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Two Essays from the Laboratory: An Experiment in Two-Sided Markets and a Meta-Analysis of Dictator Games

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ABSTRACT

TWO ESSAYS FROM THE LABORATORY: AN EXPERIMENT IN TWO-SIDED MARKETS AND A META-ANALYSIS OF DICTATOR GAMES

By

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The broad goal of this research is to understand the implications of various institutional environments on social welfare and equity through laboratory experiments. In the first chapter, I analyze pricing structures and market behavior in markets with two-sided platforms, and for my second chapter, I explore the determinants of giving behavior in dictator game experiments.

Recent court battles between Amazon and publishing companies over the control of ebook sales prices prompted the research question for my first chapter about the welfare effects of different types of pricing schemes in two-sided markets, where the presence of indirect network effects plays a crucial role unlike in traditional, one-sided markets. I conduct a novel, two-sided market experiment with competing platforms, sellers, and buyers to compare two pricing schemes: (1) the agency pricing scheme and (2) the platform pricing scheme. Under the agency pricing scheme, *sellers* retain control of prices over their goods or services (e.g. Amazon Marketplace), whereas, under the platform pricing scheme, *platforms* have control over prices (e.g. Uber). I also allow subjects to chat with one another in another set of treatments and find that communication leads to collusive behavior but only in the Agency Pricing Treatment. My findings suggest that the platforms' lack of perfect information on the sellers' costs as well as the

less accommodating learning environment for platforms under platform pricing leads to lower market efficiency under the Platform Pricing Treatment than the Agency Pricing Treatment. As a result, policymakers may want to consider the role that information asymmetry plays across the two pricing schemes in their regulations of two-sided markets.

My second chapter, joint work with Dr. James Cox, Dr. Vjollca Sadiraj, and Sean Bokelmann, is a meta-study of dictator game experiments. Using metadata collected by Engel (2011) from 620 dictator games from 131 papers, we explore the determinants of giving behavior and test the theory of "moral reference points"—introduced by Cox et. al (2017)—to explain giving behavior. Cox et al. (2017) define the moral reference points as an observable feature of opportunity sets (in dictator games) that captures information on the players' endowments and the dictator's action space. We update Engel's (2011) data with additional information on the initial endowments and the minimal expectation points (via maximum and minimum amounts that the dictators can give or take) in order to calculate the moral reference points for each treatment.

Using this updated data, we re-estimate Engel's (2011) regression and meta-regression analyses and compare results to those when we include the moral reference points as covariates. Our results support the moral monotonicity hypothesis, the main defining characteristic of Cox et al.'s (2017) theory of moral reference points. Our findings have implications for the literature on charitable giving and altruistic behavior.

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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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CHAPTER 1

AGENCY PRICING VERSUS PLATFORM PRICING IN TWO-SIDED MARKETS: AN EXPERIMENT

1.1 Introduction and Literature Review

From 2014 through 2015, Amazon fought several court cases against five publishing companies over control of ebook sales prices. These cases have prompted debates over the welfare effects of different pricing schemes in *two-sided markets*—a particular type of market in which agents on one side (e.g. sellers) must go through a platform (e.g. Amazon Marketplace) in order to interact with agents on the other side (e.g. buyers). Even before these legal disputes, the courts had deliberated on the question of who had control of ebook sales prices: the publisher or the platforms. The matter had gone to court in 2012 when the US Department of Justice (DOJ) and 33 states filed an antitrust complaint against Apple and the "Big Five" publishing companies (Hachette Book Group, HarperCollins, Macmillan, Penguin, and Simon & Schuster) for conspiring to fix ebook prices.

Prior to Apple's entrance into the ebook industry with their introduction of iBookstore in 2010, the publishing industry had followed the *platform pricing model*, under which platforms such as Amazon would negotiate to pay sellers (in this case the publishers) an amount for every unit sold while the platforms had the ability to set sales prices for consumers. However, after Apple entered the ebook industry, the whole industry rapidly adopted an *agency pricing model*, under which publishers—the "agents"—had the power to set retail prices while platforms earned a share of the retail price.[1]

[1] Publishers had wanted control of retail prices because the platform pricing model had allowed Amazon to set low retail prices, sometimes at a loss for Amazon. Amazon had steeply discounted the ebook versions of New York Times bestselling hardcovers. These low prices competed with the higher prices of printed books and threatened traditional, brick-and-mortar book distributors. Publishers feared that the weakening of brick-and-mortar bookstores

The adoption of the agency pricing model in the ebook industry and the subsequent rise in ebook prices led the DOJ to conclude that Apple and the publishers were price-fixing, and the court decided to prohibit the publishers from forming agency contracts for the following two years.[2, 3] In 2014, right before the two-year ban on agency pricing contracts was set to end, the dispute over platform pricing and agency pricing went to court again, this time over a contract dispute between Hachette Book Group and Amazon. The court eventually allowed Hachette to set retail prices in an "agency-lite" contract which gave the publisher the right to set retail prices but with certain conditions not disclosed to the public (Trachtenberg and Bensinger, 2014). By June 2015, all the Big Five publishers had signed agency pricing contracts with Amazon.[4]

These legal battles provide the background for the two questions of this research: (1) what are the advantages and disadvantages of the platform pricing scheme versus the agency pricing scheme and, more specifically, (2) how does social welfare compare between these two pricing schemes? Few researchers have examined these questions, although two, bodies of research exist that study the agency pricing model and multi-sided platforms separately.

On one hand, while several theoretical papers have looked at the agency pricing model (Johnson, 2017; Foros et al. 2014; Wirl, 2015; Hao and Fan, 2014; Abhishek et al., 2016), these papers focus solely on a retail, *one-sided* market environment. In practice, most of the companies

---

and the strengthening of Amazon's influence in the book industry would weaken publishers' bargaining powers and lead to long-run profit losses for the publishers, especially since Amazon is currently the largest ebook retailer, capturing 70% of all ebook sales based on 2012 data. (The other big players are Barnes & Noble with about 20% of ebook sales and Apple with 10% of ebook sales [Gilbert, 2015]). The publishers were also worried that consumers would come to expect low prices for ebooks in the future.

[2] Apple reached a settlement for the class action on July 2014. On March 7, 2016, the US supreme court declined to hear Apple's challenge to an appellate court decision, and Apple was fined $450 million as part of the settlement.

[3] For more information, see Department of Justice (2012, 2013a, 2013b), Gilbert (2015), De los Santos and Wildenbeest (2014), Gaudin and White (2014), and Baye, Santos, and Wildenbeest (2015).

[4] Simon & Schuster was the first publisher to make the agency contract deal with Amazon on October 20, 2014. Hachette and Amazon made their agreement on November 13, 2014; Macmillan on December 2014; HarperCollins on April 13, 2015; and Penguin Random House on June 18, 2015.

that operate under an agency pricing scheme in recent years reside in a multi-sided market environment where *indirect network effects* are crucial features of the market dynamic. *Indirect network effects* (also known as cross-group externalities) refer to the phenomenon where agents on one side (e.g. sellers) prefer to be on platforms with more agents on the other side (e.g. buyers) and vice versa.[5] In the ebook market example, joining Amazon's Kindle platform does not benefit readers if no authors are on the platform; similarly, authors do not benefit from selling ebooks on Kindle if no readers are on the platform. In contrast to retailers in a one-sided or traditional market environment, two-sided platforms that must intermediate between the two sides are known to use different pricing strategies in order to "get both sides of the market on board" (Rochet and Tirole, 2003; Mukharlyamov and Sarin, 2019).

On the other hand, the theoretical literature on multi-sided platforms (Rochet and Tirole, 2003; Rochet and Tirole, 2006; Armstrong, 2006; Weyl, 2010) solely focuses on markets where platforms set the prices (platform pricing), even though, in practice, a significant number of two-sided platforms are increasingly the ones who have adopted the agency pricing model where sellers set the prices (e.g. Amazon Marketplace, Apple's app store, Poshmark, Steam, Upwork, etc.).[6] My research aims to build on these two bodies of literature by comparing the agency pricing model and the platform pricing model in the more realistic environment of a two-sided market in the presence of network externalities. More specifically, I conduct two-sided market experiments where I compare (1) revenue-shares asked by platforms, (2) sellers' platform-entry

[5] I focus my research on two-sided markets with sellers on one side and buyers on the other side of the platform rather than multi-sided markets where platforms need to attract more than two sides of the market. For instance, YouTube is an example of a three-sided market where the YouTube as a platform must attract content creators, viewers, and also advertisers to generate profit. See Ryman (2009) for a discussion of the formal definition of a multi-sided market.

[6] The empirical side of the literature on multi-sided platforms have focused on processing fees in credit card markets (Mukharlyamov and Sarin, 2019; Agarwal et al, 2015), access fees in magazines (Song, 2013; Kaiser and Wright, 2005), and dynamic pricing/ price surging (Lu et al., 2018; Castillo et al. (2017) to name a handful. I am not aware of empirical papers that have compared the agency pricing and platform pricing schemes in two-sided markets.

decisions, (3) pricing behavior, (4) consumer, producer, and platform surpluses, and (5) market efficiency between the two pricing treatments to determine which pricing model leads to greater social welfare.

A laboratory market experiment is beneficial to answer this type of research question because the experimenter can control for the market structure and environment (such as production costs, capacity constraints, demand) and only vary the pricing schemes—something which would be almost impossible in the field where companies vary from one another in numerous ways aside from pricing structure.

I conduct multiple, two-sided market experiments with competing platforms, sellers, and buyers under the two pricing schemes. In the *Agency Pricing Treatment* (AGP for short), sellers retain control of prices over their own goods/services and have perfect information on their own costs (e.g. Amazon Marketplace), whereas, in the *Platform Pricing Treatment* (PlatP for short), platforms have control over prices and are given imperfect information on the sellers' costs (e.g. Uber). In both treatments, platforms ask sellers for a percentage share of the revenue. In particular, my experiment focuses on two-sided markets where sellers' marginal costs increase with each unit produced.[7]

The inclusion of indirect network effects is where the two-sided market experiment departs from the traditional, one-sided market experiment. I incorporate indirect network effects into the supply and demand functions such that the seller's per-unit cost decreases on a platform with more buyers whereas the buyer's per-unit value increases on a platform with more sellers. In my two-sided market experiment, I can examine the impact of network effects on sellers' choice of platforms by varying the number of buyers on each platform across the market periods.

[7] Several two-sided markets for digital products often have zero marginal costs; however, my experiment features the more traditional upward slopping supply curve.

This experimental design allows me to compare the impact that the indirect network effects have on the revenue-share split, sellers' platform entry decisions, the sales prices, and social welfare between the two pricing schemes.

My experimental results indicate that platforms ask for higher shares of the revenue when they have control of prices (platform pricing) than when sellers have control of prices (agency pricing). Positive shares for platforms act like a tax on sellers, part of which theoretically should get passed through to consumers in the form of higher prices. Consequently, we may expect higher prices in the Platform Pricing Treatment than in the Agency Pricing Treatment. However, the parameters of my market experiment are such that, conditional on shares, I theoretically predict prices in the Agency Pricing Treatment to be higher or equal to those in the Platform Pricing Treatment because sellers can collude and set the monopoly price under agency pricing, whereas platforms in my experiment always benefit from setting the competitive equilibrium price.[8] Indeed, observed prices in the experiment are lower under the Platform Pricing Treatment than in the Agency Pricing Treatment even after controlling for shares. However, platforms are also setting prices below competitive equilibrium level more so in the Platform Pricing Treatment than sellers are in the Agency Pricing Treatment. As a result, social welfare, specifically producer surplus, in my experiment is lower under the platform pricing scheme than under the agency pricing scheme.

This lower market efficiency in the Platform Pricing Treatment may be due to platforms' imperfect information on the sellers' costs when they are deciding on both the optimal revenue-share split and the price, whereas sellers have perfect information on their costs when they set the prices in the Agency Pricing Treatment. Furthermore, platforms are tasks with setting the

[8] Note that platforms do not benefit from setting the competitive equilibrium price in all cases, but they do when using the market parameters in my experiment. See 1.3 for further explanation.

revenue-share split as well as determining the appropriate price under the platform pricing scheme, whereas platforms under the agency pricing scheme only decide on the revenue-share split. This extra dimension that platforms must consider in the platform pricing scheme as opposed to the agency pricing scheme makes it more challenging for platforms to determine and converge to the more appropriate decisions. Under both treatments, we do see market prices getting closer to the competitive equilibrium price over time, suggesting that, in the Platform Pricing Treatment, platforms may just need more market periods to learn the more appropriate revenue-share split and price combinations.

In a second set of treatments, I also look at the effect of communication on sellers' and platforms' behavior. In this second set of market periods, I allow sellers to chat with other sellers and platforms to chat with the other platform in each market group. In the absence of communication, platforms in the Agency Pricing Treatment seem to exhibit more competitive behavior (because they ask for lower shares to attract sellers) than platforms in the Platform Pricing Treatment. In contrast, the addition of communication enables platforms to explicitly collude to ask for higher shares but only in the Agency Pricing Treatment. That is, when platforms can communicate with each other and must only decide on the percentage of the revenue to ask and not the price, platforms explicitly collude and agree to ask for a higher percentage share of the seller's revenue. In comparison, communication in the Platform Pricing Treatment seems to weakly decrease the shares asked by platforms. These different effects of chat between the two treatments may be because platforms in the Platform Pricing Treatment have a harder time to coordinate on both the revenue-share split and the price than in the Agency Pricing Treatment when platforms only have to coordinate on the revenue-share split offered to sellers.

This paper seeks to contribute to the small but growing empirical literature on pricing schemes in two-sided markets with network effects and is also the first market experiment, of which I am aware, to explore the two pricing models in a two-sided market environment with network effects. Hossain et al. (2011) have conducted the most relevant experiment in which subjects are asked to make entry decisions between two platforms in a two-sided market. However, their subjects do not make any pricing decisions. Studying entry decisions, while helpful for understanding the performance of perfectly competitive two-sided markets, offer limited insights for two-sided market performance in the presence of market power. Other market experiments that feature "upstream" and "downstream" firms look at vertical mergers and foreclosures in the traditional, one-sided retail environment (Durham, 2000; Martin et al., 2001; Badasyan et al., 2009; and Normann, 2011) but do not look at agency pricing, platform pricing, or two-sided markets.

The remainder of this paper is as follows. In section 1.2, I outline the timing and structure of my two-sided market and my experimental design. In section 1.3, I discuss the demand and supply functions in the market, how I model indirect network effects in the two-sided market, and the resulting theoretical predictions. Section 1.4 contains experimental results and regression analyses, and section 1.5 concludes with a discussion of the implications of my findings for antitrust policy regarding these two pricing schemes.

1.2 Two-Sided Market Structure and Experimental Design

Before I discuss the theoretical predictions in my two-sided market experiment, I first outline the structure of the two-sided market. In this paper, I consider two, "posted-offer" pricing treatments within the two-sided market: (1) the *Agency Pricing Treatment* where sellers set the price and (2) the *Platform Pricing Treatment* where platforms set the price.[9] In both treatments, each market group contains 2 platforms, 4 sellers, and 10 buyers. The computer makes decisions for the ten buyers.[10] Subjects keep their randomly assigned role of seller or platform throughout the experiment and participate in multiple periods of the computerized, market experiment followed by a demographic survey.

At the beginning of each period, all subjects are shown the number of buyers on each of the platforms in their market group. In all treatments, platforms first ask the sellers for a percentage share of the revenue from any sales made on their platform. Although sellers see the percentage shares asked by both platforms, platforms do not know what the other platform has asked the sellers.[11] After the platforms make their revenue-share split decisions, the structure of the experiment differs between the two treatments in the following ways. <u>In the Agency Pricing Treatment (AGP)</u>, *sellers* choose a platform to enter and then decide the price and quantity of a homogenous good to sell on their chosen platform. <u>In the Platform Pricing Treatment (PlatP)</u>, *platforms* set the price, after which *sellers* choose a platform and decide on the quantity of a homogenous good to sell on their chosen platform. Sellers have the option to choose "No Platform" in every period, in which case the sellers do not sell or earn anything in that period.

For every unit sold, sellers pay a production cost—the cost structure of which is based on a supply function that I specify in Table 2 and explain in Section 1.3. In both pricing treatments, the sellers know their exact marginal cost. However, in the Agency Pricing Treatment, platforms

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[9] In a "posted-offer" market, sellers post the sales price, and buyers can only make a take-it-or-leave-it purchase decision. Thus, buyers do not have the ability to make a counteroffer.

[10] I explain how the computer makes purchase decisions for buyers in the later part of this section.

[11] The original motivation behind this market experiment was to examine the Amazon vs. publishers' case. Amazon and publishers had not publicly disclosed details of their current agency pricing contract, including any information on the percentage of the revenue that Amazon receives. Following the ebook industry case, I designed the experiment such that platforms do not know the revenue-share split on the other platform. In the communication treatment, platforms may inform their competitor of the revenue-split they offer, but in all treatments both platforms make their decisions simultaneously.

are given no information on the sellers' cost structure, while in the Platform Pricing Treatment, platforms are given some ranges of values where the sellers' costs may be so that they can make more appropriate pricing decisions.[12] This information structure attempts to mimic the dynamics in real two-sided markets where a big reason platforms might choose an agency pricing scheme is to avoid having to guess or gather data on sellers' costs in order to set the optimal prices and instead allow sellers with perfect information of their own costs to set their own prices. Meanwhile, companies under the platform pricing scheme typically have a team within the company that exclusively focuses on estimating demand and supply functions in order to set their price structures—Uber being a prime example. Consequently, platforms in the Platform Pricing Treatment have imperfect information on the sellers' costs. While my experimental design does not allow platforms to choose between the platform pricing scheme and the agency pricing scheme, my design does allow me to get some insight into which of the two pricing practices is more profitable for platforms by comparing platforms' profits between the Agency Pricing Treatment and the Platform Pricing Treatment.

In my market experiment, the sellers earn money by selling units on a platform. The sellers' earnings can be calculated as their share of the revenue minus the total costs for the units sold. Platforms do not have any costs; therefore, their earnings are simply the portion of the revenue that goes to them from any unit sold on their platform.[13] The software calculates and shows all the information to subjects on the computer screen. After sellers make their decisions, the computer makes purchase decisions for the ten buyers, and everyone (sellers and platforms)

[12] In the Platform Pricing Treatment, I show platforms a bandwidth of 100 that contains the sellers' actual marginal cost for each unit. I randomly vary the distance from the start of the bandwidth to the actual marginal cost for each unit, although all platforms see the same ranges.

[13] Typically, platforms have very small or negligible costs for additional purchases made on their platform. The biggest cost for platforms comes from establishing and maintaining their platform, which could be interpreted as a fixed cost every period. In my experiment, I normalize the fixed cost to zero, since my paper does not consider platform bankruptcy.

sees all quantities and prices offered and sold by sellers on each platform. The market period ends, and the next market period starts.

I have computers make decisions for the buyers for three reasons. First, previous market experiments have found that in “posted-offer” markets, where buyers cannot make counteroffer prices, buyers do not strategize beyond deciding to purchase all the units that give them positive earnings (Smith, 1991; Ketcham et al., 1984—see Plott, 1982 for a survey of the literature). Strategically, buyers could withhold purchase and receive zero earnings for one period to punish and pressure sellers to lower their prices in the next period. However, empirical evidence strongly demonstrates that buyers act more mechanically when making purchase decisions in a “posted-offer” environment. As a result, I base the computer’s purchase decisions on this same observed, buyer behavior. Second, allowing the computer to make purchase decisions based on the exact demand function of the market (that depends on the number of sellers on the platform) reduces noise in the data, which allows me to focus my analyses on how the different treatment parameters specifically affect platforms and sellers’ behavior. Third, not having to recruit more subjects to be the buyers allows me to increase the number of buyers in each market group and the number of market groups I can have in each session of my experiment.

Given these reasons, the computer makes purchases decisions based on the following rules. First, buyers on a platform can only purchase units from sellers on that platform. As in any classic market experiment, each buyer has a set of units they wish to purchase and a value for each of those units. The value of the units is based on a demand function that I specify in Table 2 and discuss in Section 1.3. The computer only makes a purchase for a buyer if the price is below or equal to the buyer’s valuation of the unit. While sellers and platforms do not know the exact values of the units for the buyers, they can observe which units were sold and at which prices.

The computer always buys the unit with the lowest price first for the buyer with the highest valuation of the unit.[14] The computer makes purchase decisions until all purchases following the previously stated rules have been made. I give all subjects the exact information on how the computer makes purchase decisions both in the subject instructions and through a brief presentation where I give examples in the front of the laboratory to ensure that subjects clearly understand how the computer makes purchase decisions for buyers. For the exact instructions that I give to subjects, a copy of the subject instructions can be found in Appendix D.

In each session, subjects participated in a practice round that lasted three periods followed by one market round with 16 periods and a second market round with 12 periods. I rematch subjects to different market groups at the beginning of each market round. As mentioned in the introduction, a crucial feature of two-sided markets is the presence of network effects. I incorporate the network effects into the supply and demand functions such that supply on a platform increases with more buyers on the platform and demand on a platform increases with more sellers on the platform. I detail the demand and supply functions and how I incorporate the network effects in Section 1.3. To examine how network effects influence subjects' decisions, I vary the number of buyers on the platforms across periods. Table 1 has a summary of the treatments with information on the number of buyers on each platform for each period. In every period, either each platform has 5 buyers (labeled "5-5 split" in Table 1) or one platform has 2 buyers while the other has 8 buyers (labeled "2-8 split" in Table 1). In the periods with "2-8 split," the platform with 2 buyers switches every 2 periods so that both platforms have an equal number of periods with 2 buyers and 8 buyers on their platform and have an equal chance of earning the same potential profit.

[14] This procedure ensures that I can calculate consumer surplus in the traditional way as the area under the demand curve that is above the price. Therefore, my estimates will provide an upper bound of market efficiency.

In the second round of market periods, I allow sellers to chat with other sellers and platforms to chat with the other platform in each market group at the beginning of every two periods (labeled "chat" in Table 1). At the start of the chat period, I give all subjects information on the number of buyers that are on each platform. I introduce chat in the experiment to look at two outcomes. First, previous market experiments have found that in markets with only two sellers, sellers tacitly collude to increase prices even in the absence of communication (Plott, 1982; Mason et al., 1992). Because my experiment features only two platforms, I look to see if platforms tacitly collude to increase the percent shares asked in the absence of communication and see if platforms explicitly agree to collude to increase the percent shares once able to communicate with each other.

Table 1. Summary of Treatments

Round	# of Periods	AGP	PlatP	AGP ext.	PlatP ext.
Practice	3	5-5 split	5-5 split	5-5 split	5-5 split
Round 1	4	5-5 split	5-5 split	5-5 split	5-5 split
	4	2-8 split	2-8 split	5-5 split	5-5 split
	4	2-8 split	2-8 split	2-8 split	2-8 split
	4	5-5 split	5-5 split	2-8 split	2-8 split
Round 2	4	chat—5-5 split	chat—5-5 split	2-8 split	2-8 split
	4	chat—2-8 split	chat—2-8 split	2-8 split	2-8 split
	4	chat—5-5 split	chat—5-5 split	5-5 split	5-5 split

Note: "5-5 split" refers to 5 buyers on each platform; "2-8 split" refers to 2 buyers on one platform and 8 buyers on the other platform where the platform with 2 buyers switches every two periods; and "chat" refers to the chat treatment wherein subjects can chat with their competitor(s) before the start of every 2 periods.

Second, the supply and demand functions that I specify in Section 1.3 incorporate network effects that increase demand with more sellers and increase supply with more buyers on the platform. Because the network effects are such that sellers should want to be on platforms with more buyers and vice versa, we should see sellers converging to one platform, especially on a platform with more buyers. In periods with the 2-8 split, one platform has more buyers than the other, and so I expect sellers to "tip" to the platform with 8 buyers. When the number of buyers is the same on each platform, sellers still benefit from being on platforms with more sellers although, in the absence of communication, the choice of platform to converge to may not be as clear as in the 2-8 split. I introduce communication among sellers to determine if sellers choose to explicitly coordinate on their choice of platform. If the sellers do explicitly coordinate, I can gain insight into the factors that determine their choice of platform.

In two additional treatments, I extend the number of periods that subjects see the same 5-5 split or 2-8 split of buyers to see if platforms and sellers require more periods with the same parameters to learn and converge to equilibrium predictions. I label these two treatments AGP ext. and PlatP ext. in Table 1. I expect that subjects can better learn how to play the game when they are given more periods with the same market parameters.

In the next section, I discuss the demand and supply functions used in the experiment; detail how I incorporate the network effects into the two-sided market; and provide theoretical predictions for both pricing models.

1.3 Theoretical Model and Predictions

As mentioned in the previous section, I induce the seller's cost structure and the buyers' valuations. I provide the exact parameters of the supply and demand functions in Table 2.[15] Abstracting away from the discrete case, we can write the demand (Q_d) and supply (Q_s) functions as the following continuous functions

[15] Figure B.1-B.3 in Appendix B graphically illustrates the market demand and supply functions depending on the different number of buyers and sellers on the platform.

$$Q_d = \frac{n_b(450 + 150n_s - P)}{50} \text{ and } Q_s = \frac{n_s\big((1-s)P - 250 + 25n_b\big)}{100},$$

where n_s is the number of sellers, n_b is the number of buyers, s is the share of the revenue for the platform, and P is the price.[16, 17] The buyer's demand is a standard, downward-sloping function that reflects the diminishing marginal value for each additional unit demanded, and the seller's supply function is a standard upward-sloping supply curve reflecting the increasing marginal cost for each additional unit produced. Where my supply and demand structure differ from those in the traditional, one-sided market is with the incorporation of the indirect network effects, a crucial feature of two-sided markets, which is why my demand and supply functions rely on the number of buyers and sellers on the platform.

I model my two-sided market based on the theoretical framework established by Katz and Shapiro (1994), Caillaud and Jullien (2003), Rochet and Tirole (2003, 2006), and Armstrong (2006) and further developed by Weyl (2010) and White and Weyl (2016). I especially base my two-sided market structure on work by Rochet and Tirole (2006) who develop a basic framework for two-sided markets with payment between the two sides of the market. However, Rochet and Tirole (2006) only model a two-sided market under a platform pricing scheme where platforms set the sales price and subscription fees for both sellers and buyers on the platform. In both my agency pricing treatment and platform pricing treatment, I incorporate the indirect network effects into the supply and demand function by adapting the two-sided market model with payment between the two sides first introduced by Rochet and Tirole (2006).

[16] Note that Table 2B depicts the supply function when $s = 0$.
[17] I use discrete numbers in my data analyses, but the continuous case is useful to derive the comparative statics in my theoretical section.

Table 2. Supply and Demand Parameters

Table 2A. Buyer's Induced Values

Buyer's Values Minus Search Cost				
Unit	**4 sellers**	**3 sellers**	**2 sellers**	**1 seller**
1	1000	850	700	550
2	950	850	650	500
3	900	750	600	450
4	850	700	550	400
5	800	650	500	350
6	750	600	450	300
7	700	550	400	250
8	650	500	350	200

Table 2B. Seller's Induced Costs

Seller's Total Costs			
Unit	**2 buyers**	**5 buyers**	**8 buyers**
1	300	225	150
2	400	325	250
3	500	425	350
4	600	525	450
5	700	625	550
6	800	725	650
7	900	825	750
8	1000	925	850

Note that Table 2A lists the values for only one buyer, and Table 2B lists the values for only one seller. All buyers and homogeneous, and all sellers are homogenous. To create the market demand function, we multiply the number of units at the valuations in Table 2A by the number of buyers. To create the market supply function, we multiply the number of units at the costs in Table 2B by the number of sellers. Figure B.1-B.3 in Appendix B graphically illustrates the market demand and supply functions depending on the different number of buyers and sellers on the platform.

The indirect network effects in my two-sided market experiment are such that the buyer's per-unit values increase on a platform with more sellers, while the seller's per-unit costs decrease on a platform with more buyers. In my subject instructions, I explain and frame the indirect network effect as a "search cost."[18] Both buyers and sellers have a search cost that they must pay for every unit bought or sold. The seller's search cost reduces on a platform with more buyers. Graphically, the seller's supply curve shifts to the right with more buyers on the platform. Similarly, the buyer's search cost decreases for buyers on a platform with more sellers. Graphically, the buyer's demand curve shifts to the right with more sellers on the platform. Figures B.1-B.3 in Appendix B graphically depict the parameters of my supply and demand functions which incorporates the indirect network effects for all the different, possible combinations of numbers of buyers and sellers on a platform.

