ESSAYS IN HEALTH AND LABOR

by

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DISSERTATION ABSTRACT

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To explore mechanisms driving the gender gap in competitive environments, we use an experimental setting to identify how the potential for cheating affects the individual's decision to enter competition and how this effect differs by gender. We find evidence that the potential for dishonesty reduces the probability that females enter competitions whereas cheating has little effect on the decisions of men. In addressing the opioid epidemic we exploit variation in the timing of Prescription Drug Monitoring Programs (PDMPs) implementation across states to identify the effectiveness of these programs on reducing opioid abuse. Within a difference-in-differences framework, we consider the effect of heterogeneity across program attributes on opioid-related treatment admissions and overdose deaths. Results suggest only those programs that require prescribers to access the databases prior to prescribing are effective in reducing opioid-related treatment admissions. We then explore the effects of prescription drugs on health behaviors exploiting the establishment of Medicare Part D to identify the potential substitution effects between prescription drugs and preventative-care behaviors, attitudes over risk

and medical care, and other health-related behaviors. We also explore the potential for prescription-drug coverage to mitigate any preventative care or behavioral changes following a chronic or acute health shock. We find no evidence to suggest prescription-drug coverage alters the use of preventative care.

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To my family.

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CHAPTER I

INTRODUCTION

This work aims to improving our understanding of competitive behaviors and health decisions by consumers and health care providers through both experimental and applied econometric analysis.

In Chapter II we use an experimental setting to explore how the potential for cheating affects the individual's decision to enter competitive environments and how this effect differs by gender. We find evidence that the potential for dishonesty reduces the probability that females enter competitions whereas cheating has little effect on the decisions of men.

In Chapter III we exploit variation in the timing of Prescription Drug Monitoring Programs (PDMPs) implementation across states to identify the effectiveness of these programs on reducing opioid abuse. Within a difference-in-differences framework, we consider the effect of heterogeneity across program attributes on a range of health outcomes such as opioid-related treatment admissions and overdose deaths. Results suggest only those programs that require prescribers to access the databases prior to prescribing are effective in reducing opioid-related treatment admissions.

Finally, using the he establishment of Medicare Part D in Chapter IV, we identify the potential substitution effects between prescription drugs and preventative-care behaviors, attitudes over risk and medical care, and health behaviors. We also explore

the potential for prescription-drug coverage through Medicare Part D to mitigate any preventative care or changes in health behaviors following a chronic or acute health shock. We find no evidence to suggest prescription-drug coverage altered the use of preventative care or changed health behaviors. Chapter V concludes.

CHAPTER II

GENDER DIFFERENCES IN COMPETITIVENESS: THE EFFECT OF CHEATING ON OPT-OUT BEHAVIOR

Introduction

While it is welfare concerns that should govern the freedom for women to avoid environments that are to their detriment, unpacking female "preference" is key to addressing the relevant policy concerns around lagging female attainment. To the extent that preferences for competition are gender specific (Niederle and Vesterlund, 2007), for example, it follows that even in the absence of discrimination, females would continue to be under-represented in competitive environments.

Cohen and Deterding (2009) suggests that this reluctance to compete is not accounted for by ability differences, citing that while women comprise only 20% of degree recipients in engineering, among those who do select into engineering there is no evident difference in the attrition or performance of men and women. Of course, women are severely underrepresented in business leadership—they account for only 4.8% of chief executive officers, and 8.1% of top earners among Fortune 500 companies (Warner, 2014).[1]

[1]This can be attributed to the lack of female representation in the MBA programs that provide a pipeline to these positions, which is itself a potential measure of female disinterest in entering competitive environments. (The Financial Times, The Financial Times' Global MBA Rankings 2015 , Jan. 25, 2015, from The Financial Times website, `http://rankings.ft.com/exportranking/global-mba-ranking-2015/pdf` (last visited August 20, 2015.)

Here, we consider the potential that simple notions of preference for less-competitive environments may mislead policy makers from addressing an important externality. Namely, to the extent that dishonest performance or cheating of peers imposes larger costs in more-competitive environments, attributing lower rates of female participation directly to preferences for or against competitiveness is to avoid the welfare losses borne by females who would otherwise be willing and able to compete in the absence of deceit, but are deterred by the possibility that their ultimate payoffs will depend on the honesty of others (i.e., their competition).

In a survey conducted at thirty-one highly selective universities, McCabe (1997) finds that academic dishonesty is prevalent in competitive fields with 96% of business students, and 90% of engineering students reporting having engaged in any type of cheating.[2] If females are more sensitive to competing against dishonest peers, they may be minimizing the utility losses associated with cheating by entering environments where their ultimate payoffs are not as dependent on the performance of others even when these payoffs may be lower.

In an experimental setting, we explore how the potential for cheating affects the decision to opt into competitive environments and how this potential affects the decision differently by gender. That is, are females more likely to opt out of competitive environments when they anticipate that their competitors may cheat? We find modest evidence that the possibility of cheating decreases the probability women select into

[2]At the graduate level, 56% of graduate business students report having cheated in the past academic year, compared to 43% of their nonbusiness counterparts. Across all majors, there are also gender differences in reported cheating, with males having engaged in cheating more often than females.

competitive environments. The same possibility has no discernible effect on the decision for men to compete.

In Section 2.2, we review the existing literature on gender difference in competitive behaviors and how we understand levels of cheating responding to competition. In Section 2.3, we describe the experimental design used to identify how the potential for cheating affects the decision to enter competition. In Section 2.4, we develop the empirical model and offer some discussion of results, followed by concluding remarks in Section 2.5.

Literature

Existing evidence on gender gaps in competitive behavior

A growing economics literature suggests that competition may have different implications for men and women in important ways. For example, Gneezy et al. (2003) suggests that the performance of men and women is differentially sensitive to competitiveness. Using experimental participants who solved a series of mazes under competitive and noncompetitive compensation schemes, they find that men respond to competitive environments by performing better, while women perform similarly in both regimes. This suggests a fairly particular way in which women lag men in performance. In fact, any measured difference in the performance of men and women that does not account

for the competitiveness of the environment in which they performed may fail to separately identify underlying "abilities" and the sensitivity of performance to competitiveness.[3]

The literature has also identified evidence of a potential best response to this differential sensitivity to competition, with several studies now having demonstrated a gender difference in the choice to compete. Experiment participants in Niederle and Vesterlund (2007) are employed in a simple addition task and are given the opportunity to select how they will be compensated for their performance. While they see no gender differences in performance in a competitive regime, where participants compete directly against each other, or in a noncompetitive regime, where participants are paid based only on their own performance, females are 16.2% percent less likely than men to opt into competition when participants are given the choice about which regime they would like to participate in. Because there is no observed gender difference in performance, Niederle and Vesterlund (2007) posit there should also be no gender difference in the propensity to opt into a competitive environment. In that sense, this may be interpreted this as evidence of females *sub-optimally* entering into competition. In the wake of Niederle and Vesterlund (2007), it is now well established that women then to avoid competitive environments.[4]

Recent evidence suggests that there is a context-specific component to explaining this gender gap. For example, Gneezy et al. (2009) examine choices to compete across two cultures with different social structures. While a patriarchal society in Tanzania (the

[3]Although this result is consistent with women not performing as well as men in competition, there is reason to believe the maze task is not gender neutral as men have historically performed better on tasks that focus on spatial abilities (Astur et al., 1998).

[4]See Croson and Gneezy (2009) for a full review.

Masai) exhibits a gender gap in the choice to compete that is similar to that found in Niederle and Vesterlund (2007), a matrilineal society in India (the Khasi) exhibits the opposite, as women choose into competition at higher rates than men. This suggests gender differences in competitive behavior may be shaped by societal stereotypes.[5] Wieland and Sarin (2012) also consider whether context matters to the choice to compete, testing experiment participants across four different subjects areas (verbal ability, crafts, math, and fashion). Participants are randomly assigned to one of the four domains and asked to select a competitive- or noncompetitive-payment scheme by which they will be paid. Although there is no significant gender difference in the decision to compete when examined across all domains, women tend to choose to compete at significantly higher rates than men in the fashion domain and, unlike previous research, the authors find no gender differences in the decision to compete in the math domain.

Given the specific studies mentioned above as well as the wider literature, experimental work on gender differences in competitive behavior suggest that when performance is objectively measured, women tend to compete less. In the following section we explore the existing literature on how gender differences and competitive behaviors are affected when ability to cheat is introduced.

[5]A matrilineal society traces decent through the mother's line thus giving a certain level of power to women. However, men can have more power in these societies. "...a society can for example be matrilineal (trace descent through the mother) and patriarchal (men have more power)." Cárdenas et al. (2012)

The Potential for Cheating

In most experimental designs, the potential for cheating is introduced by compensating subjects based on their self-reported performance. For example, participants in Fischbacher and Föllmi-Heusi (2013) are compensated for the reported outcome of a roll of a die, being paid more for higher-value rolls. Because the die is rolled privately, participants have the incentive to report higher values to the experimenter. With no method for collecting the true value, such a design identifies cheating only in an expected sense—through deviations from the expected distribution of a die roll.

Ensuring that true performance goes unobserved by the experimenter is common to the cheating literature. In a different design, participants in Mazar et al. (2008) are allowed to self-report performance on a simple matching task, and, at the end of the experiment, are asked to shred evidence of their actual tasks instead of returning them to the experimenter for verification. Like Fischbacher and Föllmi-Heusi (2013), this experimental design eliminates the potential matching of actual and reported performance at the individual level, and the level of cheating is therefore identified only through average differences in performance across treatment and control states.

In contrast to the previous two studies, Schwieren and Weichselbaumer (2010) and Rigdon and D'Esterre (2015) employ experimental designs that allow for individual-level identification of cheating. Employing the maze task used in Gneezy et al. (2003), participants in Schwieren and Weichselbaumer (2010) can cheat either by over reporting the number of mazes solved during the experiment or by using an "auto-solve" feature,

which can finish the maze automatically or verify that the path they are taking is correct. Spyware is used to track the true performance of participants along with any help they may have received from the computer program during the task.

Participants in Rigdon and D'Esterre (2015) complete the simple matching task of Mazar et al. (2008), but instead of shredding the task sheets, they are asked to discard evidence of their performance in a recycling bin and self-report their performance separately. To link true performance to reported scores for each participant individually, they match the "discarded" evidence to reported performance using invisible ink marks designated for this purpose.

With respect to the level of cheating, an experimental literature has established that participants will tend to over report their own performance on a given task when there are rewards to doing so (Gneezy, 2005; Rigdon and D'Esterre, 2015; Mazar and Ariely, 2006; Schwieren and Weichselbaumer, 2010).[6] This is consistent with the simple notions of rationality going back to Becker (1968). However, in contrast to the literature, Rigdon and D'Esterre (2015) do not find significant differences in the level of cheating between competitive and noncompetitive regimes.

Successful efforts to reduce cheating in this literature include moral reminders (such as reading the Ten Commandments) before completing tasks, as demonstrated in Mazar et al. (2008), and making the possibility of cheating more salient to participants. That

[6]It is important to note that in the majority of studies, participants are unaware of any consequence to cheating, however, participants over report performances only slightly higher than actual performance. This may indicate a moral aversion to cheating even in the absence of an explicit threat or consequence (Rigdon and D'Esterre, 2015; Mazar and Ariely, 2006; Fischbacher and Föllmi-Heusi, 2013).

is, cheating is reduced when an actor, placed in the experiment by the authors, performs the task unrealistically fast or publicly asks the experimenter if cheating is allowed (Gino et al., 2009). This result may provide further insights into our findings as discussed in the empirical section of this paper.

Experimental Design

As in several designs, e.g., (Mazar et al., 2008; Gino et al., 2009; Rigdon and D'Esterre, 2015), we open up the possibility that participants will cheat by compensating them based only on their self-reported performance. However, unlike previous research, our objective is to identify any proclivity participants may have toward avoiding competition when the expectation is that their *competitors* may cheat (i.e., that competition could be unfair). In particular, we are interested in testing whether men and women choose to compete differently when facing the presence of anticipated cheating in competitive environments. Do women shy away from competitive environments, in part, because they are more sensitive to the potential for unfair competition?

As a backbone for our experimentation, we follow the design of Niederle and Vesterlund (2007), but vary the reporting mechanisms in order to identify the effect of suspected cheating on the participant's propensity to enter competition.

General Design

The experiment was conducted in the University of Oregon's Social Science Instructional Labs, with participants recruited from the University's introductory

economics courses. Subjects were seated randomly in groups of two males and two females, and three to four groups participated in each session. The gender composition of the group was not discussed at any time during the experiment, though participants were able to see the other members of their group, making the gender composition of the group evident. At no time were participants able to see the computer screens or work areas of others, so we believe that participants were unaware of their relative performance at all times. All tasks during the experiment were completed on Qualtrics® surveys, licensed through the University of Oregon. In total, twenty-one groups participated in the experiment, yielding 42 female and 42 male observations.

Participants were given a $5 payment for showing up to the experiment and an additional $6 for completing it. At the beginning of the experiment, each participant was given an experiment ID consisting of a number that defined his or her group and a letter that was unique to his or herself. The experiment ID was used to ensure subjects receive their payment and to ensure that a participant's identity was not linked to his or her performance. Participants were told they would be completing four tasks and at the end of the experiment one task would be randomly chosen for payment.[7]

Following Niederle and Vesterlund (2007), we ask participants to solve a series of simple addition problems under various payment schemes; two assigned by the experimenter and two chosen by the participant.[8] The first task provides the

[7]As in Niederle and Vesterlund (2007) and the larger experimental literature using the expected utility hypothesis, randomly choosing one task for payment reduces the risk that participants alter their performance or choices in one task to compensate for decisions or performances in another.

[8]Previous research suggests for simple addition tasks, men and women have similar performances (Hyde et al., 1990).

noncompetitive payment scheme; subjects are paid a piece-rate of $0.50 for each problem solved correctly. The second task is completed under a competitive payment scheme, in which subjects are paid $2.00 for each problem solved correctly conditional on having solved more problems correctly than the other three members of their group. In a third task, prior to performing the addition task for a third time, participants are asked to choose which payment scheme they wish to apply to their performance. In the fourth task, they choose which payment scheme to subject a previous (Task 1) performance to.

Each task presents the participant with twenty-two addition problems each consisting of five randomly chosen integers between one and ninety-nine. Participants are asked to solve as many of these addition problems as they can in three minutes.[9] Participants are allowed scratch paper but are not allowed to use a calculator. At the end of three minutes, they are shown the correct answers to each addition problem, and a green check mark or red cross indicates to them whether they provided the correct answer. In Figure 1 we provide an example of four addition problems and the answer screen display shown to participants.

When the answer screen is displayed after each task, participants are asked to count the number of addition problems they have correctly solved during the three minutes. Following Niederle and Vesterlund (2007), subjects receive feedback regarding only their own performance and remain unaware of the performance of all other participants throughout the experiment. Participants are not told of the payment scheme applied to

[9]Niederle and Vesterlund (2007) allow five minutes per task.

FIGURE 1.
Addition Problem and Correct Answer Display

each task until just before the task begins. In the following section we will describe in detail these payment schemes.

Task Structure and Payments

The structure of the experiment is as follows:

Task 1 - Piece Rate:

The first task consists of twenty-two addition problems to be solved in three minutes. For each addition problem the participant solves correctly during this time, they will receive $0.50 if Task 1 is randomly chosen for payment.

Task 2 -Tournament:

The second task consists of twenty-two addition problems to be solved in three minutes. However, participants are now told they will be paid $2.00 for each addition

problem they correctly solve only if their performance is the highest among their group. Ties among the winners are randomly broken. If the participant does not have the highest Task 2 performance in the group and Task 2 is chosen for payment, the participant will receive no payment for the task.

Task 3 - Choice:

The third task consists of twenty-two addition problems to be solved in three minutes. However, before beginning the task, participants are asked to choose which compensation scheme they wish to be applied to their performance in Task 3. That is, participants choose a competitive or noncompetitive environment in which to complete Task 3. If Task 3 is randomly chosen for payment and the participant has selected the piece rate payment scheme, he or she will receive $0.50 for each correctly solved addition problem in Task 3. If Task 3 is randomly chosen for payment and the participant has selected the tournament payment scheme, the participant will receive $2.00 for each correctly solved addition problem in Task 3 only if his or her performance is the highest of other three members of the group in Task 2. Comparing a participant's performance in Task 3 to the performance of the other three members of the group in a completed task (Task 2) ensures that the decision to compete is not influenced by the Task 3 decision of the other members of the group. If the decision to compete was influenced by beliefs about other's likelihood of entering a competition, we would be unable to separate the true effect of gender on propensity to enter from gender differences in beliefs about who enters competitions and gender differences in altruism.

In our attempt to identify the gender gap in propensity to compete, we follow Niederle and Vesterlund (2007) in including a fourth task. Task 4 allows us to separate aversion to risk or feedback[10] that are common to all tournament environments from preference to perform under competitive pressures. While these factors like risk and feedback aversion are present in Task 3, Task 4 does not require the subsequent participation in a competition. Thus, as described by Niederle and Vesterlund (2007), any difference in propensity to enter the Task 3 tournament, controlling for the participant's decision in Task 4, can be attributed to differences in the preference to enter competitive environments and subsequently compete under competitive pressures.

Task 4 - Choosing a Compensation Scheme for a Past Performance:

Task 4 does not require participants to solve a series of addition problems like the previous three tasks. Instead, Task 4 asks participants to choose which payment scheme they wish to apply to their performance in Task 1 (the piece rate payment scheme). If Task 4 is randomly chosen for payment and the participant has selected the piece-rate payment scheme, the participant will receive \$0.50 per correctly solved addition problem in Task 1. If Task 4 is randomly chosen for payment and the participant has selected the tournament payment scheme he or she will receive \$2.00 per correctly solved addition problem only if his or her performance in Task 1 was the highest among the group. Participants will receive no earnings for this task if they select the tournament payment scheme and do not have the highest Task 1 performance in the group.

[10]Niederle and Vesterlund (2007) call these "general factors"

Although participants do not know their own performance, or that of their group members, it is reasonable to anticipate that their decisions to enter the tournament are likely driven by a measure of participants' assessment of their relative performance. We measure this by asking participants to complete a brief survey following Task 2, collecting their believed rank among the group for both Task 1 and Task 2. Participants can select ranks one through four, with one indicating the highest performance in the group. Participants are given $1.00 in addition to any earnings during the experiment if they correctly guess their Task 1 and Task 2 ranks within their groups.[11]

The ability to cheat

Following the literature (Gneezy, 2005; Rigdon and D'Esterre, 2015; Mazar and Ariely, 2006; Schwieren and Weichselbaumer, 2010), we allow for the possibility of cheating by allowing participants in the treated session to self-report their performances in tasks one through three. The experimenter verifies reporting only in the control group. Participants in treated sessions self-report their performance and are compensated based only on these reports. Thus, we can identify the effect of cheating on the decision to opt into competition by altering the way performance in each task is reported between the control and treatment groups.

Participants in the cheating regime are given three separate answer sheets on which to record their performance in tasks one through three. Participants are also given two

[11]This follows Niederle and Vesterlund (2007) in providing an incentive to accurately judge and report performance. The self-ranking provides a control for differences in believed rank by cheating regime and by gender.

pages on which to record their payment scheme choice for Task 3 and Task 4 and a page to record their believed performance rank for Task 1 and Task 2. An example of an answer sheet is displayed in Figure 2.

Answer Sheet Task 1: **Experiment ID #: _____**

Below is an example of the type of addition problem you will be solving. The task is to add the 5 numbers you see in the row and record the answer in the blank space provided to the right of these numbers.

Sample addition problem

25	33	1	99	42	

Below is a space to write the number of problems you have correctly solved. Your payment for this task will be based on the number reported if this task is randomly chosen for payment.

Number of correctly solved addition problems for Task 1: _____

FIGURE 2.
Example Answer Sheet

Although Qualtrics® records the participant's true performance, we require participants in the control group to count and record their performance in the same way as the treatment group. This assures that if there is any difference in competitive behavior resulting from exposure to one's true performance, it is equalized across treatment and control groups. Participants in the control group are asked to confirm the number of addition problems they have correctly solved after each task. The surveys were designed to not accept incorrect entries and the program will not move ahead until the score entered by the participant matches the score recorded by the program.

Because we are investigating the decision to opt into competitive environments when cheating is present, identification comes from the participant's understanding that others in their group could potentially over report. That is, it is not necessary for our analysis that participants cheat individually, only that they understand their competitors may be cheating. We increase the salience of the potential for participants to cheat in the treatment group by emphasizing that payments will be based only on self-reported performance. For example, the experimenter reads, "If Task 2 is randomly chosen for payment, participants will receive $2.00 for each addition problem solved only if their reported performance is higher than the self-reported Task 2 performance of the other members of the group."[12]

To ensure that participants do not alter their Task 3 payment scheme choice in response to their performance in Task 3, participants return their answer sheets with their Task 1 and Task 2 performance as well as their Task 3 payment scheme choice and believed rank for tasks 1 and 2 to the experimenter before beginning Task 3. This also ensures participants are unable to alter their Task 1 performance in response to the Task 4 payment scheme choice. After participants make their payment scheme choice for Task 4, they return both the answer sheet with their self-reported Task 3 performance and their payment scheme choice for Task 4 to the experimenter.

After all performances and choices are collected, participants in both control and treatment regimes are given a brief survey that asks basic demographic questions: gender, major, and age. The survey for the treatment group also includes questions

[12]The experiment instructions are reproduced in their entirety in Appendix B.

about the believed cheating behavior of other members of the group. These questions ask participants if they were aware cheating was a possibility, if the possibility of cheating influenced their Task 3 payment scheme choice, and if they believed the other members in their group inflated their performance. Results from these surveys and average performance levels are provided in Table 1. After receiving payment and returning the second survey, participants are free to leave the experiment. Each session lasted approximately 40 minutes and participants were paid on average $16.00.

TABLE 1.
Summary Statistics

	Mean	Std. Dev.	Min.	Max.	N
Female	0.50	0.50	0	1	84
Age	19.6	1.50	18	26	84
Task 1 Score	3.63	1.67	0	7	84
Task 2 Score	5.75	1.71	2	10	84
Task 2 - Task 1	2.12	1.67	-3	6	84
Task 2 Rank	2.06	1.00	1	4	84
Tournament 3	0.44	0.50	0	1	84
Tournament 4	0.19	0.40	0	1	84
Realize Cheating was Possible	0.90	0.31	0	1	20
Cheating Influence Task 3 Decision	0.58	0.50	0	1	48
Believe Other Participants Cheated	0.21	0.41	0	1	48

Notes: Averages are pooled across gender and treatment regime. Gender, age, and responses to cheating are gathered from demographic surveys given after all tasks have been completed.

Results

The effect of cheating on the propensity to enter competition

Although participants in our study recognized the ability to cheat for both themselves and the other member of their group (Table 1), we observe no cheating during our experiment. As noted in the previous section, it is not necessary to our analysis that subjects cheat however, the fact that we see no over reporting is a unique finding of our study. Gino et al. (2009) provide an insightful analysis that may provide an explanation for lack of cheating in our results by examining the effects of increased salience on cheating behavior. The authors find that increasing the salience of the possibility for cheating significantly decreased over reporting among participants.[13] Thus, based on this result, it is possible that our method of increasing the salience of the potential for participants to cheat in the treatment group reduces the level of cheating for the individual to zero.

