitated

[6]Medicare Advantage is a special program within Medicare that allows members to join HMOs that provide coverage for a broader array of services, but with less freedom of choice, than standard Medicare.

[7]The claims that are capitated also tend to be relatively expensive services. In 2013, the average cost of each claim for capitated services was more than twice that of FFS claims, and capitated claims represented about 88% of the total value of claims (where the Medicare "allowed amount" is taken as the cost of capitated services).

services, then the IPA does not incur additional costs. Thus for ophthalmology, increases in treatment intensity have more impact on IPA costs when it happens for HMO patients. For SrHMOs, the IPA's exposure is limited due to the capitation arrangement.

Under the assumption that financial incentives are indeed salient to the PCPs, the above differences in capitation between patient types mean the incentives vary along the same dimension. Since treatment for all HMO patients directly affects IPA costs (and the PCPs' potential bonus pool), PCPs can potentially affect their bonuses by their choice of ophthalmologists for referrals. But for SrHMO patients, the high level of capitation means PCP bonuses would be much less likely to be affected by specialist choice. So, if PCPs respond to the price information they receive in this study in a way consistent with their financial incentives, they would send more of their HMO patients to cheaper ophthalmologists than if they did not have the prices.[8] However, for SrHMO patients, since there is no financial incentive (or a much smaller one), they are less likely to respond to the prices (or at least it would be a smaller effect). Thus, to the extent effects induced by this study are driven by financial incentives on the part of the physicians, the effect should be seen for HMO patient referrals but not (or should be much smaller) for SrHMO patients.

Despite the clear predictions implied by the PCPs' financial incentives, prices are only one of several considerations for PCPs in the referral process. First, a PCP must decide to make a referral and to which specialty to make it. Surveys of physicians have indicated that, at this stage, referrals are most often made for advice on diagnosis or treatment, they depend on patient symptoms or conditions, and they are more likely for uncommon medical conditions (Donohoe et al., 1999; Forrest et al., 1999; Forrest and Reid, 2001; Forrest et al., 2002, 2006). Second, having decided to make a referral, when choosing the particular specialist, PCPs weigh their previous experiences with specialists, appointment availability, specialist communication quality and relevant skills, previous patient experiences, and PCPs' own pre-dispositions towards particular specialists (Forrest et al., 2002; Starfield et al., 2002; Anthony, 2003; Kinchen et al., 2004; Barnett et al., 2012; Hackl et al., 2015). Thus, prices are only one of many possible factors that could influence referral choices, so my ability to identify an effect in this study depends on whether the financial incentives play an important enough role to avoid being lost among all these other factors.

[8] If PCPs fully optimize along financial grounds, then they would send all HMO patient referrals to the cheapest ophthalmology provider.

3 Experiment Description

The subjects of the experiment were all PCPs associated with the IPA and practiced in either the family practice or internal medicine specialties in an outpatient (ambulatory) setting.[9] Subjects assigned to the treatment group all received an informational treatment: a letter containing historical average costs of six ophthalmology practices affiliated with the IPA, the "treatment," "prices," or "cost report," which is described below. The control group subjects did not receive anything. In order to maximize the experiment sample size, all of the IPA's PCPs who were active at the time of the treatment distribution were included if they satisfied minimal criteria: they must have had at least ten claims during each calendar month from August 2013 through January 2014, and made at least one patient referral to ophthalmology during that period.[10] In the end, a total of 93 PCPs – 35 internists and 58 family practitioners – were included in the experiment. These physicians were typically organized into group practices, which I measure using the office addresses of the PCPs. Thus, a PCP practice in the context of this project is either one PCP or multiple ones that have the same office address listed with the IPA. In total, there were 55 included practices, with 24 of them being internal medicine, 30 family practice, and one mixed specialty group.

To account for the organization of PCPs into practices, assignment into treatment and control groups took place at the practice level, so that either all PCPs in a practice were assigned to the treatment group, or none were. This feature of the experimental procedure was intended to minimize control group contamination via discussion between PCPs, since if the subjects were going to discuss the information received in the treatment, it seemed likely that it would take place within the practice. To the extent, however, that discussion outside the practice indeed took place and resulted in control group contamination, the results of the experiment may understate the effects of the treatment.

I focused on ophthalmology in this project for three main reasons. First, ophthalmologists receive a large number of referrals from the IPA's PCPs. During the twelve month period from March 2012 through February 2013, the ophthalmology specialty received 3,467 referrals from Family Practitioners and 2,461 from Internists. For both types of PCPs, these figures made ophthalmology the fourth most often referred to specialty in the IPA. Second, before this experiment, the IPA PCPs had never previously received any information about ophthalmologist costs, allowing for the measurement of the effect of completely new infor-

[9]This project was reviewed by the staffs of UCI's Office of Research and Clemson's Office of Research Compliance and both confirmed that this study does not qualify as human subjects research since only de-identified data (which could be re-identified by the IPA) was available to the author.

[10]Three PCPs who were social contacts of me were also excluded. August 2013 to January 2014 was used since it was the most current data available when the distribution list was finalized.

mation. Third, as the medical specialty of physicians who treat and study diseases and functions of the eye, ophthalmology is a particularly highly specialized area of medicine, and PCPs typically cannot substitute their own services, or those of other specialists, for those of ophthalmologists. Ideally, the introduction of cost information to the PCPs would not affect the likelihood of a referral to the specialty of interest. That is, for a given patient, the probability he will be referred to ophthalmology will be the same both before and after the introduction of cost information. Since ophthalmology is so specialized, it is likely that the only margin of response available to PCPs would be to which ophthalmologist the referral is made, *not* whether or not to refer to ophthalmology at all.

During the experiment, the IPA collected data on PCP referrals as part of its normal operations. This data was generated by the PCPs' activities of seeing and treating patients as part of their usual medical practices in their regular offices. The IPA regularly collects all of this data, and all of the physicians are aware of this data collection. Moreover, none of the physicians were made aware that the distribution of the price information was related to an experiment, thereby minimizing any possible influence of the "Hawthorne effect."

3.1 Experimental Treatment: the Cost Report

The experimental treatment for this study was a report listing two numbers for each of six busy ophthalmology practices. The numbers were risk-adjusted, 180-day cost averages for newly referred patients to ophthalmology for both HMO and SrHMO patients. Together with the cost report, the treatment group PCPs also received a cover letter from the CEO of the IPA, briefly explaining the reason for receiving the report and a description of how the costs were calculated. Anonymous facsimiles of the report and cover letter are attached as Figures 1 and 2.

The cover letter was included to explain to the PCPs what the report contained and why, but its contents were crafted with several additional goals in mind. First, it was designed to convey to the PCPs that their receipt of the cost report was not out-of-the-ordinary. Hence, its first sentence stated that the report was "requested by" the PCPs of the IPA and that it was part of a "continuing" effort to "share information on specialty costs." Second, it sought to support the credibility of the cost figures. To create a sense of authority, the cover letter bore the signature of the organization CEO and was printed on IPA letterhead.[11] To emphasize accuracy, it mentioned that the figures were based on claims from recent patient encounters. To underscore that the figures were comparable across ophthalmologists, the

[11]The letterhead included a large IPA logo at the top left, and the IPA's mailing address and contact numbers running along the bottom of the page. These features of the letter, along with the CEO's signature and name, are omitted from the included copy to keep the IPA's identity private.

letter briefly described how the figures were risk-adjusted. The process was explained as having used only patient conditions that were "common across practices" and that the figures reflected "IPA-wide prevalence instead of individual practice level prevalence." The emphasis on comparability was important since the PCPs were aware of the potential for underlying patient populations to affect costs.

Third, to limit the chance that the cost report could send an unintended signal about the quality of the included ophthalmologists, the letter implied they were all of comparable quality. It mentions that the included practices were those who served many IPA patients and did so with good patient satisfaction scores. Lastly, the first sentence mentioning the continuing efforts on costs also serves as a reminder that the IPA had been emphasizing cost control and had asked the PCPs to be mindful of the cost implications of their actions. This was intended to help induce action in response to the treatment.

The actual cost report was comprised of two parts: the cost table and a set of footnotes with additional information.[12] On the left side of the table were the names of six ophthalmology practices. Below each practice name were the names of the IPA network ophthalmologists associated with each of those practices in smaller, italicized print. For the facsimile included here, these names (which are confidential) are replaced by identification numbers that reflect the ranking of the practices in terms of the cost figures contained on the report. The first digit of the ID indicates the practice's ranking in cost, from least (1) to most (6), for HMO patients. The second digit is a placeholder – a zero – for all practices. The third and last digit indicates the ranking in terms of SrHMO patients. So practice 101 is the least expensive for both types of patients, but practice 603 is the most costly in terms of HMOs and the third least expensive for SrHMOs.

In presenting cost figures, the table was designed to be simple so as to efficiently convey the information. This is why only two costs, one for each type of patient, were included for each practice. The costs were intended to estimate the average charges to the IPA over a 180-day period for a given patient generated by the ophthalmologist receiving a referral for a new ophthalmologic condition.[13] In other words, they represented the marginal cost – or, price – to the IPA of a new referral.[14] Once the costs were produced, the list was sorted in ascending fashion by HMO patient cost. The SrHMO cost did not play a role in this sorting, so the ordering of the practices on the report does not reflect any information contained in

[12]Unlike the cover letter, the cost report did not appear on IPA letterhead, but it did have a large IPA logo on the top.

[13]The 180-day period began the date of the first claim made by the ophthalmologist. A "new" condition meant that the set of visits used in the calculation had to be preceeded by a period of at least 180 days in which no ophthalmology claims were observed for the patient.

[14]HMO and SrHMO figures were calculated completely independently of each other.

the SrHMO figures.

As shown in the table, the variation in costs between the ophthalmologists is quite high. For HMO patients, practice 101's $147 figure is less than half of practice 603's $333 number. There is less variation for SrHMO patients, but the cost of the most expensive practice, 406, is more than 25% higher than the least expensive, practice 101. Variation in ophthalmologist cost does not come from the per-procedure price varying across physicians. For a given patient, these are the same across IPA ophthalmologists. Moreover, given the risk adjustment, the influence of differences in patient populations is likely small. Instead, the variation in costs primarily comes from differences in intensity-of-treatment. For example, a more costly ophthalmologist might take patients more quickly to surgery than less expensive ones. Thus, one way to think about the risk-adjusted cost estimates is to think of them as weighted measures of treatment intensity where the procedure price is the weight. In fact, for the SrHMO costs, this interpretation is particularly appropriate since the procedure price is never actually paid on capitated services – it merely represents a measure of what the procedure *would cost* if it were paid on a per-procedure basis.

The footnotes under the table provide brief descriptions of the calculation, including reiterating that the numbers were risk-adjusted and that they were based on new ophthalmologic conditions, and also mentioning the criteria for a practice to be included. These notes served to buttress the credibility of the figures provided by providing additional details suggesting the costs were calculated carefully and reasonably. Additionally, the first footnote served as an explicit reminder for the PCPs of the difference between HMO and SrHMO patients by noting that SrHMO services are highly capitated in ophthalmology.

Distribution of the experimental treatment took place on May 5th, 2014, when the IPA mailed the ophthalmologist cost report and cover letter to the treatment group subjects via the U.S. Postal Service. Regular mail was used to distribute the treatment for three main reasons. First, in its normal operations, IPA management often used mail for communication with its physicians – especially for important information. Sending the treatment in this manner, therefore, made it likely to be read by the PCPs and also unlikely to seem unusual to them or to suggest outside involvement. Second, I wanted to implement a real-world type method that would be feasible for the IPA or other groups to replicate outside of an experimental setting. Lastly, this method put little burden on IPA staff. This approach, though, has the important weakness that it is not possible to observe which PCPs actually received and opened the cost report letter.

4 Econometric Evaluation

The basis of evaluation for all the following analyses is the intention-to-treat approach since the cost reports were delivered by mail and I did not observe their receipt, nor did I take any steps to survey the PCPs to confirm their knowledge of the cost information.[15] The major disadvantage to this is that my analyses may understate the true treatment effects if some treated PCPs did not receive, understand, or retain, the cost information.

To measure the impact of the treatment on referrals to ophthalmology, I use data from the six-month period beginning May 16th and ending November 15th, 2014 (the "post-period"). For regressions that adjust for pre-treatment differences between the groups, I use the six complete months from November 2013 through April 2014 (the "pre-period"). Referrals from the fifteen days between these periods (May 1st through 15th, 2014) are excluded so that the pre-period could be based on six complete calendar months and to allow time for the reports to be delivered, opened, and read by the PCPs.