The direct result of the indirect network effects that I impose into the demand and supply functions is that buyers benefit from being on a platform with more sellers, while sellers benefit from being on a platform with more buyers. In other words, more buyers on a platform encourage more sellers to join that platform, which encourages even more sellers to join that platform, which encourages even more buyers to join that platform, …, etc., creating a feedback loop that stops when the market reaches the equilibrium number of sellers and buyers on the platform based on the pricing structure of the market. The classic, *indirect* consequence of this feedback loop is that sellers benefit from more *sellers* to be on the platform because more sellers attract more buyers to the platform. Similarly, buyers also want more buyers to be on a platform

[18] To ensure that subjects understand how the indirect network effects operate in the market experiment, I explicitly inform all subjects how the search costs affect both buyers and sellers in the subject instructions and also demonstrate with two examples in a PowerPoint presentation given during the subject instructions phase of the experiment. (Please see the subject instructions in the Appendix D for exact wording used). For sellers, I tell the subjects that they have two types of costs: a "production cost" and a "search cost" for every unit. The production costs remain the same throughout the entire experiment, but the search cost decreases with more buyers on a platform.

because more buyers on a platform attract more sellers. However, more sellers or more buyers on the platform lead the market to become more competitive on that side of the market. These two opposite effects determine the distribution of sellers and buyers across platforms and determine whether tipping occurs. In the model developed by the researchers mentioned earlier, this feedback loop dimension of two-sided markets leads to multiple market equilibria because different market equilibrium prices exist depending on the different numbers of agents on either side of the platform.

The dynamic nature of this type of two-sided market as sellers and buyers freely enter and leave a platform is difficult to emulate in an experiment, which is why my two-sided market experiment focuses exclusively on seller's entry decisions rather than both seller's and buyer's entry decisions. I control and vary the number of buyers on the platform in every period to determine precisely how the indirect network effect that I have incorporated into the supply and demand functions influence sellers' and platforms' decisions when the number of buyers is static. The indirect network effects in my demand and supply structure in my experiment are dominant enough that sellers can earn more if they all converge or "tip" to one platform, especially the one with more buyers.[19] Consequently, I formulate my first hypothesis.

Hypothesis 1.

In a Nash equilibrium and conditional on s, sellers converge or "tip" to one platform, specifically the one with more buyers. In the periods with 2-8 split, sellers choose the platform with 8 buyers.

The presence of the indirect network effects leads the market equilibrium price to vary depending on both the number of buyers and the number of sellers on the platform. An increase in the number of buyers on a platform decreases the seller's costs and, thus, leads to a decrease in

[19] See the end of Appendix A for the proof.

the competitive equilibrium price as a result of more buyers on the platform. Similarly, an increase in the number of sellers on a platform increases the buyer's per-unit valuation, which increases the competitive equilibrium price. Figures B.1-B.3 in Appendix B graphically depicts the change in equilibrium price and quantity (where supply and demand intersect) based on the number of sellers and buyers on the platform when the share for platforms (s) is zero.

Given a revenue-share split, we can determine both the competitive equilibrium outcome and the best response functions for sellers and platforms in either treatment.[20] Using the supply (Q_s) and demand (Q_d) functions mentioned earlier, we calculate the competitive equilibrium quantity Q^* and equilibrium price P^* as the following

$$Q^* = \frac{n_b * n_s(8 + n_b + 6n_s(s-1) - 18s)}{2(2n_b + n_s - n_s * s)}$$

$$P^* = \frac{900n_b + 250n_s + 275n_b * n_s}{2n_b + n_s - n_s * s}.$$

In the Agency Pricing Treatment, sellers choose the price and quantity they wish to sell on the platform. In a perfectly competitive environment, sellers would choose P^* and Q^*. However, sellers could also choose to collude and set the monopoly quantity (Q_m) and price (P_m), which is the following

$$Q_m = \frac{n_b * n_s(8 + n_b + 6n_s - 18s - 6n_s * s)}{4(2n_b + n_s - n_s * s)}$$

$$P_m = \frac{25(n_b(72 + 23n_s) + 2n_s(14 - 3n_s(s-1) - 9s)}{2(2n_b + n_s - n_s * 2)}$$

Depending on whether sellers decide to collude to set the monopoly price or converge instead to the competitive equilibrium price, we know sellers should set a price between these two prices.

[20] See Appendix A for all the derivations of the solutions that I discuss in this section.

In the Platform Pricing Treatment, platforms decide on both revenue-shares and prices. Conditional on s or the revenue-share for the platform, the platform wants to maximize the platform's profit, which is the same thing as maximizing industry revenue because the platform does not have any (direct) costs. After solving the maximization problem, we find that, for the market parameters allowed in my experiment, the competitive equilibrium price and quantity maximize the platform's profits.[21] I note, however, that this result is not generally true for all market parameters. For instance, the competitive equilibrium price does not maximize the platform's profit when the number of buyers is 2 and the number of sellers is 10.

Nevertheless, for the market parameters in my experiment, sellers in the Agency Pricing Treatment set prices between the collusive price and the competitive equilibrium price, while the platforms in the Platform Pricing Treatment set prices at the competitive equilibrium price, which is always lower or equal to the collusive, monopoly price. Consequently, we formulate our second hypothesis.

Hypothesis 2.

At any given combinations of $n_s \in \{1, 2, 3, 4\}$, $n_b \in \{2, 5, 8\}$, and $s \in [0,1]$, prices in the *Agency Pricing Treatment* are higher or equal to those found in the *Platform Pricing Treatment*.

Prices are potentially higher when sellers are setting prices than when platforms are setting prices because sellers must consider the increasing marginal cost with an additional quantity sold, whereas platforms do not experience a direct marginal cost from an increase in quantity. Instead, with the market parameters in my experiment, platforms gain from an increase in the quantity sold, which is maximized at the competitive equilibrium price.

[21] That is, for any combinations of $n_s \in \{1, 2, 3, 4\}$, $n_b \in \{2, 5, 8\}$, and $s \in [0,1]$, platforms maximize profits at the competitive equilibrium price. See Appendix A for derivations and further discussion.

Aside from the presence of indirect network effect, another factor in my two-sided market that differs from the traditional, one-sided market is the percent share asked by platforms. In either pricing treatments, the percent-share asked by platforms affects both the seller's collusive price and the competitive equilibrium price. A positive percent share for platforms act like a tax on sellers and so distort both the competitive equilibrium price and the seller's collusive price. The sellers internalize the portion of the revenue that they must give to platforms as an additional cost whose magnitude depends on both the percentage share asked and the final sales price, which is why the revenue-share s is included in the supply function Q_s. Part of this "tax" on sellers eventually gets passed through to the consumers in the form of an increase in price. Indeed, all the prices that we derive in this section increase with an increase in the percentage of the revenue for the platform. Thus, within the pricing treatments, I expect higher prices in markets where platforms have asked for higher shares of the revenue.

Hypothesis 3.

Within the same pricing treatment, prices are higher on platforms that have asked for higher shares of the revenue.

We now turn to the platforms' decision on the revenue-share split. We look first at the case when there is an equal number of buyers on each platform, under both treatments, the platforms compete to lure sellers by reducing the percent shares asked for platforms. All things equal, sellers would always prefer the platform that asks for the lower share. A platform with no sellers makes no profit. Consequently, in the perfectly competitive environment and with an equal number of buyers on each platform, platforms compete and drive the share down to 1 percent.

Hypothesis 4.

In both treatments when platforms have the same number of buyers, platforms compete for sellers and drive the share down to 1 percent.

As a counter to Hypothesis 4, previous market experiments have consistently found that when markets only have two, competing and identical sellers, the two players often tacitly collude to increase prices above competitive equilibrium levels. Because each market group in my experiment only has two platforms, the two platforms in the 5-5 split may tacitly collude to increase the percent shares asked to be above the perfectly competitive equilibrium. Given results from previous market experiments, I expect platforms to collude to set shares above the competitive equilibrium level when platforms have an equal number of buyers. I specify this collusive prediction in the following alternate hypothesis.

Hypothesis 4 – Alternate.

In both treatments when platforms have the same number of buyers, platforms collude and ask for shares above 1%.

Looking now at the case when there are different numbers of buyers on the two platforms, Hypothesis 1 already noted that, at the Nash equilibrium, sellers earn more when they "tip" to the platform with 8 buyers.[22] The platform with 8 buyers can leverage the larger quantity demanded and the sellers' lower marginal cost on their platform by asking for a higher share than the competitive equilibrium level of 1%. However, if the platform with 8 buyers asks for too high a share, the other platform can still ask for a low enough share to lure all the sellers.

In the perfectly competitive equilibrium, the platform with 2 buyers asks for a 1 percent share, while the platform with 8 buyers asks for the highest share that still gives the sellers more

[22] See Appendix A for proof and further discussion.

profit than they would get in the platform with 2 buyers. If prices are at the collusive (monopoly) level, then the platform with 8 buyers can ask for at most 3% of the share and each seller still prefers to be on the platform with 8 buyers regardless of the other sellers' platform entry decisions.[23] If instead the price is set at the competitive equilibrium level, then the platform with 8 buyers can at most ask for a 10% share and each seller still prefers to be on the platform with 8 buyers regardless of the other sellers' platform entry decisions. Consequently, I formulate my fifth hypothesis.

Hypothesis 5.

In the 2-8 split periods, platforms with 8 buyers ask for higher shares than the platform with 2 buyers.

All of the theoretical predictions I have derived so far assume that sellers and platforms have perfect information about demand and supply. However, in my experiment, while sellers have perfect information on their supply functions, platforms only have an imprecise idea of the sellers' cost structure. As a result, I do expect that platforms require more market periods to stabilize prices in the Platform Pricing Treatment than sellers do in the Agency Pricing Treatment, which brings me to my final hypothesis.

Hypothesis 6.

Platforms in the Platform Pricing Treatment require more market periods to stabilize prices than sellers in the Agency Pricing Treatment due to information asymmetry on sellers' costs across the two treatments.

In the next section, I present experimental results and discuss how the data compares to our hypotheses.

[23] See the end of Appendix A for proof.

1.4 Experimental Results

From July to October of 2019, I conducted 8 experimental sessions with 27 market groups of 6 subjects each for a total of 162 participants at Georgia State University's ExCEN laboratory. The experiments are programmed and run using the z-Tree software developed by Fischbacher (2007). I give subjects a time limit for every decision stage to ensure that subjects could fully participate within a reasonable time frame.[24] Each session took about 2 hours in total, of which the first 45 minutes are spent on subject instructions and the practice round. I increase the time limit during the practice round of the market experiment. Because this market experiment is relatively more complicated than the typical market experiment, I give subjects ample opportunity to ask questions as we go over the subject instructions and as they familiarize themselves with the software in the practice periods. The computer provides a history of the subjects' previous period decisions at every decision stage to help subjects learn over time. At the end of the experiment, I ask subjects to complete a demographic questionnaire before paying them. Subjects are paid their earnings for all rounds except for the practice round at a rate of $1 for every 1,750 points. The average earning per subject is $28.37.

Table 3 provides some demographic statistics on the subjects for each treatment. I use this demographic information as controls in my analysis. Most subjects in my experimental sessions are female, African American undergraduates. This distribution reflects the subject pool that Georgia State University's ExCEN laboratory typically receives. In the following subsections, I present and discuss experimental data and regression results on percent shares

[24] The platforms use a slider to make their revenue-share split decisions. Consequently, for platforms who run out of time, the revenue-share split is taken as whatever value the slider is on when the time ends. In the Platform Pricing Treatment, if platforms run out of time, I set the price to be 9999, which is a price more than any buyer would be willing to pay. For sellers, if time runs out, I automatically set their decision to "No Platform" for that period. There were very few periods where a subject ran out of time.

asked by platforms, the prices set by either sellers or platforms, the consumer and producer surpluses, platform profit, total social welfare, market efficiency, and the sellers' platform choice. I also discuss how these results compare to the theoretical predictions discussed in Section 1.3.

Table 3. Descriptive Statistics

	AGP	PlatP	AGP ext.	PlatP ext.
# of subjects	60	48	24	30
Black	75%	63%	67%	57%
White	8%	8%	4%	13%
Asian	6%	27%	13%	13%
Other ethnicity	10%	2%	17%	17%
Female	55%	48%	63%	73%
Freshman	5%	21%	46%	40%
Sophomore	18%	35%	29%	23%
Junior	25%	10%	13%	20%
Senior	37%	33%	8%	17%
Masters	7%	0%	0%	0%
Has prior experience with market experiment	58%	48%	30%	23%
Economics major	2%	4%	0%	3%
Business major	17%	13%	8%	17%

1.4.1 Percentage Shares Asked by Platforms

I first look at the revenue-share splits offered by platforms. Before subjects make pricing decisions, platforms must first ask the sellers for a percentage of the revenue for selling on their platform. Sellers later decide on which platform to enter after seeing these offers. Figure 1 depicts the average revenue-share asked by platforms across the market periods in the Agency Pricing Treatment (AGP) and the Platform Pricing Treatment (PlatP) on platforms with 5 buyers (Figure 1A), platforms with 8 buyers (Figure 1B), and platforms with 2 buyers (Figure 1C) separately.[25, 26] The vertical red line after period 8 indicates the start of the second market round when subjects can message their competitor(s) through a chatbox on their computer.[27]

Figure 1 clearly refutes Hypothesis 4, which states that platforms compete and drive shares to 1%. Instead, the data favors Hypothesis 4-Alternative which predicts that platforms ask for shares above the competitive equilibrium level. Focusing on the first market round (periods 1 through 8), Figures 1A illustrates that the shares asked by platforms with 5 buyers (Platform-5 for short) do tend to decrease over time under either pricing treatments and by platforms with 8 buyers (Platform-8 for short) under only the Platform Pricing Treatment, suggesting increasing competitive behavior between platforms over time. During the periods with the 2-8 split, the platform that has 2 buyers switches every two periods, which explains some of the more erratic shares asked by Platform-2 and Platform-8.

[25] Due to technical difficulties in some sessions, I had to reduce the number of periods in the Platform Pricing Treatment, which is why the number of periods differs between the two treatments in Figure 4B and Figure 4C.
[26] Figure B.4 in Appendix B illustrates the average shares in the extended treatments, where we observe similar trends as in the baseline treatments.
[27] The period numbers in the graphs do not reflect the actual period numbers in the experiment but are chronologically enumerating the periods where we see the different number of buyers on the platforms. See Table 1 for a summary of the number of buyers on the platforms at each experimental market period.

Figure 1. Average Percent Share Asked by Platform by Period

Figure 1A. Platforms with 5 Buyers

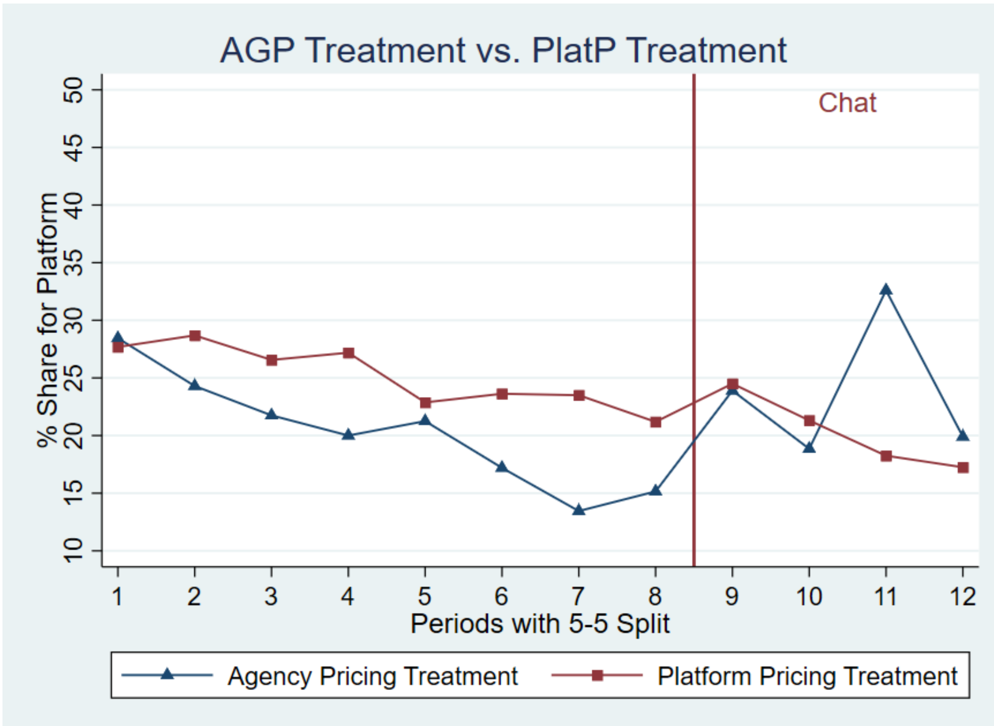


Figure 1B. Platforms with 8 Buyers

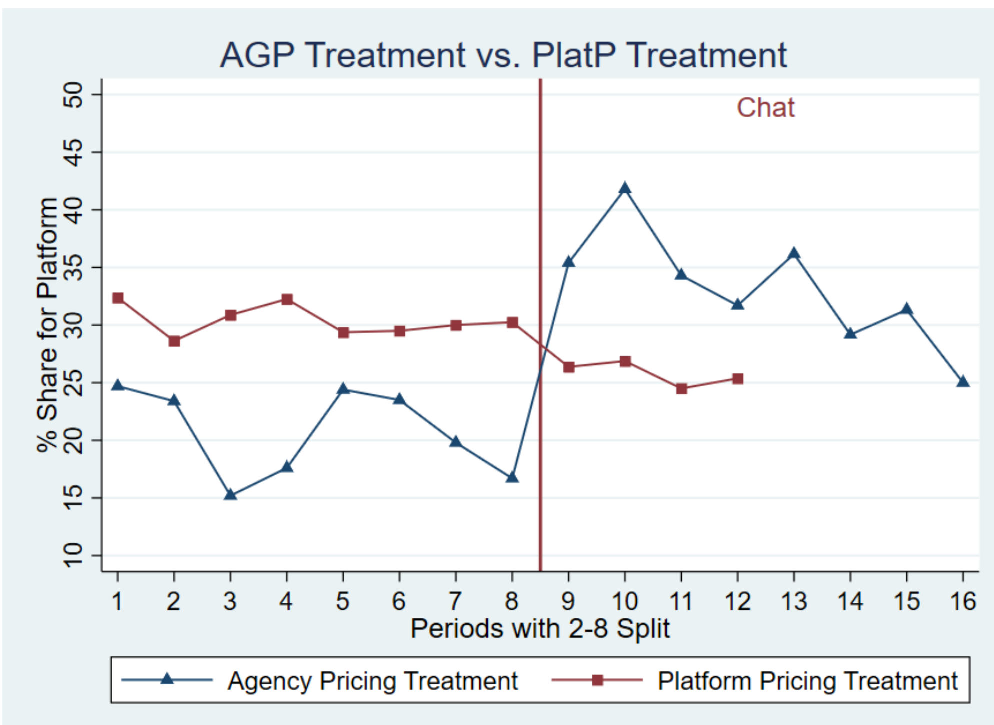
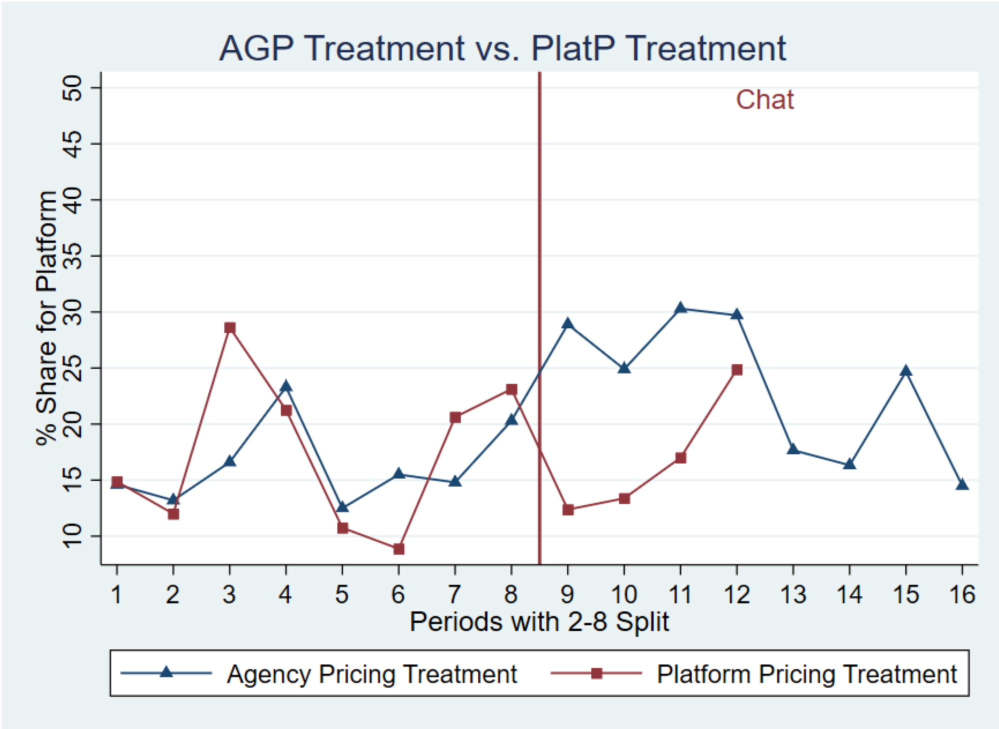


Figure 1. Average Percent Share Asked by Platform by Period (Continued)

Figure 1C. Platforms with 2 Buyers



As Hypothesis 5 states, we expect Platform-8 to take advantage of the sellers' lower search cost on their platform by asking for higher shares than the platforms with 2 buyers (Platform-2 for short). While overall the shares asked by Platform-2 on average are less than those asked by Platform-8, Platform-2 asks for a lower share than the other platform in only 34.71% of all the market group periods with the 2-8 split. Not surprisingly, Platform-2 attracts no sellers the vast majority of time across all treatments.[28] Considering Platform-2's low frequency of attracting sellers, some platforms may be asking for higher shares because they think that sellers would not choose the platform with fewer buyers regardless of the share that they ask. It is interesting to note that out of the instances when Platform-2 asks for lower shares than

[28] Table 5 in Section 1.4.2 depicts the frequencies where we observe different number of sellers on the platforms in the 5-5 split periods and the 2-8 split periods.

Platform-8, Platform-2 receives non-zero sellers about 62% of the times, and these instances explain 61% of the cases when Platform-2 receives non-zero sellers.

Comparing the shares asked between pricing treatments, Figure 1A and Figure 1B highlight the fact that the average shares asked by Platform-5 and Platform-8 are higher in the Platform Pricing Treatment than in the Agency Pricing Treatment at every period in the absence of communication. Because platforms compete to attract more sellers by reducing the shares asked and, thus, reducing the cost to sellers from selling on their platform, the platforms' revenue-share offers can be interpreted as a measure of competitiveness between platforms. Using this interpretation, platforms seem to exhibit more competitive behavior in the Agency Pricing Treatment compared to the Platform Pricing Treatment in the absence of chat. This difference may be because, in the Agency Pricing Treatment, platforms do not have to make pricing decisions and only decide on the revenue-share split. That is, platforms in the Agency Pricing Treatment only have one avenue to compete with the other platform. In contrast, platforms in the Platform Pricing Treatment make two decisions and so have two dimensions in which they can compete with the other platform: one on the revenue-share split and the other on the price. Consequently, platforms may be leveraging their control of prices to ask for higher shares in the Platform Pricing Treatment compared to the Agency Pricing Treatment.

Comparing the first market round (periods 1 through 8) with the second market round (periods 9 and beyond), Figure 1 reveals that communication seems to have affected the percentage asked by platforms differently in the two, pricing treatments. In the Agency Pricing Treatment, chat seems to have increased the percent shares asked by platforms, especially for Platform-2 and Platform-8. In fact, the chat data collected during the experiment provides further evidence that platforms in the Agency Pricing Treatment were explicitly agreeing to collude and increase their share of the revenues.[29] In the Platform Pricing Treatment, the addition of chat seems to not have made a significant difference, and we see the same decreasing trend as in the first market round for Platform-5 and Platform-8. The chat data in the Platform Pricing Treatment indicates that platforms, for the most part, attempted to coordinate but had a difficult time determining the revenue-share and price combos that yielded the highest profit—oftentimes not coming to a conclusion on one revenue-share and price combo but agreeing to a broader range of actions by the end of the chat.

Looking at instances when both platforms have asked for the same shares can also give some insight into collusive behavior. In all my treatments, roughly 12% of all market group periods with the 5-5 split had instances where shares are the same between the two platforms. However, when we look at the market group periods with the 2-8 split, we see that Platform-2 and Platform-8 ask for the same share in 19% of the periods with 2-8 split in the Platform Pricing Treatment with chat, which is much higher than the instances in the Agency Pricing Treatment without chat (7%), the Agency Pricing Treatment with chat (3%), and the Platform Pricing Treatment without chat (6%). This higher instances of equal shares in the Platform Pricing Treatment with chat compared to the other treatments suggest that the introduction of chat does allow platforms to coordinate on shares in the Platform Pricing Treatment.

[29] The following are examples of messages between platforms in the Agency Pricing Treatment: "i [*sic*] honestly think that we should increase our % [*sic*]," "yo [*sic*] let's hike up these percents," and "We should keep our percentages high and the same to make more money?".

Table 4. Regression Results on Percent Revenue-Shares Asked by Platforms

	(1) All	(2) All	(3) PlatP only	(4) PlatP only	(5) AGP only	(6) AGP only
PlatP	5.889***	6.523***				
	(1.33)	(1.22)				
PlatP*Chat	-12.404***	-11.386***				
	(2.89)	(2.62)				
Chat	8.817***	9.315***	-4.213*	-1.509	9.644***	9.396***
	(2.37)	(2.24)	(1.95)	(1.57)	(2.72)	(2.58)
2 buyers	-4.907***	-4.587***	-7.635***	-6.738***	-2.460	-2.529
	(1.04)	(1.01)	(1.28)	(1.18)	(1.54)	(1.46)
8 buyers	3.986***	4.307***	4.163**	5.060***	4.088*	4.019*
	(1.07)	(1.03)	(1.31)	(1.07)	(1.64)	(1.58)
Period	-0.118	-0.167*	-0.053	-0.174**	-0.198	-0.199
	(0.07)	(0.07)	(0.08)	(0.06)	(0.12)	(0.12)
Female		3.893***		10.170***		6.959***
		(1.06)		(1.65)		(1.86)
Demographics		X		X		X
R2	0.095	0.172	0.132	0.352	0.070	0.178
N	1,388	1,388	652	652	736	736

Robust, standard errors, clustered at the individual subject level, in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Demographic variables include dummies on ethnicity, education level, major of study, and prior experience with market experiments. The summary statistics of these variables can be found in Table 3.

Ordinary least squares regression results shown in Table 4, support the observational results from Figure 1. In the absence of communication, the Platform Pricing Treatment increases the shares asked by platforms by around 5.9-6.5 percentage points (coefficients "PlatP" in regressions (1) and (2)) from those asked in the Agency Pricing Treatment. The introduction of chat in the Agency Pricing Treatment increases the shares asked by around 9.4-9.6 percentage points (coefficients on "Chat" in regressions (5) and (6)), while the introduction of chat in the Platform Pricing Treatment reduces the shares asked by 1.5-4.2 percentage points (coefficients on "Chat" in regressions (3) and (4)). As expected, the shares asked decreases when the platform has 2 buyers as compared to the shares asked when the platform has 5 buyers. Presumably, the platforms with fewer buyers try to offset the increased seller's search cost on their platforms by reducing the shares asked. Similarly, platforms with 8 buyers asked for a higher share than platforms with 5 buyers.

As noted earlier, the shares asked decrease over time (indicated by the negative coefficients on "Period") as the subjects learn the appropriate shares to compete with the other platform. Interestingly, the regression results uncover a gender effect under both treatments. Female subjects seem to ask for higher shares of the revenue than male subjects. This discrepancy suggests that male subjects are more competitive as platforms than female subjects.

1.4.2 Number of Sellers

Sellers make platform entry decisions after platforms make their revenue-share offers, so we now look at the number of sellers on each platform. As Hypothesis 1 states, the indirect network effects in my experiment are dominant enough that sellers should "tip" to one platform at Nash equilibrium, especially to the platform with more buyers. Table 5 shows the frequency in which we see the different possible combinations of sellers on the platforms depending on the different split of buyers between platforms. As expected, we do see sellers tipping to one platform more than half the time across all treatments.

Table 6 contains regression results on the number of sellers on the platform. As expected, the number of buyers on a platform positively affects the number of sellers on the platform due to the impact of the indirect network effects. Platforms with 2 buyers see a decrease in the number sellers in comparison to platforms with 5 buyers, and platforms with 8 buyers see an increase in the number of sellers in comparison to a platform with 5 buyers. The “Lower share” variable is a dummy for whether the platform has the lower of the two shares in the market group. Not surprisingly, the platform with the lower share attracts more sellers. I do not expect any pricing treatment effects, and regression results find no statistically significant effect on either the pricing treatments or the introduction of chat. Results indicate that platforms that had more sellers in the previous period tend to be better at attracting sellers in the current period.[30, 31]

[30] Results for the other variables are similar when not including the number of sellers in the previous period.
[31] Table C.1 in the Appendix depicts results of a logit regression that looks at the characteristics that affect sellers' decision to choose one platform over the other—specifically, Platform 1 over Platform 2. Results are similar to those from Table 6. As expected, sellers prefer the platform with more buyers. Also as expected, in market periods with the same number of buyers on both platforms, sellers choose the platform that asks for a lower share of the revenue.