We identify how the possibility for cheating, i.e., the over reporting of performance, affects the propensity to enter into competition without distinguishing the differential effect by gender. We estimate the model,

$$Tournament_{ij} = \alpha + \beta_1 CheatingRegime_j + \epsilon_{ij}, \tag{2.1}$$

[13]The authors argue that increasing the salience of the possibility of cheating can alter cheating behavior by altering the perceived likelihood of being caught, increasing the saliency of ethicality, or through "changing one's understanding of social norms related to dishonesty." In the second experiment of the study, a paid actor increases the salience of dishonesty by publicly asking the experimenter "So, is it OK to cheat?" The authors find that this significantly decreased cheating.

where $Tournament_{ij}$ is an indicator equal to one if participant i in regime j choses to subject their Task 3 performance to a tournament, $CheatingRegime_j$ is equal to one if the participant is in a session in which cheating is possible and it would thus be reasonable for participants to expect that their competitors will be cheating. Given our interest in considering heterogeneous treatment effects—interactions of $CheatingRegime$ with $Female$—point estimates are obtained from linear-probability models.[14] In all specifications we report robust standard errors.

In Column (1) of Table 2 the point estimate on $CheatingRegime$ from (2.1) suggests that participants in the cheating sessions are approximately 35% percent less likely to subject their Task 3 performance to a tournament, though this is imprecisely estimated ($p = .169$). Following Niederle and Vesterlund (2007), columns (2)-(5) allow for level effects in competitiveness by gender, ability, a measure of participants assessment of their relative performance, and any aversion they may have to risk, feedback, or any other factors that are specific to competitive environments but are not related to the participant's preference for performing under competitive pressures.[15] In the fully specified model of Column (5), the point estimate of the average effect of the having introduced cheating on the choice to compete is -0.08. This is large relative to the mean rate of tournament entry (0.44), but the confidence interval spans zero at conventional levels.[16]

[14]Refer to Subsection 4.2.1 for full explanation

[15]While true performance in the cheating regime is lower, there is no statistically significant difference in the underlying performance across regimes. Thus, it may not be surprising that our measure of ability has little explanatory power in predicting tournament entry. Assessments of relative performance reveal no significant differences across $CheatingRegime$.

[16]Results are robust to the inclusion of Task 3 score as an additional measure of ability.

In contrast to Niederle and Vesterlund (2007), we do not observe an underlying gender difference in the propensity to enter competitive environments.

TABLE 2.
Effect of Treatment on Decision to Opt into Competition-Linear Model

	(1)	(2)	(3)	(4)	(5)
Cheating Regime	-0.153	-0.153	-0.110	-0.090	-0.080
	(0.11)	(0.11)	(0.11)	(0.11)	(0.10)
Female		0.071	0.032	0.040	0.074
		(0.11)	(0.10)	(0.10)	(0.10)
Task 2 - Task 1			0.039	0.025	0.047
			(0.03)	(0.03)	(0.03)
Task 2 Score			0.089**	0.048	0.030
			(0.04)	(0.05)	(0.04)
Task 2 Rank				-0.128*	-0.102
				(0.07)	(0.07)
Tournament 4					0.309**
					(0.13)
Observations	84	84	84	84	84
Effect Size	0.306	0.306	0.220	0.180	0.160
Impact	-0.347	-0.347	-0.249	-0.205	-0.181
Mean (Treatment=0)	0.528	0.528	0.528	0.528	0.528
R-squared	0.023	0.028	0.175	0.211	0.261

Notes: The dependent variable is equal to one if the participant chooses tournament for the Task 3 payment scheme, and equal to zero otherwise. Reported are estimated coefficients from linear-probability models. Effect Size corresponds to estimated coefficient on *CheatingRegime*. Standard errors (in parentheses are robust to heteroskedasticity) *** significant at 1%; ** significant at 5%; * significant at 10%.

Does gender heterogeneity explain tournament entry?

Although the average effect of expectant cheating on the choice to compete is insignificant, our primary interest is in exploring potential gender heterogeneity in the

effect that expected cheating might have on the decision to enter competitions. If females are more sensitive to competing against dishonest peers, the avoidance of competitive environments could explain the persistent gender gaps we observe in these fields.

To identify how the marginal effect of being in the cheating regime differs by gender, we re-estimate Equation 2.1 focusing on the point estimate of the interaction term $CheatingRegime \times Female$. That is, we estimate the following model:

$$\begin{aligned} Tournament_{ij} = \alpha + \beta_1 CheatingRegime_j + \beta_2 Female_{ij} \\ +\beta_3(CheatingRegime \times Female_{ij}) + \epsilon_{ij} \end{aligned} \tag{2.2}$$

Before we consider this heterogeneous treatment effect across gender, we need to address our use of a linear probability model over a non-linear alternative such as a Probit or Logit specification.

Inference around Interactions in Nonlinear Models

We will first briefly address the issues inherent in using a linear model and then address the larger issue posed by interpreting the marginal effects estimated from a nonlinear model that includes interaction terms.

There are two main issues with using a linear probability model to estimate dichotomous choice models. The first issue is the violation of the assumption of

homoskedasticity of the error term. Consider the following regression model:

$$y = x'\beta + \epsilon, \tag{2.3}$$

where $x'\beta + \epsilon$ must equal 0 or 1. Because y is dichotomous, ϵ must equal $-x'\beta$ or $1 - x'\beta$. Thus,

$$Var[\epsilon|X] = x'\beta(1 - x'\beta), \tag{2.4}$$

i.e., variance of the error term depends on $x'\beta$. Although this violation of the homoskedasticity will not bias the point estimates, OLS is no longer the lowest-variance estimator. This inefficiency may lead us to incorrect hypothesis testing. To resolve this issue, we report heteroskedasticity-consistent robust standard errors for all specifications.

The second issue concerning linear probability models is that predictions made from these models are not constrained to be within the unit interval. As Wooldridge (2010) notes, while the nature of these models may make interpretation of the marginal effects of x difficult for predicted probabilities outside of $[0, 100]$, if the goal is to estimate the marginal effect of x on the probability that $y = 1$ at average values of x, using a linear probability model will give estimates fairly close to those estimated from nonlinear specifications.

Although we recognize the potential issues in using a linear probability model to estimate the marginal effect of treatment on the choice to enter into competitive

environments, these models have a distinct advantage over nonlinear specifications with respect to our ability to interpret the marginal effects of interacted variables.

Consider Equation 2.8 in which the possibility of cheating affects the decision to compete differentially by gender. Identifying the marginal effect of $CheatingRegime \times Female$ is key. While straightforward in a linear probability model, nonlinear models introduce complications. Consider this specification in a Probit framework, where

$$E[Tournament_i = 1|X] =$$

$$\Phi(\alpha + \beta_1 CheatingRegime_i + \beta_2 Female_i + \beta_3(Female \times CheatingRegime_i)) \quad (2.5)$$

The partial effect of the interaction term (i.e., how gender affects the marginal effect of CheatingRegime) is defined as

$$\frac{\Delta^2 E[Tournament_i = 1|X]}{\Delta_{CheatingRegime}\Delta_{Female}} =$$

$$\Phi(\alpha + \beta_1 CheatingRegime_i + \beta_2 Female_i + \beta_3(Female \times CheatingRegime_i))$$

$$- \Phi(\alpha + \beta_2 Female_i) - \Phi(\alpha + \beta_1 CheatingRegime_i) + \Phi(\alpha) \quad (2.6)$$

Unlike the linear model, the effect of a change in gender on the marginal effect of treatment could be non-zero even if an interaction term is not explicitly specified in the

model. That is,

$$\frac{\Delta^2 E[Tournament_i = 1|X]}{\Delta_{CheatingRegime}\Delta_{Female}} = \Phi(\alpha + \beta_1 CheatingRegime_i + \beta_2 Female_i) - \Phi(\alpha + \beta_2 Female_i) - \Phi(\alpha + \beta_1 CheatingRegime_i) + \Phi(\alpha) \quad (2.7)$$

From the equations above one can see that it is possible to conclude the interaction effect in Equation 2.6 is zero however, this does not require that the cross difference in Equation 2.7 is also zero.

Greene (2010) notes that while one can test the hypothesis that the interaction term is zero, the intuition behind this hypothesis is unclear for this reason. Greene (2010) also notes that any interaction effect we may find using these models will be at least partially a function of the nonlinear specification we have chosen and may not provide any further intuitive understanding of the economic significance of our variables of interest.

Due to the interpretation issues with interactions in a nonlinear model, we present our results from a linear-probability model of the form in Equation 2.8. Results are similar in sign and magnitude to those found using the nonlinear Probit specification and are presented in Appendix A.

Heterogenous effect of perceived cheating on tournament entry

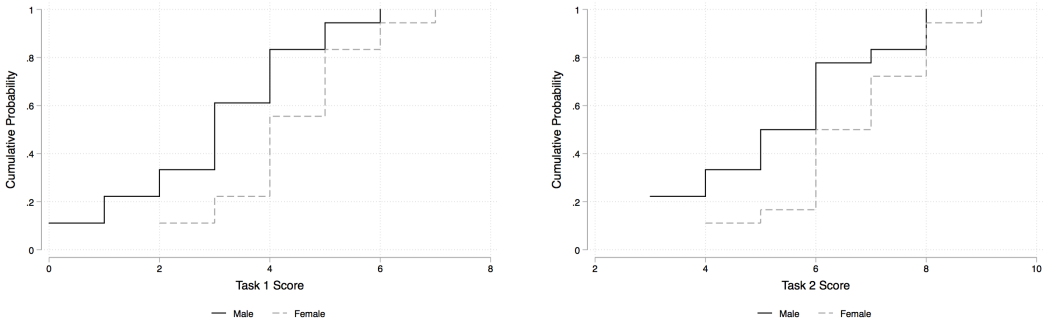
To reiterate, identification of the heterogenous treatment effect is provided by estimation of

$$Tournament_{ij} = \alpha + \beta_1 CheatingRegime_j + \beta_2 Female_{ij} + \beta_3(Female \times CheatingRegime_{ij}) + \epsilon_{ij} \quad (2.8)$$

In Table 3, we report the results from estimation of Equation 2.8. Column (1) of Table 3 points to the difference in the treatment effect between men and women. Whereas women are 27.8 percentage points more likely than men to enter competition in the control regime, the possibility of cheating reduces a female's probability of entering into the tournament by 36.1 percentage points. From the mean of 0.44, females are approximately 82% less likely to enter the tournament when exposed to the cheating regime. With a standard error of .22, the point estimate of interest is marginally significant at the 10% level.

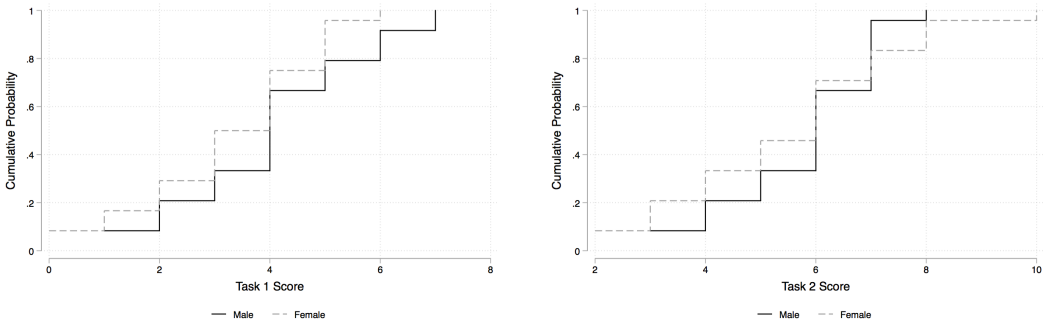
Although the magnitude of the heterogeneous effect in Column (1) is large, we expect this effect to be influenced by gender differences in performance, assessment of relative performance, and risk/feedback aversion. In Figure 3 we plot the CDFs for Task 1 and Task 2 performance by gender in the control group. Unlike the comparable performance between men and women found by Niederle and Vesterlund (2007) in the addition task, females in our control group perform significantly better than males in both

Task 1(piece rate) and Task 2 (tournament). When examining the performances on Task 1 and Task 2 for males and females in the treatment regime, as demonstrated in Figure 4, we find no statistically significant difference in the performances of males and females in Task 1 or Task 2.



Panel A: Piece rate (Task 1) Panel B: Tournament (Task 2)

FIGURE 3.
CDFs of Number of Correctly Solved Problems - Control



Panel A: Piece rate (Task 1) Panel B: Tournament (Task 2)

FIGURE 4.
CDFs of Number of Correctly Solved Problems - Treatment

Following our analysis in the previous section, we might also expect gender difference in beliefs about relative rank among the group, to influence the participant's choice. If

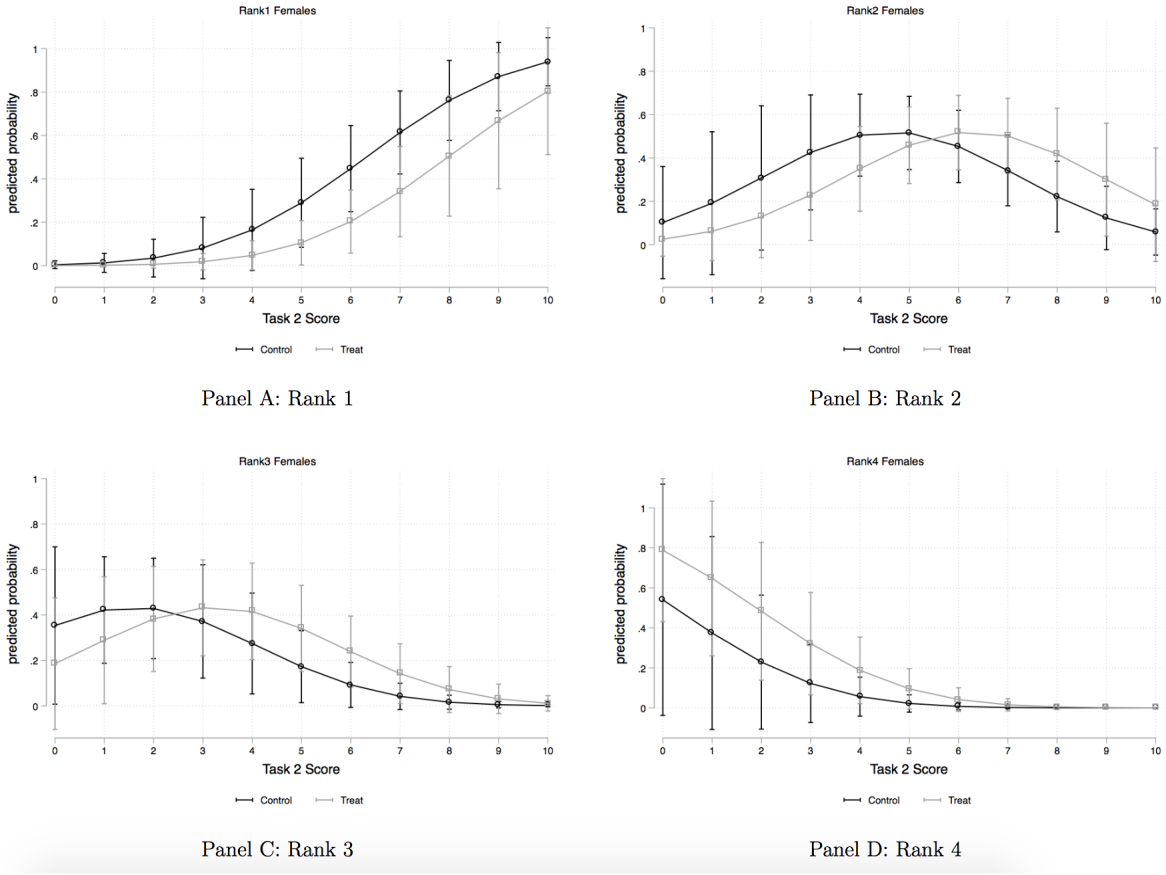
women's beliefs about relative rank among their group differ by treatment regime, failing to control for this would bias the point estimate of on $CheatingRegime \times Female$.

Figure 5 and Figure 6 demonstrate the potential for this type of omitted variable bias.[17] Panel A of Figure 5 suggests that females in control groups are more likely than females in the treatment regime to believe they are ranked first among the group for all Task 2 performance levels. Panel A of Figure 6 however, suggests that males in the treatment groups do not differ in their propensity to believe they are ranked first among the group for all Task 2 performance levels compared to the control regime. Although one should be cautious in concluding that the potential for cheating affects a the participants' assessment of their relative performance for males and females differently, as confidence intervals around the marginal effects include zero, we argue any suggestion of this gender difference merits controlling for believed Task 2 rank in the model.

Including the full set of controls (Column (4) of Table 3) increases the magnitude of the point estimate on $CheatingRegime \times Female$ slightly. After controlling for performance, believed relative rank, and risk/feedback aversion, females are approximately 50% less likely to enter the tournament when placed in the cheating regime.[18] Our results also suggest those who submit their Task 1 performance to the tournament payment scheme in Task 4 are, on average, 71.6% more likely to choose into the tournament than those who opt for the piece rate scheme in Task 4.

[17]Panels A through D in both figures plot the probability that a specific Task 2 rank is selected across possible Task 2 performance levels. That is, the probability males and females will report a Task 2 rank of one, two, three, or four controlling for actual Task 2 scores. Figure 5 plots these probabilities for females and Figure 6 plots the probabilities for the males by treatment regime.

[18]Results are robust to inclusion of Task 3 performance as an additional performance measure



Panel A: Rank 1

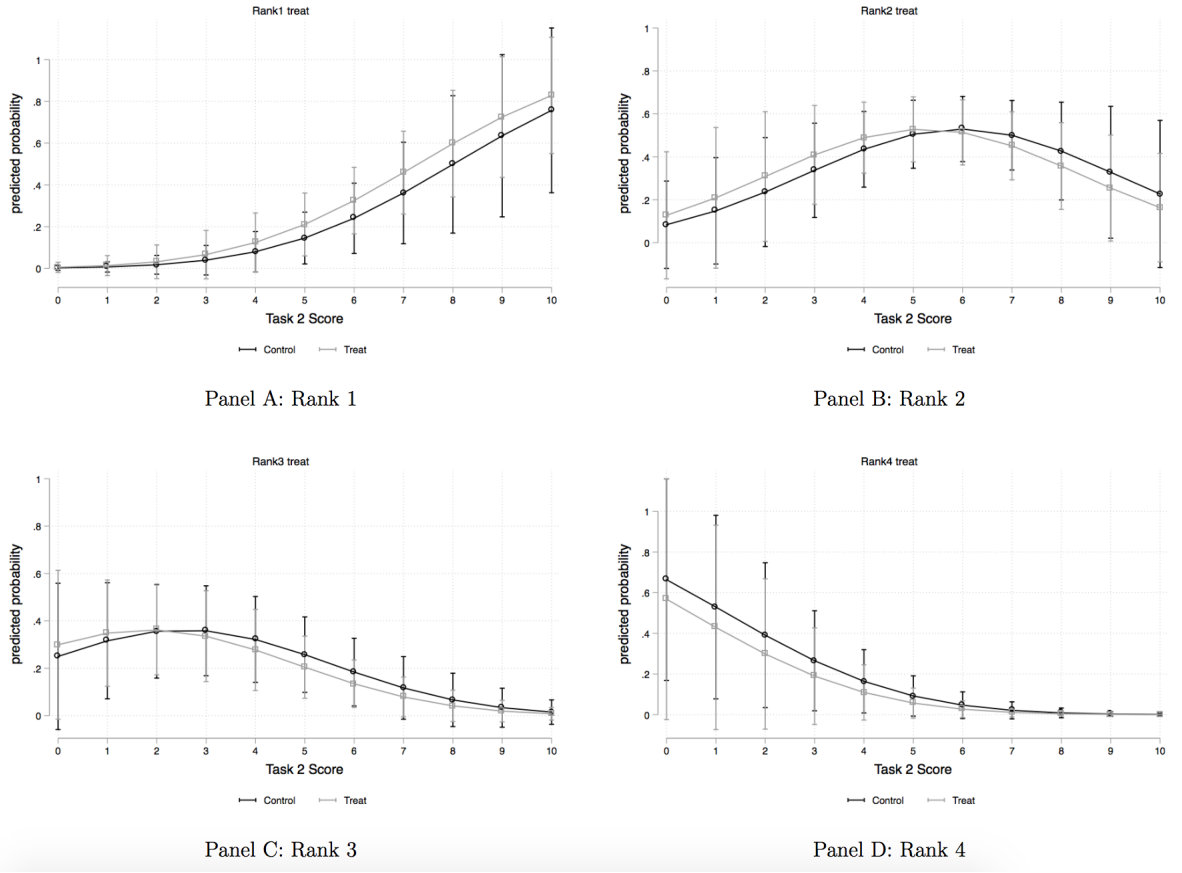
Panel B: Rank 2

Panel C: Rank 3

Panel D: Rank 4

FIGURE 5.
Probability of Reported Rank by Treatment: Females

Overall results suggest that the presence of cheating reduces female's propensity to enter into competitive environments and subsequently compete to a larger degree than their male counter parts. The magnitude of the effect remains relatively constant across columns (2)-(4) in Table 3. However, after controlling for performance levels, believed rank, and risk aversion, point estimates suggest females are more likely to enter into competitive environments and subsequently compete than males in the control group. This is in stark contrast to the findings of Niederle and Vesterlund (2007) however it is not without precedence (Wieland and Sarin, 2012). Although the sign of the point



Panel A: Rank 1

Panel B: Rank 2

Panel C: Rank 3

Panel D: Rank 4

FIGURE 6.
Probability of Reported Rank by Treatment: Males

estimates are as expected, we again reiterate the necessity of caution in drawing inference from these results as only the coefficient on the Task 4 choice is statistically significant at conventional levels.

Thus far in our analysis, we have assumed all confounding factors have the same marginal effect on entering into competitive environments for both the control and treatment regimes. However, it is reasonable to assume that the decision to submit one's Task 1 performance to a tournament maps into the propensity to compete differently

for control and treatment groups. Our fully specified model allows for this possibility by letting the marginal effect Tournament 4 differ by experimental regime.[19]

In Column (5) of Table 3 we provide the results of our fully specified model. Allowing the effect of Tournament 4 to vary by control and treatment regimes decreases the magnitude of the interaction $CheatingRegime \times Female$, but only modestly. Although statistically insignificant, the magnitude of our interaction term remains consistent across specifications. The point estimates on performance levels as well as believed rank remain small and statistically insignificant. The choice to enter one's Task 1 performance into a tournament is significantly less influential when we allow for the effect to vary between control and treatment groups. Whereas submitting to the tournament in Task 4 increases the probability of competing in Task 3 by approximately 53 percentage points in the control group, the effect is decreased to around 10 percentage points for those in the cheating regime. At a mean of .44, submitting one's Task 1 performance to a tournament increases the probability of entering competition in Task 3 by approximately 17.3% in the treatment group.

As described in Section 2, the choice to submit one's Task 1 performance to the tournament allows us to separately identify preferences for those factors common to competitive environments (e.g., risk aversion and feedback aversion) from differences in preference for entering tournaments and subsequently competing. The decision to enter competition in Task 3 requires participants to subsequently perform in the control regime

[19]Results are robust to allowing for the effect of all performance measures and rankings to vary by cheating regime

TABLE 3.
Heterogenous Treatment Effect by Gender-Linear Model

	(1)	(2)	(3)	(4)	(5)
CheatingRegime × Female	-0.361	-0.280	-0.202	-0.222	-0.233
	(0.22)	(0.23)	(0.23)	(0.23)	(0.22)
CheatingRegime	0.028	0.029	0.008	0.028	0.122
	(0.16)	(0.16)	(0.16)	(0.15)	(0.15)
Female	0.278*	0.196	0.157	0.203	0.189
	(0.16)	(0.18)	(0.18)	(0.17)	(0.16)
Task 2 - Task 1		0.051	0.035	0.058	0.044
		(0.04)	(0.04)	(0.04)	(0.04)
Task 2 Score		0.074*	0.042	0.022	0.019
		(0.04)	(0.05)	(0.04)	(0.04)
Task 2 Rank			-0.114	-0.085	-0.111
			(0.07)	(0.07)	(0.07)
Tournament 4				0.315**	0.529***
				(0.13)	(0.14)
CheatingRegime × Tournament 4					-0.453*
					(0.23)
Observations	84	84	84	84	84
Effect Size	0.723	0.562	0.404	0.445	0.466
Impact	-0.820	-0.637	-0.458	-0.505	-0.528
Mean (Treatment=0)	0.528	0.528	0.528	0.528	0.528
R-squared	0.061	0.193	0.219	0.272	0.301

Notes: The dependent variable is equal to one if the participant chooses tournament for the Task 3 payment scheme, and equal to zero otherwise. Reported are estimated coefficients from linear-probability models. Effect Size corresponds to estimated coefficient on $CheatingRegime \times Female$. Standard errors (in parentheses are robust to heteroskedasticity) *** significant at 1%; ** significant at 5%; * significant at 10%.

however, the decision to enter competition in the treatment regime requires participants to subsequently perform and choose whether or not to report that performance honestly. We might expect that the decision to submit a past performance to competition explains less

of the choice to enter competition in Task 3 when this decision now not only requires a the participant to then perform but also requires the participant to decide how truthfully to report that performance.