My primary outcome of interest is the share of a PCP practice's ophthalmology referrals that an ophthalmology practice receives. Let $p \in \{1, 2, ..., P\}$ and $s \in \{1, 2, ..., S\}$ index the PCP and specialist practices, respectively, and $t \in \{1, ..., 6\}$ index bimonthly time periods of which the first three are in the before the treatment and the last three are after.[16] I notate the referral share using $\theta_{pst} \equiv OPHREFS_{pst}/TOTOPHREFS_{pt}$, where $OPHREFS_{pst}$ is the number of ophthalmology referrals between p and s during period t, and $TOTOPHREFS_{pt}$ is the total ophthalmology referrals made by p during t.[17] The use of referral share makes the dependent variable more comparable between practices that may have different numbers of physicians and patients.

Referral share is measured at the PCP-practice level, the natural level of analysis given the study's cluster randomization design, which addresses, in a conservative fashion, the possibility of within practice correlation. Additionally, the alternative approach of measurement at the PCP-level is more prone to missing data because some PCPs may not have patients needing referral to ophthalmology during a given time period (making a referral

[15] One reason I did not administer a survey was that it did not seem feasible in the context of my relationship with the IPA. Another was that I wanted to keep the intervention as close to normal business activity as possible, and pursuing a survey or other intervention would have sacrificed (at least to some extent) that feature of this project.

[16] Precisely, the pre-period is divided into three periods, $t = 1$ for November and December 2013, $t = 2$ for January and February 2014, and $t = 3$ for March and April 2014. During the post-period, $t = 4$ for May 16th to July 15th, $t = 5$ for July 16th to September 15th, and $t = 6$ for September 16th to November 15th (all in 2014).

[17] Bimonthly periods were chosen to allow effects to vary over time but maintain a large enough sample that there were few PCP-by-period observations with no ophthalmology referrals, which would cause this calculation of θ to be undefined.

share calculation undefined). Practices, which often include more than one physician and typically have more patients, are naturally more likely to have at least one ophthalmology referral, thereby reducing the frequency of missing data.

My first method of evaluating the treatment effect is comparison of post-treatment mean referral share across groups by ophthalmology practice, focusing on the practices that were listed on the cost report.[18] That is, I take the difference of sample averages across the treatment and control groups separately for each period and each ophthalmology practice on the cost report.[19] Comparison of sample averages is straightforward to interpret and valid given the randomized assignment and the fact that, as I show below, the pre-treatment measures do not show any statistically significant differences between the groups.

In a second estimation method, I use regression to address two issues not handled by the comparison of means. The first arises because random assignment was done within strata to increase the likelihood of balanced groups. The assignment of practices to treatment status is, therefore, not independent of the randomization strata, so my confidence intervals may be incorrect. The strata controls, however, allow me to recover independent assignment *conditional* on the strata. The second issue is the possibility of pre-treatment differences in referral share between treatment and control groups. Though assignment was random, the relatively small number of PCP-practice subjects means that differences between the groups may not average out completely in practice. Further, the estimators would not be very precise. Non-significant pre-period differences, therefore, could still have meaningful implications on estimates if one controls for them. Thus, I take a conservative approach and include controls for pre-treatment differences for each ophthalmology practice in my regression model in addition to the strata controls.

To implement the second estimation method, I pool the data for all ophthalmologists and employ an econometric model that combines six separate event-study models – one for each ophthalmology practice. It allows for separate treatment effects for each practice that all vary over the three periods following treatment. It also includes controls for randomization strata, pre-period differences for each ophthalmology group, and time. It takes the following

[18]For a given PCP, referral shares must sum to one across all possible ophthalmologists, so the *change* in the distribution of share across all possible ophthalmology practices in response to the treatment must sum to zero. By focusing only on the practices listed on the cost report, there is no restriction on the sum of the estimators.

[19]These sample averages and standard errors do not adjust for the randomization strata.

form:

$$\theta_{pst} = \sum_{j=1}^{6}\left(\sum_{k=4}^{6}\left(\beta_{1jk} I_p O_{\{s=j\}} T_{\{t=k\}} + \beta_{2jk} O_{\{s=j\}} T_{\{t=k\}}\right) + \beta_{3j} I_p O_{\{s=j\}} + \beta_{4j} O_{\{s=j\}}\right) + \sum_{m=1}^{23} \alpha_m R_{\{b=m\}} + \sum_{h=2}^{6} \gamma_h T_{\{t=h\}} + u_{pst}. \tag{4.1}$$

Here the indicator I_p identifies treatment group members and the set of indicator dummies, $\{O_{\{s=j\}} | 1 \leq j \leq 6\}$, separately identifies the six ophthalmology practices listed on the cost report treatment. Similarly, $\{T_{\{t=h\}} | 1 \leq h \leq 6\}$ and $\{R_{\{b=m\}} | 1 \leq m \leq 23\}$ are sets of indicators for the six time periods and the strata – or blocks – used for randomization, respectively, with $b \in \{1, ..., 23\}$ indexing the blocks. The unobserved error term is given by u_{pst}.[20]

This model includes three coefficients of interest, indicated by the β_{1jk} parameters, for each ophthalmology practice: one for each two-month span following the treatment distribution (eighteen in total). Given the controls for pre-period differences, each β_{1jk} has a difference-in-differences-style interpretation:

$$\begin{aligned}\beta_{1jk} = E[\theta_{pst} | I_p = 1, s = j, t > 3, t = k] - E[\theta_{pst} | I_p = 0, s = j, t > 3, t = k] \\ - \left(E[\theta_{pst} | I_p = 1, s = j, t \leq 3] - E[\theta_{pst} | I_p = 0, s = j, t \leq 3]\right).\end{aligned} \tag{4.2}$$

All model parameters are estimated using ordinary least squares via Stata/MP 13.1 software for Windows StataCorp (2013), and all standard errors account for clustering at the PCP practice level. This is necessary for two reasons. First, it addresses potential serial correlation in PCP referral practices, which could arise if PCPs base referral decisions on habit or past experiences with specialists. Second, since a PCP-practice's referral shares across all ophthalmologists in the network must sum to one, when an ophthalmologist's share increases, another's must decrease. Since the ophthalmologists on the cost report accounted for the bulk of the referrals, it is therefore likely this correlation will be reflected among them.

In addition to my primary analysis, I also perform several supplemental analyses. Considering the question of capacity constraints, I focus on ophthalmology practice 101, the least expensive practice for HMOs, and the rate at which their referrals end up seeing a specialist there within 45 days. Additionally, I examine whether PCPs responded to the treatment by adjusting their overall referrals sent to any ophthalmology practice and whether periods

[20] In implementation, several dummy variables listed in equation (4.1) are dropped to avoid the dummy variable trap and allow estimation: three from the post period control group (one for each period, all coming from one opthalmology practice), one from the pre-period control group (only one since there are not separate parameters for each period in the pre-period), and one time dummy.

without any referral at all to ophthalmology became more common. To investigate the extent to which my estimates are driven by particular sub-groups, I then provide treatment effect estimates by gender and age groups, and for the three most common referral diagnoses. Finally, I also examine the effects of the treatment from an alternative perspective by estimating a simplified version of equation (4.1) on referrals sent by ophthalmology practices that were not included on the cost report (where all non-report practices are grouped together). I include gender and age breakdowns in this analysis, as well. Additional details are provided when I discuss the results of these analyses, below.

4.1 Referrals Data

The IPA operates on a system in which the PCP serves as a gatekeeper for access to specialists. Each patient is, at all times, assigned to a PCP who decides when specialists are needed. When such a decision is made, the PCP submits the referral to the IPA for approval. This allows the IPA to confirm that the service will be covered by the patient's insurance, thereby keeping costs down. This structure provides several advantages for the study of referrals. First, the approval process provides a direct manner of tracking physician referrals, implying less error in referral measurement than if they had to be inferred from claims data or other methods. Second, the approval process also implies a strong financial incentive for referrals to be reported to the IPA, since services would not be covered without approval. This improves the representativeness of the referrals observed in the data. Third, the gatekeeper aspect of the IPA increases the frequency that PCPs make referral decisions, since patients have a financial incentive to obtain referrals through their PCPs instead of self-referring. These advantages aside, though, some PCP referral decisions are not observable using the IPA data. These include referrals that are not approved because they are not covered by patient insurance, and those that are never sent for approval through the IPA system. One possible reason a referral might not be sent for approval is that the PCP or patient feels certain that the service would not be covered, and so the referral is made informally.

To implement the econometric strategy discussed above, I calculate referral shares, θ using all of the IPA's submitted and approved referrals for patients 18 years and older. Since the cost report listed costs for HMO and SrHMO patients separately, I calculate referral shares for both groups. Specifically, for HMOs, θ is calculated as the number of HMO referrals to a given specialist divided by the total number of HMO referrals, and SrHMO shares are calculated analogously. Mean differences and regression models are then estimated separately for both types of patients. An analogous approach of calculating θ by limiting the referrals to particular types of patients is used for my results by age, gender, and diagnosis

code.

Table 1 presents a breakdown of the number of referrals received by ophthalmology practice during the six-month pre-period (and includes the average costs reported to PCPs in the cost report for reference). The 93 subject PCPs made 3,147 referrals to the ophthalmology specialty, with 86.5 percent of those being directed to the practices listed on the cost report. Among these, there was fairly wide variation in the number of referrals received, ranging from 7.7 to 20.5 percent. On average, each cost report practice received about 14 percent and had similar referral shares for both types of patients, except for 603, which had a much larger share for HMOs than SrHMOs.

5 Experiment Results

5.1 Randomization

Stratified randomization was used to assign PCP practices to treatment or control groups. Practices were stratified on the basis of five pre-treatment dummy variables: whether the practice was an Internal Medicine practice; whether the per-physician count of SrHMO referrals to Ophthalmology exceeded the pre-period median of all the PCP practices, and the same for HMO referrals; and whether the per-physician count of SrHMO claims exceeded the pre-period median of all the PCP practices, and the same for HMO claims.[21] These practice-level measures were created using all IPA claims and referrals data for the six-month period from August 2013 through January 2014. This time frame does not coincide with the six-month pre-period of November 2013 to April 2014 because January was the latest month of data available to me before the distribution of the treatment.[22] The strata were created by the full interaction of the five binary control variables, of which twenty-three were non-empty.

The underlying assumption of the analyses herein is that the randomization process was successful in producing a control group that can credibility serve to estimate the counterfactual outcome of what would have happened had the treatment group not received the cost report. This assumption cannot be verified with data, but suggestive evidence can be offered

[21] Stratification by pre-period referral shares to each ophthalmology practice would have been an ideal strategy, but since this would have meant six stratification variables, each with (at least) two values, this approach did not seem feasible with only 55 subjects. Using the minimum of two values (i.e. low and high referrals) for each variable, then there would have been 36 mutually exclusive groups to split the 55 practices between, implying fewer than two subjects per strata on average.

[22] Re-randomization was not performed. The seed for the random number generator used to produce the assignment was set to 20140430, the date the randomization was implemented. The function runiform() in Stata/SE 12.1 for Windows was used for random number generation (StataCorp, 2011).

by comparison of the groups during the pre-period. To this end, Table 2 presents practice-level, pre-period sample averages by treatment status, which are measured for the whole six-month period. The first five rows contain the averages for the stratification variables, while the following nine contain referral counts, and the rest provide averages of referral shares. None of the observables reported had means that were statistically significantly different across the two groups at conventional significance levels. Importantly, this includes the bottom fourteen rows, which contain averages for the main outcome variable of my study – θ, the ophthalmology group referral share – for each specialist practice. In the next subsection I also present pre-period, practice-level averages for θ over time, which are all also not statistically significant. Additionally, Table 3 presents the pre-period distribution of PCP practice size (in terms of the number of physicians), which is similar across groups. A chi-squared test for distribution differences failed to reject at conventional significance levels.

5.2 Comparison of Sample Means

The simplest evaluation of the treatment is the post-period difference between groups, which can be justified by the lack of statistically significant differences in the pre-period. Figure 3 presents plots of the differences in average referral share between the treatment and control groups by ophthalmology practice for each two-month period, both before and after treatment. These plots allow for evaluation of the post-treatment effects and the pre-existing outcome differences in a more detailed manner than in Table 2. Results for practices are presented in the same order as they appeared on the cost report: from least expensive for HMO patients (top-left) to most costly (bottom-right) moving across the top row and down to the second row from left to right. Within each plot, the orange dividing lines divide the pre- and post-periods, and 95% confidence intervals are indicated by purple lines. The underlying numeric estimates for these plots are presented in Appendix Tables 1 and 2.

The top panel Figure 3 contains results for HMO patients. We see that for these referrals the least expensive group, practice 101, received a large spike in referrals coming from the treatment group, relative to the control group, immediately after the distribution of the cost report. This increase in referral share then dissipates partially over the next two periods. The period four difference between groups of 0.1485 is statistically significant at the 5%-level (p-value = 0.012) and is very large in magnitude, suggesting an effect of more than 310% when compared to the 0.04775 average share of referrals sent to practice 101 by the control group. Periods five and six are not statistically significant at conventional levels, but their point estimates are still relatively large at 0.08155 (143% of the 0.05706 value of the control group) and 0.05873 (88% of the control group's 0.06666), respectively.