Table 5. Frequency of Different Number of Sellers on Platforms across Treatment

# of Sellers on each Platform	AGP	PlatP	AGP Chat	PlatP Chat
5-5 Split				
4 – 0	64%	57%	77%	78%
1 – 3	26%	32%	13%	16%
2 – 2	9%	11%	10%	6%
2-8 Split				
0 – 4	63%	79%	65%	77%
1 – 3	23%	14%	17%	3%
2 – 2	9%	4%	10%	6%
3 – 1	4%	2%	5%	13%
4 – 0	1%	2%	3%	0%

For the 5-5 split, 4 – 0 indicates 4 sellers on one platform and 0 on the other; 1 – 3 indicates 1 seller on one platform and 3 sellers on the other; etc. For the 2-8 split, 0 – 4 indicates 0 sellers on Platform-2 and 4 sellers on Platform-8; 1 – 3 indicates 1 seller on Platform-2 and 3 sellers on Platform-8; etc. To calculate these frequencies, I do not include market group periods when sellers choose not to join any platform, which occurs 3% of the time in AGP, 3% of the time in PlatP, 5% of the time in AGP Chat, and 2% of the time in PlatP Chat.

Table 6. Regression Results: Number of Sellers on Platform

	(1) All	(2) All	(3) PlatP only	(4) PlatP only	(5) AGP only	(6) AGP only
2 buyers	-1.823***	-1.819***	-1.928***	-1.923***	-1.719***	-1.711***
	(0.08)	(0.08)	(0.13)	(0.12)	(0.10)	(0.10)
8 buyers	1.747***	1.747***	1.857***	1.853***	1.643***	1.637***
	(0.08)	(0.08)	(0.12)	(0.12)	(0.10)	(0.10)
Lower share	1.382***	1.371***	1.168***	1.156***	1.587***	1.543***
	(0.07)	(0.08)	(0.10)	(0.11)	(0.10)	(0.10)
# of sellers in	0.078***	0.072***	0.110***	0.104***	0.045	0.032
previous period	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
PlatP	0.002	-0.003				
	(0.06)	(0.08)				
PlatP*Chat	0.109	0.101				
	(0.14)	(0.14)				
Chat	-0.086	-0.128	0.010	-0.055	-0.080	-0.085
	(0.10)	(0.10)	(0.13)	(0.13)	(0.11)	(0.11)
Period	0.004	0.005	0.004	0.006	0.003	0.003
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Demographics		X		X		X
R2	0.577	0.582	0.556	0.562	0.615	0.622
N	1,280	1,280	600	600	680	680

Robust, standard errors, clustered at the individual subject level, in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Note: Demographic variables include dummies on ethnicity, education level, major of study, and prior experience with market experiments. The summary statistics of these variables can be found in Table 3.

1.4.3 Prices

We now look at the posted prices across treatments. After platforms make their revenue-share split offers to sellers, platforms also make their pricing decisions in the Platform Pricing Treatment, while sellers make their platform entry and pricing decisions in the Agency Pricing Treatment. Figure 2 shows the average prices in the two pricing treatments across periods depending on the number of buyers on the platform.[32] To calculate these averages, I only include prices when there are sellers on the platform, so in the Platform Pricing Treatment, if no sellers join a platform, the price on that platform is not included. In the Agency Pricing Treatment, four sellers make four pricing decisions, which means that potentially four different prices may appear in one market period. In the Platform Pricing Treatment, two platforms make two pricing decisions, which means that potentially two different prices may appear in one market period. As a result, the spread of prices in each market period is wider in the Agency Pricing Treatment than in the Platform Pricing Treatment.

[32] Figure B.5 in Appendix B depicts price by period for the extended treatments. The trends look similar to those found in the baseline treatments.

Figure 2. Average Prices by Period

Figure 2A. Platforms with 5 Buyers

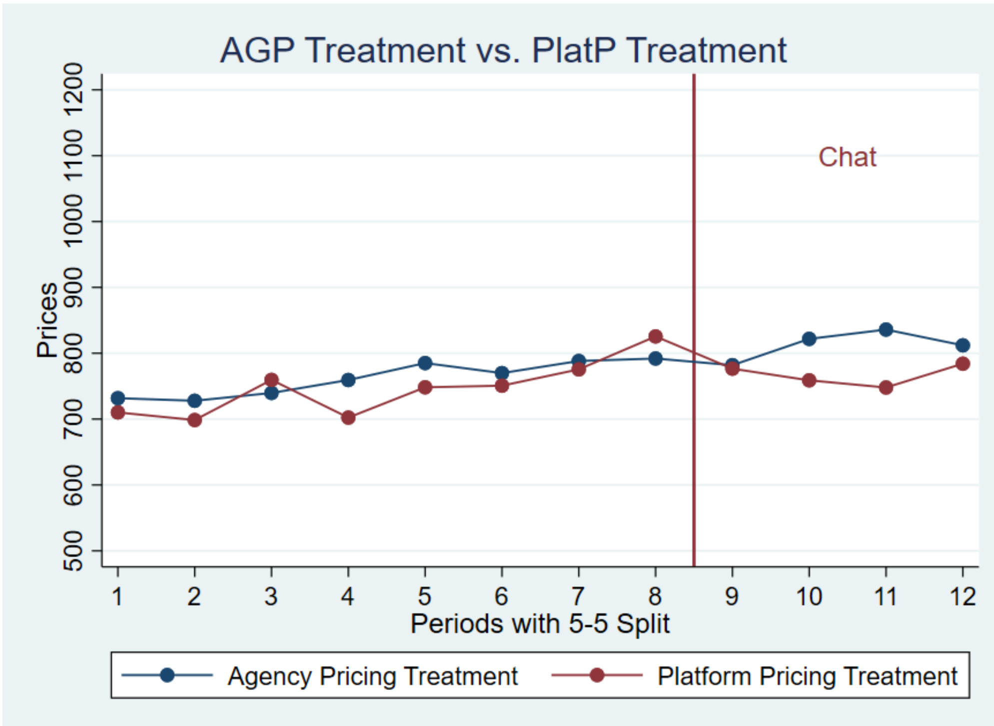


Figure 2B. Platforms with 8 Buyers

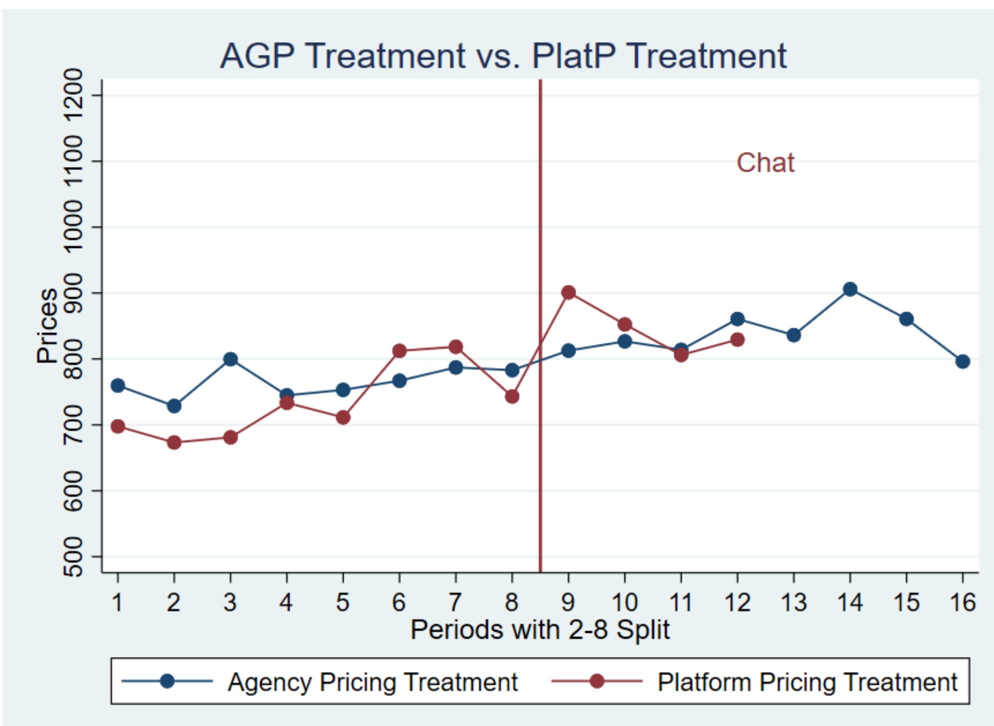
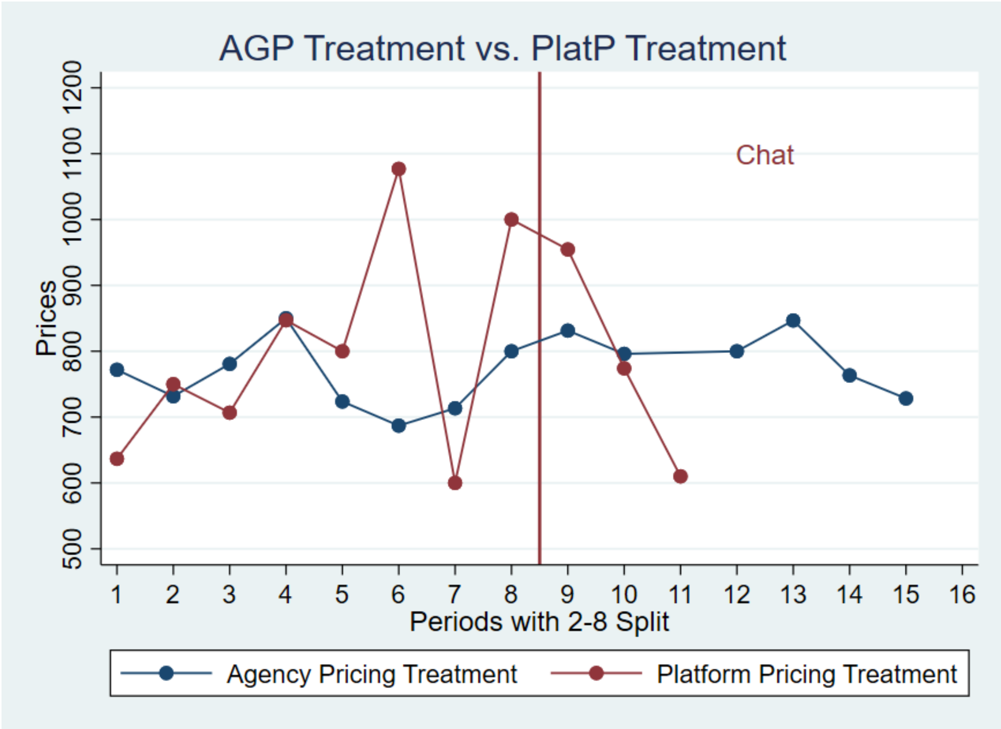


Figure 2. Average Prices by Period

Figure 2C. Platforms with 2 Buyers



In general, prices increase over time on Platform-5 and Platform-8, while prices on Platform-2 exhibit the same erratic behavior across periods as the shares did on Platform-2. On Platform-5 and Platform-8, the introduction of chat does not seem to change this increasing trend in prices in either of the pricing treatments. Average prices in the Agency Pricing Treatment seem to be slightly higher than prices in the Platform Pricing Treatment in most of the periods. Regression results in Table 7 support the graphical observations that prices are lower in the Platform Pricing Treatment, although the estimate is not statistically significant with or without demographic controls. Supporting Hypothesis 3, we that higher shares on the platform lead to higher prices, although the estimate is not statistically significant. Chat does not seem to influence prices, while prices do statistically significantly increase at later periods.

Table 7. Regression Results on Prices

	(1) All	(2) All	(3) PlatP only	(4) PlatP only	(5) AGP only	(6) AGP only
PlatP	-35.191	-36.481				
	(18.72)	(19.33)				
PlatP*Chat	26.718	31.569				
	(26.10)	(26.36)				
Chat	17.305	2.937	44.068	33.510	18.056	15.016
	(19.24)	(19.97)	(27.67)	(26.23)	(19.80)	(20.92)
Share for platform	0.300	0.594	0.347	0.970	0.260	0.574
	(0.56)	(0.51)	(0.97)	(0.83)	(0.65)	(0.60)
# of buyers on chosen platform	5.710*	4.832*	5.860	3.599	5.594	4.845
	(2.57)	(2.43)	(4.62)	(4.20)	(2.90)	(2.88)
Period	3.690**	4.278***	3.729*	4.494**	3.647**	3.897**
	(1.10)	(1.06)	(1.80)	(1.63)	(1.21)	(1.22)
Female		-17.118		-18.357		-31.359
		(16.66)		(21.61)		(23.32)
Demographics		X		X		X
R^2	0.065	0.112	0.050	0.105	0.064	0.138
N	2,745	2,745	1,291	1,291	1,454	1,454

Robust, standard errors, clustered at the individual subject level, in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Note: Demographic variables include dummies on ethnicity, education level, major of study, and prior experience with market experiments. The summary statistics of these variables can be found in Table 3.

Although analyzing observed prices can be revealing, in a two-sided market where the supply and demand functions vary depending on the number of sellers and the number of buyers on the platform, looking at the distance of these observed prices from either (1) the competitive equilibrium price or (2) the sellers' collusive (monopoly) price given the specific market parameters would be more informative. *Given* the shares asked by platforms, the number of buyers, and the number of sellers, I calculate the competitive equilibrium price (P^*) as the highest price that leads to the greatest social welfare.[33] Social welfare in my two-sided market is calculated as the sum of consumer surplus, producer surplus, and the platforms' profit within a market group. Similarly, given the same market parameters, I can also calculate the sellers' collusive (monopoly) price (P_m) that maximizes the sellers' profits.

As discussed in Section 1.3, my market parameters are such that producer surplus, consumer surplus, and total social welfare of a market are optimized when sellers tip to one platform. Consequently, to calculate the benchmark prices to compare with the observed prices, I calculate the competitive equilibrium and sellers' collusive prices on a platform with four sellers (leaving the other platform in the market with zero sellers). Given the observed number of buyers, shares, and prices on the platforms in the experiment, I calculate (1) the distance between the observed prices and the competitive equilibrium (socially optimal) price and (2) the distance between the observed prices and the sellers' collusive price.

Figure 3 depicts the average distance (in absolute value) of the observed prices from the highest competitive equilibrium price that I calculate given the number of buyers and revenue-

[33] Although the competitive equilibrium price can be a range of prices if the supply and demand functions are such that they intersect in a vertical line as opposed to a single point, I use the highest price within that range as a benchmark in my data analyses to compare the observed prices in my experiment.

share split on the platform across the periods.[34] As the figures illustrate, the prices in the Platform Pricing Treatment are further away from the highest competitive equilibrium prices than those in the Agency Pricing Treatment for most periods across the platforms with their different number of buyers, suggesting that market efficiency is lower in the Platform Pricing Treatment than in the Agency Pricing Treatment.

Figure 4 depicts the average distance (in absolute value) of the observed prices from the sellers' collusive price that I, once again, calculate given the number of buyers and revenue-shares on the platform across the periods.[35] As predicted in Hypothesis 2, platforms set prices further away from the sellers' profit-maximizing price in the Platform Pricing Treatment than sellers do in the Agency Pricing Treatment. However, taking the observations from Figure 3 and Figure 4 together, we can deduce that platforms in the Platform Pricing Treatment must be setting prices *below* the competitive equilibrium price (on average), because prices in the Platform Pricing Treatment are not only further away from the competitive equilibrium price but also from the sellers' monopoly price, which is always above or equal to the competitive equilibrium price. If prices are above the competitive equilibrium price, then those prices should also be closer to the sellers' competitive equilibrium price.

[34] Figure B.6 in Appendix B depicts the average distance from the competitive equilibrium price by period for the extended treatments. The trends look similar to those found in the baseline treatments.

[35] Figure B.7 in Appendix B depicts the average distance from the profit maximizing price by period for the extended treatments. The trends look similar to those found in the baseline treatments.

Figure 3. Average Distance of Observed Prices from Competitive Equilibrium Price by Period

Figure 3A. Platforms with 5 Buyers

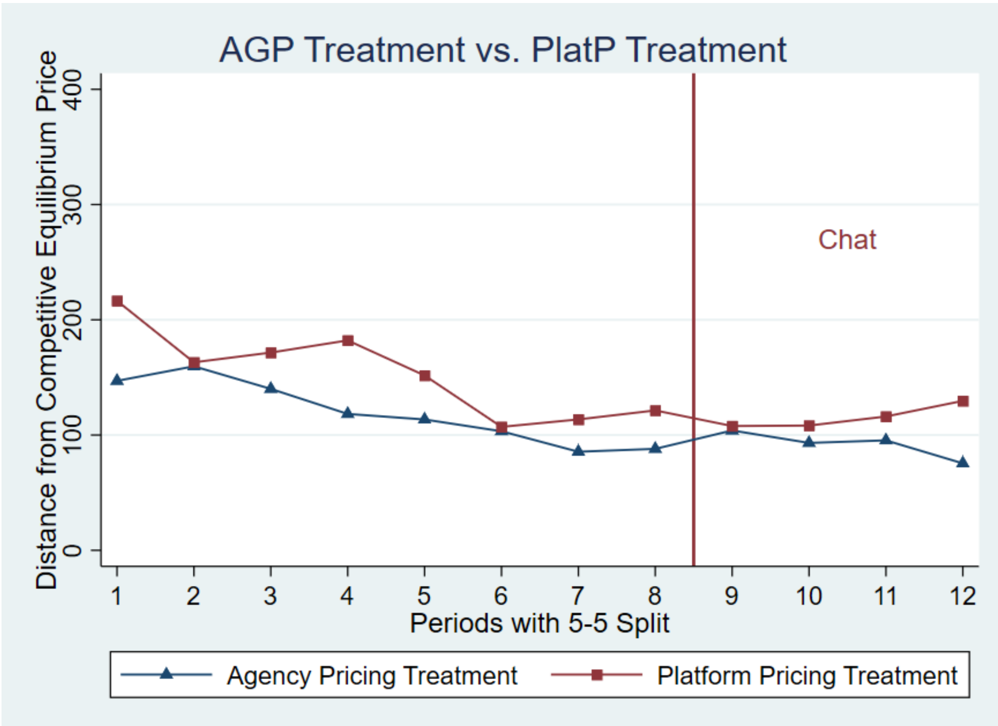


Figure 3B. Platforms with 8 Buyers

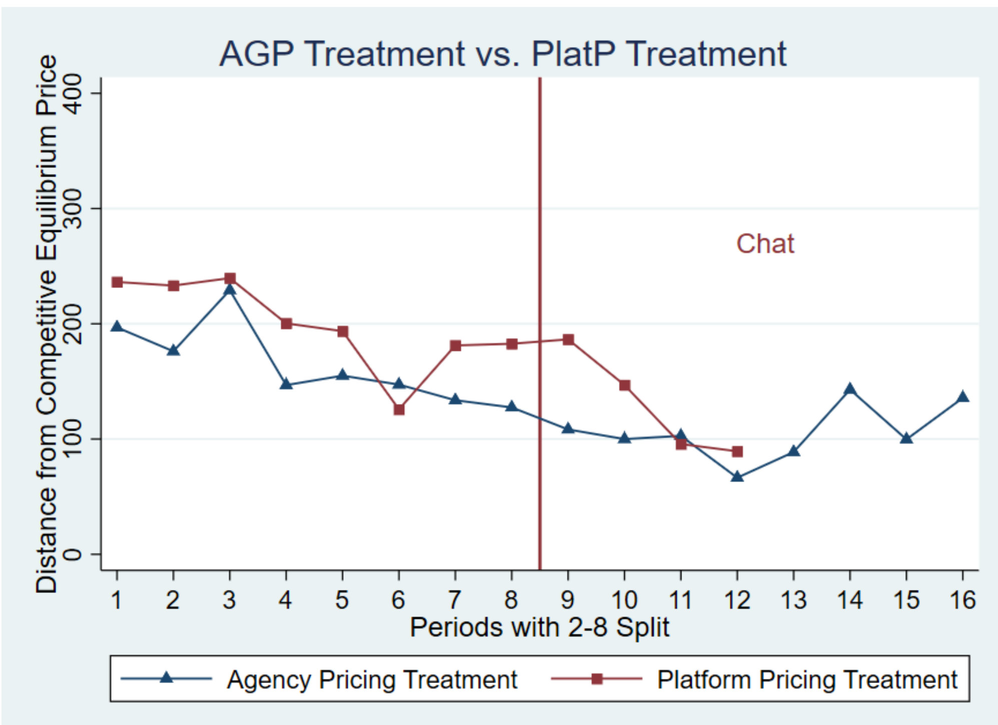


Figure 3. Average Distance of Observed Prices from Competitive Equilibrium Price by Period (Continued)

Figure 3C. Platforms with 2 Buyers

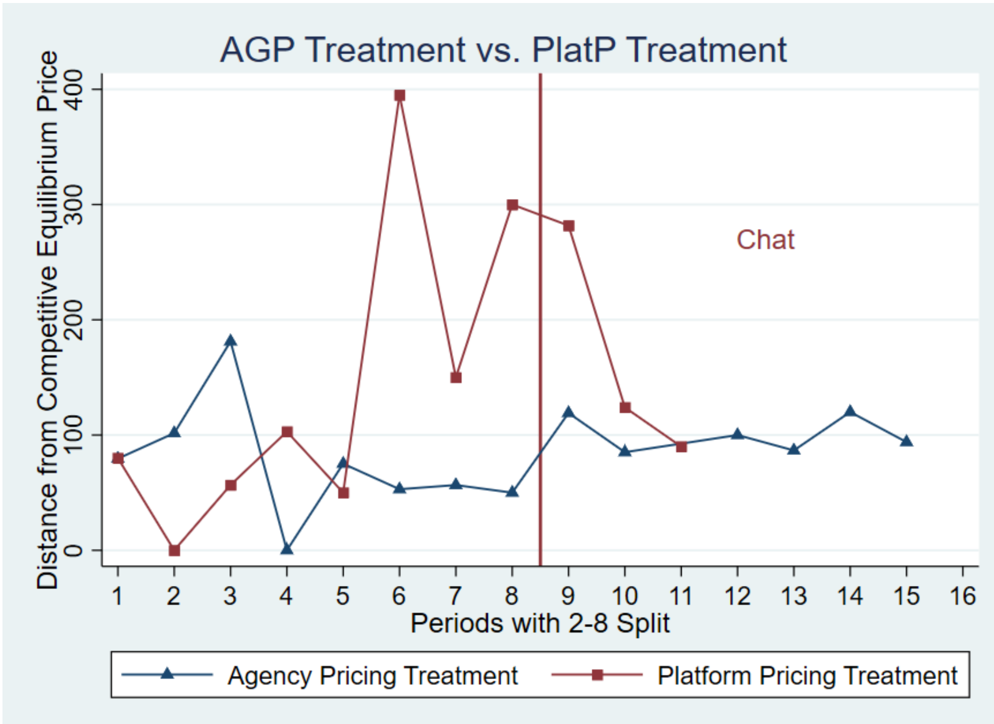


Figure 4. Average Distance of Observed Prices from Sellers' Collusive Price by Period

Figure 4A. Platforms with 5 Buyers

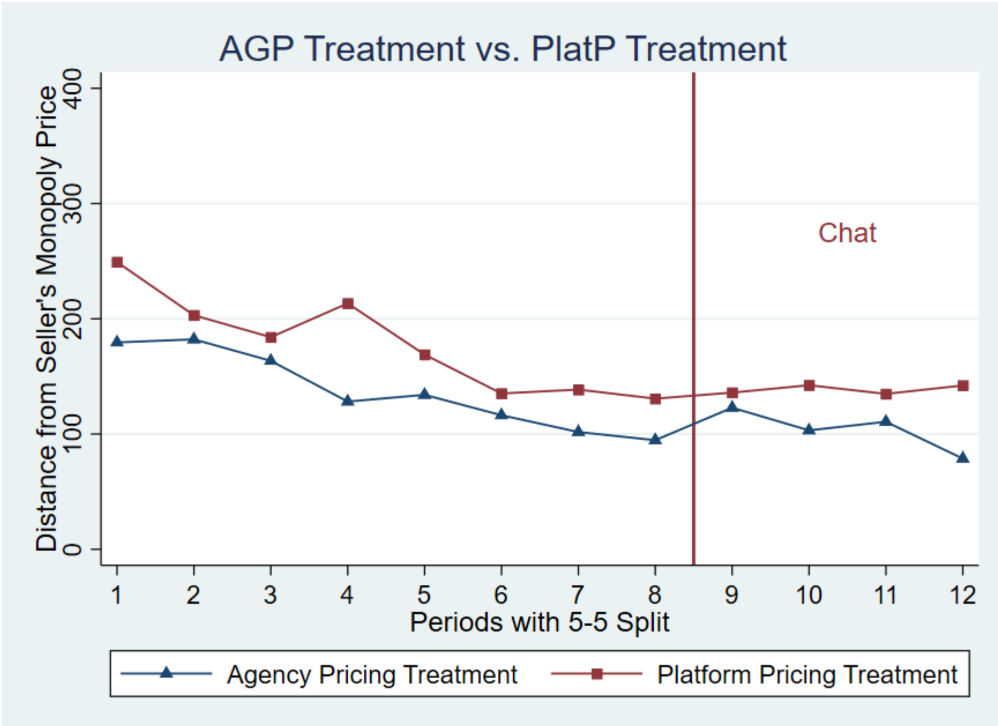


Figure 4B. Platforms with 8 Buyers

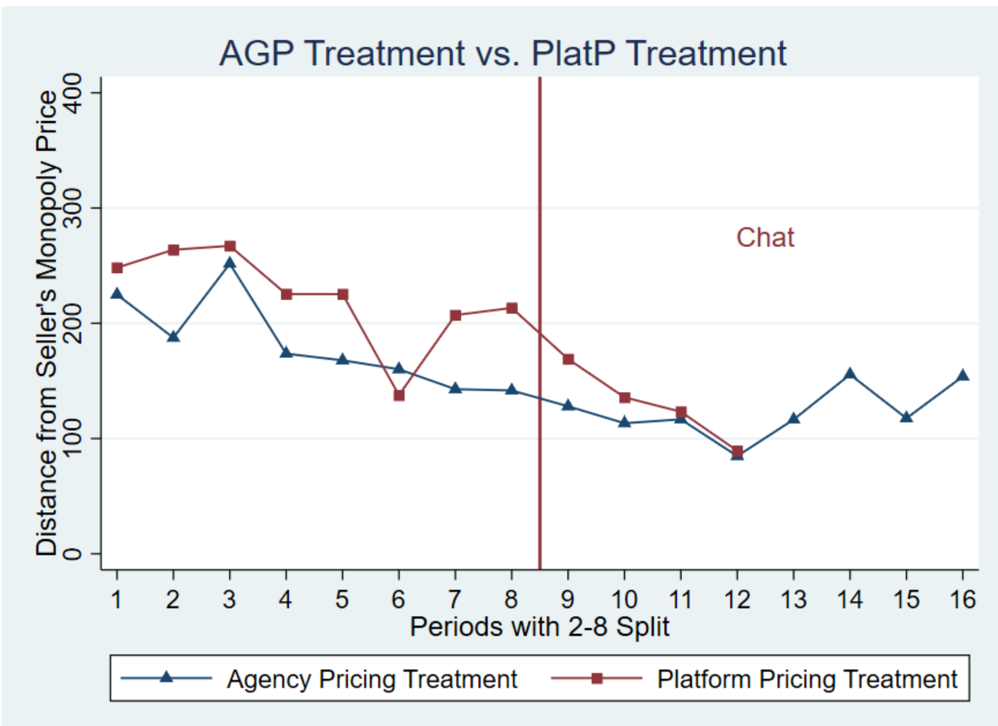


Figure 4. Average Distance of Observed Prices from Sellers' Collusive Price by Period (Continued)

Figure 4C. Platforms with 2 Buyers

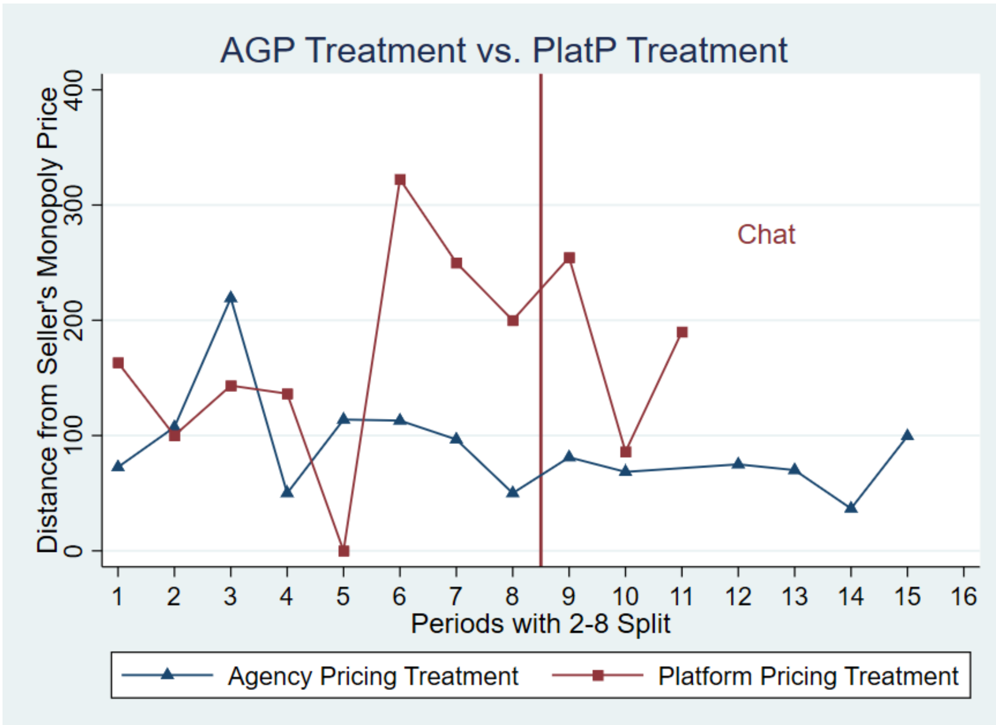


Table 8. Frequency of Distances of Observed Prices to Benchmark Prices

	AGP	PlatP	AGP Chat	PlatP Chat
Platform with 5 Buyers				
Price below P^*	70%	84%	59%	60%
$P^* \leq$ Price $\leq P_m$	53%	40%	46%	26%
Price above P_m	15%	18%	9%	18%
Platform with 8 Buyers				
Price below P^*	79%	82%	79%	60%
$P^* \leq$ Price $\leq P_m$	13%	5%	11%	21%
Price above P_m	9%	14%	10%	20%
Platform with 2 Buyers				
Price below P^*	33%	38%	26%	26%
$P^* \leq$ Price $\leq P_m$	53%	40%	46%	26%
Price above P_m	15%	23%	28%	47%

Note that P^* is the competitive equilibrium price, and P_m is the sellers' monopoly price.