It is also possible that the decision to submit one's Task 1 performance to competition explains less of the choice to enter competition in Task 3 under the cheating regime as participants may expect their competitors to cheat more under competition. If this is the case, the decision to submit one's Task 1 performance to a tournament may be more closely related to participant's beliefs about their competitors true performance whereas the decision to compete in Task 3 may be based more on participant's beliefs about how their competitors over reported performance in Task 2 due to competitive pressures.

Together, Table 3 provides evidence that cheating reduces the probability of entering into competitive environments for women however, unlike Niederle and Vesterlund (2007), we do not find that women select into competition at a lower rate than men. After for controlling for performance levels, believed rank, and risk/feedback aversion, we suggest based on consistency of estimates that treatment decreases women's propensity to enter competition relative to the effect treatment has on males. The point estimate of the differential effect however, has a standard confidence interval that includes zero meaning we cannot definitively conclude that cheating affects propensity to enter competition differentially between males and females.

Robustness by Cheating Response

To ensure that our results are driven by the possibility of cheating, we estimated our main model allowing the heterogenous effect of treatment by gender to also vary by the participant's response to the question "When choosing your payment scheme for Task 3 (tournament or piece rate), did you consider the possibility that the other members in your group might over report their performance?" The point estimates gave counter-intuitive results as they suggest individuals who were influenced by the possibility of cheating were more likely to compete than those who said the possibility did not impact their Task 3 choice. These results are difficult to explain in light of the fact that no participant in our experiment actually cheated.

This oddity warrants controlling for the survey response questions to identify any influence they may have on the robustness of our result. In Column (3) of Table 4 we control for responses to whether or not the potential for dishonestly influenced the participant's decision in Task 3 and if the participant believed others in his or her group were dishonest in reporting performance. The point estimates on both $CheatingInfluence$ and $OthersCheat$ are insignificantly different from zero at conventional levels and the point estimate on $CheatingRegime \times Female$ remains consistent in sign and only slightly smaller in magnitude. Thus, controlling for believing that others over reported performance and reporting that this possibility influenced one's decision to compete slightly attenuates the point estimate on $CheatingRegime \times Female$ as expected.

TABLE 4.
Robustness to Cheating Response

	(1)	(2)	(3)
CheatingRegime × Female	-0.233	-0.227	-0.226
	(0.22)	(0.22)	(0.22)
Female	0.189	0.192	0.189
	(0.16)	(0.16)	(0.17)
Cheating Regime	0.122	0.089	0.094
	(0.15)	(0.16)	(0.16)
Task 2 - Task 1	0.044	0.044	0.044
	(0.04)	(0.04)	(0.04)
Task 2 Score	0.019	0.017	0.016
	(0.04)	(0.04)	(0.04)
Task 2 Rank	-0.111	-0.110	-0.113
	(0.07)	(0.07)	(0.07)
Tournament 4	0.529***	0.532***	0.531***
	(0.14)	(0.14)	(0.14)
CheatingRegime × Tournament 4	-0.453*	-0.446*	-0.442*
	(0.23)	(0.23)	(0.23)
CheatingInfluence		0.048	0.070
		(0.13)	(0.13)
Others Cheat			-0.083
			(0.16)
Observations	84	84	84
Effect Size	0.466	0.454	0.452
Impact	-0.528	-0.515	-0.513
Mean (Treatment=0)	0.528	0.528	0.528
R-squared	0.301	0.302	0.304

Notes: The dependent variable is equal to one if the participant chooses tournament for the Task 3 payment scheme, and equal to zero otherwise. Reported are estimated coefficients from linear-probability models. Effect Size corresponds to estimated coefficient on $CheatingRegime \times Female$. Standard errors (in parentheses are robust to heteroskedasticity) *** significant at 1%; ** significant at 5%; * significant at 10%.

Discussion

This experiment provides additional insights into why we see gender gaps in highly competitive environments. A large body of work citing gender differences in preferences for competition and risk, ability under competitive environments, general confidence in performance, and societal norms has established that women are generally less likely to choose into competition than men. We attempt to further explain the gender gap by exploring how women and men opt into competitive environments differently when those they are competing against have the potential to cheat.

Our findings add to the literature in two ways. First, as the control regime of our experiment is as replication of Niederle and Vesterlund (2007), where large gender differences in preference to enter competition are found (with women being significantly less likely to enter even after controlling for ability, risk aversion, and believed relative rank), we do not find this result in our specification. Although the main effect of being female on entering competitions in the control group is not statistically different from zero at conventional levels, the point estimate is positive and relatively consistent across specifications, which suggests that, if anything, women are more likely than men to enter competition.

The second contribution is in the identification of how the threat of cheating differentially affects males and female's propensity to enter competitive environments. We find modest evidence that the threat of cheating and/or the ability to cheat reduces the propensity to enter into competition, and that women are more sensitive to this

than are men. This finding adds to our understanding of the gender gap in competitive environments and particularly in environments where performance measures are easily manipulated.

It is worth considering the robustness of our results to varying levels of increased salience of cheating. As described in Section 2.2 of our paper, Gino et al. (2009) find that increased salience of the possibility of cheating reduces the level of cheating during the experiment however, they do note theoretically that increased salience could potentially change "the name of the game" and thus increase cheating among all participants. The subtlety with which we introduce the possibility to cheat (by simply emphasizing the self-reported nature of performance), we believe, reflects reality more closely than an obvious invitation to cheat. To confirm that our results and observations from this experiment follow the findings of Gino et al. (2009), future sessions will introduce the ability to cheat with more salience. If results continue to suggest that women reduce their propensity to enter competition in the presence of cheating, attributing gender-differentials in part to the threat of cheating will be further justified.

CHAPTER III

PRESCRIPTION DRUG MONITORING PROGRAMS AND THE ABUSE OF PRESCRIPTION DRUGS

Introduction

Drug overdose is the leading cause of accidental death in the United States. Since 1999, rates of overdose death, drug-treatment admissions, and prescription-drug sales have increased by nearly four times, with prescription drugs now accounting for roughly 40 percent of overdose deaths.[1] Prescription-drug abuse began to escalate in the late 1990s—a time when state medical boards were moving toward relaxing restrictions on prescribing opioids for the treatment of chronic pain. Over the same time period, new pain-management standards began to focus on the patient's right to pain reduction, adding pain to a physician's standard checklist along with blood pressure, heart rate, temperature, and respiratory rate.[2] This, along with aggressive marketing and promotion of opioid pain relievers by pharmaceutical companies, physicians significantly increased the number of prescription pain relievers prescribed to patients (Manchikanti et al., 2012). In 2010, the National Survey on Drug Use and Health reported that the second-most-

[1]Center for Disease Control and Prevention, National Center for Health Statistics, National Vital Statistics System, Mortality File. (2015). Number and Age-Adjusted Rates of Drug-poisoning Deaths Involving Opioid Analgesics and Heroin: United States, 2000–2014. Atlanta, GA: Center for Disease Control and Prevention. Available at `http://www.cdc.gov/nchs/data/health_policy/AADR_drug_poisoning_involving_OA_Heroin_US_2000-2014.pdf`(last visited Nov. 1, 2016.)

[2]In 2016, the American Medical Association passed a resolution recommending that pain be removed as a vital sign.

commonly abused illicit drug—second to marijuana—was opioid pain reliever, with one-in-six users indicating that they received the drugs though a physician.[3]

In an attempt to curb growing opioid pain reliever misuse, federal and state governments have implemented legislation and allocated funding to various programs targeting the supply and demand side of the prescription opioid market. While Prescription Drug Monitoring Programs (PDMPs) had been established in many states prior to the onset of the opioid epidemic, they have since been promoted by the CDC as some of the best defenses against the impending crisis.[4] Currently, 49 states—all but Missouri—now have PDMPs (Islam and McRae, 2014). Although the specific elements of PDMPs vary widely by state—considering this heterogeneity will be my focus—these programs provide, at a minimum, an electronic database through which information is collected about patients, drugs being prescribed, and prescribing physicians. Access to these systems allows for the observation of patient-specific prescription histories, with the potential (in some states) to preempt or otherwise disrupt legal misuse, illicit acquisitions, and the reselling of prescription drugs.

Existing literature on the effectiveness of PDMPs have found little-to-no benefit associated with their broad introductions (Haegerich et al., 2014). Yet, PDMPs have varied quite widely in their implementations, with "best practices" around formal requirements and practices being slow to develop. PDMP policies are often passive and

[3]For additional consideration of the increase in abuse and in state responses, consider Jones et al. (2015); Warner et al. (2011); Compton et al. (2015); Delcher et al. (2016).

[4]Roughly 60 percent of implementing states do not mention "overdose" or related terms in their stated objectives or missions statements (Green et al., 2015).

far removed from patient interactions, so much so that passing a broad efficacy test would be surprising. For example, while all states require pharmacists to report prescription information to a database, some states have limited access to the database to law-enforcement agencies. In this context, then, I address the potential that there are yet gains associated with well-designed monitoring programs, exploring the efficacy of several component practices. This ultimately reveals that those states that require consultation with the database somewhere within the process of prescription and dispersement are more effectively curbing problematic-prescribing behavior than are states that do not make such a requirement, where they will typically require operating agencies only to *ex post* identify suspicious prescribing and use. In the end, I offer the strongest evidence to date of the potential for the most-aggressive PDMP-type policies to decrease opioid-related treatment admissions, so much so as to prescribe a potential "best practice."

In Section 3.2, I consider the related literature, where the story has thus far been somewhat discouraging in so far as opioid-related treatments and overdose deaths have not declined with PDMP implementation in general. In Section 3.3, I describe the broad patterns in the implementation of PDMSs, and consider the efficacy of specific program attributes. Recent evidence suggests the existence of gains when considering specific PDMP designs. I develop my empirical specifications and report results in Section 3.4. It is in this section that I establish the efficacy of monitoring programs—in a way that will be consistent with priors—and consider where in distributions of intensity and tenure of use the declines are arising. In short, I will look for missing mass in the distribution of

treatment admissions across intensity and tenure of use, and argue that gains are coming from among less-frequent and newer users, in states with the most-aggressive PDMP policies. Before concluding, I will also consider the fallout from PDMP implementation on overdose deaths. In Section 3.5, I summarize with a discussion of policy prescriptions.

Literature and Policy Background

While the literature related to prescription drug monitoring programs is growing, previous evaluations have focused largely on the impact of the existence of these databases on various outcomes such as prescribing behavior and patient health. In evaluating physician response to these programs, indications of database use predominantly come from voluntary surveys used to gauge, additional information they may provide, and barriers to use. Using a nationally representative survey of providers, Rutkow et al. (2015) suggest several barriers to physician use of these databases including difficulty navigating the format of programs and the time consuming nature of accessing the database. These barriers may explain low use by prescribing physicians. In an anonymous survey given to prescribers in Connecticut and Rhode Island, Green et al. (2012) finds PDMPs slightly influence physician behavior, specifically when the programs were electronically available though increase drug abuse screening, and substance abuse treatment referrals.

In evaluations using state aggregates, Simeone and Holland (2006) finds a slight decrease in prescriptions among physicians, consistent with physicians reacting to the regime change in the desirable way. However, evidence on the number of prescription

opioids dispensed are less conclusive with Brady et al. (2014) finding a reduction in per capita morphine milligram equivalents associated with PDMPs only after 2008.

The effectiveness of these programs on health outcomes is mixed, with estimates suggesting that these programs in general do not significantly affect drug-overdose rates and may have only small negative effects on drug-treatment admissions. Pacula et al. (2015) evaluates the effect of Medicare Part D introduction on substance abuse treatment admissions and overdose deaths and, in doing so, includes a control for state level PDMPs. The authors find insignificant point estimates of these program indicators suggesting these programs have no effect on the outcomes of interest.

In a difference-in-differences framework, both Radakrishnan (2013) and Paulozzi et al. (2011) directly explore the effect of PDMPs on health outcomes. Paulozzi et al. (2011) finds no significant effect on drug-related mortality or overdose rates, while Radakrishnan (2013) finds small negative effects on opioid-related treatment admissions and reported drug use associated with states having any form of these programs. After controlling for potentially confounding state laws addressing drug abuse, Radakrishnan (2013) finds no significant effects of the existence of these programs on drug-related mortality. Li et al. (2014) and Reifler et al. (2012) find opposite effects, with PDMPs being associated with increases in treatment admissions, poison center exposures, and mortality, while Maughan et al. (2015) finds no association between PDMP exposure and opioid-related emergency room visits. In a more recent analysis, Kilby (2016) explores a wider variety of health outcomes finding that the implementation of a PDMP post 2003

reduces overdose deaths through a reduction in prescribing of opioids however, this supply side restriction also leads to more invasive and expensive pain management techniques as well as more work days missed among injured workers.

While the majority of existing evidence suggests that the effects of these informational databases on the epidemic of opioid abuse are small, previous literature has by and large not accounted for the substantial variation in the attributes of PDMP across states or for the potential that there are offsetting effects on the intensive margin of use. While many public health researchers have indicated the need for detailed evaluation of PDMPs, few empirical studies have addressed individual characteristics of these programs (Griggs et al., 2015; El Burai Félix and Mack, 2014). Of course, evaluating PDMPs without regard for the cross-state variation can hide the promising effects of specific practices. Amid somewhat discouraging patterns in the aggregate, I contribute to supporting a "best practice," of a sort.[5]

Specifically, I will report on two areas of entity access—access requirements around the database by the prescribing entity, and access requirements around the database by the operating agency. To begin, my priors suggest that "must-access" provisions of a PDMP may be the most effective in curbing abuse. A "must-access" provision requires prescribers to check the database before prescribing opioids to patients.[6] If the largest

[5]With 49 states now players in this policy environment, I will collapse my reported analysis to where there is systematic variation in outcomes, which ends up being around the most-aggressive PDMP attributes, arguably. In unreported analysis, I have considered a much broader array of attributes, finding no systematic movement in outcomes through my identifying variation.

[6]Although I suggest this attribute is the most restrictive in terms of prescribing behavior, subjectivity in implementation remains for this category and thus may attenuate results

impacts of supply side restrictions come from greater information to the physician at the point of prescribing, i.e., at the point of physician-patient contact, I expect states with the "must-access" provision to show the largest declines in opioid related abuse. A less stringent but more-common attribute of state PDMPs is the ability for physicians to access the program's data if they wish to, but with no requirement to do so before prescribing (as would be required under the "must-access" provision above). For example, PDMPs that allow physician access may more-directly affect prescribing behavior than those programs that restrict access of the database to non-prescribing entities such as law enforcement. Although this is a less binding requirement, if access to the database changes prescribing behavior at the point of physician-patient contact, states with this provision may find PDMPs more effective in curbing abuse.

While the attributes of PDMPs described above have the potential to directly affect the decision to prescribe opioids at the time of contact with a patient, states also vary in their requirements that the agencies operating the PDMPs actively check the databases for suspicious prescribing and usage behavior. This requirement is post prescribing, and thus should not directly interfere at the point of prescribing between a physician and patient. Although my priors suggest this requirement may have less of an impact that those that bind at the point of physician-patient contact, required checking of the database may identify problem prescribers and users leading to reductions in overall opioid sales. In a similar vein to above, proactive checking of the database can instead be at the operating agency's discretion (i.e., proactive checking is not required but is allowed). This is a less

binding requirement on the operating agency however, if agencies sufficiently check the database, it may simply be the ability to check which becomes the most effective attribute of a state PDMP.

Patrick et al. (2016) also considers PDMPs at the attribute level, and finds larger reductions in opioid-related overdose deaths in states that monitored at least four drug schedules and updated reported drug information at least weekly. In an evaluation of opioid abuse revealed through Medicare claim patterns, Carey and Buchmueller (2016) find reductions in misuse associated with states that require prescribing entities to consult the database when issuing prescriptions under certain conditions. Both studies suggest there are gains to be found in specific attributes of PDMPs. I evaluate those programs with the most-binding requirements for physicians and then evaluate those programs that are less stringent in prescriber expectations to determine which program designs are most effective in reducing opioid misuse and overdose death.

Data

There are four sources of data brought together in the consideration of PDMP implementation and any resultant effect on opioid-related treatment and death.

PDMP implementation

Our independent variables of interest—we will be considering the "effective date" of each state's PDMP between 1998 and 2012 as well as the attributes of these programs—are obtained from the Prescription Monitoring Program database curated by Corey

Davis at The Network for Public Health Law and the PDMP Center of Excellence at The Heller School for Social Policy and Management at Brandeis University.[7] The effective dates used in this analysis are the date the statue establishing a prescription drug monitoring program was put into effect.[8] Given the potential lag between effective date and the associated policies actually being administered and/or fully implemented, measurable efficacy may not be immediate. Moreover, estimates may be attenuated to the extent resources are slow to respond to the policy change. Although a small number of states had passed legislation establishing PDMPs prior to 1998 and thus will not add to identification of the effect of the existence of a PDMP, they will provide identifying variation in considering the efficacy of post-1998 amendments.

Treatment admissions

The Treatment Episodes Data Set (TEDS) is publicly available through the Substance Abuse and Mental Health Services Administration. Collected annually, the TEDS provides information on the number of drug-treatment admissions for all treatment facilities that receive public funding, whether though federal block grants, Medicare/Medicaid payments, or state funds. Privately operated treatment facilities that do not receive public funding do not contribute to the dataset, and will therefore not identify the patterns of behavior I report. Each observation in the dataset is an admission

[7]LawAtlas. The policy surveillance portal [Internet]. Philadelphia (PA): LawAtlas. Available from `http://lawatlas.org/query?dataset=corey-matt-pmp` (last visited Mar. 1, 2016.)

[8]Although operational status of the PDMP may not occur immediately after the legislation goes into effect, conducting analysis of specific program attributes will address some of the concerns that the PDMP is not immediately effective.

to a drug-treatment facility, and the same individual may therefore contribute multiple observations to the dataset. Recorded with each admission are personal characteristics of the individuals seeking treatment including the primary, secondary, and tertiary substances abused, frequency and tenure of each user's engagement with the substance, age categories, method of payment, demographic information, and treatment setting (i.e., ambulatory, detoxification, rehabilitation). Given this information, I am able to directly analyze the effects of the PDMP on opioid related drug-treatment admissions and to identify the potential differences in selection into prescription-drug abuse based on addiction use and tenure. In addition, the TEDS allows for separate identification of treatment admissions based on the referring party. A full 60 percent of treatment admissions are from individuals seeking treatment independently or though a criminal referral, which will enable identification by referral type.

In Table 5, I present summary statistics for drug-treatment admissions in the years 1998-2012. The average number of opioid-related treatment admissions per 100,000 state residents during this time period is 71, with substantial variation given a standard deviation of approximately 80 admissions per 100,000 state residents.[9] Admissions reporting alcohol abuse are most common with an average of 505 alcohol related treatment admissions per 100,000 state residents.

[9]Because the TEDS treatment admission data restricts ages to those older than 11 years of age, I use the population over the age of 10 in a given state year to calculate the rate per 100,000 residents.

TABLE 5.
Summary Statistics

	Mean	Std. Dev.	Min.	Max.	N
Panel A: Drug-treatment admissions					
Total ($\times 10^3$)	34.76	49.83	.177	314.56	734
Opioid Related	3.05	4.42	1	43.95	734
Heroin Related	6.16	13.38	0	80.38	734
Marijuana Related	13.52	17.49	.11	126.23	734
Alcohol Related	22.30	32.75	135	236.19	734
Opioid Rate*	71.80	80.3	0.02	571.97	734
Heroin Rate	100.55	152.42	0	781.69	734
Marijuana Rate	298.72	164.62	2.63	972.56	734
Alcohol Rate	505.69	355.74	3.16	2073.78	734
Panel B: State demographics					
Total Pop ($\times 10^7$)	5.96	6.53	.49	37.99	734
% Pop Black	0.11	0.09	0.00	0.37	734
% Pop White	0.83	0.13	0.24	0.98	734
Median Income	46,194	8.10	27.67	71.84	734
Medicaid Enrollment	881.82	1,220.66	34.58	8513.32	734
Medicare Enrollment	852.86	870.12	38.23	5126.61	734
Unemployment Rate	5.61	2.08	2.3	13.7	734
Treatment Centers	290.68	295.51	9	1822	734
Pharmacies	837.63	886.51	30	4591	734
Panel C: Drug-related legislation					
Established PDMP	0.53	0.49	0	1	734
Doctor Shopping Law	0.48	0.50	0	1	734
Naloxone Availability	0.04	0.19	0	1	734
Pain Clinic Law	0.03	0.16	0	1	734
Medical Marijuana	0.19	0.39	0	1	734
Patient ID Law	0.28	0.45	0	1	734
Year	2005	4.32	1998	2012	750

Notes: * = per 100,000 residents over 10 yrs old.

Opioid-related categories reported in the TEDS include, "Non-prescription methadone," "Heroin," and "Other opioids and synthetics."[10] I adopt "other opioids and synthetics" as my dependent variable (referred to simply as opioids in the rest of the paper), which includes all commonly prescribed opioid pain killers recorded in state PDMP databases. Although substances not commonly prescribed by physicians are included in this category, the TEDS does not allow one to separate these substances from drugs PDMPs commonly target thus I am unable to avoid potential attenuation introduced by this grouping. I do not include non-prescription methadone in this analysis, as methadone is often dispensed from opioid treatment programs (OTPs) which fall under federally assisted drug abuse programs and are thus not allowed to report to PDMPs.[11] I also do not include heroin treatments, as restricting access to prescription drugs may shift users into heroin, potentially hiding any gains being made by PDMPs in curbing prescription-drug abuse.[12]

[10] "Other opioids and synthetics category includes" includes buprenorphine, codeine, Hydrocodone, hydromorphone, meperidine, morphine, opium, oxycodone, pentazocine, propoxyphene, tramadol, and any other drug with morphine-like effects.

[11] Certification of Opioid Treatment Programs (OTPs), SUBSTANCE ABUSE & MENTAL HEALTH SERVS. ADMIN., `http://www.samhsa.gov/medication-assisted-treatment/opioid-treatment-programs` (last visited Oct. 20, 2016.)

[12] Likewise, such substitution may have one overestimate the true benefit to human health associated with PDMP attributes and reductions in opioid-treatment admission. Formal analysis of this is ongoing, and will appear in "The Heroin Epidemic: Is There a Role for Supply-Side Restrictions on Prescription Drugs?"

Drug-related deaths

As a measure of drug-relate mortality I use data obtained through the restricted-use National Vital Statistics System (NVSS), which records the census of deaths in the United States from the Centers for Disease Control and Prevention. I evaluate the effect of PDMPs on opioid-related mortality, including accidental death, suicide, and undetermined intent by state of residence and year, using the International Classification of Diseases codes (ICD-10) external cause of injury codes.[13]

Unlike the TEDS, NVSS reports opioid-related deaths cause by natural and semi-synthetic opioids (e.g., oxycodone and hydrocodone), as well as fully synthetic opioids excluding methadone (e.g., fentanyl and tramadol).[14] Using this distinction, I can separately identify the effect of PDMPs on natural and semisynthetic opioids (referred to as opioids in Table 14), and on fully synthetic opioids.