Practice 204 also shows a large positive difference during period four at 0.06287, which is statistically significant at the 10%-level (p-value = 0.088) and is almost four times the 0.01595 value for the control group in that period. For the other practices during the fourth time interval, practice 302 has a smaller, but positive, estimate, while the other practices have negative estimates that increase in magnitude with their ranking in terms of cost. Thus, the relationship between cost and referral share difference during period four is strictly negative. However, other than the cases already mentioned (practices 101 and 204 in period four), only one other estimate for the HMO patients is statistically significant at a conventional level: -0.08572 for practice 302 in period six (10%-level, p-value = 0.089). Aside from the negative relationship with price in period four, it is also worth noticing that practice 603's pattern of differences is approximately the mirror image of 101's. Even though 603's differences are not statistically significant, the pattern suggests there could be some re-allocation from practice 603 (the most expensive) to 101 (the least).

The bottom panel of Figure 3 presents the same, bi-monthly mean differences for SrHMO patients. Here we do not see any drastic spikes during the post-period for practice 101, as we do for the HMOs. For the fourth period, all the estimated differences are close to zero, and this is also true for several other periods. Only one estimate is statistically significant at a conventional significance level: practice 505 has a referral-share difference of -0.1051 (10%-level, p-value = 0.086) during period six. The most notable feature of the results for the SrHMOs is that the estimates that aren't close to zero tend to be negative, particularly in periods five and six for the bottom three practices, 406, 505, and 603. That said, the differences do not align with the practice ranking in terms of costs or order on the cost report. Thus, unlike the case for the HMO patients, there does not seem to be a pattern consistent with a price response for SrHMO patients.

Overall we see that for HMOs, practice 101, the least expensive in terms of HMO patients on the cost report, received a disproportionately large shares of referrals from the treatment group immediately after the cost report was distributed. More generally, during the first two months of the post-period, the referral share difference was negatively related to ophthalmology practice price. Over the following four months, however, these patterns dissipated. Referral share differences for SrHMOs, on the other hand, seemingly had no correlation with the information on the cost report.

At this point, it is worth recalling the primary differences between the SrHMO and HMO patients: SrHMOs all have Medicare Advantage insurance, and so ophthalmology services for them are largely capitated. This implies that intensity of treatment does not affect the IPA costs nearly as much as it does for HMOs. Knowing this, one way to interpret the combined results for both patient types is that the PCPs understand the difference in financial impact

between HMO and SrHMO referrals, and incorporate that knowledge into their decisions. For patients where the intensity of medical treatment affects costs, they respond by shifting patients towards the practices they believe are more cost effective, but for patients where intensity does not have an impact, they seemingly make their decisions without concern for the relative cost of the specialist. One caveat to this, however, is the fact that, as Medicare members, SrHMO patients are also older on average than HMO ones, so age could be an alternative explanation for the differences between HMO and SrHMO referrals. Later in my analysis I explore this possibility more.

One of the interesting features of the results so far is the dissipation over time of the estimates for practice 101. There are (at least) three possible explanations. The first is that since the treatment is informational, over time the information spreads from the treatment group PCPs to those in the control group, and the difference is reduced because both groups' referral shares reflect the treatment effect. The second is that the treatment effect itself fades over time, possibly due to a capacity constraint issue. If PCP patients have trouble getting appointments with ophthalmologists after the initial surge of referrals in the post-period, PCPs may re-allocate again to account for appointment availability. The third explanation is also that the effect itself fades, but in this case due to the salience of the prices in the minds of the PCPs dissipating over time.

Based on the sample averages reported for the groups separately in Appendix Table 1, the first explanation above seems to be the least likely. After the distribution of the prices, the control group referral shares stayed relatively stable over time, while the treatment group shares spiked in period four, then fell over the next two periods. One would have expected the control group to increase like the treatment group did if contamination were the source of the fading estimates.

To investigate the possibility of a capacity constraint issue, in Figure 6 I plot the 45-day referral-follow-up rate for practice 101 by referral date and patient type.[23] That is, the share of referrals that were followed up by visits (as indicated by payment claims) within 45 days with the ophthalmology practices to which the patients were referred.[24] Here I combine referrals from the treatment and control groups since a capacity constraint would affect all patients, not only those from treatment group PCPs. The figure shows that, for HMOs, follow-up rates were below the pre-period average in each post-period for HMO patients

[23]The 45-day follow-up period was chosen to maximize the available claims data, which extended 45 days past the end of my six-month post-period.

[24]Payment claims do not identify particular referrals as being the sources from which the claims were generated, so I match referrals to claims using the patient ID (the IPA's identifier for a patient), the plan type (HMO or SrHMO), and the ophthalmology practice. A 45-day follow up is counted if I observe a claim occurring on or after the referral date but within 45 days.

(and also below the lowest of the pre-treatment periods). While this is suggestive, the same issue is not reflected in the results for SrHMO patients,[25] and the pre-period average follow-up rate lies within the 95% confidence intervals of all the post-period rate estimates for both HMOs and SrHMOs. Thus, the evidence for a capacity constraint is not strong.

Regarding the third possibility that the price information lost salience in the minds of the PCPs, given that (as noted above) I did not administer a survey to the PCPs, I am unable to formally evaluate this hypothesis. I can only note that such an explanation would be consistent with the findings of Tierney et al. (1990), where the effects of a price-transparency intervention faded after the treatment period ended and, as shown via survey, physicians were as inaccurate in their knowledge of service prices after the end of the treatment period as they were before. Additionally, a fading-memory explanation is also anecdotally consistent with my experience working with the IPA, as it was clear that the PCPs constantly dealt with a large amount of information and paperwork. Future price-transparency research that includes reminders or attempts to measure recollection could be helpful.

The results so far have suggested a sizable effect for the cost report on the referral share of practice 101, but as I noted above in Section 4, comparison of the sample means does not address the stratification in the randomization process nor pre-existing differences between the groups left after randomization. As Figure 3 shows, there are no statistically-significant pre-period differences for any period or patient type, and, in most cases, the differences in the pre-period are relatively small. Still, some of the non-significant differences have magnitudes that are not trivial. For example, during period one, practice 406's average share of the control group's HMO referrals was 0.284, which is 72% larger than its 0.165 share of the treatment group's referrals. This suggests a role for the pre-period controls in my regression analysis to ensure the robustness of the simple mean difference results.

5.3 Regression Model Estimates

Figure 5 presents estimates for the post-period differences – the β_{1jk} coefficients of equation (4.1) – with numeric values presented in Appendix Table 3.[26] It shows that the addition of controls leads to relatively minor differences from the results in Figure 3 for the sample means. Across all the estimates for both types of patients, effects are usually slightly smaller

[25] Anecdotally, the IPA viewed SrHMO patients as more profitable on average than HMO ones, but it is unclear if this was also true for the specialist practices and I have no information as to whether the ophthalmologists might have had an incentive to prioritize giving limited appointment slots to either patient type.

[26] In Appendix Table 3, each regression is presented across three different columns, with all the coefficients for a given period appearing in the same column, and all results for a given ophthalmology practice over time appearing in the same row.

and estimated with higher precision. For HMO patients, like before, the plots in the top panel show that practice 101 experienced a large spike in referral share during the first period after the treatment distribution. The point estimate of 0.1269 for period four is statistically significant at the 5%-level (p-value = 0.026), and represents an effect of 147%.[27] As before, the effect partially dissipates over the following two periods, where the point estimates have sizable magnitudes but are not statistically significant.

None of the other estimates for the other practices are statistically significant at conventional levels, and this includes practice 204's estimate for period four, which falls to about half its previous value at 0.0339. However, two features from the sample average results are preserved with the added controls – at least approximately. In the simple means results, the referral share differences were negatively related with the cost report prices for HMO patients in period four. Here that relationship continues to be seen for all but one practice (group 406's estimate is greater than that for 302). Additionally, in the previous results, those for practice 603 in the post-period approximately mirrored those for 101, a pattern that also holds in these regression-adjusted estimates.

For SrHMO patients, in the bottom panel, most estimates are fairly close to zero and are typically smaller in magnitude than the corresponding ones for the HMOs. Moreover, none are statistically significant at conventional levels. This differs from the previous results where one estimate, period six for practice 505, was significant at the 10% level. Here that estimate fell by almost half. Other than that, the estimates seen here are similar to those obtained previously via sample averages, and, like before, there are no patterns that seem to correlate with the cost report prices. Overall, the major qualitative findings of the previous results are not changed for both the HMO and SrHMO patients when the additional controls are added to the analysis.

The results in Figure 5 also help shed some insight on two important issues. The first is whether there could be differences in perceived quality of the ophthalmologists, and what impact they could have. To a large extent, the issue is likely to be addressed by the experimental design or the event-study model. If quality differences are known and result in the same quality beliefs for all PCPs, then the control group addresses the issue. If, instead, the PCPs have different perceptions about the quality of the specialists, then the pre-period controls of the event-study model subtract out the effects of the differing beliefs. A problem would arise, however, if the PCPs perceived the cost report itself as sending a signal about quality, as it would only be observed by the treatment group physicians and could not be

[27] This is based on the estimated counterfactual of 0.08648, which is obtained by taking the control group's period four sample average referral share to practice 101 of 0.04775 (from column 4 of Appendix Table 1) and adding the pre-period difference between the groups of 0.03873 (from column 4 of Table 2).

accounted for by pre-period data. If that were true, though, we would expect the effect to be observed for both HMO and SrHMO patients. The lack of effect for SrHMOs suggests the issue of quality is not a problem for my analysis.

A similar reasoning applies to the second issue. In mid-September of 2014 – four months into the post-period and the end of period five – one of practice 302's ophthalmologists left the practice and the IPA specialist network. This physician was not the only ophthalmologist in the practice, but he or she handled the bulk of its IPA patients and received the vast majority of IPA referrals (more than 78% of the practice's referrals during the pre-period). This departure, therefore, represented a significant change in the ability of practice 302 to service the IPA's patients during the last two months of the post-period. Like the first issue, the control group addresses many concerns that might arise from this since all PCPs in the network lost access to this specialist simultaneously. Nevertheless, if information about the exit disseminated differently between the treatment and control groups, my estimates could be affected. Thus, one wonders if the specialist's exit could have been a cause of the negative estimates for practice 302 HMOs seen in Figures 3 and 5 in the last two periods? This seems unlikely for two reasons. First, the negative estimates started in period five before the specialist left. Second, there are no large, negative estimates for the SrHMO patients. If the departure mattered for PCP referrals, it is hard to see why they would only matter for one type of patient. So, while one could argue that an early effect could have been a reflection of PCPs becoming aware of the specialist's impending exit, the combination of the early effect and lack of one for SrHMOs does not seem consistent with the pattern one might expect when a physician leaves the network.

5.3.1 The decision to refer to ophthalmology

Having seen that the treatment group physicians reacted to the price information by reallocating some of their referrals, Figure 6 addresses two related questions. The first is whether the treatment might have also affected the decision to refer to ophthalmology or not. As I noted in my discussion of the experiment design, the desire to avoid this possibility was one of the reasons I focused on ophthalmology, since PCPs seemed unlikely to substitute their own services, or those of other specialists, for ophthalmologists. The second question is whether the treatment affected the likelihood that a PCP does not refer to ophthalmology at all in a period. Since my main outcome of interest is calculated with total ophthalmology referrals in the denominator, observations with zero ophthalmology referrals are omitted. If periods with no referral become more common because of the treatment, my estimates might be misleading.

To address these questions, Figure 6 plots two new outcome variables. In the top panel

it is $TOTOPHREFS_{pt}/TOTREFS_{pt}$, the share of PCP practices' total referrals sent to ophthalmology. Here $TOTOPHREFS_{pt}$ is as defined above and $TOTREFS_{pt}$ is the total referrals for a PCP in a period to any specialty.[28] In the bottom panel, a dummy variable for no referral to ophthalmology at all, $\mathbb{1}(TOTOPHREFS_{pt} = 0)$, is plotted. Notice that for both outcomes the question of which practice a referral is sent to plays no role – the only concern is whether a referral is to ophthalmology generally. Thus, for both cases, a simplified version of equation (4.1) is used for the econometric model, where all the interactions needed to account for separate ophthalmology practices are eliminated.

The results in the top panel suggest there was no change in the share of total referrals sent to ophthalmology. None of the estimates are statistically significant and are relatively small, particularly for HMOs in the first two post-periods, when there is almost no difference between the treatment and control groups. The bottom panel shows that there are no group differences in the likelihood of having no ophthalmology referral that is significant at the 5% level. However, the fourth period result for SrHMOs is significant at 10% (p-value = 0.072), with the estimate suggesting that the treatment group was almost 10 percentage points *less* likely to have no referral to ophthalmology that period. Thus, Figure 6 as a whole suggests the treatment had no effect on the decision to refer to ophthalmology, and it did not cause observations to be more likely to be omitted from the analysis.