Table 8 shows the distribution of observed prices in relation to the competitive equilibrium price and the sellers' monopoly price. As the table indicates, observed prices are below the competitive equilibrium price for the majority of market group periods across the platforms with the different number of buyers as well as across treatments, and this observation is true more often in the Platform Pricing Treatment than in the Agency Pricing Treatment. One notable exception is with prices on platforms with 8 buyers with chat where prices are below the competitive equilibrium price more often in the Agency Pricing Treatment. Indeed, previous market experiments (Smith et al., 1982; Kujal, 1992) have established that when markets have asymmetric surplus distributions that favor the sellers, which is the case for any combination of buyers and sellers in my market experiment, then prices tend to converge to competitive equilibrium from below. Consequently, the prevalence of observed prices below the competitive equilibrium price is not surprising. Furthermore, previous market experiments featuring zero- or near-zero intelligence trading have established that this feature of convergence from below is accentuated when subjects set prices with little information about marginal costs (Gode and Sunder, 1993; Duff and Ünver, 2006). Under the Platform Pricing Treatment, platforms are given imperfect information on the sellers' costs, whereas sellers have perfect information on their costs. Consequently, the differences we observe such that prices are more frequently below competitive equilibrium prices in the Platform Pricing Treatment than the Agency Pricing Treatment is also not surprising.

Supporting Hypothesis 2, I do see higher frequencies of prices between P^* and P_m under agency pricing than platform pricing. Once again, the notable exception is on platforms with 8 buyers with chat, where we observe more instances where prices are between P^* and P_m in the Platform Pricing Treatment. This finding suggests that chat enables platforms to share information about more profitable prices with one another. Indeed, the chat data does reveal that platforms do share information with each other on the prices that have given them the highest profits in previous periods. We do observe prices getting closer to more profit-maximizing levels over time, suggesting that subjects learn over time.

Regression results on the distance of observed prices from the competitive equilibrium level depicted in Table 9 as well as regression results on the distance of the observed prices from the sellers' collusive (monopoly) prices depicted in Table 10 corroborate the results I have already discussed. The distance of observed prices from the competitive equilibrium price is around 55 to 59 experimental dollars higher while the distance from the sellers' monopoly price is around 58 to 63 experimental dollars higher in the Platform Pricing Treatment compared to the Agency Pricing Treatment. The introduction of chat brings prices closer to both benchmark measures under platform pricing more so than under agency pricing. Prices do get closer to the benchmark prices over time in both treatments, suggesting learning behavior. In comparison to prices on platforms with 5 buyers, subjects set prices further away from the benchmark prices on platforms with 8 buyers more so than on platforms with 2 buyers. Subjects may not have had enough market periods with the 2-8 split to learn the more appropriate price levels because the number of buyers on the platform switches every two periods during the 2-8 split.

Table 9. Regression Results: Distance of Observed Prices from Competitive Equilibrium Price

	(1) All	(2) All	(3) PlatP only	(4) PlatP only	(5) AGP only	(6) AGP only
PlatP	55.137***	59.393***				
	(13.31)	(13.43)				
PlatP*Chat	-24.249	-26.299				
	(16.34)	(16.10)				
Chat	-13.191	-2.098	-42.790**	-29.247	-7.813	3.011
	(13.36)	(13.70)	(15.96)	(15.67)	(13.93)	(12.55)
Share for platform	0.886*	0.710*	0.795	0.552	1.012*	0.815*
	(0.36)	(0.32)	(0.58)	(0.51)	(0.43)	(0.37)
2 buyers on platform	6.572 (14.62)	7.236 (14.80)	28.957 (29.61)	34.894 (29.70)	-6.798 (14.31)	-7.905 (14.92)
8 buyers on platform	24.856*** (6.72)	27.457*** (6.44)	29.038** (9.59)	34.059*** (8.59)	21.176* (8.62)	22.407** (8.39)
Period	-3.145***	-3.619***	-2.685*	-3.371**	-3.644***	-4.121***
	(0.77)	(0.75)	(1.26)	(1.18)	(0.79)	(0.76)
Female		22.811		35.399*		19.914
		(11.70)		(15.72)		(17.29)
Demographics		X		X		X
R2	0.120	0.165	0.081	0.142	0.071	0.126
N	2,743	2,743	1,291	1,291	1,452	1,452

Robust, standard errors, clustered at the individual subject level, in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Note: Demographic variables include dummies on ethnicity, education level, major of study, and prior experience with market experiments. The summary statistics of these variables can be found in Table 3.

Table 10. Regression Results: Distance of Observed Prices from Seller's Collusive Price

	(1) All	(2) All	(3) PlatP only	(4) PlatP only	(5) AGP only	(6) AGP only
PlatP	57.764***	62.806***				
	(14.38)	(14.60)				
PlatP*Chat	-25.430	-27.555				
	(18.25)	(17.71)				
Chat	-20.456	-7.945	-54.261**	-40.146*	-11.333	-1.343
	(14.59)	(14.61)	(18.29)	(17.51)	(15.70)	(14.18)
Share for platform	1.369***	1.145***	1.285*	0.980	1.496**	1.223**
	(0.38)	(0.33)	(0.62)	(0.53)	(0.44)	(0.36)
2 buyers on	-9.130	-8.915	15.907	21.979	-21.715	-24.138
platform	(12.28)	(12.25)	(23.47)	(22.93)	(12.51)	(12.58)
8 buyers on	14.119*	17.342**	9.863	15.917	18.805*	20.524*
platform	(7.00)	(6.59)	(9.96)	(8.82)	(9.02)	(8.72)
Period	-3.310***	-3.860***	-2.570	-3.319**	-4.178***	-4.652***
	(0.82)	(0.79)	(1.31)	(1.21)	(0.88)	(0.83)
Female		26.748*		37.809*		29.018
		(12.97)		(16.25)		(19.48)
Demographics		X		X		X
R2	0.147	0.202	0.102	0.173	0.094	0.168
N	2,743	2,743	1,291	1,291	1,452	1,452

Robust, standard errors, clustered at the individual subject level, in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Note: Demographic variables include dummies on ethnicity, education level, major of study, and prior experience with market experiments. The summary statistics of these variables can be found in Table 3.

1.4.4 Social Welfare

Given not only the higher frequencies of prices below competitive equilibrium but also the higher shares asked in the Platform Pricing Treatment, we would expect total social welfare and market efficiency to be lower in the Platform Pricing Treatment than the Agency Pricing Treatment. Market efficiency is defined as the sum of consumer surplus ($Earnings_{buyers}$), producer surplus ($Eearnings_{sellers}$), and platform profit ($Earnings_{platform}$) divided by the maximum social welfare possible in the market. In my market experiment, the consumer surplus is the sum of the buyers' induced valuation minus the price for each quantity sold on both platforms in one market group, and the producer surplus is the sellers' combined earnings in one market group, which is calculated as the price minus the cost for each unit sold—including the cost of the share to platforms. The platforms' profits are simply the price multiplied by the share for the platform for each unit sold on the platform in a market group. The maximum social welfare can be calculated as the sum of the consumer surplus (CS_{ce}) and producer surplus (PS_{ce}) at the competitive equilibrium price when shares for platforms are zero. Consequently, we calculate market efficiency as the following:

$$Market\ Efficiency = \frac{Earnings_{buyers} + Earnings_{sellers} + Earnings_{platform}}{CS_{ce} + PS_{ce}}$$

It is interesting to note that, because positive revenue-shares for the platforms—much like a tax—cause distortion in the market, the maximum possible social welfare in a market requires platforms to receive zero percent of the revenue. Theoretically, social welfare would be maximized if buyers and sellers could interact with one another without the existence of the platforms. In reality, two-sided markets exist precisely because they provide a bridge between buyers and sellers who often could not or would not interact without the platforms' existence (Evans and Schmalensee, 2016).

Before discussing social welfare and market efficiency, we first examine each of the components that contribute to social welfare: consumer surplus, producer surplus, and platforms' profits. Table 11 provides regression results on consumer surplus, Table 12 provides regression results on producer surplus, and Table 13 provides regression results on platforms' profits. The tables contain results for market groups with the 5-5 split and the 2-8 split separately. Consumer surplus and platforms' profits in the Platform Pricing Treatment is higher though not statistically different from those in the Agency Pricing Treatment in both types of market groups. However, producer surplus is lower in the Platform Pricing Treatment than in the Agency Pricing Treatment. These results align with our previous findings because not only do platforms set prices lower than competitive equilibrium in the Platform Pricing Treatment more frequently than sellers do in the Agency Pricing Treatment, but they also ask for higher shares. Consequently, buyers and platforms benefit in the Platform Pricing Treatment at the expense of sellers, although platforms could still increase their profits by increasing prices.

Because more sellers and more buyers on one platform increases the maximum potential social welfare due to the indirect network effects, we see that 4 sellers do increase consumer surplus, producer surplus, and platforms' profits regardless of the number of buyers on the platforms, although the magnitude is much higher on platforms with more buyers.[36] The basis of comparison for the number of sellers on each platform is the case with 2 sellers on each platform. We see that any markets with more than 2 sellers on one platform leads to an increase in social welfare.

[36] Note that the coefficient on "0 sellers on Platform-8" is equivalent to the estimate for "4 sellers on Platform-2." Note also that the coefficient on "3 sellers on one Platform-5" is the same as one on "1 seller on one Platform-5."

Table 11. Regression Results: Consumer Surplus

	(1) All 5-5	(2) All 2-8	(3) PlatP only 5-5	(4) PlatP only 2-8	(5) AGP only 5-5	(6) AGP only 2-8
PlatP	133.1	122.2				
	(193.56)	(327.67)				
PlatP*Chat	176.3	-203.5				
	(635.89)	(695.22)				
Chat	-342.7**	-169.7	-248.3	-518.1	-237.4	-23.4
	(63.47)	(257.89)	(561.73)	(907.64)	(127.85)	(125.83)
4 sellers on one Platform-5	2491.3***		2683.7***		2299.3**	
	(54.25)		(253.54)		(215.69)	
3 sellers on one Platform-5	434.8***		604.8**		300.7	
	(8.53)		(122.87)		(251.97)	
4 sellers on Platform-8		3091.6***		3305.0***		2992.1***
		(161.14)		(211.72)		(180.00)
3 sellers on Platform-8		786.4**		766.4		790.8*
		(120.18)		(389.26)		(174.97)
1 seller on Platform-8		637.7		1142.5*		344.3
		(244.44)		(332.65)		(138.90)
0 sellers on Platform-8		1439.9*		989.8*		2023.5**
		(443.47)		(348.33)		(165.87)
Period	-32.3	-52.2	-24.3	-44.0	-42.2	-65.4
	(12.75)	(24.68)	(19.74)	(29.21)	(13.55)	(24.09)
R2	0.460	0.435	0.406	0.297	0.546	0.634
N	310	361	154	163	156	198

Robust, standard errors, clustered at the market group level, in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
I drop all market groups where sellers decide to not join a platform. The basis of comparison for the number of sellers on each type of market group is 2 sellers on each platform. "4 sellers on Platform-8" means that 0 sellers are on Platform-2; "3 sellers on Platform-8" means that 1 seller is on Platform-2; "1 seller on Platform-8" means that 3 sellers are on Platform-2; and "0 sellers on Platform-8" means that 4 sellers are on Platform-2.

Table 12. Regression Results: Producer Surplus

	(1) All 5-5	(2) All 2-8	(3) PlatP only 5-5	(4) PlatP only 2-8	(5) AGP only 5-5	(6) AGP only 2-8
PlatP	-943.5**	-1703.9**				
	(188.82)	(331.92)				
PlatP*Chat	1160.2*	2234.6**				
	(268.19)	(294.66)				
Chat	-164.7	-1188.2*	1092.0	1030.9*	-354.3	-1171.2
	(285.64)	(314.06)	(568.34)	(364.86)	(319.53)	(741.74)
4 sellers on one Platform-5	1874.4***		1057.0*		2586.3***	
	(163.04)		(312.96)		(136.14)	
3 sellers on one Platform-5	554.7**		451.2		547.6**	
	(95.33)		(288.99)		(73.26)	
4 sellers on Platform-8		3979.4**		2834.1**		4539.1***
		(464.13)		(398.44)		(261.58)
3 sellers on Platform-8		1666.2*		1327.5		1756.4*
		(542.72)		(560.68)		(465.54)
1 seller on Platform-8		416.0*		-57.3		463.2*
		(141.72)		(564.97)		(106.93)
0 sellers on Platform-8		1817.6*		1368.2		1681.1*
		(493.10)		(645.43)		(434.97)
Period	46.6**	16.8	53.0*	17.6	56.4*	15.2
	(10.00)	(11.76)	(16.14)	(33.24)	(15.39)	(48.26)
R2	0.424	0.451	0.337	0.244	0.541	0.578
N	310	361	154	163	156	198

Robust, standard errors, clustered at the market group level, in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
I drop all market groups where sellers decide to not join a platform. The basis of comparison for the number of sellers on each type of market group is 2 sellers on each platform. “4 sellers on Platform-8” means that 0 sellers are on Platform-2; “3 sellers on Platform-8” means that 1 seller is on Platform-2; “1 seller on Platform-8” means that 3 sellers are on Platform-2; and “0 sellers on Platform-8” means that 4 sellers are on Platform-2.

Table 13. Regression Results: Platforms' Profit

	(1) All 5-5	(2) All 2-8	(3) PlatP only 5-5	(4) PlatP only 2-8	(5) AGP only 5-5	(6) AGP only 2-8
PlatP	213.2	148.2				
	(169.93)	(324.06)				
PlatP*Chat	-79.2	-719.3				
	(237.73)	(488.42)				
Chat	11.1	677.4	-203.0	12.5	181.3	603.8
	(235.78)	(548.97)	(420.63)	(530.44)	(277.82)	(787.02)
4 sellers on one Platform-5	1111.6***		1070.2**		1120.7**	
	(99.82)		(162.17)		(88.37)	
3 sellers on one Platform-5	351.2		466.7		285.0	
	(135.47)		(188.20)		(94.63)	
4 sellers on Platform-8		2427.9***		2072.9**		2587.4***
		(105.66)		(289.21)		(145.40)
3 sellers on Platform-8		1212.6**		1028.8		1281.1***
		(235.31)		(541.49)		(53.63)
1 seller on Platform-8		63.0		-271.3		209.3
		(170.19)		(182.09)		(79.04)
0 sellers on Platform-8		336.8		203.3		261.9
		(313.27)		(371.56)		(309.46)
Period	-0.7	2.6	18.0	-1.9	-18.8	9.0
	(9.87)	(28.92)	(16.06)	(17.75)	(10.48)	(52.95)
R2	0.216	0.306	0.199	0.203	0.263	0.383
N	310	361	154	163	156	198

Robust, standard errors, clustered at the individual subject level, in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
I drop all market groups where sellers decide to not join a platform. The basis of comparison for the number of sellers on each type of market group is 2 sellers on each platform. "4 sellers on Platform-8" means that 0 sellers are on Platform-2; "3 sellers on Platform-8" means that 1 seller is on Platform-2; "1 seller on Platform-8" means that 3 sellers are on Platform-2; and "0 sellers on Platform-8" means that 4 sellers are on Platform-2.

We noted earlier that platforms collude to ask for higher shares of the revenue in the Agency Pricing Treatment when allowed to communicate with one another, so not surprisingly the regression results reveal that the addition of chat decreases both consumer and producer surplus and increases platforms' profit in the Agency Pricing Treatment, although these effects are not statistically significant. Contrastingly, in the Platform Pricing Treatment, the introduction of chat decreases consumer surplus while increasing producer surplus, although only the estimate on producer surplus in the 2-8 market group is statistically significant. The sign of chat on platforms' profit is positive across treatments and market groups except on the 5-5 market group in the Platform Pricing Treatment, although all the estimates are not statistically significant.

Overall, the result aligns with our previous observations that chat in the Platform Pricing Treatment allowed platforms to share information on profit-maximizing prices and so lead platforms to increase prices that had been previously below the profit-maximizing price to the benefit of sellers and detriment of consumers. At the same time, chat seems to have also led to a reduction in revenue-shares asked by the platforms in the Platform Pricing Treatment (once again, to the benefit of sellers and detriment of platforms), unlike in the Agency Pricing Treatment where we see collusion and higher shares for the platforms to the detriment of sellers and the benefit of platforms.

We can combine consumer surplus, producer surplus, and platforms' profit in each market to calculate social welfare. Table 14 contains regression results on social welfare.[37] The results from Table 14 confirm our observations from the estimates in Tables 11, 12, and 13. There is a negative effect of the Platform Pricing Treatment on total social welfare, although the estimate is statistically significant only for the 2-8 market groups.

[37] Table C.2 in the Appendix contains regression results on market efficiency, which convey similar findings as Table 14.

Table 14. Regression Results: Social Welfare

	(1) All 5-5	(2) All 2-8	(3) PlatP only 5-5	(4) PlatP only 2-8	(5) AGP only 5-5	(6) AGP only 2-8
PlatP	-597.2	-1433.5*				
	(270.94)	(451.77)				
PlatP*Chat	1257.3*	1311.9				
	(368.64)	(662.62)				
Chat	-496.3	-680.5**	640.8	525.3	-410.4**	-590.8**
	(399.03)	(147.10)	(567.48)	(886.81)	(69.30)	(91.14)
4 sellers on one Platform-5	5477.3***		4810.9***		6006.3***	
	(171.17)		(416.66)		(177.91)	
1 seller on one Platform-5	1340.7**		1522.7		1133.4	
	(215.73)		(582.08)		(366.55)	
4 sellers on Platform-8		9498.9***		8212.0***		10118.6***
		(457.91)		(605.36)		(165.90)
3 sellers on Platform-8		3665.2**		3122.7*		3828.4**
		(624.95)		(839.37)		(512.98)
1 seller on Platform-8		1116.7*		813.9		1016.9**
		(310.60)		(614.21)		(166.01)
0 sellers on Platform-8		3594.4**		2561.4*		3966.5**
		(485.15)		(677.20)		(355.12)
Period	13.7	-32.8	46.7	-28.3	-4.6	-41.2
	(20.14)	(13.37)	(37.15)	(24.10)	(16.56)	(18.15)
R2	0.654	0.633	0.548	0.394	0.787	0.839
N	310	361	154	163	156	198

Robust, standard errors, clustered at the market group level, in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
I drop all market groups where sellers decide to not join a platform. The basis of comparison for the number of sellers on each type of market group is 2 sellers on each platform. “4 sellers on Platform-8” means that 0 sellers are on Platform-2; “3 sellers on Platform-8” means that 1 seller is on Platform-2; “1 seller on Platform-8” means that 3 sellers are on Platform-2; and “0 sellers on Platform-8” means that 4 sellers are on Platform-2.

In the Agency Pricing Treatment, the introduction of chat statistically significantly decreases total social welfare since platforms collude and ask for higher shares under agency pricing with chat. Positive revenue-shares for platforms act like a “tax” in the market and so leads to higher deadweight loss which reduces social welfare. In the Platform Pricing Treatment, chat does not have a statistically significant effect on social welfare. Social welfare increases on a platform with more buyers and more sellers, and these effects are statistically significant across treatments.

Three reasons may explain both the lower social welfare and the fact that prices are below the competitive equilibrium price more frequently in the Platform Pricing Treatment than in the Agency Pricing Treatment. First, platforms in the Platform Pricing Treatment are given imperfect information about the sellers’ costs when they have control of the prices, whereas sellers in the Agency Pricing Treatment have perfect information about their own costs when they have control of prices. The platforms’ lack of perfect information on the sellers’ costs may lead the platforms to set lower prices than optimal. Indeed, as already mentioned, platforms in the Platform Pricing Treatment are comparable to the “near-zero-intelligence” traders who have little information on the marginal costs. The literature surrounding near-zero intelligence traders have established that convergence occurs at a slower rate with near-zero intelligence traders compared to the case when price setters have perfect information on marginal costs (Gode and Sunder, 1993; Duffy and Ünver, 2006). Furthermore, my market demand and supply structures have surplus asymmetry distributions that favor sellers regardless of the number of buyers and sellers on the platform. The literature has established that this feature leads to convergence of prices from below the competitive equilibrium price. This convergence-from-below feature of my market together with the slower rate of convergence due to platforms’ limited information in the Platform Pricing Treatment results in the lower market efficiency that we observe in the Platform Pricing Treatment in comparison to the Agency Pricing Treatment.

To test whether or not the lower market efficiency observed in the Platform Pricing Treatment is exclusively due to the platform's lack of perfect information on sellers' costs, I would need to run a separate treatment where platforms under the Platform Pricing Treatment are given perfect information on sellers' costs and compare results with those from the Agency Pricing Treatment. However, platforms typically do not have perfect information on sellers' costs in real life. Consequently, this additional treatment is outside the scope of my paper although worthwhile to pursue in a later project.

The second reason that may explain the lower social welfare under Platform Pricing Treatment is the four different prices that may exist at every period in the Agency Pricing Treatment compared to the two different prices that may exist in the Platform Pricing Treatment. Subjects in market experiments learn to adjust their prices from previous treatments to gain the maximum amount of earnings. Subjects in the Agency Pricing Treatment are exposed to more information and see a wider range of prices than subjects do in the Platform Pricing Treatment. Consequently, platforms in the Platform Pricing Treatment may need more time than sellers in the Agency Pricing Treatment to observe and learn of the most optimal prices.

Lastly, platforms in the Platform Pricing Treatment may have a harder time to set the appropriate price when they also have to set the revenue-share split than the sellers in the Agency Pricing Treatment who take the shares across platforms as given and decide on the platform, price, and quantity. The computer calculates and displays on the screen the potential costs and potential profits for sellers given their choices so that sellers have an easier time determining which decisions lead to higher potential earnings. The platforms also see a similar calculator display on their screen but are given a range rather than the precise value of the sellers' potential costs. These three reasons may explain why we see lower prices in the Platform Pricing Treatment than expected given the higher shares asked in the Platform Pricing Treatment than in the Agency Pricing Treatment.

1.4.5 Subjects' Earnings

Perhaps of interest to the sellers and platforms in my two-sided market experiment is the earnings for each subject. Table 15 displays the regression results on the earnings depending not only on pricing treatments but also on whether the subject is a seller. Sellers earn less than platforms in both treatments. In the Platform Pricing Treatment, sellers earn much less than platforms by about 569-571 experimental dollars per period, whereas in the Agency Pricing Treatment, sellers earn less than platforms by around 215-258 experimental dollars per period. Comparing between treatments, platforms in the Platform Pricing Treatment earn more than platforms in the Agency Pricing Treatment. As previously discussed, platforms ask for higher shares in the Platform Pricing Treatment than in the Agency Pricing Treatment in the absence of chat, which leads to the overall higher earnings even though prices in the Platform Pricing Treatment are lower than the competitive equilibrium price more frequently than those in the Agency Pricing Treatment. This discrepancy means that sellers earn much less in the Platform Pricing Treatment than in the Agency Pricing Treatment.[38]

[38] Table C.3. in Appendix C looks at the determinants of the platforms' earnings. The results convey similar findings to those from Table 6 and Table C.1.

Table 15. Regression Results: Earnings between Sellers and Platforms

	(1) All	(2) All	(3) PlatP only	(4) PlatP only	(5) AGP only	(6) AGP only
PlatP	90.0	53.2				
	(79.56)	(78.17)				
PlatP*Chat	18.2	19.8				
	(117.44)	(118.95)				
Chat	-159.1*	-175.5*	163.1	131.7	-166.2	-178.9*
	(71.04)	(71.27)	(84.82)	(81.54)	(85.88)	(85.77)
Seller	-212.5**	-236.1**	-569.4***	-571.1***	-215.0**	-257.8**
	(74.16)	(75.13)	(85.17)	(80.16)	(80.08)	(84.56)
PlatP*Seller	-449.9***	-424.0***				
	(88.31)	(90.63)				
# of buyers	212.1***	211.4***	210.0***	209.6***	212.9***	212.5***
	(12.70)	(12.80)	(21.04)	(21.21)	(15.59)	(15.94)
Period	11.6***	12.7***	11.1***	12.2***	12.3**	13.3**
	(2.66)	(2.65)	(2.93)	(2.88)	(4.63)	(4.53)
Demographics		X		X		X
R2	0.230	0.242	0.250	0.264	0.202	0.219
N	4,133	4,133	1,943	1,943	2,190	2,190

Robust, standard errors, clustered at the individual subject level, in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Note: Demographic variables include dummies on ethnicity, education level, major of study, and prior experience with market experiments. The summary statistics of these variables can be found in Table 3.

1.5 Conclusion

Several legal disputes over the control of ebook prices between Amazon and publishing companies in 2014-2015 have sparked debates over the social welfare implications of different pricing schemes in two-sided markets. Should the sellers, such as the publishers, retain control of sales prices, or should the platforms, such as Amazon, have control instead? To directly address this question, I conduct a novel, two-sided market experiment that compares the agency pricing scheme—under which *sellers* set the price—with the platform pricing scheme—under which *platforms* set the price.

My findings indicate that the platform pricing scheme (1) encourages platforms to leverage their control of prices to ask for a higher percentage of the revenue, (2) leads to prices below competitive equilibrium prices more so than in the agency pricing scheme, and (3) decreases producer surplus and social welfare overall.

This discrepancy may be because the platforms struggle with imperfect information on the sellers' costs to determine the optimal price compounded by the fact that the parameters of my market promote convergence from below. In contrast to the theoretical models where subjects have perfect information, platforms do not have perfect information on the sellers' costs both in real life and in my experiment. Outside of the laboratory, I would expect platform companies who operate under the platform pricing scheme, such as Uber and Lyft, to have more resources, experience, and data at their disposal compared to the student subjects in my experiment. With their superior set of resources, platform companies should be able to more accurately ascertain the sellers' cost structure and, thus, set prices closer to optimal levels compared to those set by student subjects. Then again, supply structures outside of the laboratory have more complexity, and the literature has acknowledged that often platforms set prices below cost to lure one side of the market.

My findings suggest that regulators may want to consider the role that information asymmetry has on different pricing schemes in two-sided markets since platforms' imperfect information on marginal costs under platform pricing contributes to the more pronounced market failures in my experiment under the platform pricing scheme than the agency pricing scheme.