Controls

We obtain state-year population data from the National Cancer Institute's Surveillance Epidemiology and End Results (Cancer-SEER) program as well as median

[13]X40-X44, X60-64, X85, or Y10-Y14

[14]Drug overdose deaths involving opioids are identified using International Classification of Diseases, Tenth Revision underlying cause-of-death codes X40-X44, X60-X64, X85, and Y10-Y14 with a multiple cause code of T40.0, T40.1, T40.2, T40.3, T40.4, or T40.6.

Opioids include drugs such as morphine, oxycodone, hydrocodone, heroin, methadone, fentanyl, and tramadol.

For each type of opioid, the multiple cause-of-death code was T40.1 for heroin, T40.2 for natural and semisynthetic opioids (e.g., oxycodone and hydrocodone), T40.3 for methadone, and T40.4 for synthetic opioids excluding methadone (e.g., fentanyl and tramadol). Deaths might involve more than one drug thus categories are not exclusive.

household income and unemployment measures from the Bureau of Labor Statistics. In addition to these, I control for state level Medicaid and Medicare enrollment from the Centers for Medicare & Medicaid Services and supply of treatment centers and pharmacies by state year from the U.S. Census Bureau's County Business Patterns (CBP).

Although PDMPs are the focus of this analysis, I follow Radakrishnan (2013) in controlling for other potentially confounding state legislation affecting access to and use of prescription opioids. These include doctor shopping laws, regulation of pain clinics, medical marijuana legalization, patient identification laws and authorization for the use of Naloxone in preventing overdose. Effective dates for these alternative laws come from the CDC's Public Health Law Program.[15]

Empirics

In the sections below, I consider the efficacy of PDMP broadly. The empirical identification strategy will then be used throughout the analysis to follow, as I consider specific program attributes and the underlying pattern of efficacy across measures of use.

Do PDMPs matter to treatment admissions?

Given the variation in the timing of adoption of PDMPs by states, I adopt a difference-in-differences approach to separately identify the causal impact of program implementation on substance-abuse treatment admissions, and on overdose deaths.

[15]Effective dates of medical marijuana legalization are collected from the National Conference of State Legislatures

Specifically, I will estimate as a baseline specification,

$$Y_{st} = \alpha + \beta_1 X_{st} + \beta_2 PDMP_{st} + \gamma_s + \delta_t + \epsilon_{st}, \tag{3.1}$$

where Y_{st} is the log of state aggregate treatment admissions citing opioid abuse in state s in year t. In subsequent specifications, I will also consider two contributing paths—criminal referrals and self-referred admissions. State (γ_s) and year (δ_t) fixed effects can be included in all specifications to account for unobserved time invariant heterogeneity across states and for nationwide drug abuse campaigns. However, in my preferred specifications I will include state-specific trends. Importantly, if states with high drug-abuse rates adopting PDMPs, β_1 will be biased. Bias would also result from states tending to implement these programs differentially in response to increases in drug-abuse rates. Because of these potential biases, my preferred specification will incorporate state-specific time trends in identifying the effect of PDMPs on outcomes. I control for observable state-level heterogeneity with X_{st}, including controls for population, age and racial compositions, yearly unemployment rate, and Medicare/Medicaid enrollments. Given the potential to misidentify the effect of PDMP as related policies vary across states and time, I also control for state-level medical marijuana laws, and various other laws defined by the CDC as intended to reduce prescription-drug abuse (e.g., photo-identification requirements, Naloxone availability, and pain clinic regulation). The parameter of interest, $\hat{\beta}_2$, can be interpreted as the effect of state-level adoption of a

PDMP on treatment admissions. ϵ_{st} is a random-error term robust to heteroskedasticity, which I estimate while allowing for state-specific clustering.

As a first pass at the evaluation of PDMPs, I consider the average effect of establishing a PDMP on opioid-related treatment admissions. Although previous literature has found only small effects in similar specifications, I bring three additional years of data to the analysis, which is of particular interest as the recent uptick in heroin overdose has been associated by some with an increase in the cost of acquiring prescription drugs (Volkow, 2014).

In Panel A of Table 6, I report the coefficient estimates that capture the average effect of establishing a PDMP on the log of state-aggregate treatment admissions citing opioid abuse. In Column (1), I report the estimates of an OLS model of the form of Equation (1), controlling for year-fixed effects and time-invariant state heterogeneity. This model suggests that opioid-related treatment admissions increase approximately 14 percent with PDMP, though not significantly different from zero, statistically. Controlling for differences in state-specific trending of treatment admissions (in Column (2)) the point-estimate falls in magnitude, remaining insignificant.

In Column (3), I add demographic controls and other potentially confounding prescription-drug legislation.[16] With their inclusion, the magnitude of the effect of PDMPs

[16]These controls include: total state population, percent of the population who is black, percent of the population who is white, median income, Medicaid/Medicare enrollment levels, unemployment rate, number of drug treatment centers, number of pharmacies, and indicators for whether or not the state has one the the following drug related laws in effect: doctor shopping laws, Naloxone availability law, pain clinic regulation laws, required patient identification laws, and if medical marijuana has been legalized.

on opioid-related admissions remains small and I am unable to reject that the effect of a PDMP on opioid-related treatment admissions generally is zero.

We follow the same pattern of reporting when separately considering criminal and self referrals, in panels B and C of Table 6. Although opioid-related criminal referrals are increased by approximately 21 percent given the establishment of a PDMP in Column (1) of Panel B, the effect is not robust to controlling for state-specific time trends. A similar pattern is evident in self referrals (in Panel C). Although Radakrishnan (2013) finds small negative (though insignificant) effects of PDMPs on opioid admissions, my findings are largely consistent with the existing literature suggesting that PDMP implementation has little if any effect on drug-related health outcomes.

Program attributes

While PDMPs at a minimum require pharmacists or prescribers to report to the database, the effect these programs have on reducing opioid abuse vary substantially across the characteristics and requirements of the state-specific mandates. I evaluate these programs separately as described in Section 3.2; first addressing programs with the most-aggressive requirements on prescribers and operating agencies. I then subsequently add variation from less-aggressive programs to identify those attributes which affect opioid abuse as reflected in drug-treatment admissions.In all cases, I present varieties of specification, arriving at my preferred specification while showing the roles being played by state-specific trends, demographic controls, and other drug-related legislation.

TABLE 6.
Existence of PDMP and Opioid-Related Treatment Admissions

	(1)	(2)	(3)
Panel A: Aggregate Treatment Admissions			
PDMP	0.127	-0.020	-0.014
	(0.09)	(0.08)	(0.08)
Observations	734	734	734
Mean (PDMP=0)	1062	1062	1062
Effect Size	0.08	0.01	0.01
R^2	0.93	0.96	0.96
Panel B: Criminally Referred Treatment Admissions			
PDMP	0.191*	0.066	0.060
	(0.11)	(0.09)	(0.09)
Observations	734	734	734
Mean (PDMP=0)	210	210	210
Effect Size	0.12	0.04	0.04
R^2	0.90	0.93	0.93
Panel C: Self Referred Treatment Admissions			
PDMP	0.179*	-0.001	0.016
	(0.10)	(0.10)	(0.10)
Observations	734	734	734
Mean (PDMP=0)	487	487	487
Effect Size	0.11	0.00	0.01
R^2	0.90	0.94	0.94
Year FE	Yes	No	No
State FE	Yes	Yes	Yes
State Specific Trends	No	Yes	Yes
Demographic Controls	No	No	Yes
Other Drug Controls	No	No	Yes

Notes: In each specification, the dependent variable is equal to the (state-year) log-count of opioid-related treatment admissions (TEDS, 1998–2012). Included in "Demographic controls" are state-year observations of total population, percent of the population that is black, percent of the population that is white, median income, Medicaid/Medicare enrollment levels, and unemployment rate. Included in "Other drug controls" are the number of drug treatment centers, number of pharmacies, and indicators for whether or not the state has one of the following drug-related laws in effect: doctor-shopping laws, Naloxone availability law, pain-clinic regulation laws, required-patient-identification laws, and if medical marijuana has been legalized. Robust standard errors are reported in parentheses and in all specifications allow for clustering at the state level. *** significant at 1%; ** significant at 5%; * significant at 10%.

Likewise, I will estimate the fact of program attributes while separately identifying the potential movement in admissions attributable to PDMP alone. This is arguably the policy relevant parameter (as opposed to the joint consideration of adding "PDMP plus a given set of attributes,") as, in the end, now-49 states have active PDMPs and the only initiatives are among the specific attributes a given state might consider implementing.

Does a mandate to consider the database matter to treatment admissions?

In evaluating the effect of PDMPs, it is natural to assume that these programs would most directly affect opioid abuse in those states which require prescribers to check the database before prescribing opioids to patients (a "must-access" provision). In Table 7, I evaluate the effect of having a "must-access" provision on opioid treatment admissions generally, through criminal referrals, and though self referring individuals (Panels A, B, and C respectively). The coefficient estimate on the "must-access" indicator represents the causal impact of these provisions on opioid-related treatment admissions. The coefficient estimate on established PDMP represents the casual impact of a PDMP without this binding provision. In Panel A of Table 7, I first report the result of an OLS model that separately identifies the influence of "must-access" provisions from broader PDMPs, controlling for state and year fixed effects. With those states-years without established PDMP legislation as the comparison group, the coefficient estimate on the "must-access" provision suggests these provisions decrease overall opioid-related treatment admissions by approximately 4 percent where the estimated impact of an established PDMP suggests an increase in treatment admissions.

Recalling the potential biases discussed above, in Column (2) of Panel A I control for state-specific time trends as well as state fixed effects and state-year observations of a set of demographic characteristics. With the addition of these controls, the estimated coefficient of the "must-access" provision is -0.354, implying a statistically significant 30-percent reduction in opioid-related treatment admissions. Adding controls for other prescription drug-related legislation in Column (3), the magnitude of the coefficient of interest remains relatively stable. Estimates in Column (3) imply that treatments would fall by approximately 682 per year in the average state were they to implement "must-access" protocols.[17]

In Panels B and C of Table 7, I evaluate the effectiveness of the "must-access" provision on criminal and self referrals respectively, together accounting for roughly 60 percent of total referrals. Following the same specification described above, I find a statistically significant decrease in only opioid-related self referrals using those states without established PDMP legislation as the comparison group (columns (1), (2), and (3)). However, though the statistical significance of "must-access" provisions on criminal referrals is weak, the point estimate is arguably still economically meaningful.

That the result appears strongest among self-referred treatments is not surprising, as "must access" provisions are operational in the supply chain directly, and would only be implicated in criminal referrals indirectly.

[17]Marginal effects are calculated relative to the mean number of treatments in state-years without "must access."

TABLE 7.
"Must-Access" Provisions and Opioid-Related Treatment Admissions

	(1)	(2)	(3)
Panel A: Aggregate treatment admissions			
PDMP	0.127	0.001	-0.005
	(0.09)	(0.08)	(0.08)
+ Must access	-0.042	-0.354**	-0.362**
	(0.17)	(0.15)	(0.14)
Observations	734	734	734
Mean (Must access=0)	2247	2247	2247
Effect size (Must access)	0.03	0.24	0.24
R^2	0.93	0.96	0.96
Panel B: Criminally referred treatment admissions			
PDMP	0.189*	0.088	0.069
	(0.11)	(0.09)	(0.09)
+ Must access	-0.070	-0.358*	-0.376**
	(0.20)	(0.18)	(0.18)
Observations	734	734	734
Mean (Must access=0)	441	441	441
Effect size (Must access)	0.04	0.23	0.24
R^2	0.90	0.93	0.93
Panel C: Self referred treatment admissions			
PDMP	0.177*	0.029	0.026
	(0.10)	(0.10)	(0.09)
+ Must access	-0.096	-0.416**	-0.419**
	(0.17)	(0.17)	(0.17)
Observations	734	734	734
Mean (Must access=0)	1049	1049	1049
Effect size (Must access)	0.06	0.25	0.25
R^2	0.90	0.94	0.94
Year FE	Yes	No	No
State FE	Yes	Yes	Yes
State Specific Trends	No	Yes	Yes
Demographic Controls	No	Yes	Yes
Other Drug Controls	No	No	Yes

Notes: In each specification, the dependent variable is equal to the (state-year) log-count of opioid-related treatment admissions (TEDS, 1998–2012). Robust standard errors are reported in parentheses and allow for clustering at the state level in all specifications. *** significant at 1%; ** significant at 5%; * significant at 10%.

That they are still informative to explaining reductions in criminal referrals is nonetheless encouraging, however, it is widely anticipated that the prescription market is an input into the criminal access to opioid.

As the most-restrictive policy attribute among PDMP practices, "must-access" provisions are seemingly associated with decreases in opioid-related treatment admissions, across both criminal and self-referring users, with the larger responses coming from self-referred admissions. It is in this dimension that future policy should find encouragement, given a literature finding little efficacy in PDMP broadly. While weakly defined and passive PDMP fail to deliver, aggressive requirements matter to outcomes. In short, if these gains reflect decreases in the number of individuals abusing opioids in response to prescribers interacting with the database in this way—specifically, in the supply chain prior to the user's acquisition of the substance—the policy recommendations that follow are obvious.

Before concluding, however, I consider the sensitivity of outcomes to a slight relaxation of this constraint. In Table 8, I differentiate control-states further, by allowing opioid-related treatment admissions to vary by whether physicians and prescriber can even access this data while dealing with patients. Following the same structure as above, I reveal a very knife-edge to the "must-access" result we've established. First, I note that opioid-related treatment admissions do not systematically move with the establishment of "can-access" provisions in PDMP. Second, I note the robustness of the "must-access"

states to the inclusion of the slightly less-aggressive but similar provisions captured in the "can-access" distinction.

Does the passivity of oversight matter?

In Table 9, I evaluate the effect of provisions for the proactive checking of opioid-related treatment admissions. In particular, I exploit variation in whether and when states require that the operating agency check the database for suspicious patterns of prescribing (among physicians) and receipt of opioids (among users). The patterns in columns (1)-(3) suggest that this provision does not significantly affect treatment admissions in the either aggregate, or criminal or self referrals. The results suggest that when intervention is limited to the passive provision of information, with no mandate, the information provided by the database does itself significantly alter prescription-drug abuse.

The distribution of gains

Before considering the underlying heterogeneity—where in the intensity and tenure of use aggregate reductions are arising—I relax the constraint that is implicit in earlier results, that "must-access" provisions act similarly on opioid-treatment admission across all years of implementation. As would be consistent with changing praxis, relaxing this constraint reveals a phase-in period associated with "must-access" provisions, over which treatment admissions increasingly fall. Point estimates on years since the implementation of "must access" are shown in Figure 7. With additional time-series data available in time,

considering where this reduction "bottoms out" will be of interest to policy makers, the available times series now suggests continued declines.

In evaluating both across state and time series variation in the specific provisions of PDMPs, no programs outside of the most-aggressive PDMPs (those with "must-access" attributes) have a significant impact on drug-treatment admissions, my measure of opioid abuse. However, underlying the Treatment Episodes Dataset is a collection of information on usage intensity, by drug reported. As such, I can unpack treatment admission in a way that informs our understanding of where gains are coming from in a more nuanced way. For example, this information allows one to identify which types of users—are light users declining, or is it heavy users who are in decline—are being affected by these supply side interventions. Recall that a single "treatment admission" potentially includes primary, secondary, and tertiary substances abused, as well as the frequency with which those substances were used (e.g., "no use in the last month" through "daily"). Using these measures, I separately identify the parts of the distribution of treated users from which the overall reductions are seemingly arising.

Intensity of opioid use

In considering the above treatment effect, I make inference regarding the effect of PDMP (or specific attributes) on the log-number of treatment admissions. Making no distinction between light and heavy users—across five categories of use intensity, actually—the above analysis implicitly assumes that treatment is common across use intensity.

TABLE 8.
"Can-Access" Provisions and Opioid-Related Treatment Admissions

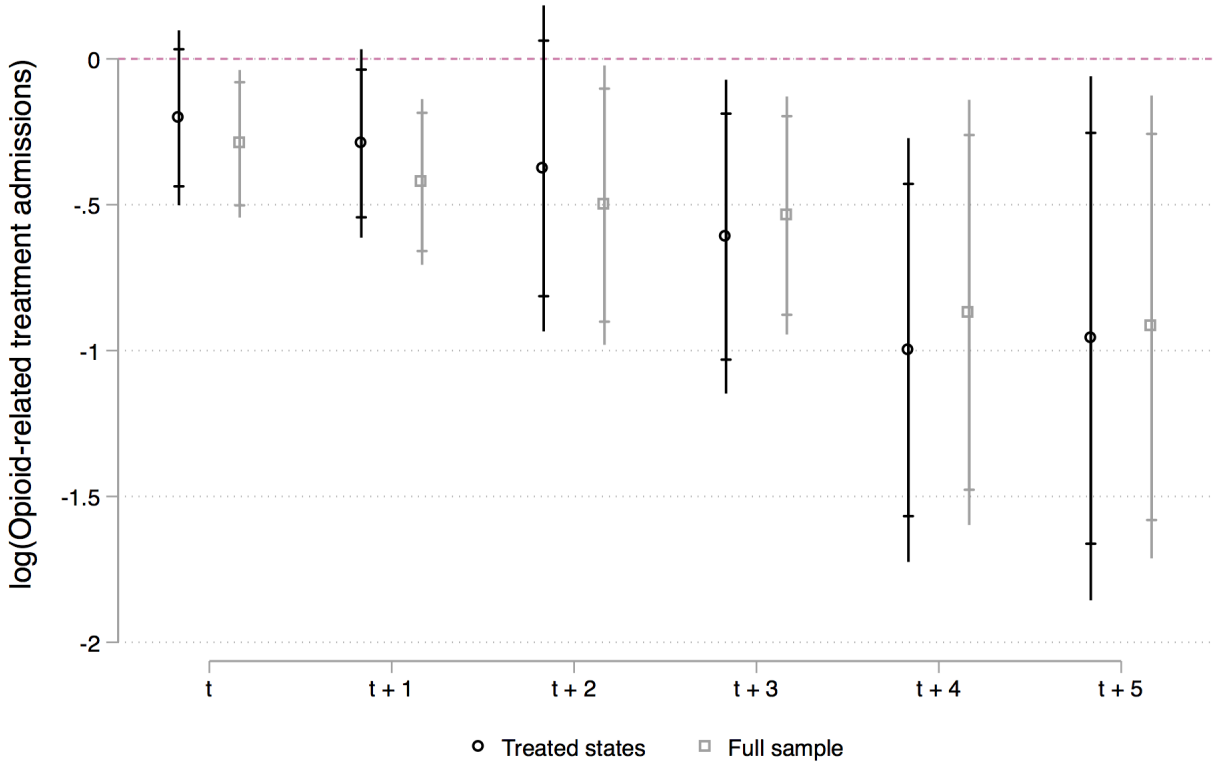
	(1)	(2)	(3)
Panel A: Aggregate treatment admissions			
PDMP	0.244*	-0.016	-0.032
	(0.12)	(0.11)	(0.11)
+ Must access	0.014	-0.357**	-0.368***
	(0.19)	(0.14)	(0.13)
+ Can access	-0.168	0.022	0.036
	(0.12)	(0.10)	(0.11)
Observations	734	734	734
Mean (Must access=0)	1299	1299	1299
Effect size (Must access)	0.01	0.24	0.24
R^2	0.93	0.96	0.96
Panel B: Criminally referred treatment admissions			
PDMP	0.256*	0.017	0.009
	(0.15)	(0.13)	(0.13)
+ Must access	-0.038	-0.372**	-0.388**
	(0.22)	(0.17)	(0.17)
+ Can access	-0.095	0.095	0.080
	(0.13)	(0.13)	(0.13)
Observations	734	734	734
Mean (Must access=0)	240	240	240
Effect size (Must access)	0.02	0.24	0.25
R^2	0.90	0.93	0.93
Panel C: Self referred treatment admissions			
PDMP	0.328**	-0.027	-0.041
	(0.13)	(0.13)	(0.13)
+ Must access	-0.025	-0.427***	-0.433***
	(0.20)	(0.16)	(0.15)
+ Can access	-0.216	0.074	0.090
	(0.13)	(0.12)	(0.12)
Observations	734	734	734
Mean (Must access=0)	616	616	616
Effect size (Must access)	0.01	0.25	0.26
R^2	0.91	0.94	0.94
Year FE	Yes	No	No
State FE	Yes	Yes	Yes
State Specific Trends	No	Yes	Yes
Demographic Controls	No	Yes	Yes
Other Drug Controls	No	No	Yes

Notes: In each specification, the dependent variable is equal to the (state-year) log-count of opioid-related treatment admissions (TEDS, 1998–2012). Robust standard errors are reported in parentheses and allow for clustering at the state level in all specifications. *** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 9.
"Proactive Checking" and Opioid-Related Admissions

	(1)	(2)	(3)
Panel A: Aggregate treatment admissions			
PDMP	0.123	0.038	0.040
	(0.08)	(0.09)	(0.08)
+ Proactive required	0.254	-0.158	-0.142
	(0.28)	(0.17)	(0.17)
+ Proactive permitted	-0.117	-0.041	-0.067
	(0.12)	(0.12)	(0.11)
Observations	734	734	734
Mean	2035	2035	2035
Effect Size (Required)	0.17	0.11	0.09
R^2	0.93	0.96	0.96
Panel B: Criminally referred treatment admissions			
PDMP	0.207**	0.164	0.171
	(0.10)	(0.10)	(0.11)
+ Proactive required	-0.147	-0.112	-0.052
	(0.33)	(0.19)	(0.20)
+ Proactive permitted	0.030	-0.164	-0.263
	(0.25)	(0.17)	(0.16)
Observations	734	734	734
Mean	414	414	414
Effect Size (Required)	0.09	0.07	0.03
R^2	0.90	0.93	0.93
Panel C: Self-referred treatment admissions			
PDMP	0.177	0.076	0.068
	(0.11)	(0.11)	(0.11)
+ Proactive required	0.096	-0.063	-0.096
	(0.32)	(0.19)	(0.19)
+ Proactive permitted	-0.041	-0.120	-0.085
	(0.22)	(0.13)	(0.12)
Observations	734	734	734
Mean	890	890	890
Effect Size (Required)	0.06	0.04	0.06
R^2	0.90	0.94	0.94
Year FE	Yes	No	No
State FE	Yes	Yes	Yes
State Specific Trends	No	Yes	Yes
Demographic Controls	No	Yes	Yes
Other Drug Controls	No	No	Yes

Notes: In each specification, the dependent variable is equal to the (state-year) log-count of opioid-related treatment admissions (TEDS, 1998–2012). Robust standard errors are reported in parentheses and allow for clustering at the state level in all specifications. *** significant at 1%; ** significant at 5%; * significant at 10%.



Notes: Point estimates are from two separate specifications, following Column (3) of Table 7, relaxing the restriction that treatment be constant across years of implementation. In this table, I plot point estimates and confidence intervals with and without restricting the sample to the five states who experience the arrival of "must access" in the time series available. Confidence intervals are 95%, with 90% indicated by hash marks.

FIGURE 7.
"Must-Access" Provisions and Opioid-Related Admissions, by Year of Implementation

In Table 10, I relax this assumption and consider the potential reductions in the treatment of opioid users across reported levels of use intensity. The TEDS data provides counts of treatment for opioid related admissions by categories of frequency of use or intensity—categories include no use in the past month, monthly use, 1-2 times weekly, 3-6 times weekly, and daily use. In Panel A of Table 10 I note that the most significant reductions are coming from the lightest users in the distribution of intensity—there are evident 41.2- and 38.3-percent reductions in users reporting only monthly or 1-2 times

weekly use report, respectively—and from those admissions reporting daily use, where reductions are 34.8 percent. Considering the average number of admissions in each of the intensity categories, the largest absolute movement of patients is clearly coming from the the two extremes of the distribution of use—were other states to likewise implement "must-access" protocols, estimates in Column (2) imply that annual treatments among monthly users would fall by 99 in the average state, and daily users by 495.[18] Although substitution across intensities in response to "must-access" provisions is possible, that point estimates are negative across all intensity levels again suggest that the net affect of such mandates is toward beneficial declines in treatment.