5.3.2 Heterogeneity

Estimates of equation (4.1) where the outcome is limited to referrals of male or female patients are presented in Figure 7. Among HMOs, in the top row, the results for male patients clearly demonstrate the patterns that were seen in my main estimates. This is especially true for practices 101 and 603, where the estimates are all larger in magnitude than in my main results. For female patients, the estimates obtained are smaller in most cases compared to the main results and none are statistically significant, though some patterns are still observable. In particular, there is still a spike – though less extreme – for practice 101 in period four, and referral shares are approximately negatively related to prices in period four, as they had been before (this is also true for males). Thus, the main results appear to be driven more by male patients, but female ones seem to have contributed, as well. For SrHMOs, the most notable difference across men and women is there is more variation in the results for men and two practice-period estimates are statistically significant: period two for practice 204 (5%) and period six for 505 (10%). Nevertheless, the breakdown by gender does not reveal any correlations between referral patterns and prices that were hidden by the aggregation in the main results.

[28] Every PCP has at least one referral to a specialist in every period.

In the next set of plots in Figure 8, estimates are split into two age subgroups per patient type. Ages were chosen to create an approximately equal division of referrals between groups.[29] The results for HMOs, in the top row, show that both age groups saw a spike of referrals in period four sent to practice 101, though it is larger and statistically significant for the younger group (5%-level, p-value = 0.029). But while the spike estimates for both age groups are similar to the main results (0.1269, 0.1609, and 0.1096, for the main, younger-, and older-group estimates, respectively), the older group more closely reflects two patterns seen in the main estimates: the dissipation of the effect over time for practice 101 and the mirroring of 101's results by practice 603. Thus, while both groups contributed to the results, the older HMOs would seem to be driving the main estimates more than the younger ones.

In contrast, for the SrHMOs, while the older and younger groups each reflect some features of the main SrHMO results, neither seems to be driving the overall main estimates more than the other. Thus, for SrHMOs, the lack of a relationship to prices in reflected in both the young and older groups, while for HMOs, the older group appears to drive more of the results. This provides evidence against the possibility mentioned above that PCPs only responded to the treatment for HMOs because HMOs are younger patients, and not because there was a cost saving incentive for them that was not there for SrHMOs.

A final heterogeneity breakdown is provided in Appendix Figures 1 and 2, where referral effects by broad diagnosis code category are estimated.[30] Results for the top three ophthalmology diagnoses are presented: cataracts, glaucoma, and diabetes.[31] Overall, there is little evidence that referrals of these patients are driven by the cost report prices, nor do patterns seen in the main results manifest themselves here for either patient type. For example, among period four HMO patients, the estimates for practice 101 are negative for cataracts and diabetes, and very close to zero for glaucoma. Thus, no surge in referral share is present immediately after the treatment as is seen in the main results. This suggests that the marginal patients affected by the treatment were not those suffering with one of these top diagnoses.

[29] More precisely, using adult ophthalmology referrals from the pre-period, the median ages for referred HMOs and SrHMOs are 54 and 74, respectively. However, due to repeated occurrences of 74, using 75 splits the SrHMO sample more equally than 74.

[30] Diagnoses codes are not included on my referral data, so I used the ICD-9 diagnosis codes assigned by the ophthalmologist to which the patient was referred in the first follow-up claim observed after the referral. ICD-9 codes are highly detailed, so I grouped the codes by their main part (the first three or four digits). The process of matching the referrals and claims was the same as used for the data underlying Figure 6, but the follow-up period was not limited to 45 days; it included whatever follow ups occurred through the end of 2014, when my claims data ends. An important weakness here is not all referrals result in an visit with the specialist (and, hence, a claim): for my post-period, almost a quarter of referrals have no observable follow-up claim.

[31] Cataracts is almost two-and-a-half more likely than the next most common diagnosis, glaucoma.

5.3.3 Non-Cost Report Ophthalmologists

A final set of estimates plotted in Figure 9 presents my results from an alternative perspective by focusing on the ophthalmology practices that were not included on the cost report. Here all these practices are grouped together, and effects are estimated using a simplified version of equation (4.1) where the interaction terms needed to account for separate ophthalmology practices are removed.[32] The top panel shows the overall results for HMO and SrHMO patients along with separate estimates by gender. The bottom panel provides the estimates by age groups. Since the cost-report- and non-cost-report practices represent the universe of in-network ophthalmologists to which the PCPs could refer, positive estimates here imply a reallocation by treatment group PCPs from cost-report ophthalmologists to those not listed on it. Conversely, negative estimates imply a reallocation to the cost-report ophthalmologists (regardless which practices to or from the referrals flow).

Focusing on HMOs, Figure 9 shows a statistically significant decrease in referral share to non-report practices during period four of -0.1269 (5%-level, p-value = 0.042), implying that the non-report practices lost almost the same number of referrals from the treatment group as practice 101 gained in period four (recall the estimate for 101 was 0.1269).[33] This trough in referral share is driven exclusively by referrals of females, as the estimates by gender show, but seems to reflect both older and younger patients approximately equally, as the age breakdown shows in the bottom panel. When considered in concert with the main results by gender in Figure 7, there is strong evidence that both male and female HMOs were affected by the treatment. They suggest that the marginal male patients that were reallocated to practice 101 in period four tended to come from practice 603, while female ones came from non-cost report ophthalmologists. This is evident from the facts that, in period four, positive estimates for practice 101 are found for both male and female patients, while the only large, negative estimate for that period for male patients was from 603 and the only one for females came from the non-report practices.[34] In the bottom panel, the similar estimates for HMOs by age groups also suggest that the differences between HMO and SrHMO patients may not be due to age differences between the groups.

For SrHMOs, the non-report estimates uncover some surprising findings. They show that the non-cost-report practices received an influx of referrals from the treatment group later in the post-period. For the pooled patients, the period six estimate of 0.1529 is statistically significant at the 5%-level (p-value = 0.006), and appears to be driven by younger females.

[32] This is the same version of the model used in Figure 6.

[33] The estimates are not exact opposites, but more than four decimal places are needed to see this.

[34] Given the positive estimates for female referrals to practices 204, 302, and 406, there might also have been some reallocation of female patients from the non-report ophthalmologists to these practices.

The results for the younger age group are particularly of note since both periods five and six are large and significant. Comparing with the main results by age in Figure 8, one can see there are corresponding large, negative estimates in at least one of the last two periods for practices 406, 505, and 603. This raises the question of how these practices could be affected by the treatment, and why it seems to be only for younger SrHMOs? One may notice that these three practices, 406, 505, and 603, are all either the most, or second most, expensive practices on the cost report for at least one of the two patient types. Could the PCPs be diverting referrals away from these practices to the non-cost-report ones, despite the high level of capitation, because they are viewed as too expensive?

Given the data available, the above scenario is one that I cannot rule out completely. However, in my evaluation of the evidence, I do not weigh this possibility heavily for two reasons. First, there is almost no response for SrHMOs during period four. Across all practices and the various sub-groups, the estimates for SrHMOs in period four are almost uniformly small. If financial concerns are driving the late-post-period results seen in Figure 9, why did the PCPs not respond earlier? Second, it is hard to see why, if the response is financial, the referrals were not reallocated to the cheaper practices instead of the non-report ones. Alternative explanations include the possibilities that these were just random changes not caused by the treatment, or that some other signal I did not anticipate was sent by the cost report. Though a result by chance is always possible, it is still a fact that the estimate reported here is quite large, making the probability it occurred by chance quite small (as the small p-value indicates). Moreover, if it was a non-financial signal that was sent by the treatment, there is still the question of why the response was delayed. Thus, I am currently unable to provide a fully satisfying explanation of this result, but it suggests there may have been more response by the treatment group in SrHMO referrals than the main results reveal. Future research on whether there are non-financial signals sent when prices for one set of doctors are made known to other physicians could provide further insight.

6 Conclusion

This study reports the results of a field experiment that took place during 2014 in the medical offices of 55 primary care practices associated with an IPA. Subject PCPs, who were all internists or family practitioners, received a report listing the per-patient average cost for six busy ophthalmology practices that were part of the IPA network of specialist physicians. These costs varied not because of differences in per-procedure prices, but because of different treatment approaches used by the ophthalmologists. The costs were listed for two separate types of patients: HMO patients, who had HMO insurance coverage through private

insurers, and SrHMO patients, who were part of the Medicare Advantage program. Since ophthalmologists were paid by the IPA for each procedure performed for HMO patients, but were paid flat rates that covered most services for SrHMOs, there were asymmetric financial incentives for the two patient types with respect to referrals. An HMO patient sent to a less expensive ophthalmologist translates to lower costs for the IPA, but the same for a SrHMO is likely to have little impact on IPA costs. This project, therefore, inspects the effect on both types of patients separately, allowing for differential responses by the PCPs.

Analyses of IPA referral data suggests that the treatment-group PCPs responded to the prices on the cost report by increasing the share of their HMO patient ophthalmology referrals they sent to the least expensive ophthalmology practice by almost 150 percent during the first two months after the distribution of the price information. This increase faded over the following four months. While there is some indication that the second least expensive practice received more HMO patient referrals initially, the other, more expensive practices all generally received fewer or a similar amount of referrals after treatment. For SrHMO patients, on the other hand, there was little change in referral patterns in most cases. For those differences that are seen, there is little evidence of a relationship with the ranking of ophthalmology practices by price on the cost report. Given the different financial incentives facing the PCPs by patient type, this differential response is consistent with a behavioral model for the PCPs in which they were cognizant of, and concerned with, the cost impacts of their referral choices. Though the medical literature on price transparency has not had uniform results, my findings are consistent with those that are more carefully done, such as Tierney et al. (1990) and Monsen et al. (2019), and are also largely in line with those of the most closely related work on referrals by Ho and Pakes (2014).

In one sense, this result is surprising given that it suggests that a large change in behavioral patterns was induced by a relatively low cost intervention. On the other hand, the PCPs' financial advantage to reallocating referrals is definitely greater than zero, even if it is small, and the IPA had been encouraging the doctors to be mindful of costs during the time period in which the experiment took place. Moreover, the costs to PCPs of changing who they referred to among network ophthalmologists is likely zero, so the PCPs only stood to gain by rearranging their referral patterns. In light of this reasoning, maybe the large response is not so surprising.

Another reason one might find the size of the results surprising is the nature of the delivery of the treatment via mail. In order for a treatment to be observed, the PCPs had to receive, open, read, understand, and remember the contents of the cost report – and any one of those steps could have failed, resulting in no treatment and, hence, no response. On the other hand, the delivery of the treatment directly to the subjects in this

case is much more convenient than occurs in typical consumer-level price transparency efforts. In those scenarios, consumers have to seek out the price information of their own accord, something few of them end up doing. Thus, along the convenience margin, my intervention was relatively strong. Additionally, for several years before the intervention, the IPA had been emphasizing the importance of costs and the PCPs influence over them. So the subjects interest in the price information may have been higher than it would have been without such preparation.

Whether one regards these findings as surprising or not, with regard to health care price transparency, they provide hope and some direction for further research. Consumer-side price transparency has, so far, failed to generate much evidence of a potential to produce cost savings in health care. Additional efforts continue to be made on the consumer side, such as the recent hospital-price-posting rule implemented by the Trump administration. However, this study, and the previous literature discussed above, suggest that physician-side price transparency has cost savings potential and deserves more attention from researchers and policymakers.

In terms of future research, the dissipation over time of the effects found in this study suggest that the issue of salience should receive particular attention. Moreover, the large estimates I obtained belie the fact that they were relatively imprecise. Given the size of the IPA, the sample size for this project was as large as it could be, but a goal for future research could be to work with larger organizations, allowing for larger samples that could improve the precision of estimates, and allow for more examination of heterogeneity. Stronger statements could then be made about the extent and persistence of responses, and the types of subjects most responsive to the treatment.

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May 1, 2014

Dear Doctor,

As requested by our primary care physicians, we are continuing our efforts to share information on specialty costs by rendering physician. To that end, please find the included report on average costs per patient for IPA Ophthalmology practices. These costs are based on actual claims from encounters with patients who were newly referred to Ophthalmology, and who had their first encounters with Ophthalmologists during the twelve-month period from July 2012 through June 2013. All claims over the 180-day period following the first encounter were used in the calculations.

Since our goal was to produce a broad measure of cost, we calculated the averages using claims for patients across a range of diagnoses. However, in order to increase the comparability of the averages, we only used diagnoses that were common across practices, and adjusted diagnosis proportions to reflect IPA-wide prevalence instead of individual practice level prevalence. As a result, for the patients included in this analysis, cataract diagnoses were the most common, occurring roughly 50% of the time. Since cataract conditions are relatively costly to treat, these patients accounted for almost 71% of the average costs reported.