My market experiment focuses exclusively on two-sided markets with increasing marginal cost structures and does not look at markets with digital products that have zero marginal costs. As a result, my results may only occur in two-sided markets with non-zero marginal cost structures. Indeed, the supply and demand structures in my two-sided market do not provide a general case for the numerous types of two-sided markets that exist. For instance, my experiment does not focus on matching markets where an agent on one side only wishes to be matched with one agent on the other side. Additionally, I do not allow platforms to set subscription or access fees nor do I allow multi-homing such that sellers and buyers can simultaneously join multiple platforms. I also do not consider the case when one platform under one pricing scheme exists alongside a platform under a different pricing scheme. These scenarios are worthwhile to explore in future experimental works.

This paper adds to the debate surrounding different pricing schemes in two-sided markets with indirect network effects. Platform companies within a two-sided market differ in a multitude of ways, which makes comparing the effects of the agency pricing scheme with the platform pricing scheme difficult. However, in a laboratory, I can control for several platform and market characteristics to precisely estimate the effect of not only the different pricing schemes but also of changes in the parameters of the market. As far as I am aware, this market experiment is the first to compare the agency pricing scheme with the platform pricing scheme within a two-sided market environment. Future market experiments could address additional questions regarding whether large platforms, like Amazon, are taking advantage of different pricing schemes to acquire monopoly power. If so, these market experiments could be used to test different regulations on the pricing structures in these markets and compare their implications to social welfare. More research must be done to answer these important questions.

CHAPTER 2

MORAL REFERENCE POINT IN DICTATOR GAMES: EXTENDING ENGEL'S (2011) META STUDY

2.1 Introduction and Literature Review

The basic dictator game involves two individuals, one of whom is given the power to determine the allocation of a sum of money between themselves and the other person. This simple game is used to study the determinants of giving (altruism, envy) by changing a variety of parameters within the game. To provide a few examples, researchers have varied the players' initial endowments, allowed dictators to take rather than give from the other, or changed the efficiency of giving by varying other's benefits from any dollar the dictator transfers, etc. Hundreds of dictator game experiments over the last few decades have highlighted some key design elements that affect giving behavior. However, few papers have tried to organize these behavioral patterns. We test one theory advanced by Cox et al. (2017) that attempts to unify some of the findings in the literature.[39] Cox et. al (2017) posit a theory of moral reference points that define an observable and thus testable measure in dictator games that synthesizes findings from two types of dictator game experiments: one that varies the dictator's feasible action space and another that varies initial endowments.

We know from the literature that increasing the recipients' initial endowments decreases the amount that dictators give (Korenok et al., 2009 and Korenok et al., 2013). We also know from another set of papers that allowing the dictators to take from rather than just to give to their partner decreases the recipient's payoff (List, 2007; Bardsley, 2008; Cappelen, et al. 2013; Korenok et al., 2014; Cox et al., 2017). Combining the intuitions behind these two ideas, Cox et

[39] Breitmoser and Tan (2014) and their theory of reference dependent altruism is another example.

al. (2017) defines moral reference points that capture information on both the players' initial endowments and the dictator's action space. We test the hypothesis that these moral reference points can explain giving behavior using metadata collected by Engel (2011) on dictator game experiments from 131 papers.

Engel's (2011) original analyses include estimates on the effect of a larger upfront endowment for the recipient and the effect of a "limited action space" for the dictator. A "limited action space" here refers to instances when the experimenter further restricts the dictators' choice such as by only giving dictators the choice between keeping everything and contributing half of the pie or excluding the equal split option, etc. Engel (2011) finds that these two factors decrease the recipient's payoff when these limitations favor the recipient. However, he does not include the minimum or maximum amount that the dictators can give and/or take in his regression analyses nor does he estimate the effect of moral reference points that capture information on both the initial endowment and the action space.

We add to his metadata more detailed information on initial endowments for both dictators and recipients and on the minimum and maximum amount the dictators can give and/or take. Following Engel (2011), we also reconstruct individual level data using the information from the original papers.[40] Using this updated metadata, we re-estimate Engel's (2011) regression and meta-regression analyses and compare estimates when we include our additional variables into the regressions.

Our findings support the theory proposed by Cox et al. (2017) and suggest that the moral reference points can help to explain giving behavior. In our regression analyses, estimated coefficients on the moral reference points are statistically significant and have the correct signs

[40] We were able to get additional data beyond those provided by Engel (2011) after contacting the authors of the papers in cases where we did not have enough information to create our reconstructed data.

predicted by the theory. In our multiple regression analyses, we also note some instances when our estimates on the other covariates fall in line with the literature but depart from the results found in Engel (2011).

We are not the first paper to reexamine Engel's (2011) metadata. Zhang and Ortman (2014) have also re-analyzed Engel's (2011) metadata to address inconsistencies they noticed with Engel's (2001) analysis of the take-option. They recode the take-option as negative giving instead of zero giving and find, contrary to Engel's initial report, a statistically significant negative effect of the take-option on giving, which is more in line with results from studies of the take-option (List, 2007; Bardsley, 2008; Cappelen, et al. 2013; Cox et al., 2017). Section 2.2 explains how we address this inconsistency.

Aside from Engel's (2011) meta-study, Camerer (2003) and Cardenas and Carpenter (2008) have also conducted similar meta-studies of dictator games. However, these two papers look at smaller samples of experimental results than Engel (2011). Camerer's (2003) meta-study analyzes results from 11 experiments, and Cardenas and Carpenter (2008) look at 10 field experiments of dictator games played in developing countries.

The proceeding sections are as follows. In section 2.2, we discuss Engel's (2011) metadata and the additional information we have included in the data. In section 2.3, we summarize Cox et al. (2017)'s moral reference point. Section 2.4 reports the results from our regression analyses, and section 2.5 concludes.

2.2 Data

Engel's (2011) original dataset contains information on 620 dictator games from 131 papers published between 1992 and 2009, including 4 papers published in 2010.[41] He collects

[41] See Appendix F for the list of papers included in the data.

information on the average share of the stake that the dictator gives to the recipient for each treatment as well as information on the variety of design elements that each treatment was testing.[42] For his meta-regression analyses, Engel (2011) records the standard errors when the information is available.[43] Of particular interest to us, Engel (2011) creates a dummy variable for whether the dictator's action space was "limited" or not. However, as explained in Cox et al. (2017), the effect on the recipient's payoff can be positive or negative depending on the type of limitations placed on the dictator's action space. Furthermore, Engel (2011) does not collect information on the actual size of the action space.

We add to his dataset information on the maximum amount that the dictators can give and the maximum amount the dictators can take in all the treatments. Furthermore, although the original data set does include a dummy variable on whether the recipient receives an upfront endowment and another variable that captures the size of the recipients' endowments, we find several inconsistencies in the data. Consequently, we correct these values and add more detailed information on the initial endowments for the dictators and recipients.[44]

For our analyses instead of using the average share of the stake that the dictator *gives* to the recipient as our dependent variable, we calculate instead the average share of total endowments (dictator's endowment plus recipient's endowment) that the dictator transfers to the recipient. Consequently, unlike in Zhang and Ortman (2014), we do not code the take option as negative giving. Instead, our variable captures information on the recipient's payoff relative to

[42] As Zhang and Ortman (2014) note, Engel's (2011) original data censors the take option as zero giving. We do not censor any of the values in our data. Instead we create a new variable that captures giving as the share of the total endowment (dictator's endowment plus recipient's endowment) given to the recipient.
[43] A substantial number of standard errors recorded are actually standard deviations. Consequently, Engel's (2011) meta-regression analyses were underweighted. We have corrected these values in our data.
[44] We further check the values of all the original variables and correct any inconsistencies we find in the data that do not match the information in the original papers. All corrections can be found in the Stata do file used to create the new data set.

the entire pie instead of just the portion of the pie that the dictators can give or take. We use this variable as our dependent variable of interest in our analyses.

All of our data are in units transferred that have not been multiplied by the price of giving nor the price of taking. That is, we look at units given by the dictator (taken from the recipient) instead of units received by the recipient (received by the dictator). These two values would be different if, for instance, the price of giving is such that one unit given results in two units received. We do this so that our dependent variable would be comparable across treatments. Additionally, we control for the efficiency of giving in our regression analyses to estimate the effect that efficiency has on giving behavior. We also code the players' initial endowments as a percentage of the total endowments to make the variables comparable across the different treatments.

For our regressions, we reconstruct individual-level data using the subject sample size and reported distributions of subjects' giving behavior as well as any individual-level data reported in the papers. For each treatment, we collect when reported data on how many dictators gave 0%, greater than 0% and less than 10%, greater than 10% and less than 20%, etc. We use these buckets to reconstruct the individual-level data. We were further able to obtain more data beyond those provided by Engel (2011) after we contacted some of the authors in cases where the paper did not contain enough information for us to reproduce individual-level data.

We do drop a few observations in some of our analyses due to the complexity of some of the treatments. In six papers, subjects receive varying amounts of endowments within one treatment (Cox, 2004; Cox et al., 2008; Farina et al., 2008; Fisman et al., 2007; List and Cherry, 2008; and Harbaugh et al., 2000). Because we cannot appropriately code the size of the initial endowments at the treatment level, we drop these treatments from our meta-regression analyses, although we include them as individual-level data in our regressions and structural estimations. Additionally, one of the treatments for Fisman et al. (2007) is a three-person dictator game in which the price of giving differs for each recipient. We focus on behavior in two-person dictator games in this paper, so we drop observations when dictators give to two recipients. Lastly, we drop Dalbert and Umlauft (2009) from both our analyses because they do not have information on average giving behavior.

Our final dataset comes from 126 papers and 599 treatments. Our meta-regression analyses work with the 440 data points that contain information on standard errors. The resulting, reconstructed individual-level data comes from 83 papers and 323 treatments.

In the next section, we discuss Cox et al.'s (2017) theory of moral reference points.

2.3 Theory of Moral Reference Points

Cox et al. (2017) develop a theory of "moral reference points" that builds upon the notion of a moral cost associated with behaving in a socially inappropriate way or failing to behave in a way that complies with social norms. This idea of moral cost has been explored in works by Levitt and List (2007), List (2007), Lazear et al. (2012), and DellaVigna et al. (2012). However, Cox et al. (2017) are the first to develop an axiomatic foundation that links the idea of moral cost with observable features called "moral reference points" to explain giving behavior. Because the moral reference points are measurable features of dictator games, we can empirically test the effects of reference points on individual choices using our metadata.

Cox et al. (2017) defines two dimensions that form the moral reference points in dictator games labeled r_1 and r_2. To identify these two points, they first define the *minimal expectation point*, which is the payoff for one player when the other player gets her maximum feasible payoff. They argue that the recipient's minimal expectation point (m_2) affects the dictator's choice —that is, the dictator's giving behavior would depend on the amount that the recipient would receive were the dictator to keep as much as possible for herself. Presumably, the closer the recipient's final allocated payoff to her minimal expectation payoff, the higher the moral cost of the dictator's choice. Consequently, they define r_2 as the (recipient's dimension of) moral reference points as $r_2 = m_2$.

The dictator's dimension of the moral reference point in their theory concerns both the dictator's own-payoff and the dictator's initial endowments. Cox et al. (2017) argue that the moral cost of the dictator's choice decreases with the closeness to the dictator's minimal expectation point, m_1, associated with the most generous action to the recipient. At the same time, the dictator's moral cost may also be inversely affected by the sense of entitlement the dictator feels towards the dictator's initial endowment (e_1). Presumably, the dictator with higher initial endowment would feel entitled to a larger final payoff. Cox et al. (2017) synthesizes these two concepts by taking the convex combination and defining r_1 as the midpoint between the dictator's minimal expectation point (m_1) and the dictator's initial endowment (e_1). They define the dictator's moral reference point as $r_1 = \frac{1}{2}m_1 + \frac{1}{2}e_1$.

In summary, they define the moral reference points as

$$r = (r_1, r_2) = \left(\left(\frac{1}{2}m_1 + \frac{1}{2}e_1\right), m_2\right).$$

Cox et al. (2017) argue that these two points together can explain giving behavior because they capture a measure of the dictator's moral cost. Keeping r_2 constant, Cox et al. (2017) moral monotonicity choice theory predicts an increase in r_1 to decrease the dictator's transfer to the recipient due to the dictator's sense of entitlement. Keeping r_1 constant, Cox et al.'s (2017) moral monotonicity choice theory requires an increase in r_2 to decrease the dictator's transfer to the recipient.[45] In section 2.4, we test to see if the estimated coefficients on r_1 and r_2 are negative as predicted when regressing on the share allocated to recipients.

In the next section, we discuss results from our meta-regression and regression analyses.

2.4 Multiple Regression Results

With our updated data, we run similar multiple regression analyses as those from Engel (2011) to compare our results with his original estimates. Columns (1) and (2) in Table 16 show results for the meta-regression and OLS estimation when we control for original covariates found in Engel's (2011) original analyses. Columns (3) and (4) contain estimates when we do not include "recipient endowment" to Engel's (2011) original covariates. Columns (5) and (6) contain estimates when we include the dictator's and recipient's minimal expectation points. Columns (7) and (8) contain estimates when we replace the minimal expectation points with the two moral reference points.[46] Unlike in Engel's (2011) original estimation, our dependent variable is not the average share of the stake that the dictator gives to the recipient. Our dependent variable is the average percentage of the total endowment that the dictator transfers to the recipient. Consequently, the interpretation of Engel's (2011) original estimates are not directly comparable to our estimates.

We estimate the meta-regression at the treatment level using the standard errors that we recorded for the 440 treatments that had information on standard errors. All other regressions use reconstructed individual-level data. We cluster the standard errors for all the regressions (aside from the meta-regression) at the treatment level.

[45] We derive that the sign of the estimates on m_1, m_2 or r_2, and r_1 should be negative on transfers in Appendix E.
[46] Tables F.1-F.4 in Appendix F contains results when we run the other regression analyses found in Engel's (2011) paper.

Table 16. Multiple Regression Results

	(1) Meta-regression	(2) OLS no treat dummies	(3) Meta-regression	(4) OLS no treat dummies	(5) Meta-regression	(6) OLS no treat dummies	(7) Meta-regression	(8) OLS no treat dummies
m_1					-0.101+	-0.155+		
					(-1.66)	(-1.91)		
m_2 or r_2					-0.341***	-0.197***	-0.429***	-0.309***
					(-5.94)	(-3.55)	(-5.84)	(-4.18)
r_1							-0.188+	-0.238+
							(-1.81)	(-1.76)
limited action space	-0.067***	-0.046	-0.064**	-0.047	-0.042+	-0.011	-0.043+	-0.021
	(-3.55)	(-1.54)	(-3.31)	(-1.59)	(-1.79)	(-0.24)	(-1.92)	(-0.52)
degree of uncertainty	-0.135	-0.151**	-0.144+	-0.151**	-0.136	-0.154**	-0.137	-0.154**
	(-1.59)	(-2.86)	(-1.65)	(-2.85)	(-1.62)	(-2.95)	(-1.63)	(-2.93)
incentive	-0.016	-0.016	-0.015	-0.016	-0.012	-0.012	-0.012	-0.013
	(-1.41)	(-0.93)	(-1.36)	(-0.95)	(-1.12)	(-0.78)	(-1.13)	(-0.81)
repeated game	-0.061***	-0.018	-0.079***	-0.016	-0.057**	-0.021	-0.057**	-0.020
	(-3.41)	(-1.03)	(-4.37)	(-0.94)	(-3.20)	(-1.22)	(-3.23)	(-1.18)
group decision	-0.015	-0.098*	-0.007	-0.095*	-0.016	-0.097*	-0.016	-0.097*
	(-0.63)	(-2.09)	(-0.29)	(-2.07)	(-0.70)	(-2.07)	(-0.70)	(-2.07)
identification	0.073***	0.077***	0.079***	0.078***	0.071***	0.075**	0.071***	0.076***
	(4.03)	(3.41)	(4.24)	(3.47)	(3.97)	(3.32)	(3.99)	(3.35)
social cue	-0.008	-0.043	-0.002	-0.041	-0.013	-0.046	-0.013	-0.045
	(-0.35)	(-1.37)	(-0.10)	(-1.31)	(-0.58)	(-1.47)	(-0.58)	(-1.44)
concealment	-0.065**	-0.047+	-0.060**	-0.047+	-0.068**	-0.050*	-0.067**	-0.050*
	(-2.95)	(-1.97)	(-2.65)	(-1.94)	(-3.10)	(-2.07)	(-3.08)	(-2.05)
double blind	-0.038**	-0.062**	-0.042**	-0.067**	-0.029*	-0.059**	-0.028*	-0.061**
	(-2.88)	(-2.96)	(-3.15)	(-3.31)	(-2.23)	(-2.99)	(-2.15)	(-3.03)
take option	0.016	-0.030	-0.108**	-0.093***	-0.054	-0.073*	-0.088*	-0.110**
	(0.37)	(-0.92)	(-2.84)	(-3.91)	(-1.42)	(-2.46)	(-2.28)	(-3.10)

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deserving recipient	0.142***	0.197***	0.137***	0.197***	0.142***	0.190***	0.142***	0.192***
	(9.25)	(4.88)	(8.76)	(4.88)	(9.37)	(4.74)	(9.37)	(4.79)
recipient earned	0.160***	0.191***	0.153***	0.194***	0.148***	0.191***	0.144***	0.191***
	(4.82)	(4.79)	(4.50)	(4.64)	(4.50)	(4.94)	(4.40)	(4.88)
efficiency recipient	0.041***	0.024**	0.038***	0.023**	0.040***	0.023**	0.040***	0.023**
	(4.15)	(2.94)	(3.74)	(2.98)	(4.14)	(2.92)	(4.13)	(2.92)
multiple recipients	0.123***	-0.077	0.120***	-0.077	0.113**	-0.066	0.114***	-0.069
	(3.58)	(-1.54)	(3.39)	(-1.56)	(3.25)	(-1.32)	(3.32)	(-1.38)
recipient endowment	-0.268***	-0.179***						
	(-4.86)	(-3.39)						
dictator earned	-0.144***	-0.170***	-0.163***	-0.188***	-0.149***	-0.170***	-0.151***	-0.170***
	(-5.86)	(-8.05)	(-6.56)	(-8.43)	(-6.17)	(-8.38)	(-6.24)	(-8.29)
real money	0.032*	0.044	0.034*	0.049	0.023	0.044	0.021	0.045
	(2.20)	(1.39)	(2.25)	(1.54)	(1.60)	(1.41)	(1.46)	(1.42)
degree of social proximity	-0.016	0.021	-0.011	0.023	-0.018	0.020	-0.017	0.020
	(-1.51)	(1.03)	(-0.98)	(1.14)	(-1.64)	(0.97)	(-1.63)	(1.01)
student	-0.160***	-0.210**	-0.166***	-0.210**	-0.156***	-0.213**	-0.156***	-0.212**
	(-5.49)	(-2.97)	(-5.51)	(-2.96)	(-5.44)	(-3.00)	(-5.43)	(-2.98)
child	-0.157***	-0.163*	-0.159***	-0.165*	-0.152***	-0.169*	-0.151***	-0.168*
	(-4.26)	(-2.19)	(-4.21)	(-2.20)	(-4.18)	(-2.22)	(-4.16)	(-2.22)
middle age	-0.036	0.019	-0.034	0.023	-0.043	-0.010	-0.043	-0.002
	(-0.87)	(0.23)	(-0.80)	(0.27)	(-1.06)	(-0.12)	(-1.07)	(-0.02)
old age	0.260***	0.153*	0.273***	0.147*	0.265***	0.150*	0.268***	0.150*
	(3.91)	(2.06)	(4.04)	(1.98)	(4.02)	(2.01)	(4.06)	(2.01)
developing country	-0.023	-0.001	-0.014	0.002	-0.024	-0.003	-0.023	-0.002
	(-0.92)	(-0.02)	(-0.52)	(0.06)	(-0.96)	(-0.11)	(-0.92)	(-0.09)
indigenous society	-0.046	-0.088	-0.050	-0.089	-0.041	-0.096	-0.040	-0.094
	(-1.34)	(-1.25)	(-1.43)	(-1.25)	(-1.23)	(-1.34)	(-1.19)	(-1.32)
adj. R2/ pseudo R2	0.560	0.203	0.530	0.201	0.574	0.205	0.575	0.204
N	440	18,708	440	18,708	440	18,708	440	18,708

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

I focus now on results from (1) and (2). In line with Engel's (2011) findings, the meta-regression model with an adjusted R-squared value of 0.560 still has the best fit and can explain more than half of the variance. Consequently, we focus our attention mainly on the meta-regression results. We note that the recipient's initial endowment ("recipient endowment"), which is coded as a percentage of the total endowment, is negative and significant. Because of how we normalize the initial endowments as a share of the total endowments, the regression including the dictator's initial endowment would yield a coefficient that is the same but of opposite sign as the coefficient on the recipient's endowment. This estimate means that the larger the recipient's initial endowment, the less that dictator transfers to the recipients.

Using Engel's (2011) original covariates, we find that the "take option" does not have a statistically significant effect on giving. However, the "take option" is highly correlated with the "recipient endowment" variable since treatments with the take option require recipients to have a positive initial endowment. We run the same regressions without including "recipient endowment" in columns (3) and (4) and find that the estimate on the "take option" becomes negative and statistically significant, which is consistent with results from Zhang and Ortmann (2014).

Departing from Engel's (2011) original meta-regression results, the estimate on "identification" is positive and significant, meaning that dictators give more when recipients can identify their dictators. Our results fall in line with those from Frey and Bohnet (1995) who make dictators stand up for the recipients to identify them in their experiment. As expected and aligning with Engel's (2011) results, the dictator's ability to conceal her choices ("concealment"), a limited action space, and the "double blind" design negatively affect giving behavior, whereas a deserving recipient and recipient who has earned her endowment positively affect the amount transferred to recipients—all of these estimates are statistically significant. Also as expected, the "efficiency recipient" variable is positive and statistically significant, which means that dictators give more to the recipient if the recipient receives more than one unit from one unit given. The interpretation of the majority of the other estimates is similar to those found in Engel (2011) although the degree of significance may vary.

We also estimate similar regressions with all the covariates but including the two minimum expectation points to test the effect that this additional information has on giving behavior. Columns (5) and (6) contain the results. For these regressions, we remove the "recipient endowment" variable that captures the recipient's initial endowment due to a strong correlation with the recipient's minimal expectation point. To make the values across treatments comparable, we normalize the value of the minimum expectation points by dividing by the total endowment like how we normalize the players' initial endowments. As in columns (1) and (2), the meta-regression results yield the best fit and explains more than half the variation in the data. The meta-regression model in column (5) also has a better fit than the one in column (1).

Following the predictions from Cox et al. (2017), the recipient's minimal expectation point (m_2 also known as r_2) and the dictator's minimal expectation point (m_1) negatively affects the recipient's share. Both estimates are statistically significant—with the estimate on m_2 significant at the 0.1% level and the estimate on m_1 significant at the 10% level. Once again, the estimate on "identification" is positive and significant which is as expected. The coefficient on the "take option" is correctly negative though only statistically significant for the OLS model (6). The other results in the model with the minimal expectation points are very similar to those when using just the covariates from Engel's (2011) original model.

Columns (7) and (8) show results when we replace the two minimum expectation points with the two moral reference points (r_1 and r_2) which we normalize in the same way as mentioned for the minimum expectation points and initial endowments. Like in the model with the minimal expectation points, we remove the "recipient endowment" variable from the list of covariates because the information from that variable is already captured in the dictator's minimal reference point (r_1) and so is highly correlated with r_1. Once again, the meta-regression model has the best fit out of the models in Table 16, explaining more than half of the variation in the data.

The results from this model support Cox et al.'s (2017) theory of moral reference points. The signs on the coefficients on the two points are as predicted. The coefficient on the dictator's moral reference point r_1 is negative and statistically significant at the 10% level, while the coefficient on the recipient's moral reference point r_2 is negative and statistically significant at the 0.1% level. In line with expectations, the coefficients on the take option are negative and statistically significant. Identification still has a positive and statistically positive effect on the share transferred to recipients, and the rest of the results are similar to those found in the other models.

2.5 Conclusion

We take advantage of metadata collected by Engel (2011) on dictator game experiments to test Cox et al.'s (2017) theory of moral reference points. After correcting any inconsistencies that we find in the data and collecting additional information on the dictator's action space, we re-estimate the regression analyses using the same covariates as in Engel (2011) and find results for some covariates that are more in line with results from the literature. We further compare these results to those from a model where we include the dictator's and the recipient's minimal expectation points and another model where we include Cox et al.'s (2017) moral reference points. Our estimates align with the predictions of Cox et al.'s (2017) moral monotonicity theory. We confirm that when we hold the recipient's moral reference point r_2 constant, the share transferred to the recipients decreases as the dictator's moral reference point r_1 decreases. Moreover, when we hold the dictator's moral reference point r_1 constant, we see that the share transferred to the recipients decrease with an increase in the recipient's moral reference point r_2.

APPENDIX A – DERIVATIONS FOR CHAPTER 1

Let n_s = number of sellers, n_b = number of buyers, s = share of the revenue for platforms, p = price, and q = quantity.

The demand for one buyer is $p = 450 + 150n_s - 50q$.

The supply for one seller is $(1-s)p = 250 - 25n_b + 100q$.

Consequently, the total quantity demanded Q_d and the total quantity supplied Q_s are the following

$$Q_d = \frac{n_b(450 + 150n_s - P)}{50} \qquad (1)$$

$$Q_s = \frac{n_s\big((1-s)P - 250 + 25n_b\big)}{100} \qquad (2)$$

Solving for the *competitive equilibrium*, we set (1) = (2) and get the following equilibrium quantity Q^* and equilibrium price P^*

$$Q^* = \frac{n_b * n_s(8 + n_b + 6n_s(s-1) - 18s)}{2(2n_b + n_s - n_s * s)}$$

$$P^* = \frac{900n_b + 250n_s + 275n_b * n_s}{2n_b + n_s - n_s * s}.$$

To find the collusive price in the *Agency Pricing Treatment*, we can solve for the price that maximizes the seller's profit (monopoly price). The seller's total revenue TR and total cost TC are the following

$$TR = \frac{50(3\text{nb}(3 + \text{ns}) - Q)Q}{\text{nb}}$$

$$TC = \frac{25((\text{nb} - 10)\text{ns} - 4Q)Q}{\text{ns}(s-1)}.$$

Note that I have included the revenue-share as part of TC instead of TR. Consequently, $TR = Q * P$, where $P = \frac{50(9\text{nb}+3\text{nbns}-Q)}{\text{nb}}$, derived from solving for P from Q_d. The results are the same if we set the revenue-share as part of TR instead of TC.[47]

Taking the derivatives with respect to Q, we get

$$\frac{\partial TR}{\partial Q} = \frac{50(3n_b(3+n_s) - 2Q)}{n_b} \tag{3}$$

$$\frac{\partial TC}{\partial Q} = \frac{25((-10+n_b)n_s - 8q)}{n_s(-1+s)} \tag{4}$$

Solving for profit maximization by setting (3) = (4), we get the following profit-maximizing quantity (Q_m) and price (P_m)

$$Q_m = \frac{n_b * n_s(8 + n_b + 6n_s - 18s - 6n_s * s)}{4(2n_b + n_s - n_s * s)}$$

$$P_m = \frac{25(n_b(72 + 23n_s) + 2n_s(14 - 3n_s(s-1) - 9s)}{2(2n_b + n_s - n_s * 2)}$$

Depending on whether sellers decide to collude to set the monopoly price or converge instead to the competitive equilibrium, we know sellers set a price between these two prices, which leads us to our conclusion that, in a Nash equilibrium and at any given combinations of $n_s \in \{1, 2, 3, 4\}$, $n_b \in \{2, 5, 8\}$, and $s \in [0,1]$, the sellers in the *Agency Pricing Treatment* set prices that are somewhere between the competitive price and the collusive (monopoly) price.

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[47] If we put revenue-share as part of total revenue instead of total cost, we would solve for the following total revenue and total cost functions:

$$TR = \frac{(1-s)50(3n_b(3+n_s) - Q)Q}{n_b}$$

$$TC = 25\left(10 - nb + \frac{4Q}{ns}\right)Q,$$

where the quantity supplied function without revenue-share is $Q_{s\ without\ share} = \frac{n_s}{100}(-250 + 25n_b + P)$. Solving for P from $Q_{s\ without\ share}$, we get $P = 25\left(10 - nb + \frac{4Q}{ns}\right)$. Using this setup, the resulting collusive price and quantity are the same as the ones we derive above.

In the *Platform Pricing Treatment*, we want to maximize the platform's profit conditional on s, which is the same thing as maximizing industry revenue. The platform does not have any (direct) costs, so the platform's total revenue (TR_p) is

$$TR_p = s * P * Q_{sold}(P, s).$$

For price-taking sellers and given P and s, the quantity sold (Q_{sold}) is the minimum of $Q_d = \frac{n_b(450+150n_s-P)}{50}$ and $Q_s = \frac{n_s((1-s)P-250+25n_b)}{100}$ when gains from trade are possible.[48]

At prices above competitive equilibrium price (P^*), $Q_s > Q_d$, and so $Q_{sold} = Q_d = \frac{n_b(450+150n_s-P)}{50}$.