As an attempt to account for the potential substitutions across category, in Panel B of Table 10, I include lagged counts of contiguous densities. That is, when predicting counts of admissions reporting "3-6 times weekly" use in year t, for example, I include $t-1$ counts of admissions reporting "1-2 times weekly" and counts of admissions reporting "daily" use, as these are the most-likely category from which substitution may originate. These controls prove informative in predicting treatment counts and, while the magnitudes fall across all intensity levels, I again find significant declines in admissions of those users reporting monthly, 1-2 times weekly, and daily use (30.5-, 26.7-, and 27.9-percent declines, respectively).

Across Table 10, impact estimates at the (untreated) mean suggest reductions from 30 to 41 percent among the lightest users, and 27 to 34 percent among daily users, dipping

[18]In both cases, marginal effects are calculated relative to the mean number of treatments reporting monthly (or daily) use in state-years without "must access."

slightly in the middle of the distribution. The economic significance of this is further exaggerated by the smaller densities in the middle of the distribution, making it quite reasonable to consider efficacy following a roughly "U-shaped" pattern in use intensity. Similarly, the available policy variation is explaining more of the variation in treatment admissions in the tails of the distribution of use-intensity, where effect sizes are upwards of 0.20 to 0.26.

As variation across intensity levels could suggest differential selection into categories, in Table 11 I consider known personal characteristics across similar categories.[19] Comparing those categories displaying the largest impacts of PDMPs (i.e., the tails of the distribution) to those where PDMPs have insignificant effects, I do not find striking differences in demographic characteristics. Across all intensity levels, approximately 50 percent of admissions are male and 78 percent are white. Approximately 30 percent of admissions are unemployed and 13 percent report having public insurance while close to 8 percent report being privately insured. Individuals seeking treatment of opioid abuse fall into the 35-44 years age group at approximately 30 percent which seems to be the commonly reported age bin across all intensity levels. Given this information, one cannot attribute the differential impact of "must access" to sorting based on selection on such characteristics.

[19]Personal characteristics reported at the time of admission include gender, entity of reference (i.e., criminal, self, school, employer, health care or alcohol counseling referrals), employment and insurance status, age, race, prior treatment admissions, and type of treatment facility. Characteristics are not exhaustive, and need not therefore sum to one.

TABLE 10.
"Must-Access" Provisions and Opioid-Related Treatment Admissions, by Intensity of Use

	No use in last month	Monthly use	Weekly (1-2 times)	Weekly (3-6 times)	Daily use
	(1)	(2)	(3)	(4)	(5)
Panel A: Contiguous categories (lagged) not included					
PDMP	0.034	0.123	0.136	0.089	0.064
	(0.10)	(0.10)	(0.11)	(0.10)	(0.11)
+ Must access	-0.283	-0.532***	-0.483**	-0.279	-0.428**
	(0.17)	(0.11)	(0.22)	(0.24)	(0.17)
Observations	734	734	734	734	734
Mean (Must access=0)	739	239	170	277	1420
Effect Size (Must access)	0.20	0.38	0.34	0.19	0.26
R^2	0.93	0.92	0.92	0.92	0.93
Panel B: Contiguous categories (lagged) included					
PDMP	-0.023	0.051	0.042	0.020	-0.015
	(0.06)	(0.08)	(0.08)	(0.06)	(0.09)
+ Must access	-0.115	-0.364***	-0.311**	-0.083	-0.328**
	(0.14)	(0.10)	(0.15)	(0.18)	(0.12)
Observations	726	726	726	726	726
Mean (Must access=0)	746	242	172	279	1433
Effect Size (Must access)	0.08	0.26	0.22	0.06	0.20
R^2	0.94	0.94	0.94	0.95	0.95

Notes: In each specification, the dependent variable is equal to the (state-year) log-count of opioid-related treatment admissions (TEDS, 1998–2012), by use intensity. All specifications include state FE, state-specific trends, demographic controls, and controls or other drug legislation. Robust standard errors are reported in parentheses and allow for clustering at the state level in all specifications. *** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 11.
Demographics Across Intensity of Use, Pre-Treatment

	No use in last month	Monthly use	Weekly (1-2 times)	Weekly (3-6 times)	Daily use
	(1)	(2)	(3)	(4)	(5)
Male	0.540	0.519	0.525	0.513	0.508
Crime referral	0.302	0.195	0.169	0.151	0.127
Self referral	0.297	0.376	0.385	0.409	0.470
Alcohol referral	0.122	0.098	0.096	0.103	0.105
Health referral	0.091	0.106	0.100	0.114	0.13
School referral	0.007	0.013	0.007	0.006	0.004
Employer referral	0.008	0.014	0.012	0.014	0.010
Unemployed	0.289	0.29	0.284	0.297	0.296
Private insurance	0.069	0.075	0.075	0.083	0.084
Public insurance	0.138	0.133	0.128	0.134	0.145
Age 18-24	0.125	0.144	0.148	0.138	0.108
Age 25-34	0.290	0.294	0.28	0.291	0.299
Age 35-44	0.357	0.308	0.297	0.326	0.363
Age 45-54	0.121	0.088	0.093	0.104	0.134
White	0.829	0.782	0.745	0.788	0.818
Black	0.059	0.062	0.068	0.056	0.054
No prior	0.265	0.295	0.293	0.301	0.310
Ambulance	0.694	0.556	0.501	0.505	0.474
Rehab	0.193	0.196	0.199	0.206	0.198
Detox	0.069	0.157	0.180	0.195	0.266

Source: Treatment Episodes Data Set (TEDS), Substance Abuse and Mental Health Services Administration, 1998–2012.

As one last consideration of the potential error structure across categorical intensities of use, in Table 12 I fully model the simultaneity by three-stage least squares. Doing so accounts directly for the potential that errors across intensities correlate, and movement

with "must access" in one category might well drive movement in other categories. Doing so, I find that opioid-treatment admissions are similarly responsive to the policy variation, which suggests that the independence assumption (of Table 10) is not overly restrictive. In particular, across all four approaches in Panel A, point estimates among light users (e.g., monthly and 1-2 weekly) and heavy users (i.e., daily use) associated with "must-access" provisions range from -0.532 to -0.425, suggesting decreases of approximately 32 to 37 percent. Including the lagged-neighboring categories in Panel B as described above, the magnitude of the "must-access" provisions again attenuates slightly, though there is significant movement again among monthly, 1-2 times weekly, and daily users.

Tenure of opioid use

In addition to providing frequency of use information, the TEDS includes information on the self-reported "age at first use" for each of the three substances reported by an individual seeking treatment. As this age report is categorical in nature, I consider all possible-but-latent truths (i.e., the four combinations of youngest and oldest starting age and youngest and oldest treatment age). As results are not sensitive to this categorization, in Table 13 I report on the responsiveness of opioid-related treatment admissions by tenure of use using the mid points of all age bins.

As is evident in Table 13, around PDMP-induced supply side restrictions there are differential effects on short-term and long-term users. While point estimates suggest reductions coming from across the distribution of tenure, I see the largest reductions in admissions which report having used for less than six years, and, in particular, 0-3 years of

use, where impact at the (untreated) mean is 42.3 percent. Overall, the range of impacts is monotonically declining in tenure of use, bottoming out at roughly 15-percent reductions in opioid-related treatment admissions among those reporting 16-or-more years of opioid use. "Must-access" provisions are also explaining more of the variation in treatment admissions at the lower-tenure end of the distribution, where effect sizes are upwards of 0.30.

Must-access PDMP provisions and opioid-related overdose deaths

Thus far, I have established that amid the general lack of sensitivity in opioid-related treatments with implementations of prescription drug monitoring programs, there are areas of encouragement, albeit very specific and narrow avenues of encouragement. Namely, I find a knife-edge result where efficacy is seemingly strong and economically significant. Where states implement PDMPs requiring physicians to access the database before prescribing opioids, I see reductions in opioid-related treatment admissions—a pattern that is not even evident among those merely allowing similar access.

In Table 14, I follow up on the same variation in PDMP to consider the implications on opioid-related deaths. Previous research has found little systematic variation in death around the introductions of PDMP provisions, yet, without distinguishing these most-aggressive practices.[20] In Column (1) of Table 14, I find no statistically significant

[20]In recent work, Ruhm (2016) considers the assignment of death to specific drugs involved in drug-poisoning fatalities, recognizing the potential implication of multiple substances. While clearly germane to any consideration of the potential substitution from prescription-opioid to heroin, I find no reason to anticipate that measurement in the Multiple Cause of Death files is systematic with "must-access" provisions, and anticipate that (if anything) the movement in the measure of opioid-related death may

explanatory power coming from the general establishment of these programs. However, the effectiveness of the mandated interaction with Prescription Drug Monitoring Programs is again demonstrated in Column (2) and Column (3) of Table 14. While PDMPs generally do not affect overall opioid-related overdose deaths, states adopting a "must-access" provision in their program is associated with an approximately 33-percent reduction in opioid-related overdose deaths. When I evaluate those state with the more relaxed provision, "can-access," the results closely follow the pattern found in opioid-related treatment admissions. That is, while the effect of "must-access" remains strongly negative and statistically significant, the effect of the "can-access" provision is positive and statistically insignificant suggesting that, when the information provided in these databases is costly to access, allowing access to the information is not sufficient to reduce prescription drug abuse.

Discussion

I offer strong evidence of efficacy in prescription drug monitoring programs, in a large literature of weak associations between PDMPs and outcomes. I find that PDMPs with "must-access" attributes—getting between prescribers and patients—lead to a significant reductions in opioid-related drug-treatment admissions. Merely allowing this access cannot be associated with similar decreases, which points further to the need for

miss some deaths categorized as exclusively heroin deaths over time and thereby yield a lower-bound of the true effect.

strict mandates as the knife-edge nature of this result suggests that effective PDMPs are those that actively interfere with the supply chain, often at the point of consultation.

In addition to documenting the extensive margin of episodes of drug treatment, I demonstrate that reductions in treatment admissions are originating from less-attached users—less attached in both intensity of use and tenure of use—in states with the most-aggressive PDMP policies. I also find evidence of these specific monitoring practices driving overdose deaths down, significantly so among states with at least one year of experience with "must access" PDMP provisions. Estimates imply that treatments would fall by 561 per year in the average state were they to implement "must-access" protocols, with the bulk of these coming from reductions in those individuals reporting to have sought treatment following a period of daily opioid use.

Identifying which aspects of the PDMPs are most effective in curbing prescription-drug abuse is crucial to informing policy. In light of existing evidence that suggests only small benefits associated with broader PDMP implementation, the data are a clear encouragement toward requiring prescribers to consult these databases at the point of contact with the patient.

TABLE 12.
"Must-Access" Provisions and Opioid-Related Treatment Admissions by Intensity of Use: Simultaneous Equations (3sls)

	No use in last month	Monthly use	Weekly (1-2 times)	Weekly (3-6 times)	Daily use
	(1)	(2)	(3)	(4)	(5)
Panel A: Contiguous categories (lagged) not included					
PDMP	0.027	0.123**	0.126**	0.072	0.057
	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)
+ Must access	-0.279*	-0.532***	-0.478***	-0.271	-0.425**
	(0.17)	(0.16)	(0.17)	(0.17)	(0.18)
Observations	734	734	734	734	734
Mean (Must access=0)	741	240	170	277	1421
Effect Size (Must access)	.12	.32	.30	.17	.24
R^2	0.93	0.92	0.92	0.92	0.93
Panel B: Contiguous categories (lagged) included					
PDMP	0.013	0.098*	0.093*	0.051	0.036
	(0.06)	(0.05)	(0.05)	(0.06)	(0.06)
+ Must access	-0.204	-0.449***	-0.375***	-0.173	-0.381**
	(0.15)	(0.14)	(0.14)	(0.14)	(0.15)
Observations	726	726	726	726	726
Mean (Must access=0)	749	242	172	280	1435
Effect Size (Must access)	.08	.27	.24	.11	.21
R^2	0.94	0.94	0.94	0.94	0.95

Notes: In each specification, the dependent variable is equal to the (state-year) log-count of opioid-related treatment admissions (TEDS, 1998–2012), by use intensity. All specifications include state FE, state-specific trends, demographic controls, and controls or other drug legislation. Robust standard errors are reported in parentheses and allow for clustering at the state level in all specifications. *** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 13.
"Must-Access" Provisions and Opioid-Related Treatment Admissions, by Tenure of Use

	0-3 years	4-6 years	7-10 years	11-15 years	ge16 years
	(1)	(2)	(3)	(4)	(5)
PDMP	-0.108	-0.045	0.097	0.069	0.038
	(0.09)	(0.10)	(0.09)	(0.09)	(0.08)
+ Must access	-0.551***	-0.460***	-0.313**	-0.232*	-0.167
	(0.12)	(0.16)	(0.15)	(0.13)	(0.18)
Observations	734	734	734	734	734
Mean (Must access=0)	728	577	412	299	32865
Effect Size (Must access)	0.36	0.30	0.21	0.16	0.15
R^2	0.94	0.94	0.95	0.95	0.95

Notes: In each specification, the dependent variable is equal to the (state-year) log-count of opioid-related treatment admissions (TEDS, 1998–2012), by tenure of use. All specifications include state FE, state-specific trends, demographic controls, and controls or other drug legislation. Robust standard errors are reported in parentheses and allow for clustering at the state level in all specifications. *** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 14.
Opioid-Related Deaths, 1999-2012

	PDMP	Must Access	Can Access
	(1)	(2)	(3)
PDMP	-0.056	-0.045	-0.141
	(0.10)	(0.10)	(0.18)
+ Must access		-0.399*	-0.416*
		(0.22)	(0.22)
+ Can access			0.124
			(0.12)
Observations	700	700	700
Mean (Must access=0)		257	257
Effect Size (Must access)		.32	.33
State FE	Yes	Yes	Yes
State-specific trends	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes
Other drug policy	Yes	Yes	Yes

Notes: In each specification, the dependent variable is equal to the (state-year) log-count of deaths involving natural and semi-synthetic opioids (opioid) and fully synthetic opioids (synthetic), from the Vital Statistics of the United States (MCOD, 1999-2012). The mean number of deaths (PDMP=1) is equal to that in state-years with active PDMPs but no "must-access" provision. All specifications include state FE, state-specific trends, demographic controls, and controls or other drug legislation. Robust standard errors are reported in parentheses and allow for clustering at the state level in all specifications. *** significant at 1%; ** significant at 5%; * significant at 10%.

CHAPTER IV

PRESCRIPTION-DRUG COVERAGE AND PREVENTATIVE HEALTH BEHAVIORS: EVIDENCE FROM MEDICARE PART D

Introduction

As part of the Medicare Modernization Act of 2003, Medicare Part D offered government subsidized drug coverage to Medicare beneficiaries beginning January 1, 2006. Part D is an optional provision under which eligible individuals enroll in either a stand-alone or a Medicare Advantage Plan.[1][2] Medicare requires a standard minimum benefit for all Part D plans with enrollees paying an average premium of $42.17 per month in 2017, and standard deductible and coinsurance payment.[3] The share of Medicare beneficiaries enrolled in Part D was 71% in 2016 up from 52% in 2006-the year of Part D's establishment. The literature suggests the establishment of Part D increased coverage for those aged 65 and older however, Engelhardt and Gruber (2011) demonstrate that although there was a 50% increase in government coverage for the elderly, increases in

[1]Any individual who is entitled to Medicare Part A or enrolled in Part B is eligible to enroll in Part D coverage

[2]Stand-alone plans are typically utilized by those who utilize the traditional Medicare structure for health services -(6 in 10 beneficiaries use this enrollment plan). Medicare Advantage Plans are typically utilized by those covered by an HMP or PPO including prescription drugs in a comprehensive package. http://kff.org/medicare/fact-sheet/the-medicare-prescription-drug-benefit-fact-sheet/ (last visited May 1, 2017.) http://kff.org/medicare/issue-brief/medicare-part-d-a-first-look-at-prescription-drug-plans-in-2017/ (last visited May 1, 2017.)

[3]For the standard Medicare Part D plan, $400 deductible and 25% coinsurance up to an initial coverage limit of $3,700 in total drug costs http://kff.org/report-section/medicare-part-d-in-2016-and-trends-over-time-section-1-part-d-enrollment-and-plan-availability/ (last visited May 1, 2017.)

any prescription-drug coverage was only 10% for this group which suggests that much of the increase in coverage can be attributed to switching between private and public plans and not necessarily a change from no drug coverage to Part D. The largest increases in government coverage stem from those individuals at the top distributions of prescription-drug expenditures (Levy and Weir, 2010).

There is a substantial literature exploring the role of prescription-drug coverage on ex-post health and financial outcomes.[4] Much of this literature has explored the enactment of the Medicare Modernization Act of 2003 -which added prescription-drug benefits to the Medicare program through Medicare Part D. While prescription-drug coverage is associated with improved ex-post health outcomes and financial stability, less is known about the effect prescription drugs have on ex-ante health decisions i.e., preventative-care measures and health behaviors.

In this paper, we explore the potential substitution effect between prescription drugs and these ex-ante health measures. Specifically, we identify the reduced form effect of prescription-drug coverage on preventative care and health utilization measures such as routine checkups and screenings, physical activity, and office visits, insurance attitudes, and risky behaviors such as smoking. Additionally, we identify the effect of prescription-drug coverage through Medicare Part D on physician behaviors by evaluating diagnoses and recommendations to patients such as reducing fat intake and exercising more often. Given the potential for health shocks to trigger changes in health behaviors, we also

[4]Finkelstein and McKnight (2008) Ayyagari and Shane (2015) Afendulis et al. (2011) Kaestner et al. (2014)

explore the potential for prescription-drug coverage to mitigate an individual's healthcare and behavioral decisions following a health shock.

The pathway by which prescription-drug coverage may change an individual's preventative care and health behavior decisions is through reducing the costs of poor health for an individual. When making decisions surrounding health, whether though preventative-care measures or risky behavioral choices, an individual weighs the utility loss or gain of these activities today against their potential future consequences. The establishment of Part D mitigates the costs of poor health by allowing the individual to effectively "risk-share" with the federal government. Thus, if preventative care is costly, the ability to share the risk of poor health in the future may deter an individual from engaging in it.[5]

The paper proceeds as follows. In Section 4.2, we discuss the existing literature evaluating the impact of prescription-drug coverage on health outcomes, financial security, and crowd-out in the insurance market. In Section 4.3, we discuss the data used in the analysis. In Section 4.4, we describe the empirical strategy as well as potential selection issues inherent in the reduced form parameter as well as in the current literature. In Section 4.5, we present the empirical results, including the primary effect of Part D expansion on preventative care and risky health behaviors and exploring the potential mediating effects prescription drugs have in responses to health shocks. We conclude in Section 4.6.

[5]It could also be the case that prescription-drug coverage reduces the effectiveness of preventative care such as in the case of statin/ET interactions Deichmann et al. (2015).

Literature

There are multiple studies evaluating the effect Medicare Part D had on prescription-drug coverage and utilization with the majority suggesting that both coverage and utilization rates increased after its establishment. Evaluating survey waves directly pre and post Part D establishment in the Health and Retirement Study, Levy and Weir (2010) find a significant increase (50% to 60%) in prescription-drug coverage among those without coverage in 2004. They find evidence that those who remain uninsured had low levels of prescription-drug utilization pre Part D and thus were less likely to gain from this coverage offering. In line with this finding, Heiss et al. (2006) suggest that the largest gains in coverage stem from "vulnerable" subpopulations. Engelhardt and Gruber (2011) suggest the true gains in coverage are much smaller, finding 80% crowd-out in insurance coverage. The authors also find only small reductions in out-of-pocket (OOP) costs concentrated in the top expenditure distribution. In contrast to the magnitudes found by Engelhardt and Gruber (2011) and Finkelstein and McKnight (2008) suggest a much larger decrease in OOP costs with a 40% decline for the highest expenditure group. The larger literature has also found significant reductions in OOP costs associated with the establishment of Part D (Ketcham and Simon, 2008) (Duggan and Morton, 2010).

The literature evaluating the impact of Part D on drug utilization and total expenditures is positive. Using prescription-drug records and a similar difference-in-difference strategy as employed by Engelhardt and Gruber (2011), Ketcham and Simon (2008) find an increase of use of prescription drugs by 4.7% for the first year of the

program. These findings on increased drug utilization are consistent throughout the literature with Liu et al. (2011),Duggan and Morton (2010), Zimmer (2015), and Alpert (2016) finding similar results although often with larger magnitudes. Kaestner (2012) find a 30% increase in drug utilization and a significant reduction in socioeconomic disparities in prescription-drug insurance. The authors also find that gaining prescription-drug coverage through Medicare Part D is associated with a 40% increase in total drug expenditures suggestive of crowd-out of alternative private insurance sources. When evaluating health specific drug utilization, Blumberg et al. (2015) find a reduction in cost-related nonadherence for Medicare beneficiaries with glaucoma pre and post Part D while Hanlon et al. (2013) find increased use of antilipemics among black Medicare recipients in the post Part D regime.

The evidence of direct health impacts following prescription-drug coverage is mixed. Evaluating the effect of Part D on a sample of Medicare beneficiaries, Kaestner (2012) find no evidence of increased inpatient or outpatient services while Finkelstein and McKnight (2008) and Kaestner et al. (2014) find Part D had no impact on the mortality rates of Medicare beneficiaries. The majority of the studies evaluating the impact of Part D on health outcomes employ a similar difference-in-difference strategy as used by (Engelhardt and Gruber, 2011), comparing the effect of Part D establishment on the Medicare eligible to a slightly younger ineligible cohort. In an evaluation of Medicare Part D's effect on mental health, Ayyagari and Shane (2015) uses the Health and Retirement Study to find that the establishment of Part D was associated with significant reductions in depressive

symptoms for those of Medicare eligibility age. They argue that potential mechanisms may include the increased use of antidepressants after the establishment of Part D. Using the same identification strategy, Ayyagari et al. (2016) find declines in the number of non-emergency care visits to emergency departments while Afendulis et al. (2011) find a 4.1% reduction in hospitalizations associated with Part D.

In addition to the reduced form estimates of the impact of Part D on direct health outcomes, measures of drug utilization after 2006 provide insight into mechanisms by which health outcomes may be affected. Madden et al. (2008) compare pre and post Part D cost-related medication non-adherence finding a slight decrease in non-adherence rates following drug coverage while Zivin et al. (2009) finds no differential effect of Part D on non-adherence rates for those with and without depressive symptoms. Mahmoudi and Jensen (2014) find that Part D significant reduced racial disparities in number of prescriptions and out-of-pocket spending for Hispanics.

While the majority of studies on the effect of Part D explores health outcomes, this research adds to the existing literature by exploring the potential substitution effects between prescription drugs and preventative-care utilization and health behaviors. This contribution is important in understanding the true welfare implications of providing prescription drugs. If individuals rely more heavily on prescription drugs and forego costly preventative-care screenings, exercise, or continue risky health behaviors, evaluating ex-post health outcomes may not represent the true welfare gain of Part D as increases in total prescription-drug expenditures would pose a higher cost to the public.

In addition to exploring the direct effects of Part D, we also investigate the potential for prescription-drug coverage to mitigate an individual's preventative care and health behavior choices following a health shock. The existing literature suggests behavioral and psychological responses to health shocks for example, suggesting an increase in risk aversion following a health event Decker and Schmitz (2016) and a higher likelihood of remaining employed for those with employment-contingent health insurance (Bradley et al., 2012). If having prescription-drug coverage acts as a substitutable good for health improvement behaviors, an individual may be less inclined to seek preventative screenings or reduce risky health behaviors following a shock. We explore this possibility below.