Lastly, to further improve comparability, only practices that saw more than 300 newly referred patients and had patient satisfaction scores above 80% were included.

Sincerely,

Chief Executive Officer

Figure 1: Cover Letter for Ophthalmologist Cost Report

Average 180-Day Cost for Newly Referred Patients to Ophthalmology

For patients with first encounters with Ophthalmology during the twelve-month period from July 2012 through June 2013

Practice / Physician Name	HMO Patients	SrHMO Patients
101	$147	$450
204	$215	$502
302	$230	$456
406	$270	$575
505	$292	$561
603	$333	$470

Notes:

(1) Ophthalmology is paid fee-for-service for HMO patients. For SrHMO patients, approximately 75% of procedure codes are capitated, with the rest being fee-for-service. Costs for capitated codes are based on the Medicare fee schedule for claims submitted.

(2) Averages have been adjusted for observable differences in the underlying health of specialist patient populations, and rounded to the nearest dollar.

(3) Newly referred patients are those that had not had a claim in Ophthalmology for the previous 180-days and were referred to Ophthalmology by an internist or FP during the previous 180-days.

(4) Ophthalmology practices included on this report all had at least 300 newly referred patients, had patient satisfaction scores for all practice ophthalmologists exceeding 80%.

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Figure 2: Ophthalmologist Cost Report

Table 1: Characteristics of ophthalmology practices

	Pre-Period Referrals Received						Average Cost ($) Reported to PCPs	
Ophthalmology	All		HMO Patients		SrHMO Patients		HMO	SrHMO
Practice ID	Count	Share	Count	Share	Count	Share	Patients	Patients
101	341	10.8%	102	9.3%	239	11.7%	147	450
204	241	7.7%	80	7.3%	161	7.9%	215	502
302	459	14.6%	140	12.7%	319	15.6%	230	456
406	636	20.2%	242	22.0%	394	19.2%	270	575
505	646	20.5%	191	17.4%	455	22.2%	292	561
603	400	12.7%	197	17.9%	203	9.9%	333	470
Cost report practices	2,723	86.5%	952	86.6%	1,771	86.5%	–	–
All others	424	13.5%	147	13.4%	277	13.5%	–	–
All ophthamologists	3,147	100%	1,099	100%	2,048	100%	–	–

Table 2: Comparison of PCP-practice observables for the six-month pre-period

	Group Mean			Std.			95% Conf. Int.	
Variable	Treat	Ctrl	Diff.	Err.	T-stat.	P-val.	Low	High
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Internal Medicine*	.3571	.5185	-.1614	.1346	-1.199	.2358	-.4313	.1086
Ophthalmology referrals per-PCP $\geq$ all-practice median								
– HMO patients*	.5714	.5556	.01587	.1363	.1165	.9077	-.2574	.2892
– SrHMO patients*	.5	.5556	-.05556	.137	-.4057	.6866	-.3303	.2191
Claims per-PCP $\geq$ all-practice median								
– HMO patients*	.5357	.4815	.05423	.1372	.3954	.6941	-.2209	.3294
– SrHMO patients*	.4643	.5556	-.09127	.1368	-.6673	.5075	-.3656	.1831
All referrals	604.4	567.4	36.99	140.6	.263	.7937	-245.7	319.7
Ophthalmology referrals	58.96	55.41	3.557	12.75	.2789	.7814	-22.06	29.17
– HMO patients	21.46	18.44	3.02	4.984	.6059	.5474	-7.002	13.04
– SrHMO patients	37.5	36.96	.537	8.638	.06217	.9507	-16.8	17.87
– Female patients	35.54	34.3	1.239	8.055	.1539	.8783	-14.93	17.4
– Male patients	23.43	21.11	2.317	5.146	.4503	.6546	-8.038	12.67
– Patients aged 60+	42.89	41.81	1.078	9.996	.1078	.9145	-18.99	21.14
– Patients ages 40-59	12.32	10.07	2.247	2.648	.8486	.4004	-3.08	7.575
– Patients ages 18-39	3.75	3.519	.2315	1.217	.1902	.8499	-2.211	2.674
Ophthalmology referrals as share of all referrals								
– HMO patients	.07445	.07949	-.005038	.007915	-.6366	.5275	-.02097	.01089
– SrHMO patients	.1312	.1367	-.005544	.01311	-.4227	.6742	-.03185	.02076
Share of ophthalmology HMO patient referrals to:								
– Practice 101	.09824	.05951	.03873	.03215	1.205	.2341	-.02588	.1033
– Practice 204	.08529	.04852	.03677	.02849	1.29	.2027	-.02042	.09395
– Practice 302	.1165	.1219	-.005428	.05592	-.09706	.9231	-.1177	.1069
– Practice 406	.1735	.2121	-.03856	.06075	-.6347	.5284	-.1605	.08335
– Practice 505	.1536	.2232	-.06955	.07264	-.9574	.3431	-.2155	.07643
– Practice 603	.1704	.194	-.02354	.06917	-.3403	.735	-.1623	.1152
– All others	.2024	.1408	.06158	.0602	1.023	.311	-.05917	.1823
Share of ophthalmology SrHMO patient referrals to:								
– Practice 101	.1307	.08262	.04807	.03849	1.249	.2176	-.02924	.1254
– Practice 204	.05497	.06718	-.01221	.02638	-.4628	.6454	-.06512	.0407
– Practice 302	.1095	.1021	.007414	.04575	.162	.8719	-.0845	.09932
– Practice 406	.1833	.2025	-.01912	.05217	-.3665	.7155	-.1238	.08555
– Practice 505	.1685	.2206	-.05211	.06741	-.773	.4429	-.1873	.0831
– Practice 603	.1752	.1557	.01947	.06705	.2903	.7727	-.115	.154
– All others	.1778	.1693	.008491	.0583	.1456	.8848	-.1085	.1255

* Indicates stratification variable. Standard errors for group mean differences assume unequal variances between groups and critical values are based on degrees of freedom approximated via Satterthwaite (1946).

Table 3: PCP practice size distribution by treatment status

Number of PCPs in practice	Control Group	Treatment Group	Any Treatment Status
1	16 59.26%	17 60.71%	33 60.00%
2	7 25.93%	4 14.29%	11 20.00%
3	3 11.11%	5 17.86%	8 14.55%
4	1 3.70%	1 3.57%	2 3.64%
6	0 0.00%	1 3.57%	1 1.82%
All Practice Sizes	27 100.00%	28 100.00%	55 100.00%

Percentages are relative to column totals. Pearson's chi-square statistic equals 2.3311 and P-value equals 0.675.

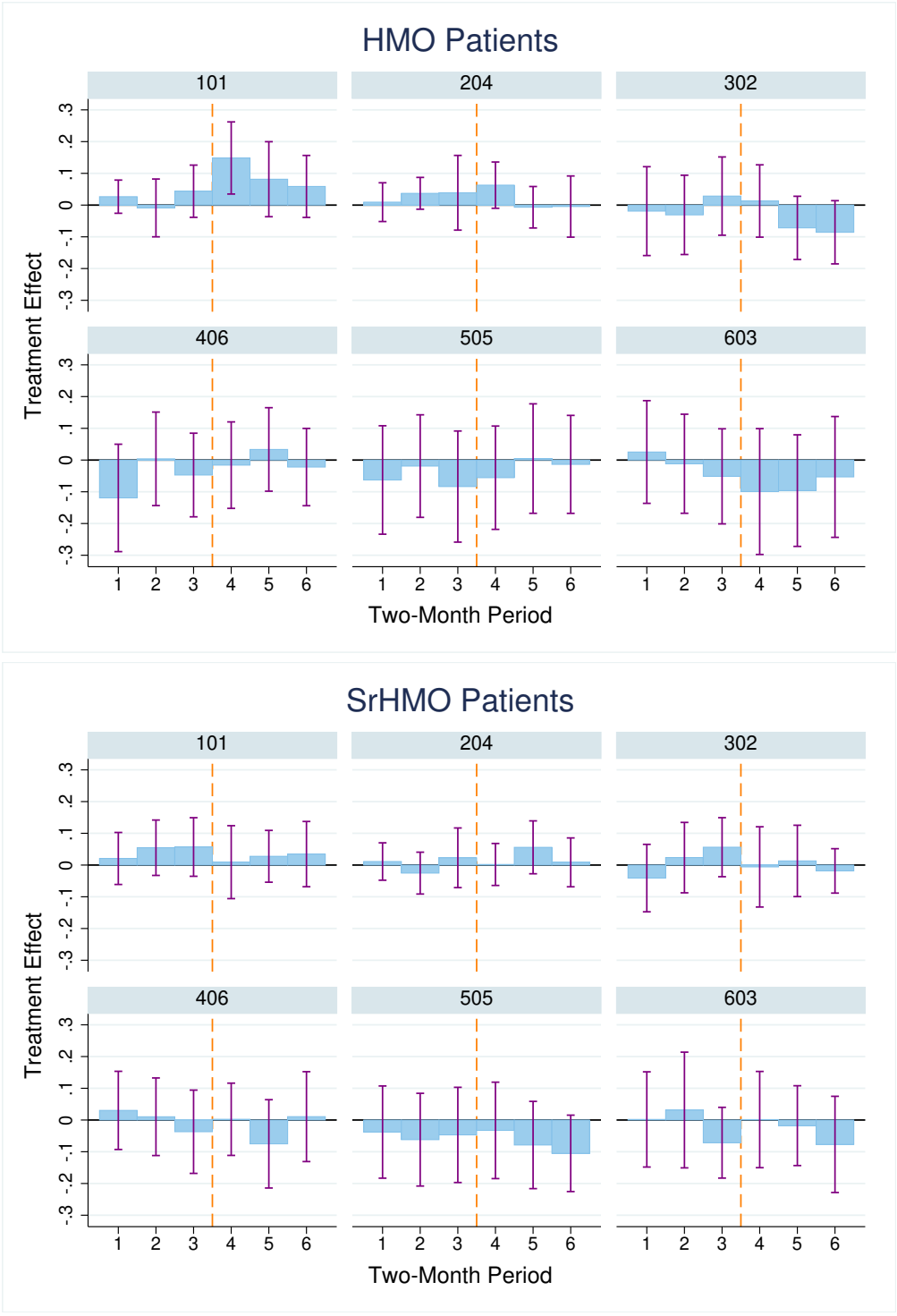


Figure 3: Difference in mean referral share between groups (treatment minus control). Periods are two months. Purple lines indicate 95% confidence intervals. Orange, dashed vertical lines divide the pre- and post-periods, Nov. 2013 to Apr. 2014 and May 16th to Nov. 15th, 2014, respectively. Appendix Tables 1 and 2 present calculations underlying these plots.

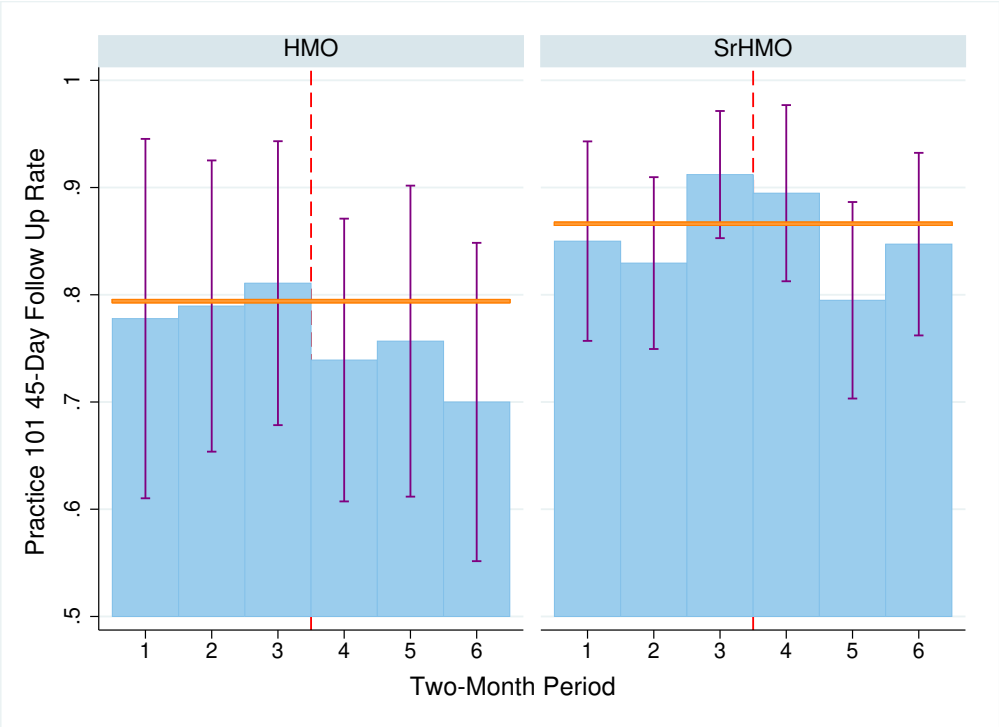


Figure 4: Mean 45-day follow-up rate for ophthalmology practice 101. Red, dashed, vertical line divides the pre- and post-periods. Purple bands mark 95% confidence intervals. Orange horizontal line represents the pre-period average follow-up rate for practice 101.