At prices below the competitive equilibrium price, $Q_s < Q_d$, and so $Q_{sold} = Q_s = \frac{n_s((1-s)P-250+25n_b)}{100}$.

At the competitive equilibrium price, $Q_{sold} = \frac{n_b*n_s(8+n_b+6n_s(s-1)-18s)}{2(2n_b+n_s-n_s*s)}$.

A platform would never want to choose prices below the competitive equilibrium price because the amount sold is determined by Q_s, so they can do better by charging a higher price that demand allows at that same Q.

At prices *above the competitive equilibrium price*, the total revenue is

$$TR_{p+} = \frac{s * P * n_b(450 + 150n_s - P)}{50}.$$

At the equilibrium price, the total revenue is

$$TR_{p*} = s * P^* * Q^*, \text{or}$$

$$TR_{p*} = s * \frac{900n_b + 250n_s + 275n_b * n_s}{2n_b + n_s - n_s * s} * \frac{n_b * n_s(8 + n_b + 6n_s(s-1) - 18s)}{2(2n_b + n_s - n_s * s)}$$

[48] Note that, when s is large enough, Q_s can exceed Q_d for all Q value, in which case no trade is possible.

Let's try to find the cases where $TR_{p+} > TR_{p*}$ hold. We get the following

$$\frac{P * n_b(450 + 150n_s - P)}{50} >* \frac{900n_b + 250n_s + 275n_b * n_s}{2n_b + n_s - n_s * s} * \frac{n_b * n_s(8 + n_b + 6n_s(s-1) - 18s)}{2(2n_b + n_s - n_s * s)}$$

Solving for P, we get

$$P < \frac{25\left(10n_s + n_b(36 + 11n_s)\right)}{2n_b + n_s - n_s * s} \qquad (5)$$

and

$$P > \frac{25 * n_s(8 + n_b - 6n_s(-1 + s) - 18s)}{2n_b + n_s - n_s * s} \qquad (6)$$

Note that (5) is the same as $P < P^*$. Consequently, the prices under which (5) hold cannot be the profit-maximizing price for the platforms since we noted that the profit-maximizing price for the platform cannot be below the competitive equilibrium price.

It can also be easily proven that for all combinations of $n_s \in \{1, 2, 3, 4\}$, $n_b \in \{2, 5, 8\}$, and $s \in [0,1]$, (6) is the same as $P > P^* > \frac{25*n_s(8+n_b-6n_s(-1+s)-18s)}{2n_b+n_s-n_s*s}$. Indeed, let P_{low} denote the right-hand-side of the last inequality. Note that s is bounded between 0 and 1, the denominators of both P_{low} and P^* are positive, and 25 is common. Consequently, $P_{low} < P^*$ if and only if

$$n_s(8 + n_b - 6n_s(-1 + s) - 18s) < 10n_s + n_b(36 + 11n_s)$$

Note that the left-hand-side is decreasing in s, so if the inequality is true for $s = 0$, then the inequality is true for all positive s. At $s = 0$, the inequality becomes

$$8n_s + n_b n_s + 6n_s^2 < 10n_s + 36n_b + 11n_b n_s$$

which is equivalent to

$$2n_s + 36n_b + 10n_b n_s - 6n_s^2 > 0.$$

The left-hand-side is decreasing in n_b, so if the inequality is true for $n_b = 2$, then it is true for all $n_b > 2$. At $n_b = 2$,

$$72 + 22n_s - 6n_s^2 > 0$$

The last inequality is satisfied for all $n_s \in \{1, 2, 3, 4\}$ since the above inequality holds for $n_s \in [-2.09, 5.75]$.

From (5) and (6), we get $P^* < P < P^*$, which cannot be true. Consequently, we find that $TR_{p+} > TR_{p*}$ cannot be true for the market parameters in my experiment. That is, for $n_s \in \{1, 2, 3, 4\}$, $n_b \in \{2, 5, 8\}$, and $s \in [0,1]$, $TR_{p*} > TR_{p+}$ must be true, and the platform's profit is maximized at the competitive equilibrium price and quantity in my experiment.

Since sellers in the Agency Pricing Treatment sets prices between the collusive price and the competitive equilibrium price and the platforms in the Platform Pricing Treatment set prices at the competitive equilibrium price, we formulate our second hypothesis.

Hypothesis 2.

At any given combinations of $n_s \in \{1, 2, 3, 4\}$, $n_b \in \{2, 5, 8\}$, and $s \in [0,1]$, prices in the *Agency Pricing Treatment* are higher or equal to those found in the *Platform Pricing Treatment*.

Looking at the market where one platform has 2 buyers (Platform-2) and the other has 8 buyers (Platform-8), I show that sellers converge to the platform with 8 buyers at Nash equilibrium. Indeed, given the same revenue-share on both platforms, sellers can always earn more by converging to Platform-8 over Platform-2 because not only are there more buyers but the search cost on Platform-8 is lower by 150 per unit. To give an example, Table A.1 shows the maximum profit that one seller could get on the platform depending on the number of buyers and sellers on the platform when $s = 0$. Table A.1 shows that a seller earns more on Platform-8 with 1 seller than she could on Platform-2 with 4 sellers. A seller's profit increase with more sellers and more buyers on the platforms. Consequently, sellers could always earn more by converging to a platform with 8 buyers than a platform with 2 buyers. For all revenue-share splits, if s is the same in both platforms, the sellers could always earn more by converging to Platform-8 than converging to Platform-2.

Table A.1. Seller's Profit on Platform-2 and Platform-8 at Collusive Price

	Platform-2				Platform-8			
Scenario	n_b	n_s	s	Seller's Profit at P_m	n_b	n_s	s	Seller's Profit at P_m
1	2	4	0%	800	8	1	0%	1,000
2	2	3	0%	600	8	2	0%	1,500
3	2	2	0%	600	8	3	0%	2,100
4	2	1	0%	300	8	4	0%	2,800

Platform-8 can leverage the larger quantity demanded and the seller's lower cost on their platform by asking for a higher share than the competitive equilibrium share of 1%. However, if Platform-8 asks for too high a share, Platform-2 can ask for a low enough share to lure all the sellers. In the competitive equilibrium, Platform-8 would set the largest share that still gives the sellers more profit than they would get in Platform-2 if Platform-2 asks for a 1 percent share.

If prices are set at the collusive (monopoly) level, then it turns out that the highest share that Platform-8 can ask that still leads sellers to prefer Platform-8 is 3%. Table A.2. depicts the seller's profits at the collusive price on Platform-2 when $s = 1\%$ and on Platform-8 when $s = 3\%$. Table A.2 shows that a seller's profit at P_m is always higher on Platform-8 with $s = 3\%$ than Platform-2 with $s = 1\%$ regardless of the number of sellers. Indeed, as we already noted, assuming prices at P_m, a seller's profit increases with the number of sellers on the platform. Consequently, assuming prices are at P_m, a seller will always choose Platform-8 if the seller's profit at Platform-8 as the only seller is more than the seller's profit at Platform-2 with 3 other sellers.

Table A.2. Seller's Profit on Platform-2 and Platform-8 at Collusive Price

	Platform-2				Platform-8			
Scenario	n_b	n_s	s	Seller's Profit at P_m	n_b	n_s	s	Seller's Profit at P_m
1	2	4	1%	926	8	1	3%	934
2	2	3	1%	815	8	2	3%	1,516
3	2	2	1%	587	8	3	3%	2,172
4	2	1	1%	389	8	4	3%	2,838

Note that, on Platform-8 with 1 seller, the seller's profit at P_m decreases as s increases. Indeed, on Platform-8 with 1 seller, $P_m = 550$ for $s \in [0\%, 72\%]$, and the seller would not choose to sell anything at $s > 72\%$. As s increases, not only is P_m constant, but the share of the revenue that goes to sellers and the quantity sold also decreases. Consequently, the seller's profit clearly decreases as s increases on Platform-8 with 1 seller.

I now look at the case when $s = 4\%$ on Platform-8 to verify that $s = 3\%$ is the highest value of s such that Platform-8 will always attract sellers regardless of the number of sellers on the other platform. Assuming the price is at P_m, $s = 4\%$ on Platform-8, and $s = 1\%$ on Platform-2, then a seller's profit on Platform-8 as the only seller is 912 which is lower than 926 which is the seller's profit on Platform-2 with 3 other sellers. Consequently, assuming price at P_m, $s = 3\%$ is the highest value of s on Platform-8 that leads sellers to always prefer Platform-8 over Platform-2 regardless of $s \in [1\%, 100\%]$ on Platform-2 or the number of sellers on the platforms.

We can follow the same logic as before to determine the highest share that Platform-8 can ask and always lure all the sellers if prices are at the competitive equilibrium level instead of at the sellers' monopoly price. It turns out that this highest share is 10%. Table A.3 depicts a seller's profits at the competitive equilibrium price on Platform-2 when $s = 1\%$ and on

Platform-8 when $s = 10\%$. Table A.3 shows that a seller's profit at P^* is always higher on Platform-8 with $s = 10\%$ than Platform-2 with $s = 1\%$ regardless of the number of sellers.

Table A.3. Seller's Profit on Platform-2 and Platform-8 at Competitive Equilibrium Price

	Platform-2				Platform-8			
Scenario	n_b	n_s	s	Seller's Profit at P^*	n_b	n_s	s	Seller's Profit at P^*
1	2	4	1%	774	8	1	10%	780
2	2	3	1%	745	8	2	10%	1,175
3	2	2	1%	582	8	3	10%	1,650
4	2	1	1%	389	8	4	10%	2,205

Similar as before, note that on Platform-8 with 1 seller, $P^* = 550$ for $s \in [0\%, 72\%]$, and sellers would not choose to sell anything at $s > 72\%$. Using the same logic as before, on Platform-8 with 1 seller, the seller's profit decreases as s increases. I now look at the case when $s = 11\%$ on Platform-8 to verify that $s = 10\%$ is the highest value of s such that Platform-8 will always attract sellers regardless of the number of sellers on the other platform. When price is P^*, s is 11% on Platform-8, and s is 1% on Platform-2, then a seller's profit as the only seller on Platform-8 is 758 which is lower than 774 which is the seller's profit on Platform-2 with 3 other sellers. Consequently, when prices are at competitive equilibrium level, $s = 10\%$ is the highest value of s on Platform-8 that leads sellers to always prefer Platform-8 over Platform-2 regardless of $s \in [1\%, 100\%]$ on Platform-2 or of the number of sellers on the platforms.

These results lead to our first and fifth hypotheses.

Hypothesis 1.

In a Nash equilibrium and conditional on s, sellers converge or "tip" to one platform. In the periods with 2-8 split, sellers choose the platform with 8 buyers.

Hypothesis 5.

In the 2-8 split periods, platforms with 8 buyers ask for higher shares than the platform with 2 buyers.

APPENDIX B – ADDITIONAL FIGURES FOR CHAPTER 1

Figure B.1 Demand and Supply on Platform with 2 Buyers

Figure B.1A: demand and supply with 2 buyers & 4 sellers

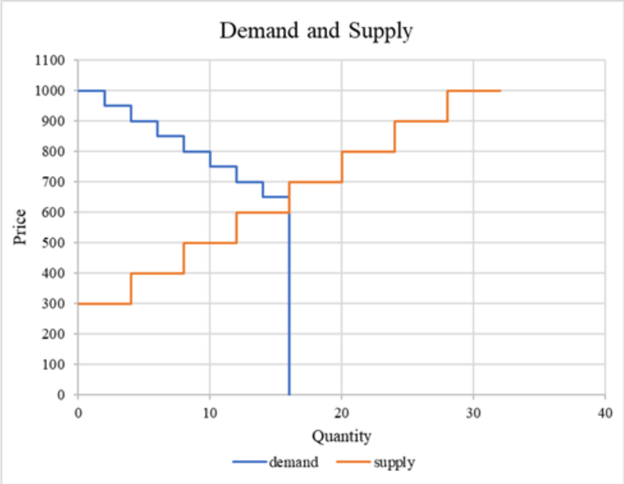


Figure B.1B: demand and supply with 2 buyers & 3 sellers

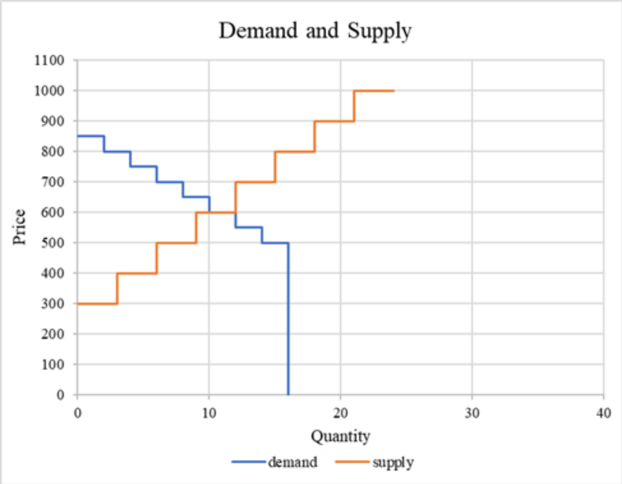


Figure B.1C: demand and supply with 2 buyers & 2 sellers

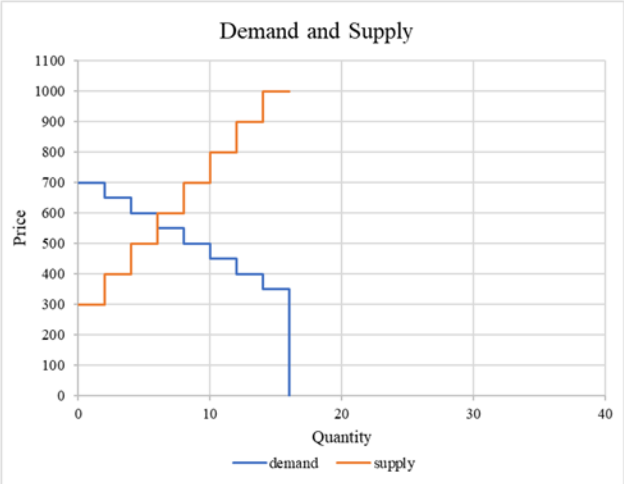


Figure B.1D: demand and supply with 2 buyers & 1 seller

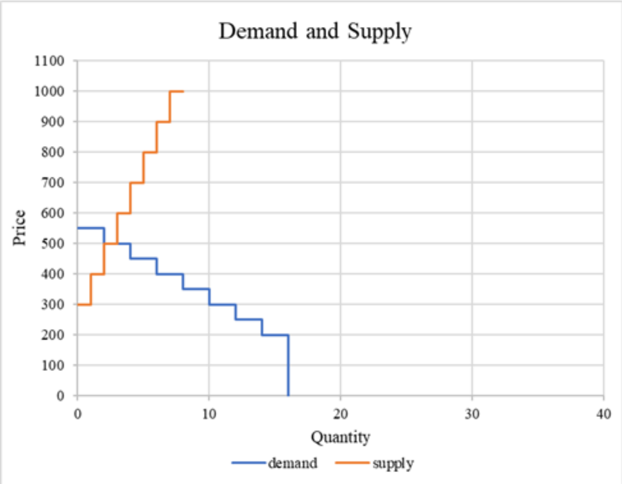


Figure B.2 Demand and Supply on Platform with 5 Buyers

Figure B.2A: demand and supply with 5 buyers & 4 sellers

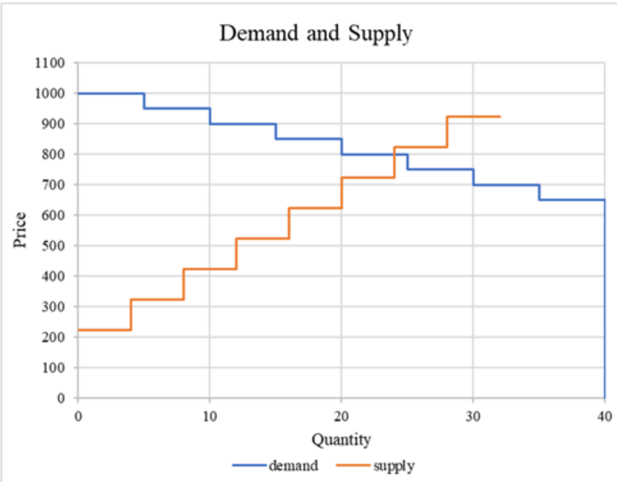


Figure B.2B: demand and supply with 5 buyers & 3 sellers

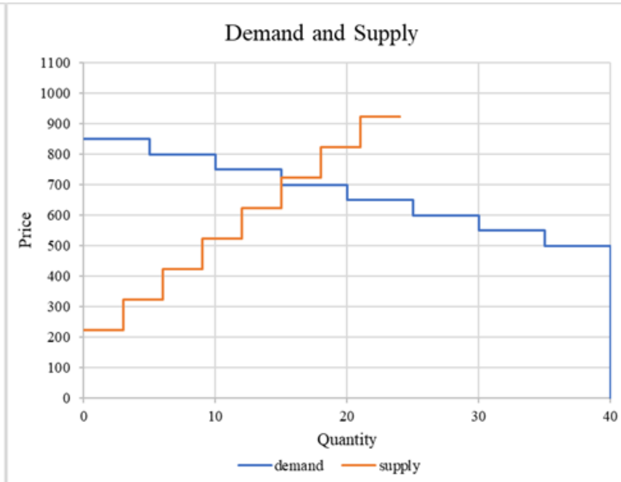


Figure B.2C: demand and supply with 5 buyers & 2 sellers

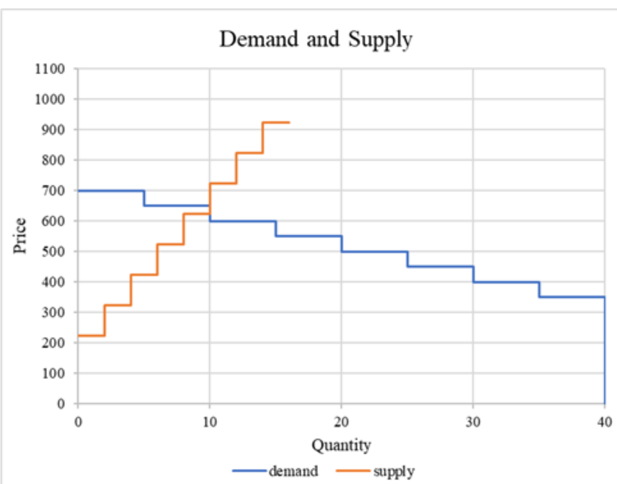


Figure B.2D: demand and supply with 5 buyers & 1 seller

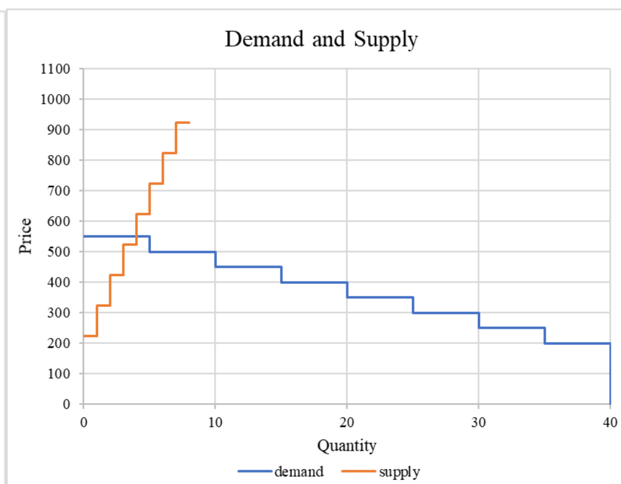


Figure B.3 Demand and Supply on Platform with 8 Buyers

Figure B.3A. Demand and Supply with 8 Buyers & 4 Sellers

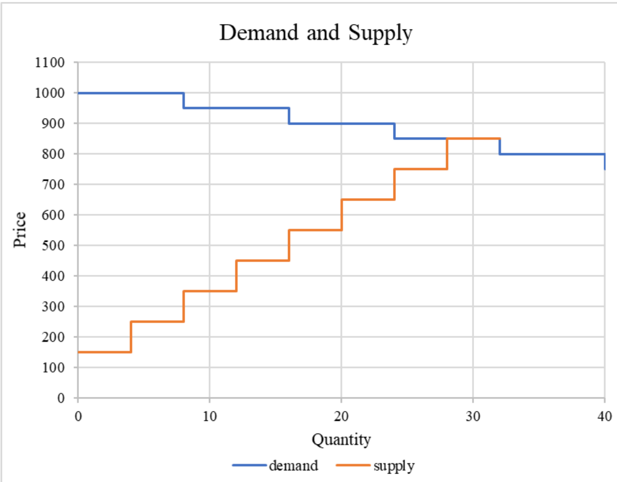


Figure B.3B. Demand and Supply with 8 Buyers & 3 Sellers

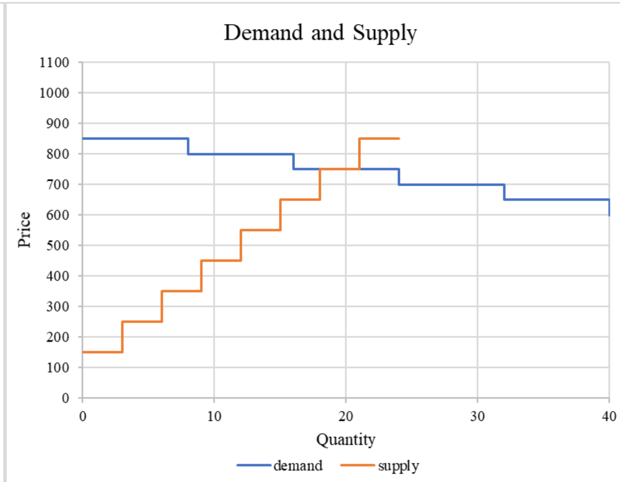


Figure B.3C. Demand and Supply with 8 Buyers & 2 Sellers

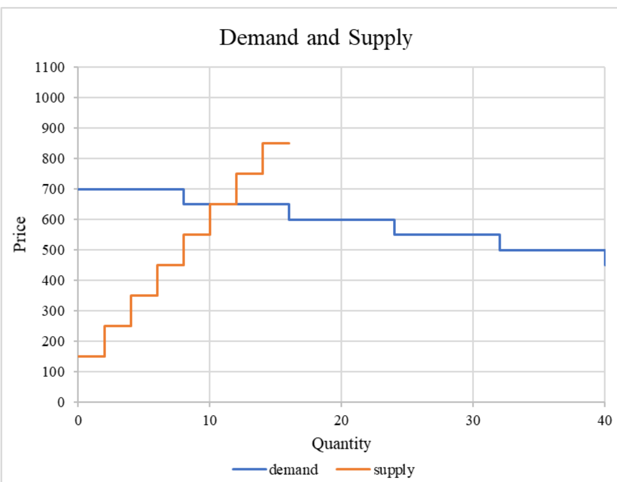


Figure B.3D: Demand and Supply with 8 Buyers & 1 Seller

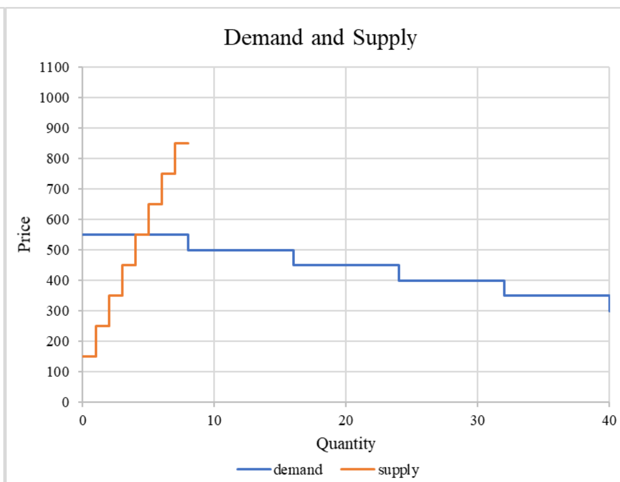
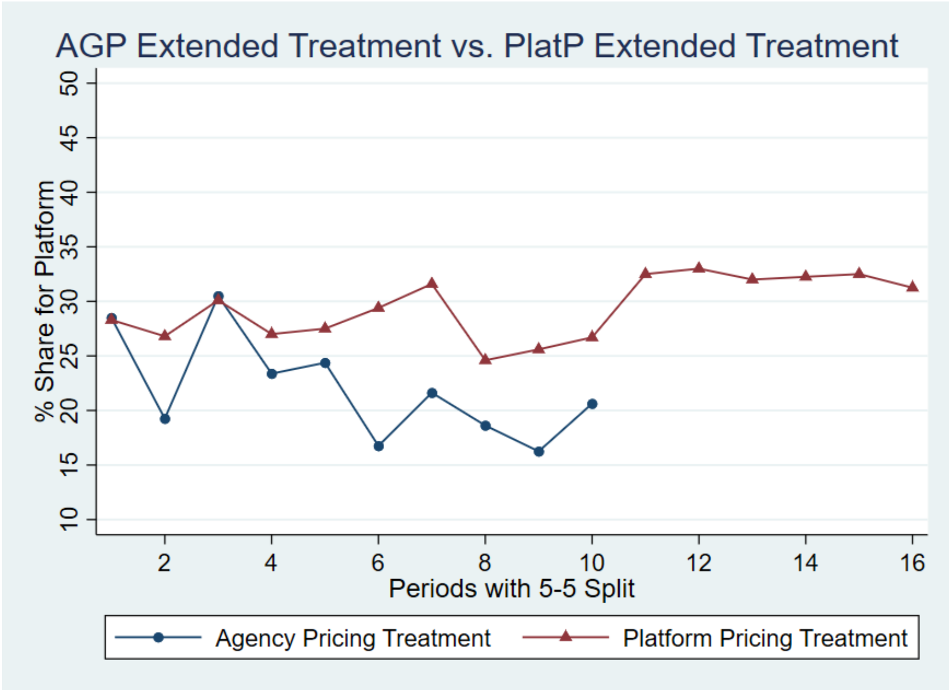


Figure B.4. Average Percent Share Asked by Platform by Period – Extended Treatments

Figure B.4A. Platforms with 5 Buyers



Note that there are fewer periods in the Agency Pricing Treatment due to technical issues.