Data

Medical Expenditure Panel Survey (MEPS)

To study the effects of Medicare Part D establishment on preventative healthcare and risky health behaviors, we employ 2001-2014 waves of the publicly available Medical Expenditure Panel Survey (MEPS)- Household Component data set administered by the Agency for Healthcare Research and Quality (AHRQ). The MEPS is a nationally representative subsample of those individuals participating in the National Health Interview Survey's two year panel.

The MEPS is a two-year overlapping panel with surveys including information on health expenditures and payment sources as well as insurance coverage and most importantly for this analysis, questions on preventative-care actions as well as attitudes

towards insurance and risky health behaviors. We use variable measures from the last interview of the year for each individual.[6] To establish Medicare Part D's effect on prescription-drug coverage, we follow Engelhardt and Gruber (2011) in constructing measures of coverage from both private and public sources. The household component of the MEPS provides information on both insurance coverage generally and on explicit expenditures for prescription drugs by source. This includes total expenditures, out-of-pocket expenditures, and the portion of the costs covered by third-party-payers including public and private insurers.[7] This information allows the analysis to identify a first stage of the effect of Part D establishment on prescription-drug coverage as well as expenditures-including total, out-of-pocket, medicare, and private.

Ultimately, the analysis will focus on the reduced form parameter of Part D establishment on preventative care, utilization, and risky health behaviors. The household component of the MEPS includes information on number of physician visits, cholesterol checks, flu shots, and routine checkups, adverse health diagnoses including high blood pressure, diabetes, arthritis, and stroke. In addition to these utilization measures, personal health behaviors such as physical activity, aspirin use, and smoking are included in the analysis. To explore the degree to which prescription-drug coverage affects attitudes, we also include a measure of self-reported risk aversion and attitudes towards the effectiveness of medical assistance generally.

[6]Results are robust inclusion of either first or second year of the panel and the inclusion of both.

[7]This expenditure measure does not include insurance premiums

In line with the literature, we restrict the sample to individuals ages 55-75 and exclude those individuals who are on Medicare prior to age 65.[8]

Heath and Retirement Study

To assess the potential mitigating effect prescription-drug coverage may have on health behaviors and choices after an adverse health event, we use the 1996 - 2012 waves of the Health and Retirement Study (HRS). The HRS is a nationally representative longitudinal survey of individuals over the age of 50. Topics in the HRS include healthcare , assets and investments, employment, housing, and attitudes. For this analysis, we employ the RAND HRS (version P) data file which contains panel consistent cleaned versions of the most frequently asked questions in the HRS. In addition to these existing variables, we include raw variables from the original HRS files.[9]

Although the MEPS includes more detailed measures of drug coverage and a larger number of preventative-care measures, the panel structure of the HRS allows us to explore the effects of a health shock (defined as being diagnosed during the time the individual participates in the survey) on preventative care and health behaviors and, importantly, how the establishment of prescription-drug coverage through Part D may mitigate any of these effects. The analysis uses four major health shocks broken into two categories: chronic and acute. Diagnoses of high blood pressure or diabetes are considered chronic

[8]Individuals can qualify for Medicare benefits prior to 65 in the case of End Stage Renal Disease or Amyotropic Lateral Sclerosis

[9]The RAND HRS version P was developed at RAND with funding from the National Institute on Aging and the Social Security Administration.

shocks where a heart attack or stroke are considered acute. we analyze the extent to which experiencing one of these shocks affects frequency of cholesterol checks, flu shots, physical activity, and smoking status.

Empirics

Medical Expenditure Survey

The fundamental variation exploited is the establishment of Medicare Part D, January 1st, 2006. Specifically, the establishment of Part D affects those who are eligible for Medicare i.e., those aged 65 and older. Restricting the sample to those of Medicare eligibility age, we ask whether there are identifiable differences in pre- and post-2006 preventative care and health behaviors among this population with the general prior that Part D reduces health enhancing actions in these two groups. The baseline econometric specification for this difference estimator is,

$$\mathrm{Y}_{it} = \alpha + \beta_1 Post06_{it} + \beta_2 X_{it} + \epsilon_{it},$$

where Y_{it} is a placeholder for the outcome of interest of individual i in year t. $Post06_{it}$ is the variable of interest and identifies the average treatment effect of Part D establishment for the sample of those aged 65 and older assuming no other factors affecting preventative care or health behaviors occurred during this time. X_{it} is a vector of controls including age, martial status, race, employment status, income, health status, region fixed effects and a linear time trend.

The challenge in using a pre-post design to identify the causal effect of Part D is that we would attribute any unobserved shocks to the healthcare system during this time to Part D establishment and thus to prescription-drug coverage. In the ideal experiment, we would identify the causal impact of Part D by comparing the preventative care and health behavior measures of individuals randomly assigned Part D coverage to a group of same-aged individuals not receiving Part D coverage in the same year.

To alleviate the concern that confounding factors affected preventative care and health behaviors post 2006, we employ a difference-in-difference strategy using those who are Medicare eligible (ages 65-75) to a comparable younger cohort who are ineligible for traditional Medicare coverage (ages 55-64). For this difference-in-difference estimator our econometric specification is,

$$\begin{aligned} \mathrm{Y}_{it} = \alpha + \beta_1(Age \geq 65_{it} X Post06_t) + \beta_2(Age \geq 65_{it}) + \beta_3(Post06_t) \\ + \beta_4 X_{it} + \epsilon_{it}, \end{aligned} \tag{4.1}$$

In this specification, the parameter of interest is β_1 which represents the causal effect of Medicare Part D on the outcome of interest Y_{it} under the assumption that these cohorts would, absent the establishment of Part D, trend similarly. To the extent that shocks to preventative care and health behaviors post 2006 equally affected both the younger Medicare ineligible and Medicare eligible cohorts, including the younger cohort as the control allows us to net out these effects. In Figure 8 we plot measures of prescription-drug coverage (including private only, public only, and dual coverage) pre

and post Part D for both cohorts from 2001-2014. Pre trends suggest the younger cohort provides a reasonable counterfactual to the Medicare eligible group. X_{it} is a vector of demographic, employment, income, region fixed effects, and age group specific linear time trends accounting for potential differences in pre-treatment trending between the Medicare eligible and younger cohort groups.

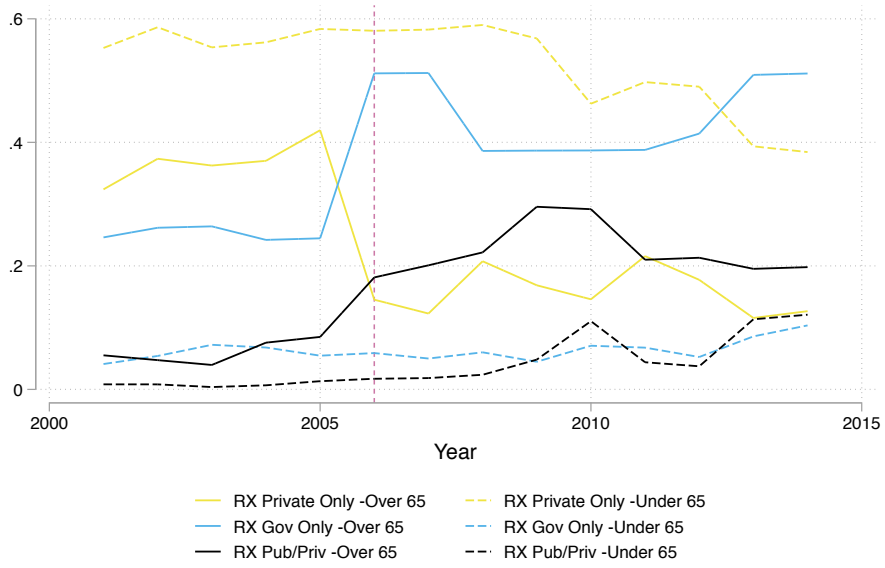


FIGURE 8.
Coverage Public, Private, Dual

The difference-in-difference estimator assumes no unobserved factor is driving the differential between the Medicare eligible group and the younger cohort. Additionally, for β_1 to represent the true causal estimate of Part D, it must be the case that selection into Medicare cannot change differentially across the older and younger cohorts with the establishment of Part D. Given that the younger cohort cannot select into Medicare, this restriction implies that selection into Medicare must remain constant across the 2006

establishment date. If the establishment of Part D triggers a non-random differential selection into Medicare coverage, for instance, if the establishment of Part D causes relatively sicker individuals to move into coverage this will bias our estimates. While some outcomes such as prescription-drug utilization should unambiguously be biased upward, other estimates-particularly those on preventative-care engagement are ambiguous as to the direction.

Under the assumption that Medicare Part D establishment does not also induce a change in transition into Medicare coverage for the treatment group, β_1 can be interpreted as the causal impact of Part D on preventative care and health behaviors.

Health and Retirement Study

In addition to the general effect of Part D on preventative care and health behaviors, we also explore the effect prescription-drug coverage has on the individual's choices over preventative care and health behaviors following a health shock. Health shocks as described in Section 4.3 may trigger changes in preventative care and health behaviors by making health issues more salient to the individual and thus potentially increasing the desire to engage in health improving behaviors. If there exists a substitution effect between prescription drugs and preventative care and health behaviors, we may see a differential response to health shocks for the Medicare eligible population post 2006. That is, the response to experiencing a health shock prior to Part D's establishment may differ from the response to the same health shock post Part D. To estimate the mitigating effect Part D has on health choices following a health shock, we estimate the following model,

$$\mathrm{Y}_{it} = \alpha + \beta_1(HealthShock_{it}XPost06_t) + \beta_2(HealthShock_{it}) + \beta_3(Post06_t) \qquad (4.2)$$
$$+ \beta_4 X_{it} + \epsilon_{it},$$

where Y_{it} is a place holder for preventative care or health behavior for person i in year t. $HealthShock_{it}$ represents one of four possible health shocks a person can experience as described in Section 4.3. The parameter of interest, β_1 represents the differential effect Medicare Part D has on the individual's health choices following a health shock. As we wish to explore the mitigating effect prescription-drug coverage has on health decisions after a shock, we include only those individuals who are over the age of 65 prior to Part D's establishment.[10] X_{it} is a vector of controls including age, race, marital status, income, and region. Time invariant controls for the individuals are absorbed into the error structure by an individual level fixed effect.

Again, although we have restricted the sample to those eligible for Medicare coverage, any differential non-random selection into Medicare following a health shock triggered by the establishment of Part D will threaten the non-biasedness of our results. For β_1 to represent the true causal mitigating effect of Part D on health choices following a health shock, one must be willing to assume that selection into Medicare is not systematic with the behavioral responses to health shocks pre and post Part D. One must also be

[10]Sample is restricted to those individuals who are at least 65 in the year 2000

willing to assume health shocks are not differentially selected into based on an individual's response to these health shocks.[11]

Results

For each outcome category in this section we first consider the baseline specification separately for the Medicare eligible and ineligible cohorts. We subsequently present estimates from the difference-in-difference specification and briefly discuss results.

RX Coverage and Expenditures

Although we will be reporting the reduced form parameter of Part D coverage on preventative care and health behaviors, it is worth establishing (as a notion of first stage relevance) the extent to which Part D affected prescription-drug costs and coverage as well as drug utilization.

In Panel A of Table 15, we report the difference estimator specification of prescription-drug coverage and expenditures around the introduction of Medicare Part D. We report our estimates first for the Medicare eligible cohort (65-75) and again for the younger comparison group (55-64). For all specifications, gender, race, marital, employment, and health status, age, age squared, census region fixed effects and a year time trend are included as controls. Robust standard errors are reported and allow for clustering at the age level in all specifications. As described in Section 4.4,

[11] Although the establishment of Part D allows for heart-attack mitigation through preventative medications, identification is preserved if individuals experiencing health shocks do not systematically differ in their response to these shocks pre- and post-Part D establishment.

causal identification of Part D relies on the younger cohort remaining unaffected by this legislation and thus we should see a level shift for the Medicare eligible cohort around 2006 and no change for the younger group.

Overall, the set of results in Panel A of Table 15 suggest Part D did increase both coverage for and utilization of prescription drugs. In Column (1) of Table 15, we report the estimate of total number of prescription drugs around the implementation of Part D. The establishment of Part D in 2006 is associated with a significant increase in prescription-drug fills for both the Medicare eligible and ineligible cohorts, although we see a larger increase of approximately three drug fills per year for the eligible group. In columns (2), (3), and (4) of Table 15, we report the effect of Part D establishment on total prescription-drug expenditures as well as out-of-pocket (OOP) costs and expenditures covered by Medicare. In line with previous literature, total expenditures and expenditures covered by Medicare increase ($335.85 and $910.59 respectively) while OOP costs fall by at statistically significant $123.11 for the Medicare eligible group. For the ineligible group, the establishment of Part D has no statistically significant effect on total expenditures but did affect OOP costs and Medicare spending to a much smaller magnitude.

In columns (5) and (6) of Table 15 we report estimates from a linear probability specification describing the effect of Part D on having any prescription-drug coverage (Column (5)) and having government drug coverage (Column (6)). In the same vein as the previous estimates, Part D is associated with a 16 percentage-point increase in having any drug coverage and a 48 percentage-point increase in having government drug coverage

for the Medicare eligible cohort. For the ineligible cohort, results are not suggestive that Part D affected prescription-drug coverage. This finding is consistent with the findings of Engelhardt and Gruber (2011) and suggestive of crowd-out in private insurance coverage.

The difference estimates of Panel A are identified solely off of variation in the Part D provisions introduced in 2006. In particular, with the sample restricted to the Medicare eligible, aged 65 through 75 from 2001 through 2014, the desire would be to identify off of the variation in the Medicare expansion into prescription-drug coverage without the confoundedness that would arise from considering a sample that included newly eligible. We also separately consider any coincident change in outcomes for a population that was neither Medicare eligible nor directly influenced by the 2006 policy change. Of course, they may well constitute a valid control group for the treated group, which we consider directly as part of the identification strategy of Panel B. However, the legitimacy of the design hinges on the assumption that these cohorts are trending similarly into treatment and would, absent the establishment of Part D continue trending in parallel.

In Panel B of Table 15 we report the difference-in-difference estimates of the effect of Part D on total expenditures and types of coverage. For all difference-in-difference specifications gender, race, marital, employment, and health status, age, age squared, census region fixed effects and an age cohort-specific linear year time trend are included as controls. Robust standard errors are reported and allow for clustering at the age level in all specifications. Results remain similar to the Medicare eligible results in Panel A. Total expenditures and expenditures covered by Medicare increase by $359.76 and $815.89

respectively and OOP costs fall by $139.62. The impact on prescription-drug coverage also remains consistent with results in Panel A suggesting a 14 percentage-point increase in any drug coverage and a 47 percentage-point increase in government coverage.[12]

[12]The large level effects represented in Panel B of Table 15 are an artifact of the age cohort-specific linear time trend. Results remain qualitatively similar restricting all observations to a common trend. We present estimates allowing for differences in trends between the near elderly and elderly groups.

TABLE 15.
Prescription-Drug Payments and Coverage

		RX Total Expend			RX Coverage	
	RX Total Num.	Total	OOP	Medicare	Any	Government
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Pre/Post Estimates						
			Medicare Eligible (65-75)			
Post 06	3.01*	335.85**	-123.11**	910.59***	0.16***	0.43***
	(1.46)	(121.95)	(53.87)	(75.96)	(0.01)	(0.01)
Observations	12860	12860	12860	12860	12860	12860
Mean	24.60	1488.58	712.50	134.28	0.74	0.29
Effect Size	0.11	0.18	0.11	1.83	0.36	0.96
			Medicare Ineligible (55-64)			
Post 06	1.28**	40.06	40.51***	13.19**	0.01	-0.04***
	(0.44)	(70.96)	(9.74)	(5.22)	(0.01)	(0.01)
Observations	19024	19024	19024	19024	19024	19024
Mean	16.25	1025.17	389.50	0.13	0.80	0.04
Effect Size	0.06	0.02	0.05	1.56	0.04	0.19
Panel B: Difference-in-Differences Estimates						
≥ Age 65 X Post 06	1.94	359.76**	-139.62**	815.89***	0.14***	0.47***
	(1.49)	(144.86)	(53.75)	(84.87)	(0.01)	(0.01)
Post 06	1.17**	6.03	28.51**	45.42**	0.01	-0.04***
	(0.44)	(66.03)	(10.60)	(16.92)	(0.01)	(0.01)
≥ Age 65	-441.04*	-7039.58	8378.58	-60873.55**	-5.24*	21.35***
	(240.86)	(31304.21)	(10399.64)	(26254.67)	(2.84)	(4.83)
Observations	31884	31884	31884	31884	31884	31884
Mean	19.75	1219.33	524.83	56.34	0.78	0.15
Effect Size	0.08	0.20	0.15	2.49	0.35	1.31

Notes: For all specifications, gender, race, marital, employment, and health status, age, age squared, census region fixed effects and a year time trend are included as controls. Panel A displays the difference estimator-restricting the sample to the Medicare eligible population (65-75) and to the Medicare ineligible population (55-64). Panel B displays the difference-in-difference estimates and adds a cohort specific time trend in the model.*** significant at 1%; ** significant at 5%; * significant at 10%.

We can explore a more non-parametric approach to modeling the differential response between the treatment and control groups to Part D pre and post 2006. The estimating equation becomes,

$$\mathrm{Y}_{it} = \alpha + \sum_{t=2001}^{2014} \beta_{1t}(Age \geq 65_{it})XYear_t + \beta_2(Age \geq 65_{it}) + \sum_{t=2001}^{2014} \beta_{3t}Year_t \qquad (4.3)$$
$$+ \beta_4 X_{it} + \epsilon_{it},$$

In this specification, β_{1t} represents the yearly differential between the Medicare eligible and younger cohort. Estimates of β_{1t} pre 2006 with confidence intervals inclusive of zero are suggestive of parallel pre-treatment trends giving us confidence that any differences seen after the establishment of Part D is not driven by differential trends between the age cohorts. Figure 9 provides evidence of parallel pre-treatment trends as well as increasing Medicare payment over time using 2005 as the reference year. Figure 10 again provides confidence in similar pretreatment trends and suggests a larger increase in government coverage post 2006 relative to any prescription-drug coverage.

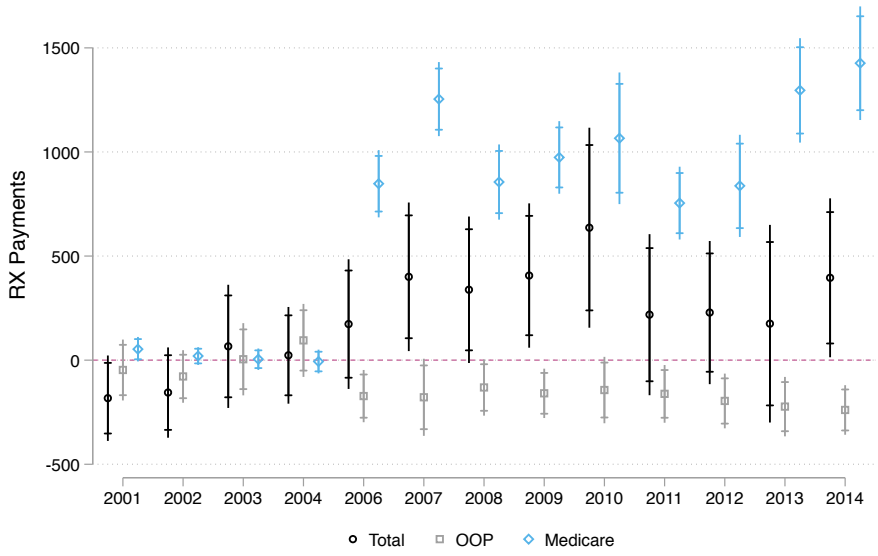


FIGURE 9.
Prescription-Drug Expenditures

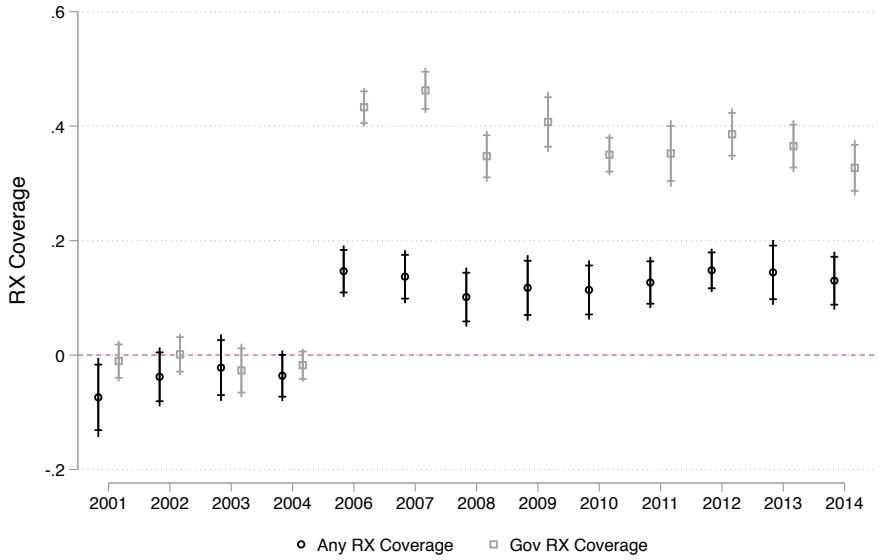


FIGURE 10.
Prescription-Drug Coverage

Preventative Care

In Table 16, we explore the potential for Medicare Part D to affect preventative-care measures and health behaviors. Specifically, we explore the extent to which preventative healthcare through recent (within the past year) cholesterol and routine checkups and flu shots have been utilized. We also explore the extent to which health behaviors including reporting joint pain, taking Aspirin, engaging in physical activity at least 3x weekly, smoking, and having a usual care provider change with the establishment of Part D.

Because the difference-in-difference estimates allow us to control for any shocks that may have affected both the Medicare eligible and ineligible cohorts post Part D, in this subsection and the subsections that follow, we focus on the difference-in-difference estimators around the establishment of Part D. However, because the baseline specification displayed for both cohorts in Panel A is informative in identifying which group is contributing to the effect, we present these estimates for all outcomes.

In columns (1), (2), and (3), the coefficient on the interaction represents the average treatment effect of Part D on cholesterol and routine checkups and flu shots for the Medicare eligible cohort. Results are small in magnitude and statistically insignificant suggesting no real substitution effect between prescription drugs and cholesterol checks or routine checkups. The coefficient on Flu Shots is negative and statistically significant at the 1% level suggesting that Part D reduces your likelihood of receiving a flu shot by 6 percentage-points.

In columns (4)-(8), results suggest the estimates on the other measures of health behavior remain small and statistically insignificant, again suggesting a lack of substitution between prescription drugs and health behaviors.

To evaluate the appropriateness of the younger ineligible cohort as the control group, we include Figure 11 and Figure 12. For all outcomes, the pretreatment estimates have confidence intervals spanning zero and, a described above, we do not find significant evidence of a post 2006 effect on these outcomes.

Insurance Attitudes

In Table 17, we investigate the extent to which Part D coverage affects attitudes towards risk and medical interventions. If gaining prescription-drug coverage motivates an individual to take more risks, this may be captured by the individual agreeing with the statement "I am more willing to take risks than the average person." Additionally, if prescription-drug coverage alters reliance on medical care or triggers selection into Medicare based on this reliance, individuals' response to the question "I can overcome ill's without medical help" may change.