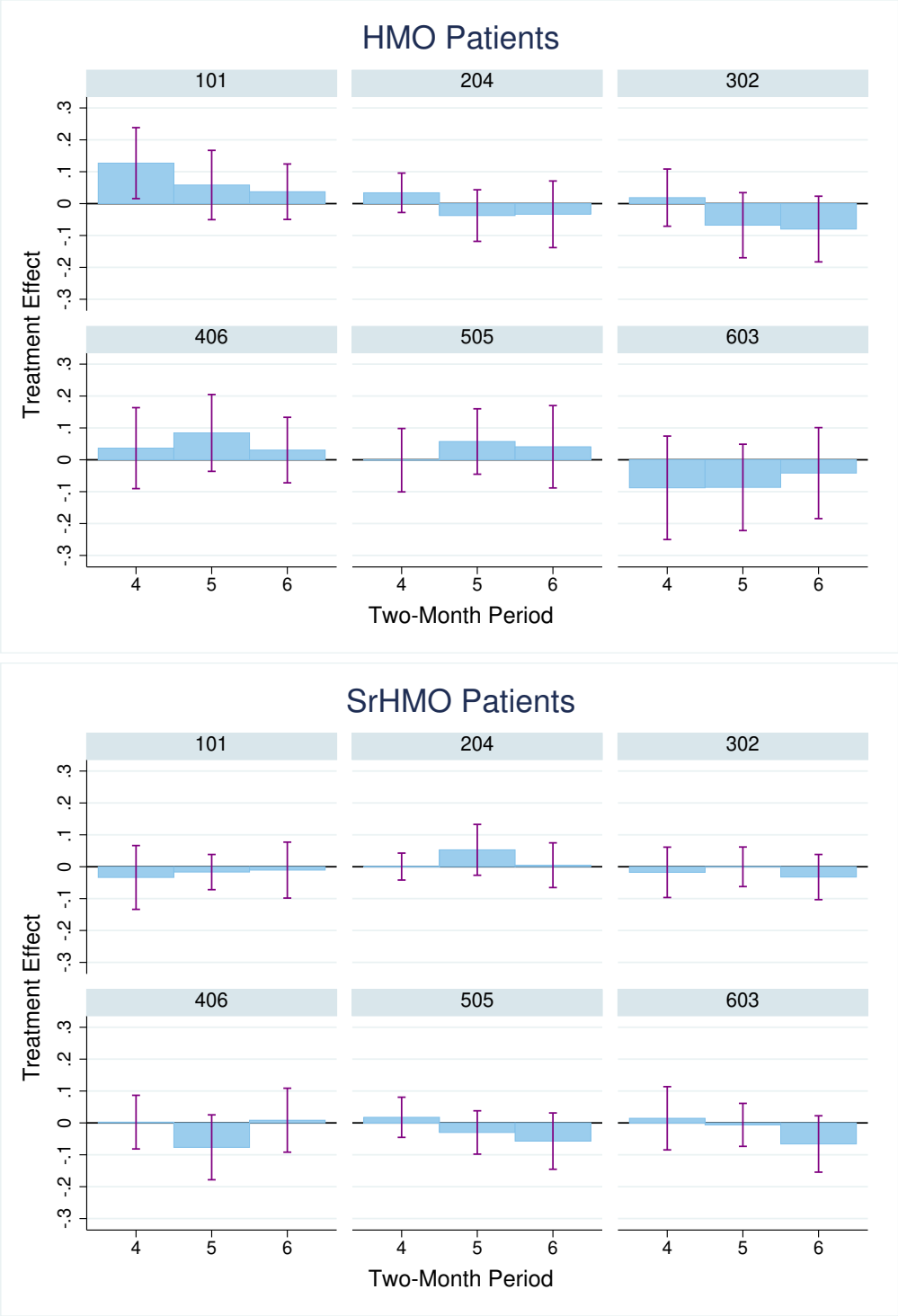


Figure 5: Regression estimated treatment effects on referral share. Model is equation (4.1). Purple lines indicate 95% confidence intervals. See Appendix Table 3 for tabular presentation of these estimates.

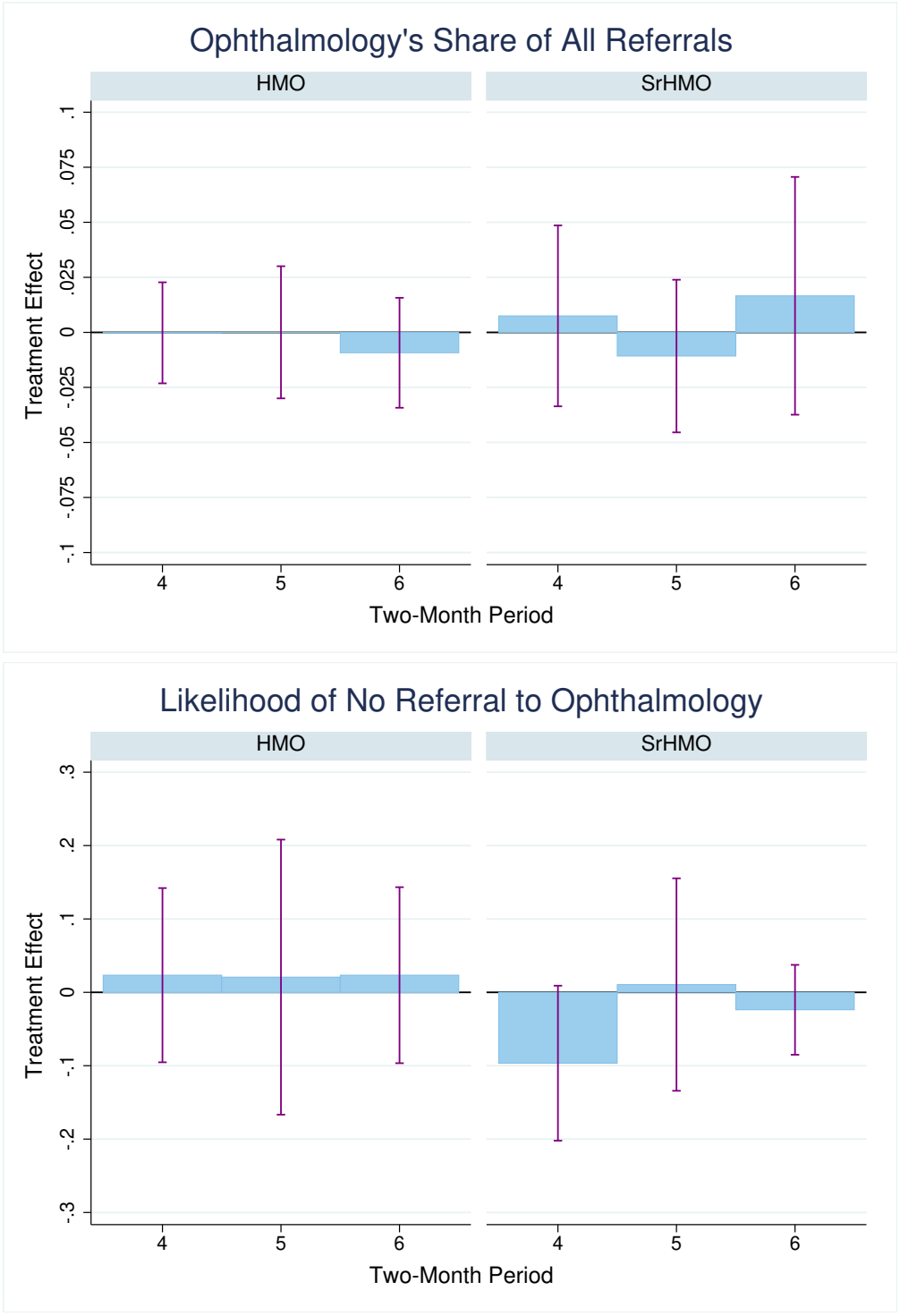


Figure 6: Regression estimated treatment effects on share of all referrals (of any type) sent to ophthalmology (top) and the likelihood of sending no referral to ophthalmology for an entire period (bottom). Model is a simplified version of equation (4.1) that does not separate estimates by practice. Purple lines indicate 95% confidence intervals.

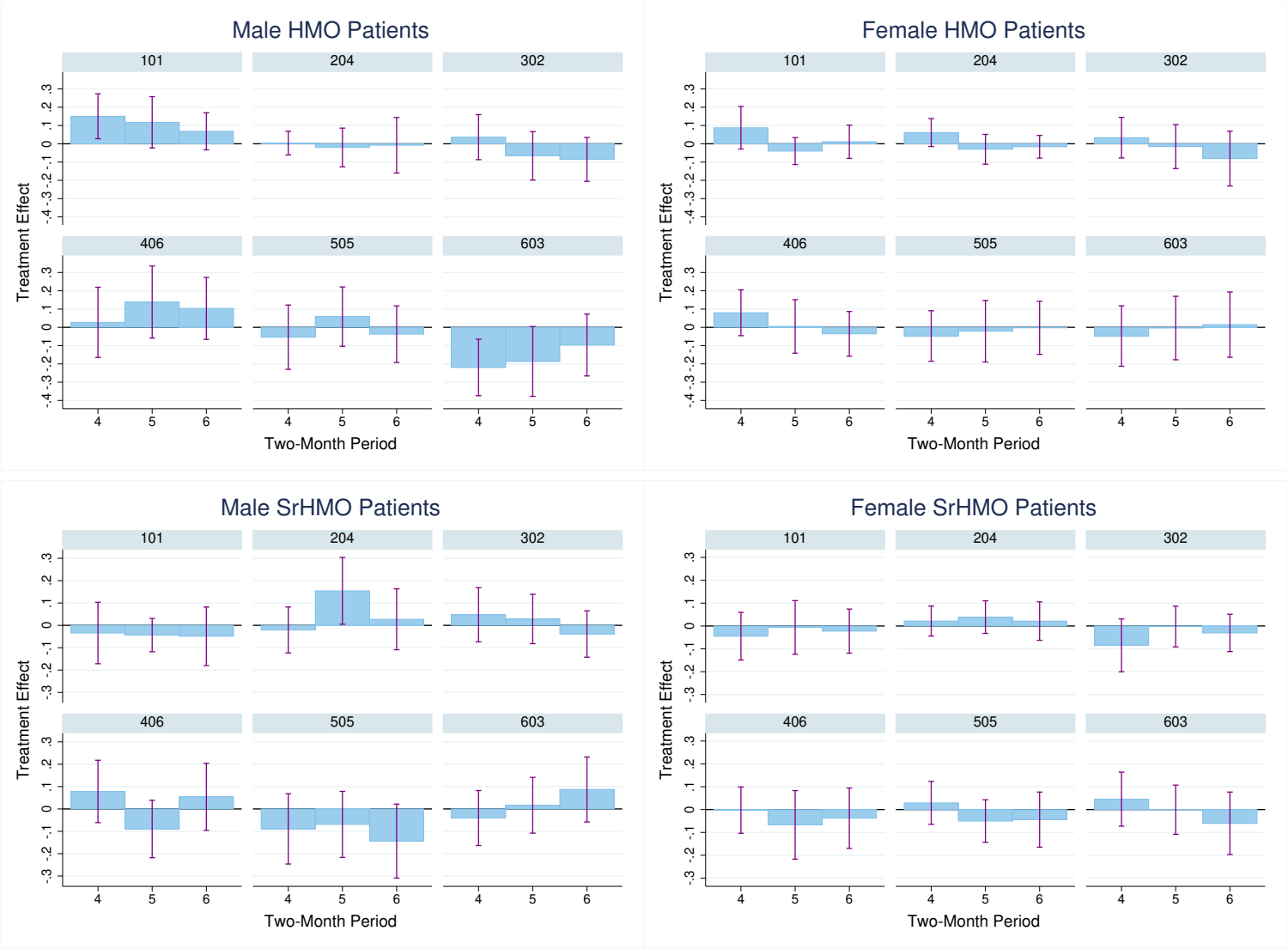


Figure 7: Regression estimated treatment effects on referral share for male and female patients. Note that the y-axis scale is slightly larger in the top row (HMO) figures. Model is equation (4.1). Purple lines indicate 95% confidence intervals.