Figure B.4B. Platforms with 8 Buyers

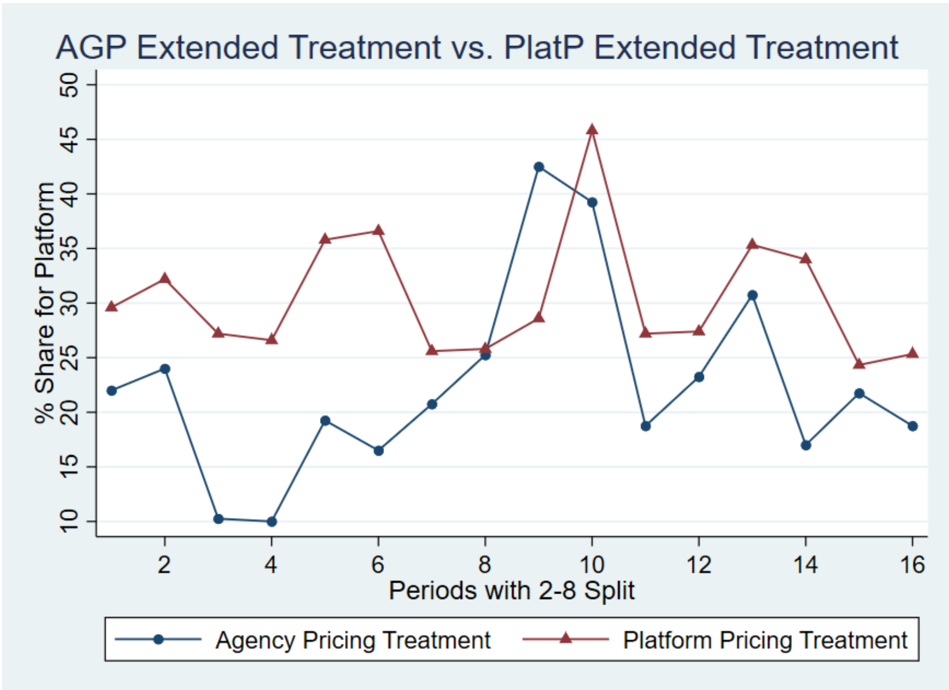


Figure B.4. Average Percent Share Asked by Platform by Period – Extended Treatments (Continued)

Figure B.4C. Platforms with 2 Buyers

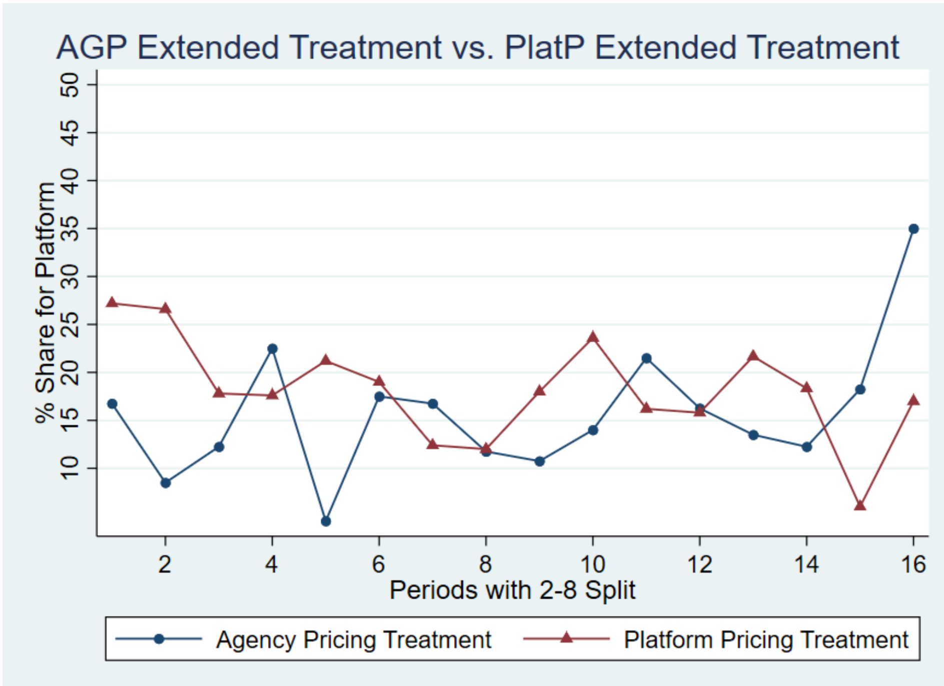
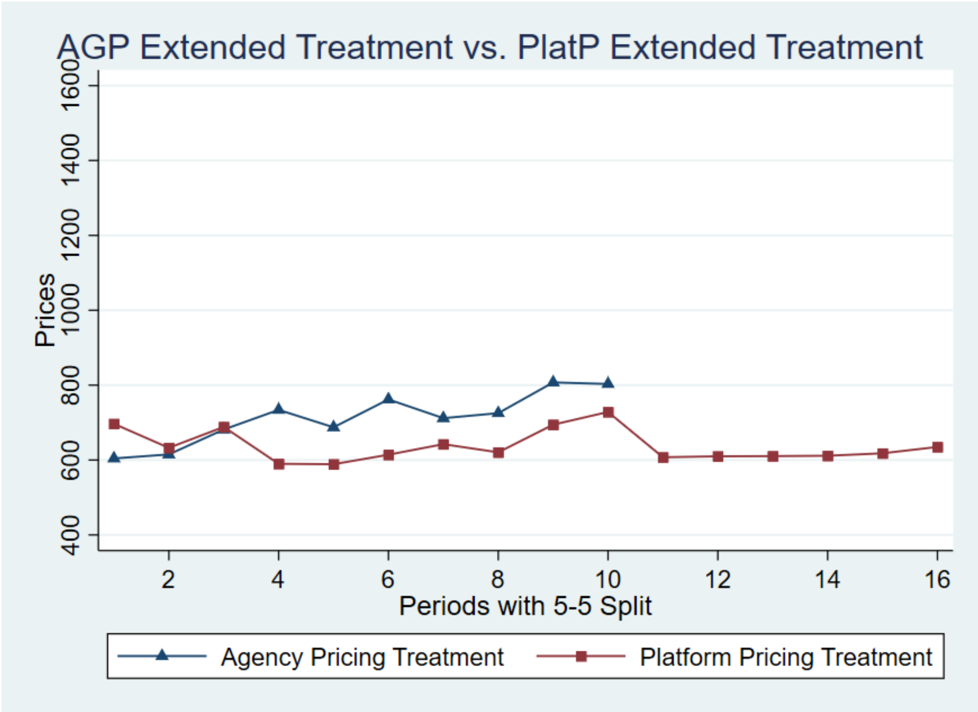


Figure B.5. Average Prices by Period – Extended Treatments

Figure B.5A. Platforms with 5 Buyers



Note that there are fewer periods in the Agency Pricing Treatment due to technical issues.

Figure B.5B. Platforms with 8 Buyers

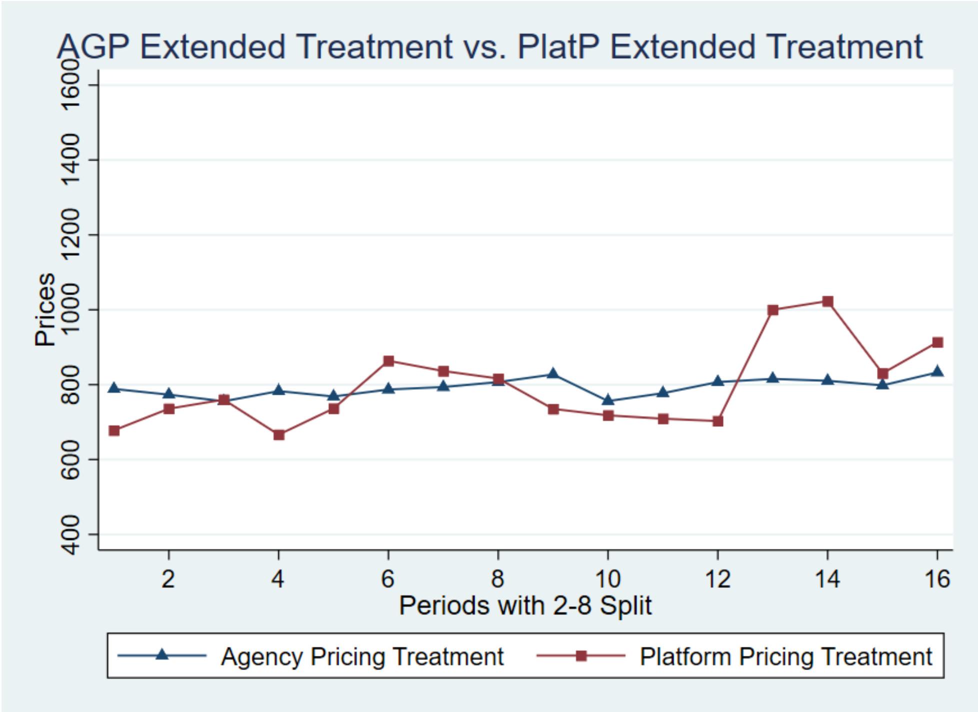
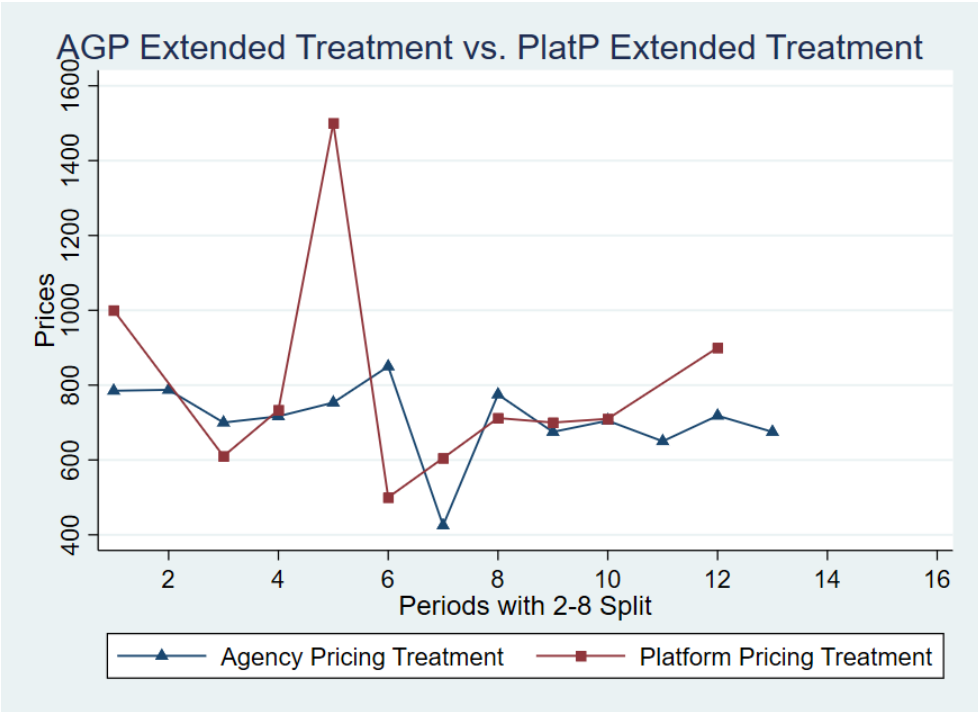


Figure B.5. Average Prices by Period – Extended Treatments (Continued)

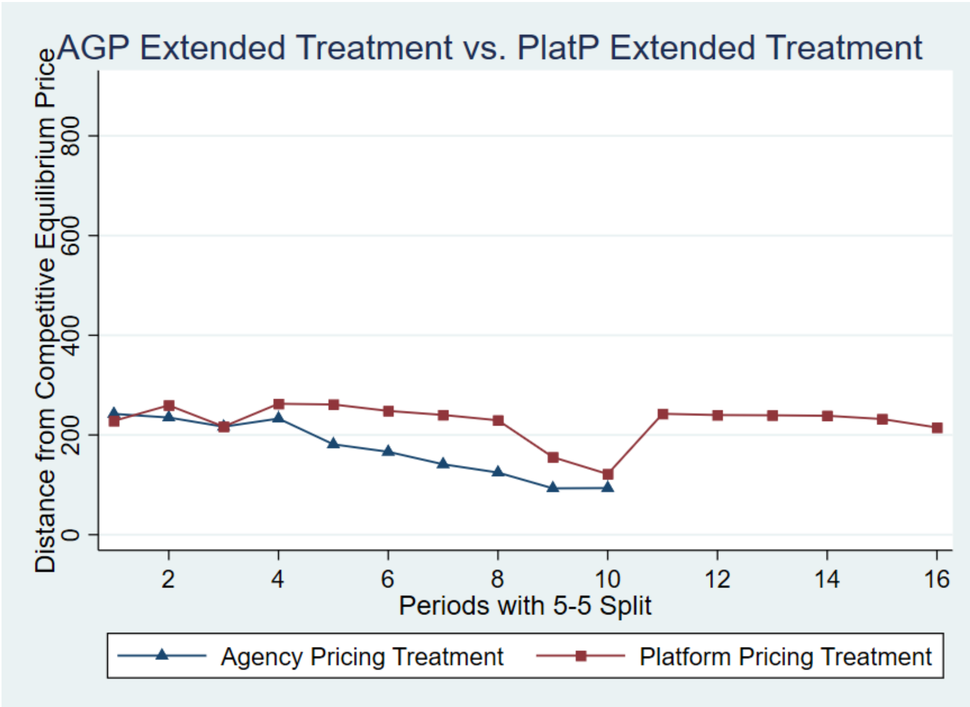
Figure B.5C. Platforms with 2 Buyers



Note that I only display prices when a seller is on the platform. The missing points in graphs indicates periods where no seller has chosen to sell on the platform with 2 buyers.

Figure B.6. Average Distance of Observed Prices from Competitive Equilibrium Price by Period – Extended Treatments

Figure B.6A. Platforms with 5 Buyers



Note that there are fewer periods in the Agency Pricing Treatment due to technical issues.

Figure B.6B. Platforms with 8 Buyers

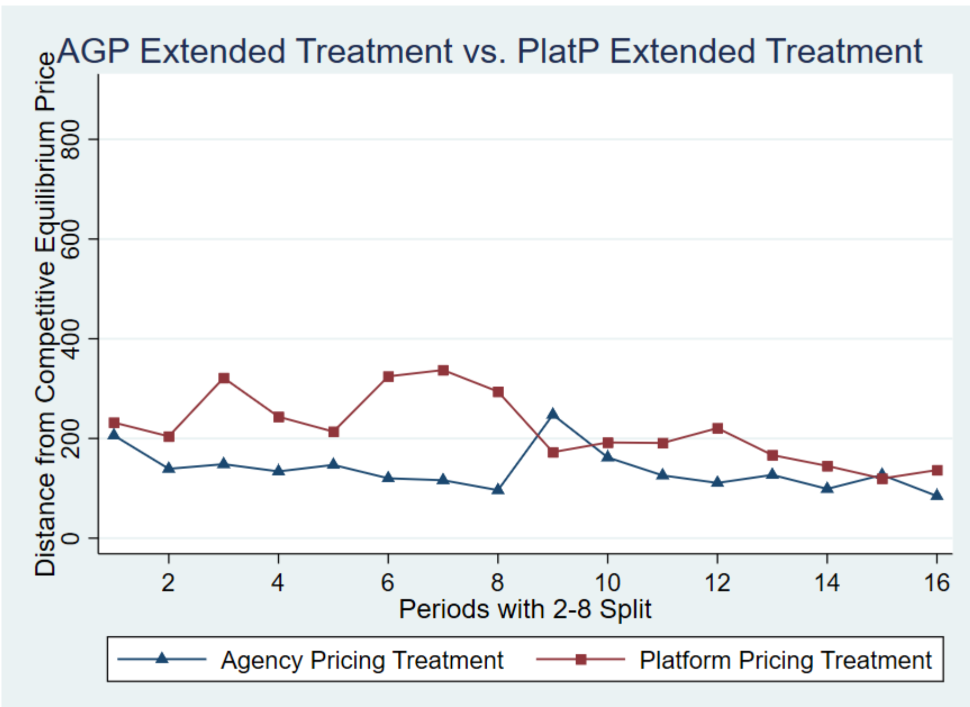
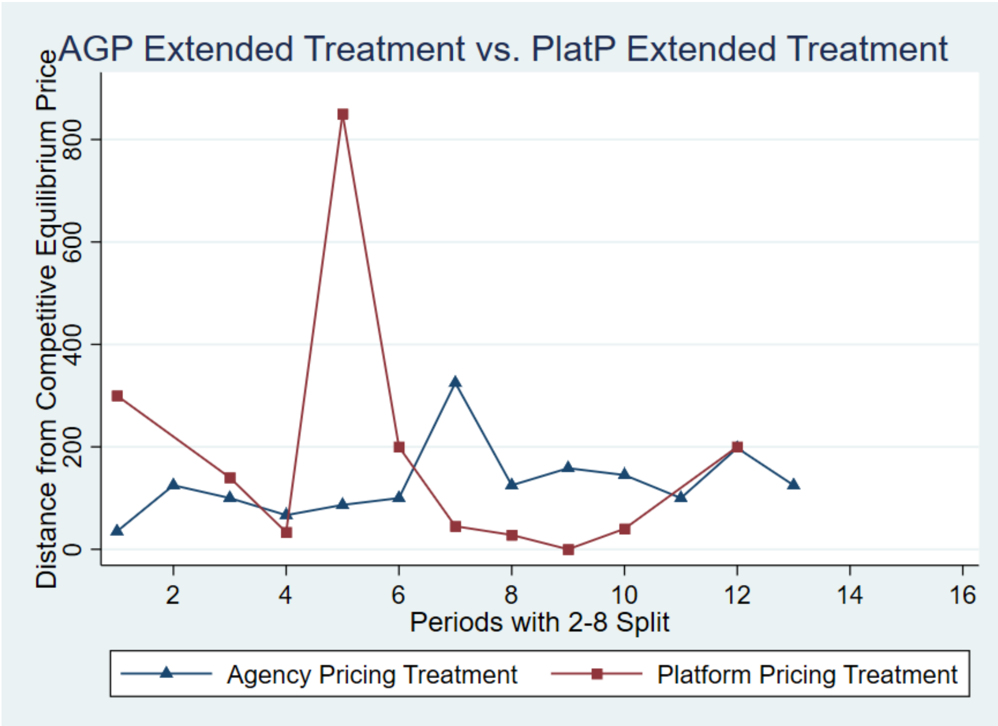


Figure B.6. Average Distance of Observed Prices from Competitive Equilibrium Price by Period – Extended Treatments (Continued)

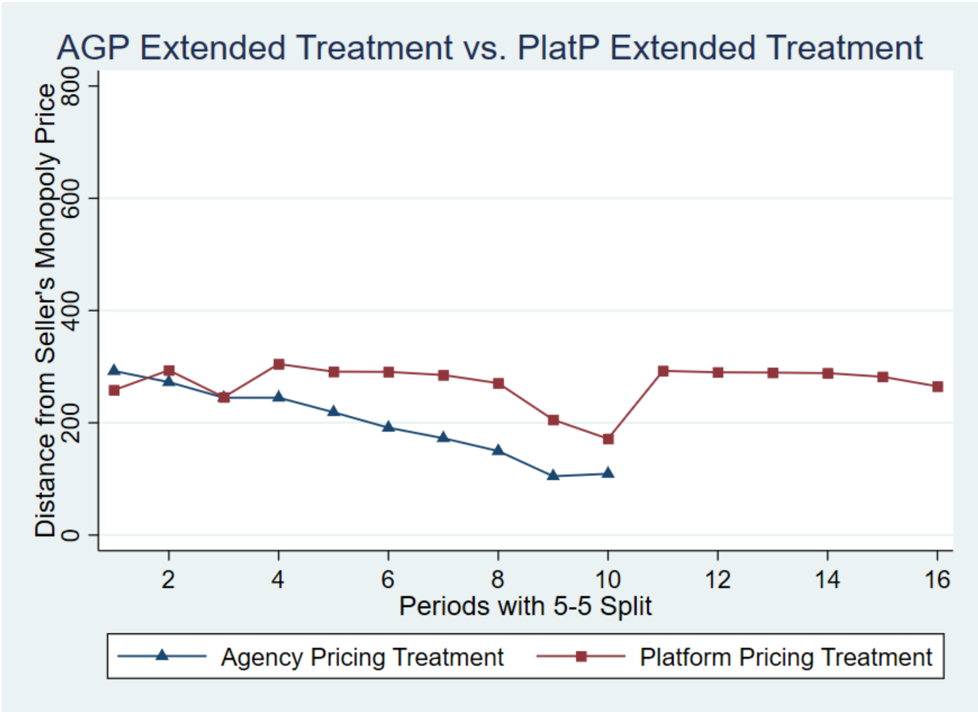
Figure B.6C. Platforms with 2 Buyers



Note that I only display prices when a seller is on the platform. The missing points in graphs indicates periods where no seller has chosen to sell on the platform with 2 buyers.

Figure B.7. Average Distance of Observed Prices from Sellers' Collusive Price by Period – Extended Treatments

Figure B.7A. Platforms with 5 Buyers



Note that there are fewer periods in the Agency Pricing Treatment due to technical issues.

Figure B.7B. Platforms with 8 Buyers

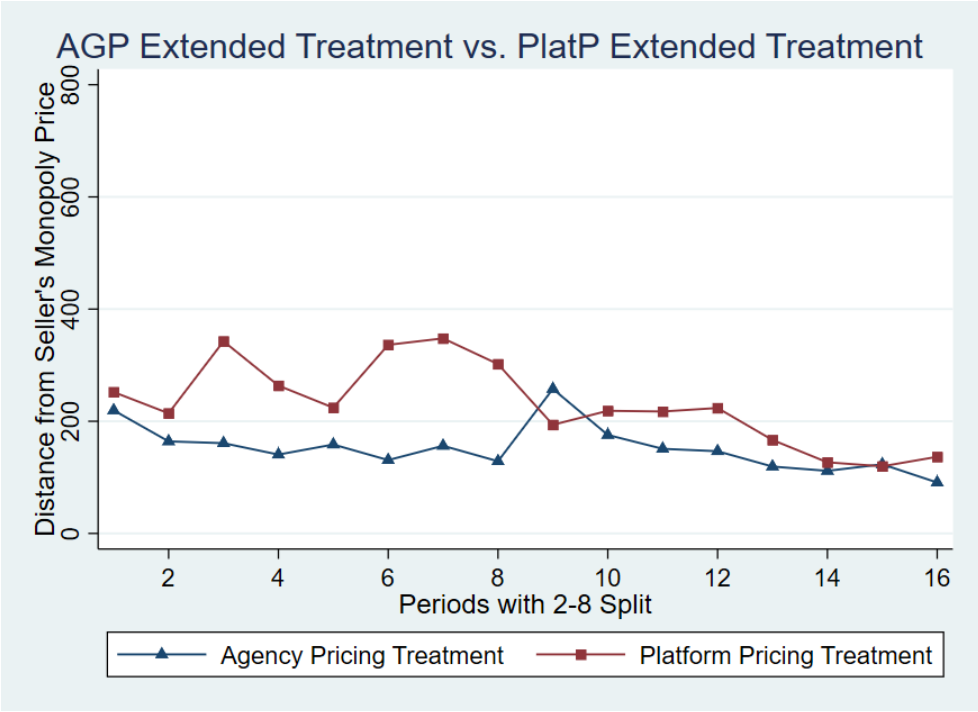
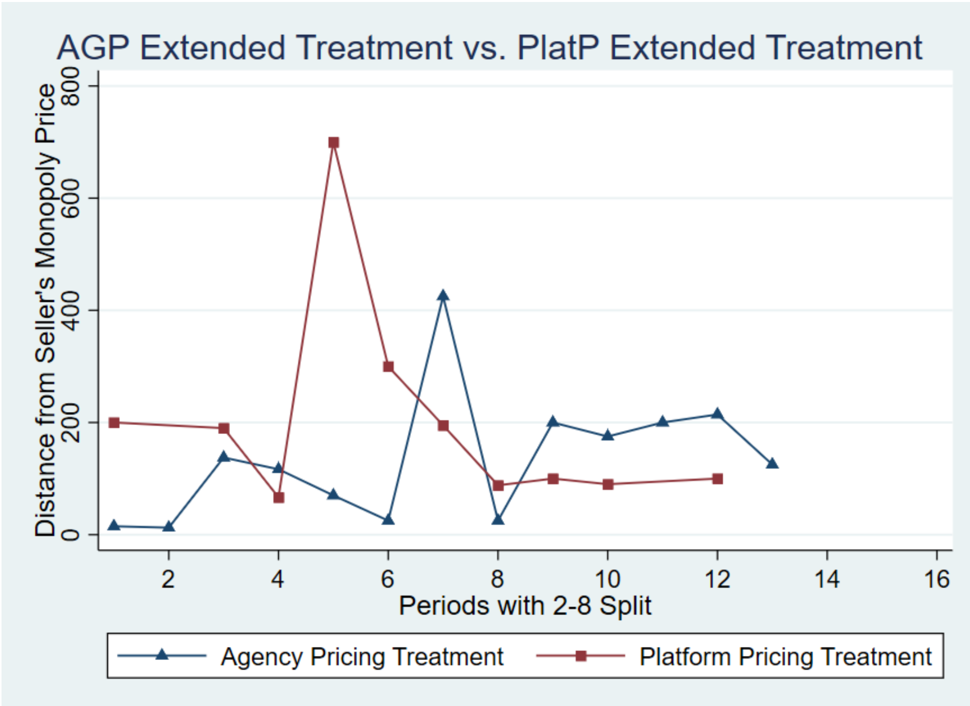


Figure B.7. Average Distance of Observed Prices from Sellers' Collusive Price by Period – Extended Treatments (Continued)

Figure B.7C. Platforms with 2 Buyers



Note that I only display prices when a seller is on the platform. The missing points in graphs indicates periods where no seller has chosen to sell on the platform with 2 buyers.

APPENDIX C – ADDITIONAL TABLES FOR CHAPTER 1

Table C.1. Logit Regression Results: Platform Choice

	(1) All	(2) All	(3) PlatP only	(4) PlatP only	(5) AGP only	(6) AGP only
Lower share	0.324***	0.328***	0.263***	0.266***	0.381***	0.383***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.04)	(0.04)
2 buyers	-0.379***	-0.381***	-0.376***	-0.377***	-0.390***	-0.392***
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
8 buyers	0.397***	0.399***	0.431***	0.432***	0.359***	0.360***
	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)
# of sellers in	0.010*	0.011*	0.019**	0.019**	0.001	0.001
previous period	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
PlatP	-0.036*[49]	-0.033				
	(0.02)	(0.02)				
PlatP*Chat	0.051	0.058				
	(0.04)	(0.04)				
Chat	0.021	0.022	0.069	0.076*	0.035	0.037
	(0.02)	(0.03)	(0.04)	(0.04)	(0.03)	(0.02)
Period	0.000	0.000	0.001	0.001	-0.001	-0.001
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Demographics		X		X		X
N	2,627	2,627	1,231	1,231	1,396	1,396

Robust, standard errors, clustered at the individual subject level, in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Note: Demographic variables include dummies on ethnicity, education level, major of study, and prior experience with market experiments. The summary statistics of these variables can be found in Table 3.

[49] I would not expect any pricing treatment effects on whether or not sellers choose one platform over another; however, sellers in the Platform Pricing Treatment seem more likely to choose Platform 1 than sellers in the Agency Pricing Treatment, although this effect is no longer statistically significant when controlling for demographics.

Table C.2. Regression Results: Market Efficiency

	(1) All 5-5	(2) All 2-8	(3) PlatP only 5-5	(4) PlatP only 2-8	(5) AGP only 5-5	(6) AGP only 2-8
PlatP	-0.058	-0.102*				
	(0.03)	(0.03)				
PlatP*Chat	0.122*	0.094				
	(0.04)	(0.05)				
Chat	-0.048	-0.049**	0.062	0.038	-0.040**	-0.042**
	(0.04)	(0.01)	(0.06)	(0.06)	(0.01)	(0.01)
4 sellers on one Platform-5	0.532***		0.467***		0.583***	
	(0.02)		(0.04)		(0.02)	
1 seller on one Platform-5	0.130**		0.148		0.110	
	(0.02)		(0.06)		(0.04)	
4 sellers on Platform-8		0.678***		0.587***		0.723***
		(0.03)		(0.04)		(0.01)
3 sellers on Platform-8		0.262**		0.223*		0.273**
		(0.04)		(0.06)		(0.04)
1 seller on Platform-8		0.080*		0.058		0.073**
		(0.02)		(0.04)		(0.01)
0 sellers on Platform-8		0.257**		0.183*		0.283**
		(0.03)		(0.05)		(0.03)
Period	0.001	-0.002	0.005	-0.002	-0.000	-0.003
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
R2	0.654	0.633	0.548	0.394	0.787	0.839
N	310	361	154	163	156	198

Robust, standard errors, clustered at the market group level, in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
I drop all market groups where sellers decide to not join a platform. The basis of comparison for the number of sellers on each type of market group is 2 sellers on each platform.

Table C.3. Regression Results: Platform Earnings

	(1) All	(2) All	(3) PlatP only	(4) PlatP only	(5) AGP only	(6) AGP only
Lower share	549.7***	533.9***	502.5***	508.0***	611.3***	586.6***
	(57.62)	(58.14)	(85.96)	(88.04)	(77.39)	(77.81)
2 buyers	-859.9***	-845.1***	-933.0***	-908.1***	-798.1***	-798.0***
	(47.15)	(47.02)	(71.37)	(72.92)	(60.94)	(60.33)
8 buyers	1528.6***	1536.4***	1641.2***	1669.2***	1439.3***	1431.1***
	(90.08)	(89.16)	(132.64)	(128.71)	(122.30)	(121.94)
PlatP	144.3*	70.0				
	(61.18)	(65.58)				
PlatP*Chat	-198.8	-197.1				
	(140.94)	(141.89)				
Chat	80.8	51.0	-144.5	-115.8	118.5	63.5
	(108.21)	(111.87)	(129.02)	(136.45)	(123.31)	(129.64)
Period	9.9*	10.5*	12.8	10.9	6.7	8.5
	(4.79)	(4.85)	(6.73)	(7.14)	(6.80)	(6.73)
Demographics		X		X		X
R2	0.409	0.425	0.420	0.454	0.404	0.421
N	1,388	1,388	652	652	736	736

Robust, standard errors, clustered at the individual subject level, in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Note: Demographic variables include dummies on ethnicity, education level, major of study, and prior experience with market experiments. The summary statistics of these variables can be found in Table 3.

APPENDIX D – SUBJECT INSTRUCTIONS

Market Experiment Instructions for Platforms
Agency Pricing Treatment

You will participate in a market experiment in which some of you will be sellers and some of you will be platforms in a sequence of several "trading periods." Each market will have 4 sellers, 2 platforms, and 10 buyers. The computer will make decisions for the 10 buyers. You are a **platform**, and you will keep this same role for the entire experiment.

As a platform, you earn money by attracting sellers to sell units on your platform and receiving a share of any resulting revenue. Sellers can only earn money by selling units on one of the two platforms.