TABLE 16.
Preventative Care and Health Behaviors

	Past Year Utilization							
	Cholesterol Check	Flu Shot	Checkup	Joint Pain	Aspirin	Phys. Act	Smoker	Usual Care
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Pre/Post Estimates								
				Medicare Eligible (65-75)				
Post 06	0.03*	0.02	-0.00	0.01	0.04**	-0.05**	0.01	-0.00
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
Observations	12509	12678	12643	12810	12341	7862	11891	12743
Mean	0.84	0.62	0.82	0.56	0.49	0.54	0.14	0.93
Effect Size	0.07	0.04	0.01	0.01	0.09	0.10	0.03	0.00
				Medicare Ineligible (55-64)				
Post 06	0.02	0.09***	0.00	-0.04**	0.02**	-0.02	-0.02	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Observations	18278	18594	18571	18950	18172	11372	17309	18781
Mean	0.73	0.36	0.72	0.49	0.36	0.56	0.20	0.86
Effect Size	0.03	0.18	0.01	0.08	0.05	0.05	0.05	0.02
Panel B: Difference-in-Differences Estimates								
$\geq$ Age 65 X Post 06	0.00	-0.06***	-0.01	0.03	0.01	-0.02	0.02	-0.01
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
Post 06	0.02*	0.08***	0.01	-0.03**	0.03***	-0.03*	-0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
$\geq$ Age 65	-1.28	2.87	-7.85**	-4.82	-7.85*	-0.14	-0.73	-6.44**
	(3.69)	(3.09)	(3.62)	(4.32)	(4.12)	(8.80)	(4.41)	(2.28)
Observations	30787	31272	31214	31760	30513	19234	29200	31524
Mean	0.77	0.47	0.76	0.52	0.41	0.55	0.17	0.89
Effect Size	0.00	0.13	0.03	0.07	0.03	0.03	0.06	0.03

Notes: For all specifications, gender, race, marital, employment, and health status, age, age squared, census region fixed effects and a year time trend are included as controls. Panel A displays the difference estimator-restricting the sample to the Medicare eligible population (65-75) and to the Medicare ineligible population (55-64). Panel B displays the difference-in-difference estimates and adds a cohort specific time trend in the model.*** significant at 1%; ** significant at 5%; * significant at 10%.

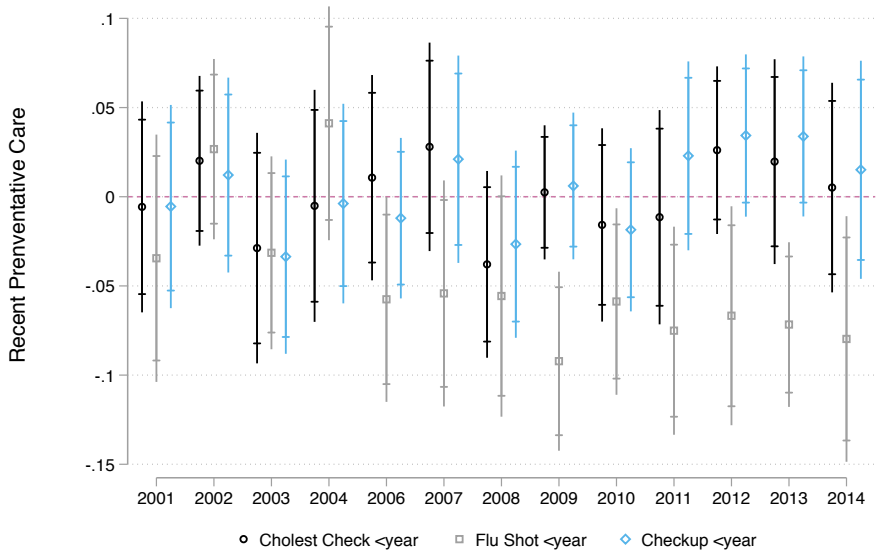


FIGURE 11.
Checkups

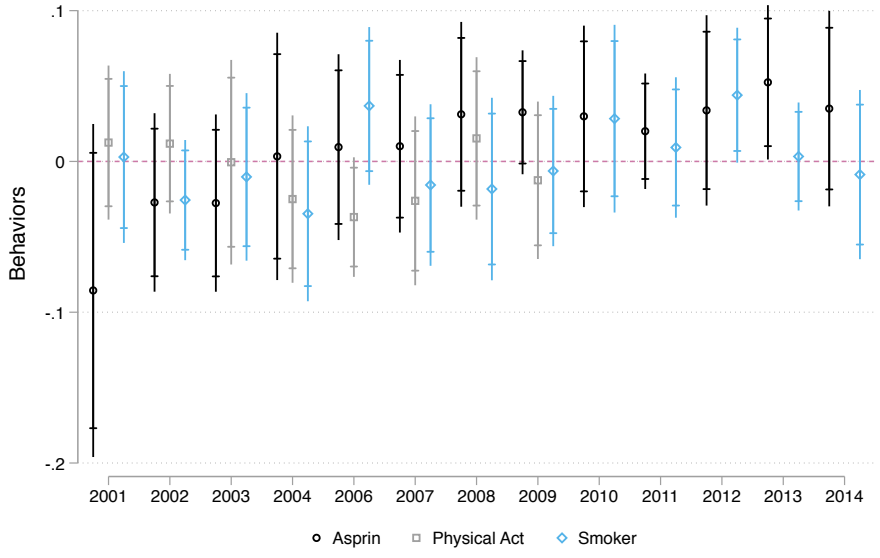


FIGURE 12.
Health Behaviors

For both outcomes, (columns (1) and (2) of Table 17) we do not find evidence to suggest individual attitudes towards risks and medical help change given the establishment of part D. The result is consistent when estimating the yearly effect post Part D in Figure 13.

TABLE 17.
Insurance Attitudes

	More Risks	Overcome w/out Meds
	(1)	(2)
Panel A: Pre/Post Estimates		
	Medicare Eligible (65-75)	
Post 06	0.01	-0.01
	(0.01)	(0.01)
Observations	11712	11755
Mean	0.17	0.14
Effect Size	0.01	0.03
	Medicare Ineligible (55-64)	
Post 06	-0.00	-0.01**
	(0.01)	(0.01)
Observations	17103	17164
Mean	0.19	0.18
Effect Size	0.01	0.04
Panel B: Difference-in-Differences Estimates		
$\geq$ Age 65 X Post 06	0.00	0.00
	(0.01)	(0.01)
Post 06	-0.00	-0.01*
	(0.01)	(0.01)
$\geq$ Age 65	-1.03	-1.58
	(3.67)	(3.07)
Observations	28815	28919
Mean	0.18	0.16
Effect Size	0.01	0.00

Notes: All specifications include demographic controls and regional FE. Panel ii of both specifications include linear time trends with a age group specific time trend in the difference in difference specification. Robust standard errors are reported in parentheses and allow for clustering at the age level in all specifications. *** significant at 1%; ** significant at 5%; * significant at 10%.

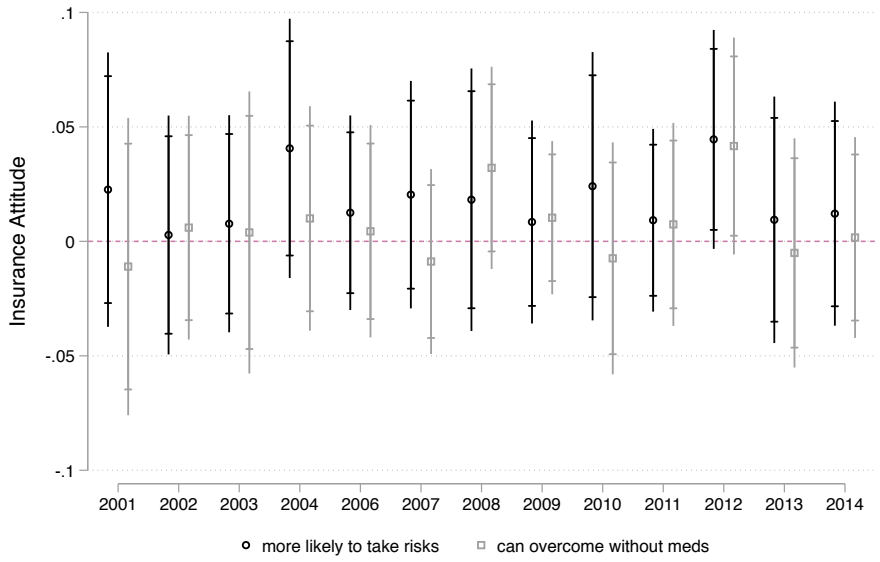


FIGURE 13.
Risk and Medical Attitudes

Healthcare Utilization

In Table 18, we explore the impact Part D had on healthcare utilization in the form of physical/occupational therapy, chiropractor, emergency room, home health, and dental visits. If Part D induced individuals to rely more heavily on prescription drugs, we may see a decrease in the number of physical/occupational therapist or chiropractor visits. As emergency room visits tend to be associated with more acute health conditions, our prior is that there will be less movement in this measure associated with the establishment of Part D. Home health visits as well as dental visits may be more closely associated with the potential increase in income associated with a reduction in out-of-pocket costs.

In columns (1)-(4) in Panel B, the sign on the effect of Part D, while negative for office based and ambulatory chiropractor visits and ambulatory physical/occupational therapy, are statistically insignificantly different from zero. Following suit, the coefficient estimates on emergency room, home health, and dental visits (columns (5)-(7)) are all have confidence intervals spanning zero and thus provide no evidence of substitution between prescription drugs and healthcare utilization.

In allowing for a flexible yearly response to Part D implementation, we do not see evidence of an effect on physical/occupational therapy and chiropractor visits. This is shown in Figure 14.

Physician Behavior

In Table 19, we shift from evaluation of the individual's behavior and care choices to those of the physician. prescription-drug coverage should affect the physician through a patient's ability to pay and thus may have consequences on physician diagnosis behavior (particularly for those diseases that are more easily treated with medication) and on more general health recommendations such as suggesting reducing fat intake and exercising more often.

TABLE 18.
Healthcare Utilization

	Office Based		Ambulatory				
	PT/OT	Chiropractor	PT/OT	Chiropractor	Emergency Room	Home Health	Dental
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Pre/Post Estimates							
				Medicare Eligible (65-75)			
Post 06	0.05	-0.17	-0.08	-0.14	-0.03	-1.90*	-0.04
	(0.13)	(0.17)	(0.15)	(0.18)	(0.02)	(1.04)	(0.07)
Observations	12860	12860	12860	12860	12860	12860	12860
Mean	0.47	0.56	0.71	0.56	0.23	4.42	1.25
Effect Size	0.02	0.04	0.02	0.04	0.04	0.06	0.02
				Medicare Ineligible (55-64)			
Post 06	-0.11	-0.06	-0.16	-0.06	-0.04*	0.28	-0.00
	(0.10)	(0.09)	(0.09)	(0.09)	(0.02)	(0.36)	(0.05)
Observations	19024	19024	19024	19024	19024	19024	19024
Mean	0.50	0.55	0.62	0.55	0.16	0.89	1.28
Effect Size	0.03	0.02	0.04	0.02	0.07	0.02	0.00
Panel B: Difference-in-Differences Estimates							
$\geq$ Age 65 X Post 06	0.04	-0.12	-0.05	-0.10	-0.01	-1.56	-0.05
	(0.17)	(0.16)	(0.18)	(0.17)	(0.03)	(1.01)	(0.07)
Post 06	-0.07	-0.06	-0.11	-0.06	-0.03	0.00	-0.01
	(0.10)	(0.09)	(0.09)	(0.09)	(0.02)	(0.39)	(0.05)
$\geq$ Age 65	-42.02	-48.44	-52.30	-42.78	-7.28	-341.51*	-37.59*
	(34.02)	(41.20)	(41.17)	(41.47)	(6.36)	(164.74)	(19.70)
Observations	31884	31884	31884	31884	31884	31884	31884
Mean	0.49	0.55	0.66	0.56	0.19	2.37	1.27
Effect Size	0.01	0.03	0.01	0.03	0.02	0.07	0.02

Notes: For all specifications, gender, race, marital, employment, and health status, age, age squared, census region fixed effects and a year time trend are included as controls. Panel A displays the difference estimator-restricting the sample to the Medicare eligible population (65-75) and to the Medicare ineligible population (55-64). Panel B displays the difference-in-difference estimates and adds a cohort specific time trend in the model.*** significant at 1%; ** significant at 5%; * significant at 10%.

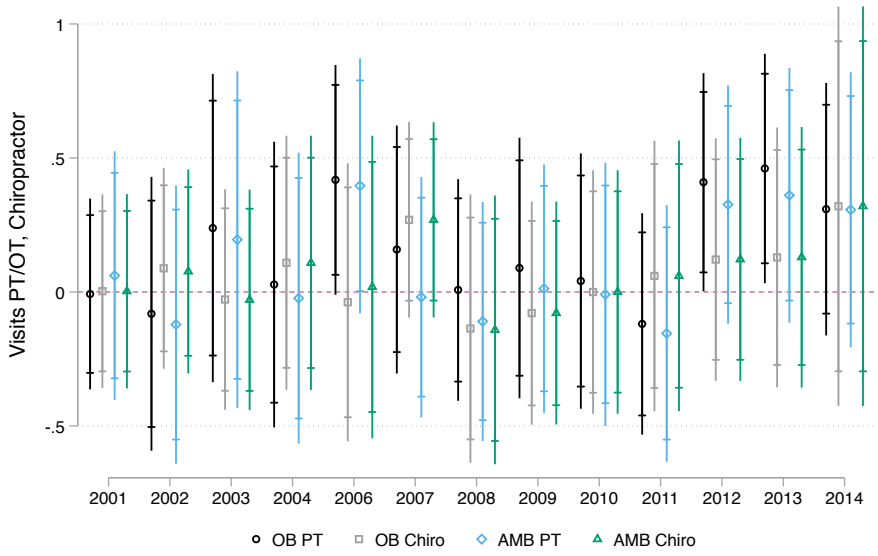


FIGURE 14.
Physical Therapy and Chiropractor Visits

In columns (1)-(4) of Panel B, we report the estimates associated with diagnoses of high blood pressure, stroke, diabetes, and arthritis. The significant effect of Part D is only visible for diabetes diagnoses- decreasing the likelihood of a physician diagnosing and individual with this disease. Although part of the effect may be attributed to physicians responding to a change in patient's drug coverage, we can not rule out the potential that access to prescription drugs mitigates pre-diabetic indicators.

In columns (5) and (6) there is nothing to suggest physicians changing their suggestions to patients after the establishment of part D. Overall, Part D does not seem to have a substantial effect on either patient preventative care, health behaviors, and attitudes nor does it have an effect on physician diagnostic and recommendation behavior.

Our confidence in the null result over time for these outcomes is demonstrated in Figure 15 and Figure 16.

TABLE 19.
Diagnosis and Doctor Behaviors

	Diagnosis					
	High Blood Pres	Stroke Diag.	Diabetes Diag.	Arthritis Diag.	No Fat	Exercise More
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Pre/Post Estimates						
			Medicare Eligible (65-75)			
Post 06	0.03*	0.01	0.00	-0.01	0.05**	0.03
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	12835	12830	12842	12812	12255	12271
Mean	0.58	0.07	0.20	0.49	0.54	0.54
Effect Size	0.06	0.02	0.00	0.02	0.10	0.06
			Medicare Ineligible (55-64)			
Post 06	0.03**	0.00	0.04***	-0.03*	0.02	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Observations	18982	19005	18998	18936	17989	18000
Mean	0.42	0.03	0.13	0.35	0.49	0.52
Effect Size	0.06	0.03	0.12	0.07	0.05	0.02
Panel B: Difference-in-Differences Estimates						
≥ Age 65 X Post 06	-0.01	0.00	-0.05***	0.01	0.01	0.01
	(0.02)	(0.01)	(0.01)	(0.03)	(0.02)	(0.03)
Post 06	0.03**	0.00	0.04***	-0.03*	0.03**	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
≥ Age 65	-8.48**	-1.79	-18.20***	-11.29**	1.44	2.32
	(3.84)	(2.80)	(4.22)	(5.40)	(4.54)	(5.89)
Observations	31817	31835	31840	31748	30244	30271
Mean	0.49	0.05	0.16	0.41	0.51	0.53
Effect Size	0.02	0.02	0.13	0.02	0.01	0.02

Notes: For all specifications, gender, race, marital, employment, and health status, age, age squared, census region fixed effects and a year time trend are included as controls. Panel A displays the difference estimator-restricting the sample to the Medicare eligible population (65-75) and to the Medicare ineligible population (55-64). Panel B displays the difference-in-difference estimates and adds a cohort specific time trend in the model.*** significant at 1%; ** significant at 5%; * significant at 10%.

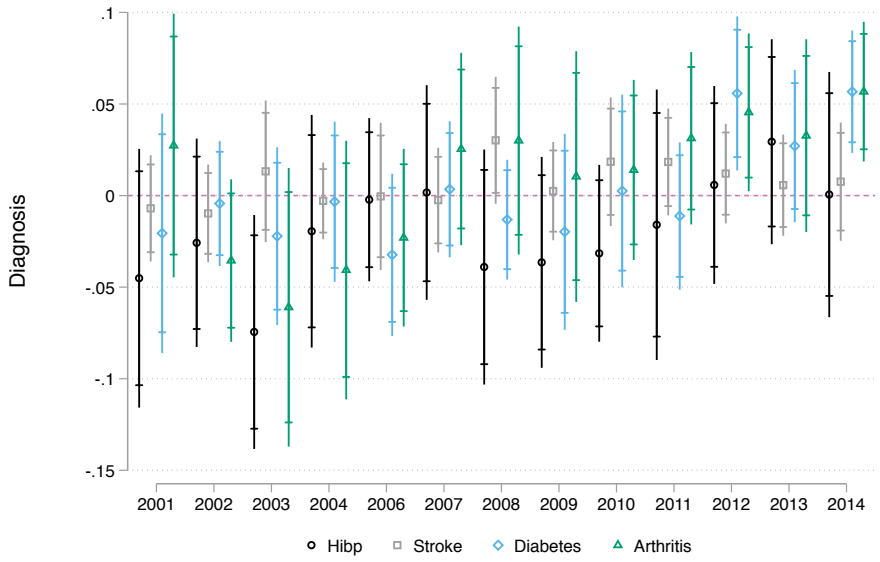


FIGURE 15.
Physician Diagnoses

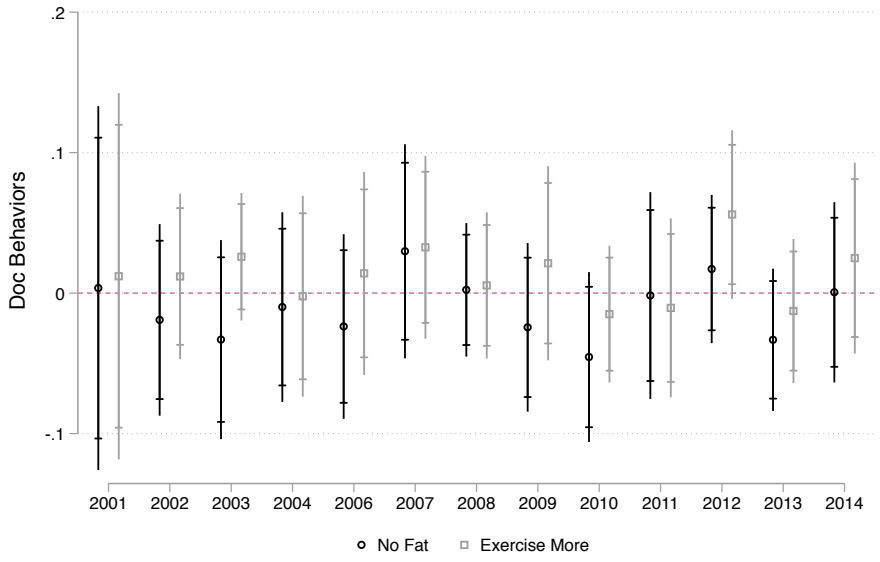


FIGURE 16.
Physician Behaviors

The Mitigating Response to Health Shocks

Before concluding our analysis of Part D's expansion on preventative care and health behaviors, we explore how preventative care and health behavior choices after a health shock change under the Part D regime. That is, we evaluate the potential for Medicare Part D to mitigate health choices after health shocks by affecting the individual's decision to engage in cholesterol checks, flu shots, smoking behavior and daily exercise. Recall, our estimates represent the causal parameter of Part D establishment on responses to health shocks if we assume there is not differential selection into health shocks based on responses to these shocks pre- and post- Part D establishment.

As described above, we exploit the panel nature of the Health and Retirement Study to explore the impact of a health shock on healthcare utilization. Restricting the sample to individuals aged 65 or older in the year 2000.

In Table 20 we explore the effect Part D has on the response to a chronic health shock (defined as a diagnosis of diabetes or high blood pressure). Although there seems to be no significant differential response to a health shock pre and post Part D establishment for flu shots or daily physical activity, results suggest a slight increase (of 7 percentage-points) in cholesterol checks and a small (3 percentage-point) increase in likelihood of smoking following a diabetes diagnosis in the post-2006 era relative to experiencing this diagnosis prior to Part D establishment. In Panel B of Table 20 we evaluate the effect to which health choices after a high blood pressure diagnosis differ pre and post Part D. In columns (1) and (2) we find no evidence to suggest a differential response in flu shots

or cholesterol checks pre and post Part D. Unlike in Panel A, the likelihood of smoking slightly decreases (by 3 percentage-points) however, there is no significant mitigating effect on physical activity.

In Table 21 we again explore the mitigating effect of Part D in health choices after health shock however, we now focus on acute shocks defined as stroke and heart attack. Similarly to our largely statistically insignificant findings in Table 20, we do not find evidence of Part D having a mitigating effect of health decisions following a stroke or a heart attack.

Discussion

While the literature on the health impacts of Part D establishment are mixed but suggest ex-post health outcomes do not decline after 2006, this paper adds the evaluation of Part D's effect on preventative care and health behaviors. We also investigate the effect to which Part D mitigates preventative care and health behavior choices following a chronic and acute health shocks. We find no evidence to suggest prescription-drug coverage through Part D reduces individuals' willingness to engage in preventative-care measure nor does it reduce the likelihood of participating in risky health behaviors. Given this, we do not find evidence to suggest that individuals rely more heavily on prescription drugs both generally and in the presence of a health shock.

TABLE 20.
Preventative Care Following Chronic Diagnosis

	Flu Shot	Cholesterol Check	Current Smoke	Physical Activity
	(1)	(2)	(3)	(4)
Panel A: Difference-in-Differences Estimates -Diabetes Diagnosis				
Diabetes X Post 06	-0.03	0.07**	0.03*	-0.05
	(0.03)	(0.03)	(0.02)	(0.06)
Post 06	-0.06***	-0.00	0.00	0.00
	(0.01)	(0.01)	(0.00)	(0.01)
Diabetes	0.01	-0.02	-0.01	0.03
	(0.02)	(0.02)	(0.01)	(0.05)
Observations	29064	28687	29046	8807
Mean	0.71	0.79	0.11	0.11
Effect Size	0.08	0.18	0.10	0.16
R^2	0.06	0.01	0.02	0.02
Panel B: Difference-in-Differences Estimates -High Blood Pressure Diagnosis				
High BP X Post 06	0.01	0.03	-0.03**	0.04
	(0.03)	(0.03)	(0.02)	(0.04)
Post 06	-0.06***	-0.01	0.00	-0.00
	(0.01)	(0.01)	(0.00)	(0.01)
High BP	-0.01	0.00	0.00	-0.00
	(0.01)	(0.01)	(0.01)	(0.04)
Observations	23462	23143	23513	8807
Mean	0.73	0.80	0.10	0.11
Effect Size	0.03	0.07	0.11	0.14

Notes: All specifications include controls for age, race, marital status, income, and region. Time invariant controls for the individuals are absorbed into the error structure by an individual level fixed effect. Sample is restricted to those individuals who are at least 65 in the year 2000. *** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 21.
Preventative Care Following Acute Diagnoses

	Flu Shot	Cholesterol Check	Current Smoke	Physical Activity
	(1)	(2)	(3)	(4)
Panel A: Difference-in-Differences Estimates -Stroke Diagnosis				
Stroke X Post 06	0.00	0.01	0.01	-0.01
	(0.02)	(0.03)	(0.01)	(0.04)
Post 06	-0.06***	-0.00	0.00	0.00
	(0.01)	(0.01)	(0.00)	(0.01)
Stroke	0.01	0.03*	-0.00	-0.01
	(0.02)	(0.02)	(0.01)	(0.04)
Observations	23462	23143	23513	8807
Mean	0.73	0.80	0.10	0.11
Effect Size	0.01	0.04	0.03	0.03
Panel B: Difference-in-Differences Estimates -Heart Attack Diagnosis				
Heart Atck X Post 06	-0.00	-0.03	0.00	-0.02
	(0.02)	(0.02)	(0.01)	(0.04)
Post 06	-0.06***	-0.00	0.00	0.00
	(0.01)	(0.01)	(0.00)	(0.01)
Heart Attack	0.00	0.02	-0.03***	0.03
	(0.01)	(0.01)	(0.01)	(0.04)
Observations	23462	23143	23513	8807
Mean	0.73	0.80	0.10	0.11
Effect Size	0.01	0.06	0.00	0.06

Notes: All specifications include controls for age, race, marital status, income, and region. Time invariant controls for the individuals are absorbed into the error structure by an individual level fixed effect. Sample is restricted to those individuals who are at least 65 in the year 2000. *** significant at 1%; ** significant at 5%; * significant at 10%.