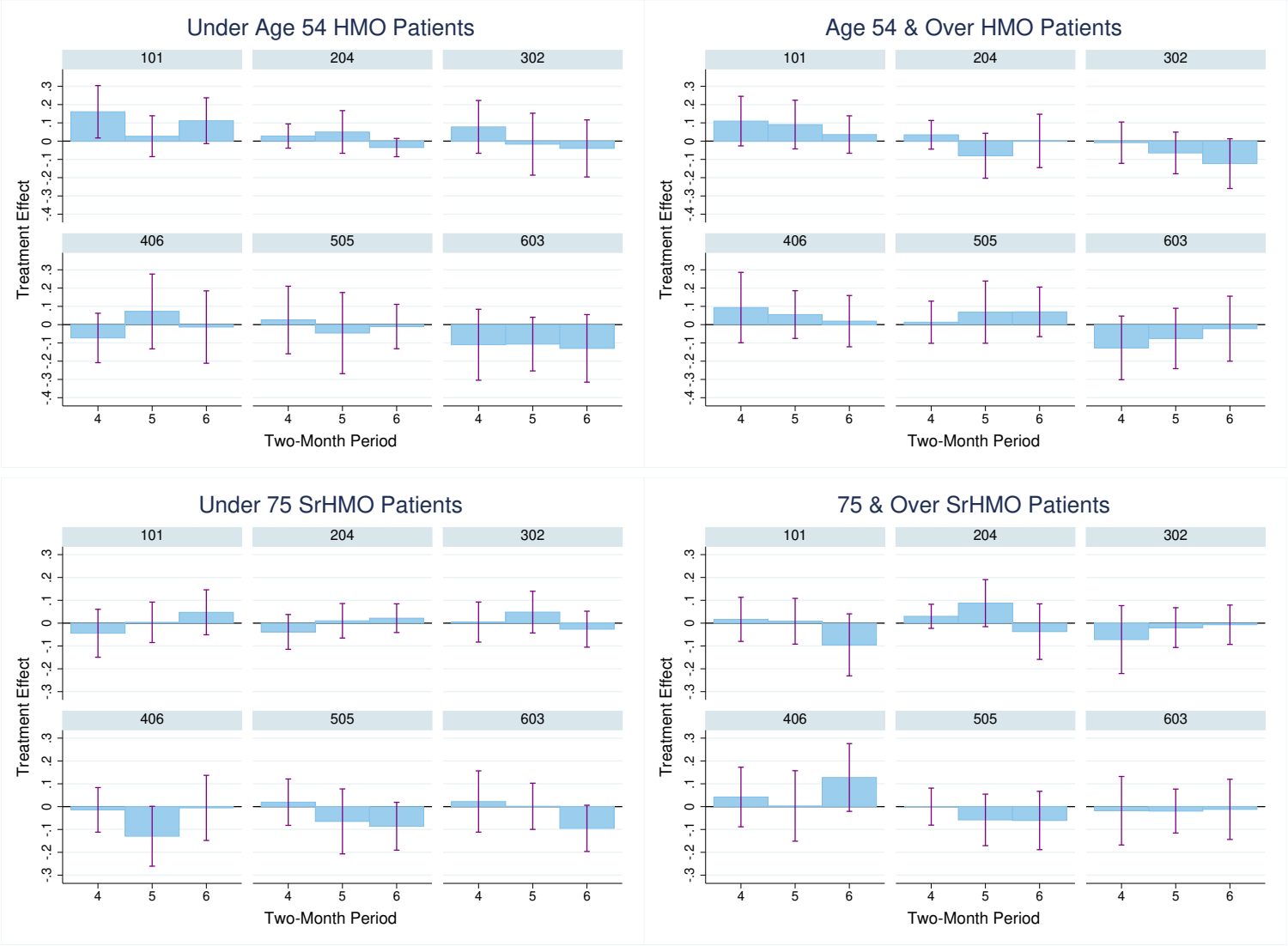


Figure 8: Regression estimated treatment effects on referral share by age splits. Note that, consistent with Figure 7, the y-axis scale is slightly larger in the top row (HMO) figures. Model is equation (4.1). Purple lines indicate 95% confidence intervals.

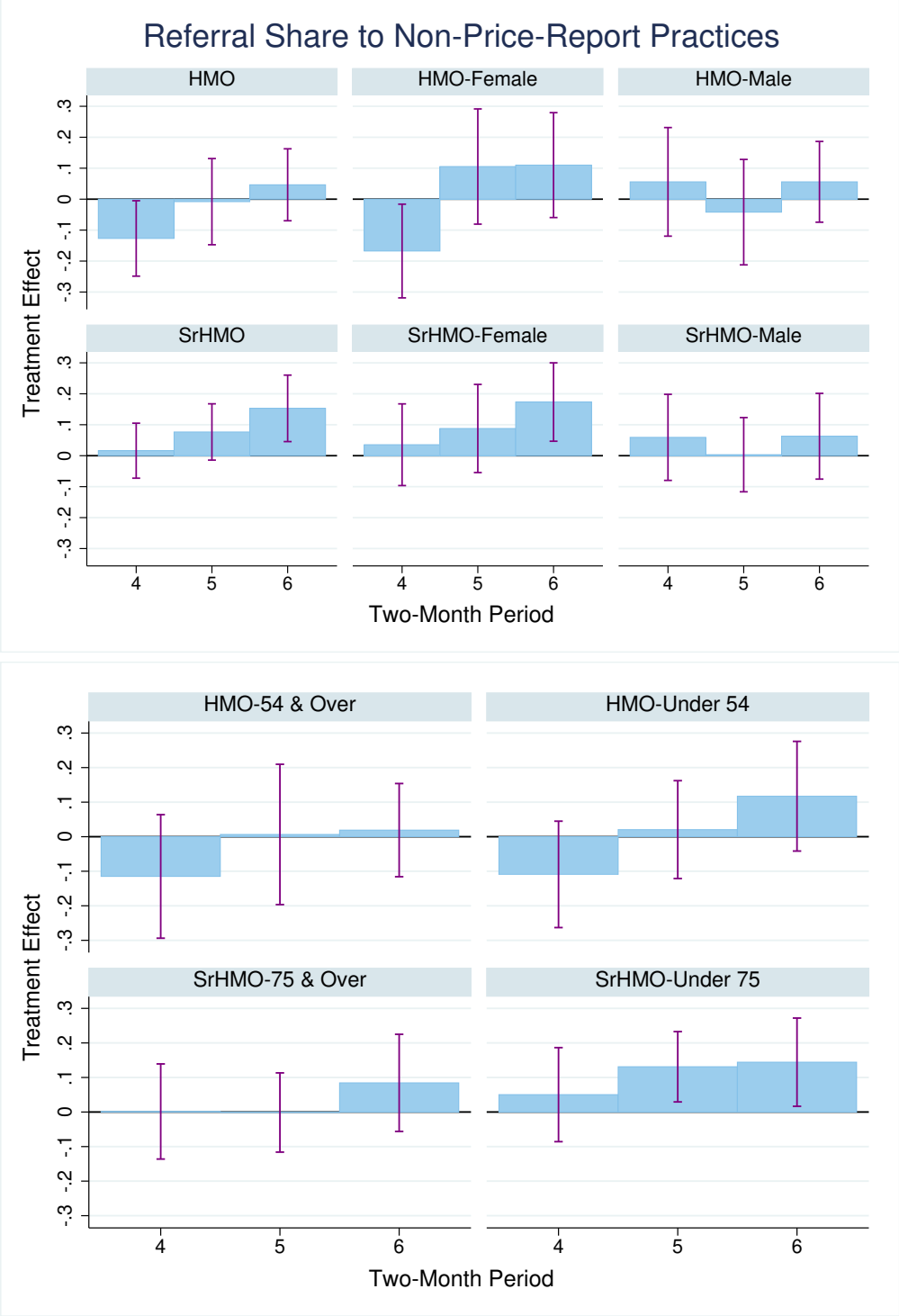


Figure 9: Regression estimated treatment effects on combined referral share for all ophthalmology practices not listed on the cost report. Model is a simplified version of equation (4.1) that does not separate estimates by practice. Purple lines indicate 95% confidence intervals.

Appendix

Appendix Table 1: Mean referral shares for HMO patients

Oph.	Time	Group Mean			Std.			95% Conf. Int.	
Pract.	Period	Treat	Ctrl	Diff.	Err.	T-stat.	P-val.	Low	High
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
101	1	.07106	.04455	.0265	.02604	1.018	.3137	-.02581	.07882
101	2	.07675	.08572	-.008972	.04533	-.1979	.8439	-.1001	.08211
101	3	.1004	.05675	.04365	.04086	1.068	.2911	-.03867	.126
101	4	.1962	.04775	.1485	.05611	2.647	.01184	.03484	.2621
101	5	.1386	.05706	.08155	.0582	1.401	.17	-.03662	.1997
101	6	.1254	.06666	.05873	.04812	1.22	.2304	-.03892	.1564
204	1	.05635	.04708	.009268	.03039	.305	.7616	-.05175	.07028
204	2	.05293	.01589	.03704	.02474	1.497	.1428	-.01308	.08715
204	3	.129	.09024	.03879	.0586	.662	.511	-.07889	.1565
204	4	.07882	.01595	.06287	.03567	1.762	.08842	-.01005	.1358
204	5	.03735	.04421	-.006863	.03245	-.2115	.8334	-.07218	.05846
204	6	.06118	.06583	-.004647	.04784	-.09714	.9231	-.1011	.09176
302	1	.1064	.1254	-.01907	.06965	-.2738	.7854	-.1591	.121
302	2	.1142	.1451	-.03089	.06207	-.4977	.621	-.1558	.09399
302	3	.1274	.09911	.02834	.06143	.4613	.6466	-.09501	.1517
302	4	.1126	.09968	.01291	.05672	.2275	.8209	-.101	.1269
302	5	.05772	.1296	-.07185	.04924	-1.459	.1527	-.1715	.0278
302	6	.03313	.1188	-.08572	.04908	-1.747	.08929	-.1853	.01384
406	1	.1646	.284	-.1194	.08414	-1.419	.1625	-.2885	.04985
406	2	.2165	.2126	.003828	.07339	.05215	.9586	-.1435	.1512
406	3	.1299	.1771	-.04716	.06565	-.7183	.4758	-.1789	.0846
406	4	.1556	.1717	-.01604	.06775	-.2367	.8139	-.1523	.1202
406	5	.1569	.1235	.03342	.06527	.512	.6112	-.09816	.165
406	6	.1553	.1774	-.0221	.06061	-.3646	.717	-.1438	.09965
505	1	.1553	.2181	-.0628	.08507	-.7383	.4638	-.2337	.1081
505	2	.168	.187	-.01903	.08044	-.2365	.814	-.1806	.1425
505	3	.1596	.2431	-.08358	.08703	-.9603	.3418	-.2586	.09148
505	4	.1511	.2067	-.05561	.08104	-.6863	.4957	-.2184	.1072
505	5	.2022	.1976	.004648	.08566	.05427	.957	-.1679	.1772
505	6	.1675	.1811	-.01364	.07684	-.1775	.8598	-.168	.1407
603	1	.1911	.1657	.02537	.08057	.3149	.7542	-.1364	.1872
603	2	.1923	.204	-.01169	.07773	-.1504	.881	-.1677	.1444
603	3	.1281	.1794	-.05134	.07465	-.6877	.4947	-.2012	.0985
603	4	.1687	.2679	-.09928	.09857	-1.007	.3189	-.2975	.09896
603	5	.1669	.2632	-.09627	.08708	-1.106	.2753	-.2721	.07952
603	6	.2279	.2811	-.0532	.09471	-.5617	.5769	-.2436	.1372

Standard errors for group mean differences assume unequal variances between groups and critical values are based on degrees of freedom approximated via Satterthwaite (1946).

Appendix Table 2: Mean referral shares for SrHMO patients

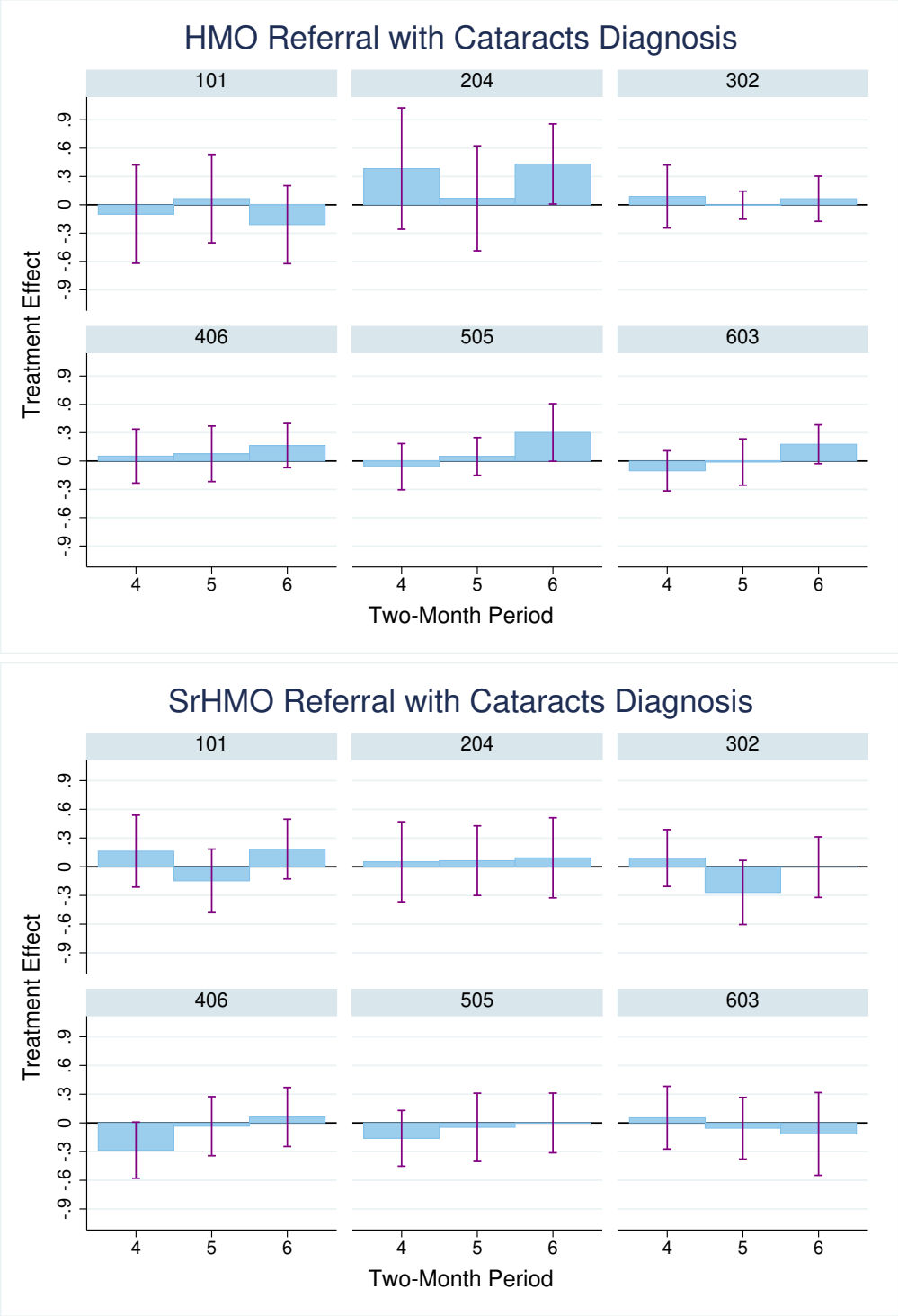
Oph.	Time	Group Mean			Std.			95% Conf. Int.	
Pract.	Period	Treat	Ctrl	Diff.	Err.	T-stat.	P-val.	Low	High
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
101	1	.09813	.07741	.02073	.04068	.5094	.6129	-.06119	.1026
101	2	.1255	.07095	.05457	.0434	1.257	.2147	-.03269	.1418
101	3	.1548	.09791	.05687	.04594	1.238	.2217	-.03545	.1492
101	4	.1015	.09232	.009137	.0569	.1606	.8732	-.1058	.1241
101	5	.1151	.08758	.02756	.04057	.6793	.5003	-.05404	.1092
101	6	.1258	.09131	.03448	.05107	.6751	.503	-.06836	.1373
204	1	.0637	.05257	.01113	.02924	.3805	.7054	-.04779	.07005
204	2	.04803	.07342	-.02539	.03279	-.7743	.4425	-.09131	.04053
204	3	.08879	.06592	.02287	.04655	.4913	.6258	-.07112	.1169
204	4	.06763	.06576	.001863	.0329	.05665	.9551	-.06426	.06799
204	5	.1182	.06238	.05585	.04132	1.351	.1836	-.02748	.1392
204	6	.07278	.06433	.008449	.03834	.2204	.8265	-.06849	.08539
302	1	.09216	.1333	-.04111	.05293	-.7767	.4409	-.1474	.06515
302	2	.1228	.0994	.02344	.05522	.4245	.6731	-.08749	.1344
302	3	.1255	.0692	.05628	.04596	1.225	.2283	-.03675	.1493
302	4	.1278	.1337	-.005866	.06287	-.0933	.9261	-.1323	.1206
302	5	.1258	.1128	.01304	.05585	.2336	.8164	-.0994	.1255
302	6	.06794	.08635	-.01841	.03477	-.5294	.599	-.08835	.05154
406	1	.1904	.1601	.03032	.06127	.495	.6229	-.09284	.1535
406	2	.1813	.171	.0103	.06091	.1691	.8664	-.1119	.1325
406	3	.1873	.2242	-.03696	.06519	-.5671	.5734	-.1682	.09422
406	4	.1654	.1631	.00233	.05672	.04109	.9674	-.1115	.1161
406	5	.1768	.252	-.07514	.06922	-1.085	.2831	-.2143	.06402
406	6	.2091	.1984	.01068	.07055	.1514	.8803	-.1309	.1523
505	1	.1799	.218	-.03803	.07236	-.5256	.6015	-.1833	.1072
505	2	.1735	.2355	-.06198	.07278	-.8516	.3984	-.2081	.08411
505	3	.1787	.2257	-.04696	.07468	-.6288	.5324	-.197	.1031
505	4	.1724	.2051	-.0327	.07573	-.4318	.6677	-.1847	.1193
505	5	.1432	.222	-.07876	.06833	-1.153	.2549	-.2162	.05872
505	6	.1269	.2319	-.1051	.05989	-1.754	.08568	-.2255	.01531
603	1	.1374	.1356	.001815	.07471	.02429	.9807	-.1482	.1519
603	2	.2197	.1882	.03149	.09085	.3467	.7302	-.1508	.2138
603	3	.09619	.1677	-.07156	.0554	-1.292	.2029	-.1831	.03995
603	4	.1669	.1655	.001392	.07549	.01845	.9854	-.1503	.153
603	5	.1294	.1472	-.01781	.06252	-.2849	.777	-.1436	.1079
603	6	.1401	.217	-.07689	.07515	-1.023	.312	-.2285	.07471

Standard errors for group mean differences assume unequal variances between groups and critical values are based on degrees of freedom approximated via Satterthwaite (1946).

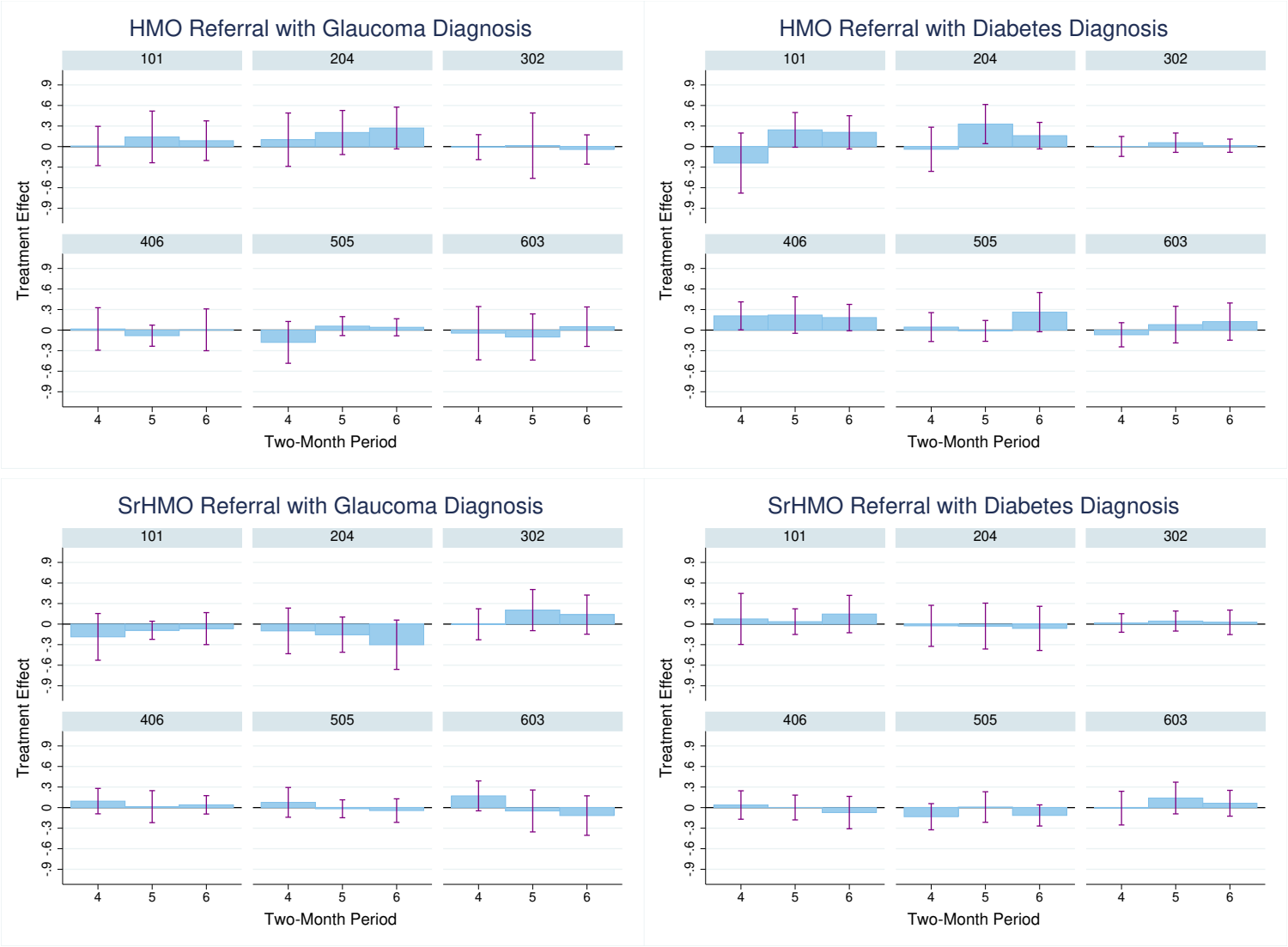
Appendix Table 3: Effect of treatment on referral share.
Each set of three columns presents one regression.

Oph.	HMO Patients (N=1,872)			SrHMO Patients (N=1,914)		
Pract.	t=4	t=5	t=6	t=4	t=5	t=6
	[1]	[2]	[3]	[4]	[5]	[6]
101	0.1269**	0.0583	0.0373	-0.0336	-0.0167	-0.0106
	(0.056)	(0.054)	(0.043)	(0.050)	(0.028)	(0.044)
	[0.026]	[0.287]	[0.394]	[0.503]	[0.546]	[0.809]
204	0.0339	-0.0375	-0.0335	0.0005	0.0530	0.0048
	(0.031)	(0.040)	(0.052)	(0.021)	(0.040)	(0.035)
	[0.277]	[0.356]	[0.523]	[0.981]	[0.189]	[0.892]
302	0.0187	-0.0677	-0.0798	-0.0176	-0.0002	-0.0324
	(0.045)	(0.051)	(0.051)	(0.039)	(0.031)	(0.035)
	[0.678]	[0.189]	[0.126]	[0.657]	[0.995]	[0.363]
406	0.0365	0.0843	0.0305	0.0024	-0.0765	0.0085
	(0.063)	(0.060)	(0.051)	(0.042)	(0.051)	(0.050)
	[0.567]	[0.167]	[0.554]	[0.954]	[0.138]	[0.866]
505	-0.0012	0.0573	0.0408	0.0176	-0.0300	-0.0571
	(0.050)	(0.051)	(0.064)	(0.031)	(0.034)	(0.044)
	[0.980]	[0.267]	[0.529]	[0.578]	[0.381]	[0.201]
603	-0.0879	-0.0866	-0.0417	0.0145	-0.0062	-0.0661
	(0.081)	(0.068)	(0.071)	(0.049)	(0.034)	(0.044)
	[0.283]	[0.206]	[0.561]	[0.770]	[0.855]	[0.140]

Model is equation (4.1). Statistical significance for two-sided t-tests indicated by *=p<0.1, **=p<0.05, and ***=p<0.01. Standard errors are adjusted for within-PCP-practice clustering and presented in parentheses, while p-values for two-sided tests are given in brackets.



Appendix Figure 1: Regression estimated treatment effects on referral share for cataract diagnosis. Model is equation (4.1). Purple lines indicate 95% confidence intervals. Note that the vertical scale here is larger than in the main results.



Appendix Figure 2: Regression estimated treatment effects on referral share by diagnosis. Model is equation (4.1). Purple lines indicate 95% confidence intervals. Note that the vertical scale here is larger than in the main results.