CHAPTER V

CONCLUSION

Using both experimental and applied econometric techniques, we provide insights into several policy relevant issues-namely influences on competitive behaviors and health incentives.

In Chapter II we use a lab experiment to identify the effect of dishonest competition on the decision to opt into competitive environments paying close attention to the potential for heterogeneity by gender. We find that although, on average, cheating does not significantly affect the decision to compete, women opt out of competition more often than they do in the objectively measured environment. The potential for cheating does not seem to impact male decisions. Through this experiment we are able to provide insight into mechanisms that contribute to the gender gap in competitive behaviors.

In Chapter III we explore the impact of Prescription Drug Monitoring Programs which are state-level databases providing information on patients' drug histories, on prescription-drug abuse and overdose deaths. While these databases should allow physicians to identify risky drug use behaviors and improve prescribing behaviors generally, we find that only those programs that require prescribers to access the database at the point of prescribing reduce measured drug abuse and overdose deaths. Policy implications from this work are clear in suggesting only mandates are effective in correcting the agency problem faced in the market for prescription drugs.

Finally, in work related to the availability of prescription drugs, we explore the impact prescription-drug coverage has on health decisions related to preventative care and on health decisions following an adverse health shock. As prescription drugs became less expensive to Medicare recipients after the establishment of Part D, we explore health decisions following this legislation and find that while out-of-pocket costs and drug utilizations increase, results do not suggest any changes to preventative-care behaviors generally or following a health shock. This suggests welfare measures from increasing access to prescription drugs (through insurance coverage) are not reduced by declines in preventative-care behaviors.

APPENDIX A

SUPPLEMENTAL TABLES

Marginal Effect of Treatment: Probit Specification

TABLE A1.
Effect of Treatment on Decision to Opt into Competition-Probit Model

	(1)	(2)	(3)	(4)
Cheating Regime	-0.155	-0.109	-0.090	-0.080
	(0.11)	(0.10)	(0.10)	(0.10)
Task 2 - Task 1		0.039	0.025	0.046
		(0.04)	(0.04)	(0.04)
Task 2 Score		0.090***	0.049	0.033
		(0.03)	(0.04)	(0.04)
Task 2 Rank			-0.127*	-0.101
			(0.07)	(0.07)
Tournament 4				0.294**
				(0.13)
Observations	84	84	84	84

Notes: The dependent variable is equal to one if the participant chooses tournament for the Task 3 payment scheme, and equal to zero otherwise. Reported are average marginal effects calculated from Probit model evaluated at a participant in the control group. *** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE A2.
Heterogenous Treatment Effect by Gender-Probit Model

	(1)	(2)	(3)	(4)
CheatingRegime × Female	-0.3611*	-0.2763*	-0.1929 *	-0.2236*
	(0.14)	(0.15)	(0.15)	(0.16)
Female	0.2341**	0.1685	0.1314	0.1827*
	(0.10)	(0.12)	(0.13)	(0.11)
Cheating Regime	0.0236	0.0197	-0.0070	0.0076
	(0.13)	(0.13)	(0.14)	(0.12)
Task 2 - Task 1		0.0507	0.0383	0.0692**
		(0.03)	(0.04)	(0.03)
Task 2 Score		0.0665*	0.0390	0.0162
		(0.04)	(0.04)	(0.03)
Task 2 Rank			-0.1139*	-0.0793
			(0.07)	(0.06)
Tournament 4				0.3231***
				(0.12)
Observations	84	84	84	84

Notes: The dependent variable is equal to one if the participant chooses tournament for the Task 3 payment scheme, and equal to zero otherwise. Reported are average marginal effects calculated from probit model evaluated at a male in control group. Average marginal effects of the interaction are calculated using the Stata Inteff command developed by Ai and Norton (2003). *** significant at 1%; ** significant at 5%; * significant at 10%.

APPENDIX B

EXPERIMENT SCRIPT AND INSTRUCTIONS

Experiment Script and Instructions

Control Group

You will be handed a card when you come into the room. On it will be your experiment ID. Please find the computer station with your experiment ID attached. At your keyboard you will have a consent form that I want you to read and sign. Once these are all turned in, we can begin the experiment. Please do not touch move off the screen that is currently displayed until told to do so.

Please follow along as I read the instructions. Do not skip ahead to the next page until told to do so. Please hold all questions until the end of the experiment.

Instructions

Welcome

In the experiment today you will be asked to complete four different tasks. None of these tasks will take more than 3 minutes. At the end of the experiment you will receive $6.00 for having completed the four tasks, in addition we will randomly select one of the tasks and pay you based on your performance in that task. Once you have completed the four tasks we will determine which task counts for payment by drawing a number between 1 and 4. The method we use to determine your earnings varies across the each task.

Depending on the task, your earnings will depend on your performance only or on your performance relative to the other members in your group. Your group consists of the four people in your row. Before each task we will describe in detail how your payment will be determined.

Your total earnings from the experiment are the sum of your payment for the randomly selected task, your $6.00 payment for completing these tasks, and a $5.00 payment for participating in the experiment. At the end of the experiment you will be paid privately.

The first three tasks consist of 22 addition problems. Tasks 1, 2 and 3 differ in the payment structure that will be applied to your performance in each of these tasks. In the fourth task you will be asked to make a decision on the type of payment structure you wish to be applied to your performance on a previous task. After each task, your score for each problem will be displayed and you will be asked to confirm your performance.

Below is a space for you to enter the number and letter of the experiment ID given to you when you arrived. The number defines your group and the letter printed next to the number will assure that you receive your payment without revealing your identity. Your performance will always remain anonymous to the other participants in the experiment.

Please enter your experiment ID now and click the green next button in the lower right hand corner to continue to the first task

Task 1 - Piece Rate

For Task 1 you will be asked to calculate the sum of five numbers. You will be given 3 minutes to calculate the correct sum of a series of these problems. You cannot use a calculator to determine this sum. However, you are welcome to make use of the provided scratch paper. You will then record your answer in the blank space provided under the 5 numbers. At the end of the 3 minutes, your score for each addition problem will be displayed as well as the correct answer to these problems.

An example of the type of addition problem is displayed on your screen.

If Task 1 is randomly selected for payment, you will receive 50 cents per problem you solve correctly in the 3 minutes. Your payment does not decrease if you provide an incorrect answer to a problem and your payment for this task does not depend on the performance of the other members in your group. We refer to this payment scheme as the piece rate payment scheme.

At the end of 3 minutes, your score for each problem as well as the correct answer to these problems will be displayed on your screen. Do NOT advance past this screen until given further instructions. Please do not talk to one another for the duration of the experiment.

When I say begin, please click the next button to begin task 1.

3-minutes given to complete Task 1

You should now see the correct answers to the addition problems in task 1 as well as your answers to these questions. Please stay on this page and count the number of addition problems you have solved correctly in this task

After you have counted the number of addition problems you have solved in this task, please click the green button to confirm your performance

Task 1 - Reporting

The number of addition problems you correctly solved in Task 1 is displayed on your screen Recall that the payment scheme for this task is piece rate. That is, if this task is randomly selected for payment, you will receive 50 cents per solved addition problem regardless of the performance of the other members in your group.

In the space below, please confirm the number of questions you correctly answered in Task 1 and click the next button to continue to the next task.

Task 2 - Tournament

As in Task 1 you will be given 3 minutes to calculate the correct sum of a series of five numbers. However, if Task 2 is randomly selected for payment, your earnings depend on your performance relative to the performance of the other members in your group. The individual who correctly solves the largest number of addition problems in the group will receive $2 per solved problem, while the other three members of the group will receive no payment. We refer to this as the tournament payment scheme. You will not be informed of how you did in the tournament until all four tasks have been completed. If there are ties the winner will be randomly determined among the tied individuals.

At the end of 3 minutes, your score for each problem as well as the correct answer to these problems will be displayed on your screen. Do NOT advance past this screen until given further instructions.

When I say begin, please click the next button to begin task 2.

3-minutes given to complete Task 1

You should now see the correct answers to the addition problems in task 2 as well as your answers. Please stay on this page and count the number of addition problems you have solved correctly in this task.

After you have counted the number of addition problems you have solved in this task, please click the green next button to confirm your performance.

Grading

You will now be asked to confirm your performance on these tasks in the space provided on the following pages.

Please click the next button to continue. You will be confirming your task 1 performance first.

Task 2 - Reporting

The number of addition problems you correctly solved in Task 2 is displayed on your screen Recall that the payment scheme for this task is tournament. That is, if this task is randomly selected for payment, you will receive $2.00 per solved addition problem only if your performance is higher than the performance of the other members of your group.

In the space below, please confirm the number of questions you correctly answered in Task 2 and click the next button to continue to the first survey.

Ranking

Recall your performance in both Task 1 (the piece rate condition) and Task 2 (the tournament condition) and respond to the questions on this page. You will receive $1.00 in addition to your earnings from this experiment if you correctly guess your rank among your group in Task 1 AND in Task 2. You will not receive any additional payment if you incorrectly guess your rank for either Task 1 or Task 2. Once you have responded to the questions on this page, please click the next button to continue.

Task 3 - Choice

As in the previous two tasks, you will be given 3 minutes to calculate the correct sum of a series of five numbers. However, you will now get to choose which of the two previous payment schemes (piece rate or tournament) you prefer to apply to your performance on the third task.

If Task 3 is randomly selected for payment, then your earnings for this task will be determined as follows: If you choose the piece rate you will receive 50 cents per problem you solve regardless of the performance of the other members of your group in Task 2 or Task 3.

If you choose the tournament, your performance in Task 3 will be evaluated relative to the performance of the other three participants of your group in the Task 2 tournament. This will not include your own performance in Task 2. If you solve more problems in Task 3 than the other participants in your group solved in Task 2, then you will receive $2 for every problem you solved in Task 3. That is, your performance in Task 3 will be compared only to the Task 2 performance of the other members of your group.

You will receive no earnings for this task if you choose the tournament payment scheme and do not solve more problems now than the other members in your group did in the Task 2 tournament. Please click the next button to continue to the task 3 choice.

Task 3 - Choice

Please choose which of the two payment schemes you prefer to apply to your performance on Task 3 by circling either tournament or piece rate on this page. Once your choice has been made, please wait to continue on until told to do so.

Select the payment structure you wish to be applied to your performance in Task 3. Once your choice has been made please click the next button to continue to Task 3.

Task 3

As in the previous two tasks, you will be given 3 minutes to calculate the correct sum of a series of five numbers.

Recall, the payment scheme applied to your performance in Task 3 is determined from the choice you just made.

At the end of 3 minutes, your score for each problem as well as the correct answer to these problems will be displayed on your screen. Do NOT advance past this screen until given further instructions.

When I say begin, please click the next button to begin task 3.

3-minutes given to complete Task 1

You should now see the correct answers to the addition problems in task 3 as well as your answers. Please stay on this page and count the number of addition problems you have solved correctly in this task.

After you have counted the number of addition problems you have solved in this task, please click the green next button to confirm your performance.

Grading

The number of addition problems you correctly solved in Task 3 is displayed on your screen.

Recall that you chose the payment scheme to be applied to your performance in Task 3 prior to completing the task. In the space below, please confirm the number of questions you correctly answered in Task 3 and click the next button to continue to Task 4.

Task 4 - Submit Piece Rate

You do not have to add any numbers for the fourth and final task of the experiment. Instead, you will be paid based on your performance in Task 1. You can choose to be paid for this performance according to the piece rate or the tournament payment scheme.

If you choose the piece rate, and Task 4 is randomly selected for payment, you will receive 50 cents for each correct answer in Task 1 regardless of the performance of the other members of your group in this task.

If you choose the tournament, your performance will be evaluated relative to the performance of the other members of your group in Task 1. If your performance in Task 1 is higher than the performance of the other three participants in your group in Task 1,

then you will receive $2 dollar for each question you answered correctly. You will receive nothing for this task if you choose the tournament and your performance in Task 1 is not the highest in your group.

Recall your performance in Task 1. Please choose which payment scheme you will apply to you your performance in Task 1 by selecting either tournament or piece rate.

Once you have made your choice, please click the next button to submit your performance and choices in the experiment.

I will then give you a brief survey to complete while your payment is being calculated.

Please write your experiment ID in the space provided in the upper right hand corner and answer the questions on this survey now.

When your group number is called, please bring this survey to the experimenter in the front of the room in exchange for your payment.

At this point you will receive a payment confirmation form. You will return to your desk to fill out this form and be asked to return it to the experimenter in order to be excused from the experiment.

Treatment Group

You will be handed a card when you come into the room. On it will be your experiment ID. Please find the computer station with your experiment ID attached. At your keyboard you will have a consent form that I want you to read and sign. Once these are all turned in, we can begin the experiment. Please do not touch move off the screen that is currently displayed until told to do so.

At your desk there is a packet of papers, a manila folder, and scratch paper. You will be told when to use these as the experiment continues.

Please follow along as I read the instructions. Do not skip ahead to the next page until told to do so and please hold all questions until the end of the experiment.

Instructions

Welcome

In the experiment today you will be asked to complete four different tasks. None of these tasks will take more than 3 minutes. At the end of the experiment you will receive $6.00 for having completed the four tasks, in addition we will randomly select one of the tasks and pay you based on your performance in that task. Once you have completed the four tasks we will determine which task counts for payment by drawing a number between 1 and 4. The method we use to determine your earnings varies across the each task. Depending on the task, your earnings will depend on your performance only or on your performance relative to the other members in your group. Your group consists of the four

people in your row. Before each task we will describe in detail how your payment will be determined.

Your total earnings from the experiment are the sum of your payment for the randomly selected task, your $6.00 payment for completing these tasks, and a $5.00 payment for participating in the experiment. At the end of the experiment you will be paid privately.

The first three tasks consist of 22 addition problems. Tasks 1, 2 and 3 differ in the payment structure that will be applied to your performance in each of these tasks. In the fourth task you will be asked to make a decision on the type of payment structure you wish to be applied to your performance on a previous task. After each task, your score for each problem will be displayed and you will record your performance on the corresponding answer sheet. Below is a space for you to enter the number and letter of the experiment ID given to you when you arrived. In addition to this, there will be a space for you to record this ID in the upper right hand corner of the answer sheets for each task. The number defines your group and the letter printed next to the number will assure that you receive your payment without revealing your identity. Your performance will always remain anonymous to the other participants in the experiment.

Please enter your experiment ID now and click the green next button in the lower right hand corner to continue to the first task.

Task 1 - Piece Rate

For Task 1 you will be asked to calculate the sum of five numbers. You will be given 3 minutes to calculate the correct sum of a series of these problems. You cannot use a calculator to determine this sum. However, you are welcome to make use of the provided scratch paper. You will then record your answer in the blank space provided under the 5 numbers. At the end of the 3 minutes, your score for each addition problem will be displayed as well as the correct answer to these problems.

An example of the type of addition problem is displayed on your screen.

If Task 1 is randomly selected for payment, you will receive 50 cents per problem you solve correctly in the 3 minutes. Your payment does not decrease if you provide an incorrect answer to a problem and your payment for this task does not depend on the performance of the other members in your group. We refer to this payment scheme as the piece rate payment scheme.

At the end of 3 minutes, your score for each problem as well as the correct answer to these problems will be displayed on your screen. Do NOT advance past this screen until given further instructions, as you will need to use this page to receive payment. Please do not talk to one another for the duration of the experiment.

When I say begin, please click the next button to begin task 1.

3-minutes given to complete Task 1

You should now see the correct answers to the addition problems in task 1 as well as your answers to these questions. Please stay on this page and count the number of addition problems you have solved correctly in this task.

At the end of the experiment, we will pay you based only on your self-reported performance for each task. You will report your performance in each task on the answer sheets provided at your desk.

Recall that the payment scheme for the task you have just completed is piece rate. That is, if this task is randomly selected for payment, you will receive 50 cents per solved addition problem reported on your task 1 answer sheet regardless of the performance reported by the other members in your group.

Please write your experiment ID in the space provided in the upper right hand corner of the Task 1 answer sheet indicated by the header Answer Sheet Task 1 and report your performance on Task 1 in the space provided on this sheet. After you have reported your performance, please click the green button to continue to the next task.

Task 2 - Tournament

As in Task 1 you will be given 3 minutes to calculate the correct sum of a series of five numbers. However, if Task 2 is randomly selected for payment, your earnings depend on your reported performance relative to the reported performance of the other members in your group. The individual who solves the largest number of addition problems in the group will receive $2 per solved problem, while the other three members of the group will receive no payment.

We refer to this as the tournament payment scheme. You will not be informed of how you did in the tournament until all four tasks have been completed. If there are ties the winner will be randomly determined among the tied individuals.

At the end of 3 minutes, your score for each problem as well as the correct answer to these problems will be displayed on your screen. Do NOT advance past this screen until given further instructions, as you will need to use this page to receive payment.

When I say begin, please click the next button to begin task 2.

3-minutes given to complete Task 1

You should now see the correct answers to the addition problems in task 2 as well as your answers. Please stay on this page and count the number of addition problems you have solved in this task.

Recall that the payment scheme for this task is tournament. That is, if this task is randomly selected for payment, you will receive $2.00 per solved addition problem reported on your task 2 answer sheet only if your reported performance is higher than the reported performance of the other members of your group.

Please write your experiment ID in the space provided in the upper right hand corner of the Task 2 answer sheet indicated by the header Answer Sheet Task 2 and report your performance on Task 2 in the space provided on this sheet. After you have reported your performance, please click the green button to continue to the next task.

After you have reported your score, please place only the answer sheets with your reported performance for Task 1 and Task 2 in the manila folder at your desk and click the next button to continue to the first survey.

Ranking

Please refer to the Task 1 and 2 ranking survey indicated by the heading Survey 1 and write your experiment ID in the space provided in the upper right hand corner of this page. Recall your recorded performance in both Task 1 (the piece rate condition) and Task 2 (the tournament condition) and respond to the questions on this page. You will receive $1.00 in addition to your earnings from this experiment if you correctly guess your rank among your group in Task 1 AND in Task 2. You will not receive any additional payment if you incorrectly guess your rank for either Task 1 or Task 2.

Once you have completed this survey, please place this survey in the manila folder with your answer sheets for Task 1 and Task 2 and click the next button to continue.

Task 3 - Choice

As in the previous two tasks, you will be given 3 minutes to calculate the correct sum of a series of five numbers. However, you will now get to choose which of the two previous payment schemes (piece rate or tournament) you prefer to apply to your performance on the third task.

If Task 3 is randomly selected for payment, then your earnings for this task will be determined as follows: If you choose the piece rate you will receive 50 cents per solved addition problem reported on your task 3 answer sheet regardless of the performance reported by the other members in your group in Task 2 or Task 3.

If you choose the tournament, your reported performance in Task 3 will be evaluated relative to the reported performance of the other three participants of your group in the Task 2 tournament. This will not include your own reported performance in Task 2. If you

solve more problems in Task 3 than the other participants in your group reported solved in Task 2, then you will receive $2 per solved addition problem reported on your task 3 answer sheet. That is, your reported performance in Task 3 will be compared only to the Task 2 reported performance of the other members of your group.

You will receive no earnings for this task if you choose the tournament payment scheme and your reported performance in task 3 is not higher than the Task 2 reported performance of the other members of your group. Please click the next button to continue to the task 3 choice.

Task 3 - Choice

Please refer to the Task 3 payment scheme choice page indicated by the header Task 3 Payment Scheme Choice and write your experiment ID in the upper right hand corner of this page.

Choose which of the two payment schemes you prefer to apply to your reported performance on Task 3 by circling either tournament or piece rate on this page. Once your choice has been made, please place this payment scheme choice page in the manila folder with your answer sheets for Task 1 and Task 2 and survey 1.

When your group number is called, please gather your manila folder containing your answer sheets for Task 1, Task 2, your payment scheme choice for Task 3 and survey 1. You will give the experimenter at the front of the room only this manila folder. You will then return to your seat to continue the experiment. Do not move ahead in the experiment until told to do and please do not talk to one another during this time.

Please click the next button to continue to Task 3.

Task 3

As in the previous two tasks, you will be given 3 minutes to calculate the correct sum of a series of five numbers.

Recall, the payment scheme applied to your reported performance in Task 3 is determined from the choice you just made.

At the end of 3 minutes, your score for each problem will be displayed as well as the correct answer to these problems. Do NOT advance past this screen until given further instructions, as you will need to use this page to receive payment.

When I say begin, please click the next button to begin task 3.

3-minutes given to complete Task 1

You should now see the correct answers to the addition problems in task 3 as well as your answers. Please count the number of addition problems you have solved in this task

Recall that you chose the payment scheme to be applied to your reported performance in Task 3 prior to completing the task.

Please write your experiment ID in the space provided in the upper right hand corner of the Task 3 answer sheet indicated by the header Answer Sheet Task 3 and report your performance on the Task 3 in the space provided on this sheet.

Once you have reported your performance, please place the answer sheet with your reported performance for Task 3 in the manila folder at your desk and click the next button to continue to Task 4.

Task 4 - Submit Piece Rate

You do not have to add any numbers for the fourth and final task of the experiment. Instead, you will be paid based the performance you recorded on your Task 1 answer sheet. You can choose to be paid for this performance according to the piece rate or the tournament payment scheme.

If you choose the piece rate, and Task 4 is randomly selected for payment, you will receive 50 cents for each solved problem reported on your Task 1 answer sheet regardless of the reported performance of the other members of your group in this task.

If you choose the tournament, the reported performance on your Task 1 answer sheet will be evaluated relative to the reported performance of the other members of your group. If your performance on your Task 1 answer sheet is higher than the reported performance of the other three participants in your group, then you will receive $2 dollar per solved addition problem reported on your task 1 answer sheet. You will receive nothing for this task if you choose the tournament and the performance on your Task 1 answer sheet is not the highest in your group. Recall your reported performance in Task 1.

Refer to Task 4 indicated by the header Answer Sheet Task 4.

Please write your experiment ID in the space provided in the upper right hand corner and choose which payment scheme you will apply to your performance from the Task 1 answer sheet by circling either tournament or piece rate.

Please place this answer sheet with your Task 4 choice in the remaining manila folder at your desk and click the next button.

Reporting

When your group number is called, please gather the manila folder containing your answer sheet for Task 3 and your payment scheme choice for Task 4. You will give the experimenter at the front of the room only this folder containing the answer sheets. You will then return to your seat to complete a brief survey while your payment is calculated. Please do not talk to one another during this time.

Please click the next button to complete the task portion of the experiment.

I will then give you a brief survey to complete while your payment is being calculated.

Please write your experiment ID in the space provided in the upper right hand corner and answer the questions on this survey now.

When your group number is called, please bring this survey to the experimenter in the front of the room in exchange for your payment. At this point you will receive a payment confirmation form. You will return to your desk to fill out this form and be asked to return it to the experimenter in order to be excused from the experiment.

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