Each trading period will consist of four phases.

- First phase: 10 buyers are divided between the 2 platforms.
- Second phase: platforms ask the sellers for a share of the revenue from any sales on their platform.
- Third phase: sellers may choose a platform and decide the price and quantity they wish to sell on that platform.
- Fourth phase: the computer makes purchase decisions for the buyers, and revenues are split between platforms and sellers.

PLATFORM DECISIONS

As a platform, you ask the sellers for a percentage of the revenue made on your platform. You can ask for any number between 0% and 100% of the revenue. All sellers observe your decision and the other platform's decision, but you will not see the other platform's decision.

Note that sellers must pay a cost for every unit sold. This cost decreases with more buyers on the platform.

PLATFORM EARNINGS

As a platform, you can earn money if sellers make a sale on your platform.

Profits are computed by multiplying the unit's selling price with the percentage of the revenue that you ask from the sellers. Consequently,

Your Profit from a unit sold = (Selling Price) × (Percentage You Ask for Yourself)
Example. Suppose you ask for 30% of the seller's revenue per transaction. A seller chooses your platform to sell one unit set at a price of 900. Another seller chooses your platform to sell one

unit set at a price of 1,000. Suppose that buyers buy the two units. No other sales occur on your platform this period. Then,

Profit from 1st transaction = 900 × 0.30 = 270
Profit from 2nd transaction = 1,000 × 0.30 = 300

Your total earnings for the period = 270 + 300 = 570

You will participate in 3, unpaid practice periods before the actual market experiment begins. You will be paid for all subsequent periods at the rate of $1 for every 1,750 points you earn. The computer keeps track of all trades and earnings. This information will be shown on your screen.

COMPUTERIZED BUYERS

The computer will make the buyers' purchase decisions.

In the beginning of every period, the number of buyers on each platform will be publicly known to everyone. Buyers in your platform can only purchase units from sellers on your platform.

<u>How are purchase decisions made?</u>
Each buyer has a **value** for each unit they wish to purchase. The computer will only make a purchase for a buyer if the price is below the buyer's valuation for that unit.

The buyer's value for each unit increases with more sellers on the platform. You will <u>not</u> know the exact values.

<u>In what order are purchase decisions made?</u>
The computer will always buy the unit with the lowest price first. The computer will make purchase decisions for each of the 10 buyers in a random order until all purchases have been made.

At the end of the period, everyone will see all quantities and prices offered by sellers and the quantities sold on each platform.

TIME LIMIT

You will have a time limit to make your decisions. The computer will display the amount of time remaining on your screen. Once you hit the time limit, whatever the current value is on your decision screen will be taken as your decision.

Market Experiment Instructions for Sellers
Agency Pricing Treatment

You will participate in a market experiment where some of you will be sellers and some of you will act as platforms in a sequence of several "trading periods." Each market will have 4 sellers, 2 platforms, and 10 buyers. The computer will make decisions for the 10 buyers. You are a **seller**, and you will keep this same role for the entire experiment.

As a seller, you earn money by selling units of a good through one of the two platforms. The platform you choose will receive a share of any resulting revenue.

Each trading period will consist of four phases.

- First phase: 10 buyers are divided between the 2 platforms.
- Second phase: platforms ask the sellers for a share of the revenue from any sales on their platform.
- Third phase: sellers may choose a platform and decide the price and number of units they wish to sell on that platform.
- Fourth phase: the computer makes purchase decisions for the buyers, and revenues are split between platforms and sellers.

SELLER DECISIONS

In the third phase, you and other sellers will see the percentage of the revenue asked by each platform. As a seller, you may choose a platform and decide the price and number of units to sell on that platform. You may also choose not to sell anything by clicking the “No Platform” button.

SELLER EARNINGS

As a seller, you earn money by selling units on a platform at prices that are above their costs. Sellers have two types of costs: a **production cost** for each unit and a **search cost** for matching with a buyer for each unit. **Your <u>search cost</u> decreases with more buyers on the platform.** These costs are private information only shown to you. Consequently,

Your Profit = (Units Sold) × (Selling Price) × (100% – Percentage for Platform)
– (Total Production Cost) – (Total Search Cost)

You do not pay the costs for a unit unless you sell the unit. However, you can make negative profit if you sell at a price lower than the cost.

Example. Suppose the **production cost** is 100 for the first unit, 400 for the second unit, and 500 for the third unit. You choose to sell on a platform that has 3 buyers and that asks for 30% of your revenue. The **search cost** on the platform is 100. You post a price of 1,000 and offer 3 units for sale. Suppose that buyers buy only 2 of your units. Then,

Profit from 1^{st} unit = (1,000 × 0.70) – 100 – 100 = 500
Profit from 2^{nd} unit = (1,000 × 0.70) – 400 – 100 = 200

Your total earnings for the period = 500 + 200 = 700

You will participate in 3, unpaid practice periods before the actual market experiment begins. You will be paid for all subsequent periods at the rate of $1 for every 1,750 points you earn. The computer keeps track of all trades and earnings. This information will be shown on your screen.

COMPUTERIZED BUYERS

The computer will make the buyers' purchase decisions.

In the beginning of every period, the number of buyers on each platform will be publicly known to everyone. Buyers in a platform can only purchase units from sellers on the same platform.

How are purchase decisions made?
Each buyer has a **value** for each unit they wish to purchase. The computer will only make a purchase for a buyer if the price is below the buyer's valuation for that unit.

The buyer's value for each unit increases with more sellers on the platform. You will not know the exact values.

In what order are purchase decisions made?
The computer will always buy the unit with the lowest price first. The computer will make purchase decisions for each of the 10 buyers in a random order until all purchases have been made.

At the end of the period, everyone will see all quantities and prices offered by sellers and the quantities sold on each platform.

TIME LIMIT

You will have a time limit to make your decisions. The computer will display the amount of time remaining on your screen. If you run out of time, the "No Platform" option will be taken as your decision, and you will not be able to earn anything in that period.

Market Experiment Instructions for Platforms

Platform Pricing Treatment

You will participate in a market experiment in which some of you will be sellers and some of you will be platforms in a sequence of several "trading periods." Each market will have 4 sellers, 2 platforms, and 10 buyers. The computer will make decisions for the 10 buyers. You are a **platform**, and you will keep this same role for the entire experiment.

As a platform, you earn money by attracting sellers to sell units on your platform and receiving a share of any resulting revenue. Sellers can only earn money by selling units on one of the two platforms.

Each trading period will consist of four phases.

- First phase: 10 buyers are divided between the 2 platforms.
- Second phase: platforms ask the sellers for a share of the revenue and sets the price for any sales on their platform.
- Third phase: sellers may choose a platform and decide the quantity they wish to sell on that platform.
- Fourth phase: the computer makes purchase decisions for the buyers, and revenues are split between platforms and sellers.

PLATFORM DECISION

As a platform, you ask the sellers for a percentage of the revenue made on your platform. You can ask for any number between 0% and 100% of the revenue. You must also decide on the sales price of any units sold on the platform. All sellers observe your decision and the other platform's decision, but you will not see the other platform's decision.

Note that sellers must pay a cost for every unit sold. This cost decreases with more buyers on the platform.

PLATFORM EARNINGS

As a platform, you earn money if sellers make a sale on your platform.

Profits are computed by multiplying the unit's selling price with the percentage of the revenue that you ask from the sellers. Consequently,

Profit from a unit sold = (Selling Price) × (Percentage You Ask for Yourself)

Example. Suppose you ask for 30% of the seller's revenue per transaction, and you price each unit at 1,000. Two sellers choose your platform to sell one unit each. Suppose that buyers buy the two units. No other sales occur this period. Then,

Profit from 1st transaction = 1,000 × (1 – 0.70) = 300
Profit from 2nd transaction = 1,000 × (1 – 0.70) = 300

Your total earnings for the period = 300 + 300 = 600

You will participate in 3, unpaid practice periods before the actual market experiment begins. You will be paid for all subsequent periods at the rate of $1 for every 1,750 points you earn. The computer keeps track of all trades and earnings. This information will be shown on your screen.

COMPUTERIZED BUYERS

The computer will make the buyers' purchase decisions.

In the beginning of every period, the number of buyers on each platform will be publicly known to everyone. Buyers in your platform can only purchase units from sellers on your platform.

How are purchase decisions made?
Each buyer has a **value** for each unit they wish to purchase. The computer will only make a purchase for a buyer if the price is below the buyer's valuation for that unit.

NOTE: The buyer's value for each unit increases with more sellers on the platform. You will not know the exact values.

In what order are purchase decisions made?
The computer will always buy the unit with the lowest price first. The computer will make purchase decisions for each of the 10 buyers in a random order until all purchases have been made.

At the end of the period, everyone will see all quantities and prices offered by sellers and the quantities sold on each platform.

TIME LIMIT

You will have a time limit to make your decisions. The computer will display the amount of time remaining on your screen. Once you hit the time limit, whatever the percentage is on your decision screen and a price of 9,999 will be taken as your decision.

Market Experiment Instructions for Sellers

Platform Pricing Treatment

You will participate in a market experiment where some of you will be sellers and some of you will act as platforms in a sequence of several "trading periods." Each market will have 4 sellers, 2 platforms, and 10 buyers. The computer will make decisions for the 10 buyers. You are a **seller**, and you will keep this same role for the entire experiment.

As a seller, you earn money by selling units of a good through one of the two platforms. The platform you choose will receive a share of any resulting revenue.

Each trading period will consist of four phases.

- First phase: 10 buyers will be divided to the 2 platforms.
- Second phase: platforms ask the sellers for a share of the revenue and sets the price for any units sold on the platform.
- Third phase: sellers may choose a platform and decide the number of the units they wish to sell on that platform.
- Fourth phase: the computer makes purchase decisions for the buyers, and revenues are split between platforms and sellers

SELLER DECISIONS

In the third phase, you and other sellers will see the percentage of the revenue asked by each platform. As a seller, you may choose a platform and decide on the quantity to sell at the price chosen by the platform. All units posted will be at the same price chosen by the platform. You may also choose not to sell anything by clicking the “No Platform” button.

SELLER EARNINGS

As a seller, you earn money by selling units on a platform at prices that are above their costs. Sellers have two types of cost: a **production cost** for each unit and a **search cost** for matching with a buyer for each unit. **Your <u>search cost</u> decreases with more buyers on the platform.** These costs are private information only shown to you. Consequently,

Your Profit = (Units Sold) × (Selling Price) × (100% – Percentage for Platform)
– (Total Production Cost) – (Total Search Cost)

You do not pay the costs for a unit unless you sell the unit. However, you can make negative profit if you sell at a price lower than cost.

Example. Suppose the **production cost** is 100 for the first unit, 400 for the second unit, and 500 for the third unit. You choose to sell on a platform that has 3 buyers and that asks for 30% of the revenue, and the platform sets a sales price of 1,000. The **search cost** on the platform is 100. Suppose that buyers buy only 2 of your units. Then,

Profit from 1^{st} unit = (1,000 × 0.70) – 100 – 100 = 500
Profit from 2^{nd} unit = (1,000 × 0.70) – 400 – 100 = 200

Your total earnings for the period = 500 + 200 = 700

You will participate in 3, unpaid practice periods before the actual market experiment begins. You will be paid for all subsequent periods at the rate of $1 for every 1,750 points you earn. The computer keeps track of all trades and earnings. This information will be shown on your screen.

COMPUTERIZED BUYERS

The computer will make the buyers' purchase decisions.

In the beginning of every period, the number of buyers on each platform will be publicly known to everyone. Buyers in a platform can only purchase units from sellers on the same platform.

How are purchase decisions made?
Each buyer has a **value** for each unit they wish to purchase. The computer will only make a purchase for a buyer if the price is below the buyer's valuation for that unit.

NOTE: The buyer's value for each unit increases with more sellers on the platform. You will not know the exact values.

In what order are purchase decisions made?
The computer will always buy the unit with the lowest price first. The computer will make purchase decisions for each of the 10 buyers in a random order until all purchases have been made.

At the end of the period, everyone will see all quantities and prices offered by sellers and the quantities sold on each platform.

TIME LIMIT

You will have a time limit to make your decisions. The computer will display the amount of time remaining on your screen. If you run out of time, the "No Platform" option will be taken as your decision, and you will not be able to earn anything in that period.

APPENDIX E – DERIVATIONS FOR CHAPTER 2

In this section, we derive the signs of the estimates of the minimal expectation points (m_1, m_2) and the moral reference points (r_1, r_2) on transfers. Let t be the transfer amount by which the recipient's payoff exceeds her minimum expectations payoff, T be the range of possible transfers allowed, and let π_1 and π_2 be the recipient payoffs for the dictator and the recipient, respectively. Let be p_1 and p_2 be the price of taking and price of giving respectively, such that $p_1\pi_1 + p_2\pi_2 = \omega$ is the dictator's budget constraint. Note that $\pi_1 = \frac{\omega - -p_2\pi_2}{p_1}$ and $\pi_2 = m_2 + p_1 t$.

Let the dictator's preferences be represented by a reference-dependent utility function $u(\pi_1 - r_1, \pi_2 - r_2)$ that is concave, increasing, continuously differentiable, and with positive cross derivatives (*). Consequently, we can write the dictator's decision problem as the following

$$\max_{t \in T} u(\pi_1, \pi_2) = u\left(\frac{\omega - p_2(m_2 + p_1\, t)}{p_1} - r_1, m_2 + p_1 t - r_2\right) \qquad (1)$$

Cox et al.'s (2017) moral reference points are $(r_1, r_2) = \left(\frac{1}{2}(e_1 + m_1), m_2\right)$, where e_1 and e_2 be the dictator's and recipient's initial endowments, respectively. Note that $p_1 e_1 + p_2 e_2 = \omega$. Consequently, the dictator's decision problem can be rewritten as the following:

$$\max_{t \in T} u\left(e_1 + \frac{p_2}{p_1} e_2 - \frac{p_2}{p_1} m_2 - p_2 t - \frac{e_1}{2} - \frac{m_1}{2}, p_1 t\right)$$

The first-order condition (which is also sufficient by properties (*) of u(.)) is the following:

$$-p_2 u_1\left(e_1 + \frac{p_2}{p_1} e_2 - \frac{p_2}{p_1} m_2 - p_2 t - \frac{e_1}{2} - \frac{m_1}{2}, p_1 t\right)$$
$$+ p_1 u_2\left(e_1 + \frac{p_2}{p_1} e_2 - \frac{p_2}{p_1} m_2 - p_2 t - \frac{e_{1_1}}{2} - \frac{m_1}{2}, p_1 t\right) = 0$$

Let $F(t, m_2)$ represent the left-hand side of the first-order condition, and let t^* be the optimal transfer that solves the first-order condition. Taking the partial derivative with respects to m_2, we get the following

$$F_{m_2}(t^*, m_2) = p_2\left(\frac{p_2}{p_1}\right)u_{11}(.) - p_1\left(\frac{p_2}{p_1}\right)u_{21}(.) = -\frac{p_2}{p_1}\big(-p_2u_{11}(.) + p_1u_{21}(.)\big) < 0$$

where the inequality follows from $u_{11}(.) < 0$, and $u_{21}(.) \geq 0$. By implicit function theorem, $\frac{\partial t^*}{\partial m_2} = -\frac{F_{m_2}(t^*, m_2)}{F_t(t^*, m_2)}$, and by properties (*) of $u(.)$, $sign\left(\frac{\partial t^*}{\partial m_2}\right) = sign\big(F_{m_2}(t^*, m_2)\big)$.

Consequently, we have the following hypothesis.

<u>Hypothesis 1</u>: The optimal transfer, t^*, decreases in m_2 $(= r_2)$

We can do the same to determine the sign of m_1 on t^*. Note that

$$F_{m_1}(t^*, m_1) = -p_2\left(-\frac{1}{2}\right)u_{11}(.) + p_1\left(-\frac{1}{2}\right)u_{21}(.) = -\frac{1}{2}\big(-p_2u_{11}(.) + p_1u_{21}(.)\big).$$

Applying the implicit theorem and properties (*) of *u*(.) again, we have $sign\left(\frac{\partial t^*}{\partial m_1}\right) = sign\big(F_{m_1}(t^*, m_1)\big) < 0$. Consequently, we have the following hypothesis.

<u>Hypothesis 2</u>: The optimal transfer, t^*, decreases in m_1.

We can also derive the sign of r_1 on t. Note that

$$F(t, r_1) = -p_2u_1\left(e_1 + \frac{p_2}{p_1}e_2 - \frac{p_2}{p_1}m_2 - p_2t - r_1, p_1t\right) + p_1u_2\left(e_1 + \frac{p_2}{p_1}e_2 - \frac{p_2}{p_1}m_2 - p_2t - r_1, p_1t\right)$$

Taking the partial derivative with respects to r_1, we get $F_{r_1}(t^*, r_1) = p_2u_{11}(.) - p_1u_{21}(.)$.

Applying the implicit theorem and properties (*) of *u*(.), we have $sign\left(\frac{\partial t^*}{\partial r_1}\right) = sign\big(F_{r_1}(t^*, r_1)\big) < 0$. Consequently, we have the following hypothesis.

<u>Hypothesis 3</u>: The optimal transfer, t^*, decreases in r_1.

APPENDIX F – ADDITIONAL TABLES FOR CHAPTER 2

Table F.1. Multiple Regression Results: Updated Version of Engel (2011)'s Analysis

	(2) OLS treat dummies	(3) Tobit	(5) Logit 0	(6) Truncated OLS	(7) Logit 50	(8) Logit 100
limited action space	0.119***	-0.101*	0.154***	0.051+	-0.042+	-0.113**
	(1.18e+10)	(-2.22)	(4.50)	(1.68)	(-1.73)	(-2.87)
degree of uncertainty	-0.289***	-0.235*	0.348*	-0.061	0.005	—
	(-4.84e+10)	(-2.36)	(2.02)	(-0.68)	(0.08)	
incentive	-0.006***	-0.007	-0.043	-0.046**	-0.032+	-0.011
	(-1.27e+09)	(-0.31)	(-1.56)	(-3.11)	(-1.68)	(-1.16)
repeated game	-0.175***	-0.007	-0.052	-0.071***	-0.083***	-0.001
	(-2.37e+11)	(-0.27)	(-1.61)	(-3.83)	(-3.68)	(-0.07)
group decision	0.025***	-0.130*	0.076	-0.015	0.055+	—
	(5.79e+09)	(-2.04)	(1.43)	(-0.59)	(1.93)	
Identification	0.166***	0.107***	-0.157**	0.050*	0.089**	0.003
	(5.87e+10)	(3.67)	(-3.02)	(2.03)	(3.05)	(0.23)
social cue	0.119***	-0.073	0.081	-0.012	0.075*	-0.014
	(3.45e+10)	(-1.48)	(1.19)	(-0.28)	(2.50)	(-0.88)
concealment	-0.271***	-0.081*	0.074**	0.008	0.015	-0.066**
	(-2.37e+10)	(-2.34)	(2.79)	(0.26)	(0.65)	(-2.99)
double blind	0.032***	-0.097**	0.063*	-0.027	-0.012	-0.053*
	(3.13e+09)	(-3.24)	(2.52)	(-1.55)	(-0.53)	(-2.24)
take option	-0.234***	-0.025	-0.014	-0.058	-0.049	—
	(-13.20)	(-0.45)	(-0.28)	(-1.09)	(-0.54)	
deserving recipient	-0.081***	0.275***	-0.163***	0.061*	-0.082**	0.087***
	(-7.63e+09)	(5.12)	(-3.94)	(2.17)	(-2.92)	(6.20)
recipient earned	0.083***	0.262***	-0.138**	0.204***	0.136***	0.066**
	(7.64e+09)	(5.02)	(-2.59)	(4.83)	(3.44)	(3.20)
efficiency recipient	0.024*	0.035**	-0.043*	-0.002	-0.007	0.016***
	(2.51)	(2.79)	(-2.54)	(-0.23)	(-0.81)	(4.88)
multiple recipients	0.079***	-0.132+	0.190+	0.124+	0.096+	-0.087***
	(3.10e+10)	(-1.93)	(1.87)	(1.90)	(1.83)	(-3.40)
recipient endowment	-0.046	-0.257**	0.135	-0.273*	-0.555**	—
	(-0.87)	(-2.66)	(1.58)	(-2.52)	(-2.86)	
dictator earned	-0.053***	-0.329***	0.272***	-0.214***	-0.290**	-0.076+
	(-1.51e+10)	(-7.48)	(6.68)	(-4.00)	(-2.76)	(-1.64)
real money	0.301***	0.062	0.010	0.004	0.010	0.097***
	(2.30e+10)	(1.39)	(0.31)	(0.25)	(0.48)	(3.38)
degree of social proximity	0.503***	0.041	-0.034	-0.004	-0.022	0.085
	(2.19e+10)	(1.44)	(-0.76)	(-0.16)	(-0.85)	(1.28)
student	-0.244***	-0.271**	0.089	-0.082**	0.141+	-0.073***
	(-2.50e+10)	(-2.79)	(0.56)	(-2.92)	(1.77)	(-6.11)

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child	-0.426***	-0.214*	-0.003	0.007	0.175*	-0.214***
	(-1.97e+10)	(-2.08)	(-0.02)	(0.17)	(1.97)	(-3.89)
middle age	-0.237***	0.048	-0.266	0.046	0.356***	-0.035
	(-1.19e+10)	(0.42)	(-1.47)	(1.35)	(4.20)	(-0.83)
old age	0.194***	0.169	—	0.208***	0.259**	-0.054
	(1.48e+10)	(1.63)		(4.67)	(2.84)	(-1.50)
developing country	-0.385***	0.025	-0.122***	-0.054	0.016	-0.017
	(-1.67e+10)	(0.81)	(-3.64)	(-1.20)	(0.51)	(-0.90)
indigenous society	-0.691***	-0.107	-0.234	-0.013	0.169*	-0.159***
	(-3.09e+10)	(-1.10)	(-1.49)	(-0.44)	(1.97)	(-4.23)
adj. R2/ pseudo R2	0.311	0.175	0.086		0.081	0.344
N	18,708	18,708	18,521	12,854	18,708	17,144

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Our dependent variable for all the non-logit regressions are the reconstructed share of the total endowment that the dictator allocates to the recipient. The dependent variable for "Logit 0" (5) is a dummy for whether the dictator gives nothing to the recipient; the dependent variable for "Logit 50" (7) is a dummy for whether the dictator allocates equal amount to the recipient and herself; and the dependent variable for "Logit 100" (8) is a dummy for whether the dictator allocates the maximum amount to the recipient.

Table F.2. Multiple Regression Results without "Recipient Endowment" Variable

	(2) OLS treat dummies	(3) Tobit	(5) Logit 0	(6) Truncated OLS	(7) Logit 50	(8) Logit 100
limited action space	0.119***	-0.103*	0.155***	0.051+	-0.042+	-0.117**
	(1.19e+10)	(-2.25)	(4.51)	(1.71)	(-1.76)	(-3.15)
degree of uncertainty	-0.289***	-0.234*	0.347*	-0.060	0.005	—
	(-4.83e+10)	(-2.34)	(2.02)	(-0.67)	(0.09)	
incentive	-0.006***	-0.008	-0.042	-0.047**	-0.034+	-0.010
	(-1.27e+09)	(-0.33)	(-1.53)	(-3.16)	(-1.73)	(-1.16)
repeated game	-0.175***	-0.005	-0.054	-0.069***	-0.082***	-0.002
	(-2.30e+11)	(-0.19)	(-1.63)	(-3.79)	(-3.68)	(-0.14)
group decision	0.025***	-0.125*	0.074	-0.011	0.059*	—
	(5.79e+09)	(-2.02)	(1.40)	(-0.45)	(2.14)	
identification	0.166***	0.108***	-0.158**	0.051*	0.091**	0.004
	(5.78e+10)	(3.74)	(-3.04)	(2.10)	(3.11)	(0.31)
social cue	0.119***	-0.070	0.079	-0.010	0.079**	-0.013
	(3.45e+10)	(-1.43)	(1.16)	(-0.22)	(2.64)	(-0.86)
concealment	-0.271***	-0.081*	0.074**	0.008	0.017	-0.066**
	(-2.36e+10)	(-2.33)	(2.79)	(0.29)	(0.72)	(-2.98)
double blind	0.032***	-0.105***	0.068**	-0.035*	-0.021	-0.057**
	(3.13e+09)	(-3.62)	(2.73)	(-2.02)	(-0.93)	(-2.75)
take option	-0.250***	-0.115**	0.032	-0.156***	-0.225**	—
	(-2.28e+10)	(-2.82)	(0.81)	(-4.42)	(-2.70)	
deserving recipient	-0.081***	0.276***	-0.163***	0.061*	-0.083**	0.084***
	(-7.62e+09)	(5.13)	(-3.93)	(2.18)	(-2.93)	(6.17)
recipient earned	0.083***	0.268***	-0.140*	0.212***	0.142***	0.067***
	(7.63e+09)	(5.00)	(-2.56)	(5.03)	(3.47)	(3.34)
efficiency recipient	0.024*	0.034**	-0.042**	-0.003	-0.009	0.016***
	(2.51)	(2.86)	(-2.61)	(-0.31)	(-0.87)	(4.90)
multiple recipients	0.079***	-0.132+	0.189+	0.124+	0.098+	-0.087***
	(3.11e+10)	(-1.96)	(1.85)	(1.92)	(1.89)	(-3.64)
dictator earned	-0.053***	-0.356***	0.286***	-0.238***	-0.329**	-0.083+
	(-1.52e+10)	(-8.08)	(7.46)	(-4.10)	(-3.04)	(-1.91)
real money	0.301***	0.068	0.005	0.010	0.017	0.099***
	(2.29e+10)	(1.55)	(0.17)	(0.56)	(0.81)	(3.86)
degree of social proximity	0.503***	0.044	-0.035	-0.000	-0.018	0.089
	(2.19e+10)	(1.57)	(-0.80)	(-0.00)	(-0.68)	(1.32)
student	-0.244***	-0.271**	0.089	-0.083**	0.141+	-0.071***
	(-2.49e+10)	(-2.78)	(0.56)	(-2.91)	(1.80)	(-6.26)
child	-0.426***	-0.216*	-0.002	0.005	0.174*	-0.215***
	(-1.96e+10)	(-2.09)	(-0.01)	(0.13)	(1.98)	(-4.12)

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middle age	-0.237***	0.053	-0.269	0.050	0.362***	-0.031
	(-1.18e+10)	(0.46)	(-1.49)	(1.45)	(4.33)	(-0.75)
old age	0.194***	0.161	—	0.201***	0.254**	-0.057+
	(1.47e+10)	(1.55)		(4.56)	(2.81)	(-1.77)
developing country	-0.385***	0.028	-0.124***	-0.052	0.020	-0.016
	(-1.67e+10)	(0.92)	(-3.68)	(-1.15)	(0.61)	(-0.86)
indigenous society	-0.691***	-0.108	-0.234	-0.014	0.169*	-0.159***
	(-3.08e+10)	(-1.11)	(-1.49)	(-0.46)	(1.99)	(-4.53)
adj. R2/ pseudo R2	0.311	0.173	0.085		0.078	0.348
N	18,708	18,708	18,521	12,854	18,708	17,719

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Our dependent variable for all the non-logit regressions are the reconstructed share of the total endowment that the dictator allocates to the recipient. The dependent variable for “Logit 0” (5) is a dummy for whether the dictator gives nothing to the recipient; the dependent variable for “Logit 50” (7) is a dummy for whether the dictator allocates equal amount to the recipient and herself; and the dependent variable for “Logit 100” (8) is a dummy for whether the dictator allocates the maximum amount to the recipient.

Table F.3. Multiple Regression Results with Minimal Expectation Points

	(2) OLS treat dummies	(3) Tobit	(5) Logit 0	(6) Truncated OLS	(7) Logit 50	(8) Logit 100
m_1	-0.631***	-0.160	-0.158	-0.286***	-0.128	—
	(-2.85e+10)	(-1.31)	(-1.46)	(-3.68)	(-1.44)	
m_2 or r_2	-0.011***	-0.275**	0.145	-0.296**	-0.582*	—
	(-6.46e+08)	(-2.70)	(1.54)	(-2.58)	(-2.43)	
limited action space	0.119***	-0.066	0.201***	0.115**	-0.010	-0.110*
	(9.98e+09)	(-1.02)	(3.72)	(2.97)	(-0.29)	(-2.39)
degree of uncertainty	-0.289***	-0.238*	0.346*	-0.064	0.006	—
	(-3.58e+10)	(-2.41)	(2.01)	(-0.71)	(0.09)	
incentive	-0.006***	-0.005	-0.040	-0.044**	-0.032+	-0.009
	(-1.05e+09)	(-0.20)	(-1.44)	(-3.08)	(-1.66)	(-0.97)
repeated game	-0.175***	-0.010	-0.055+	-0.075***	-0.084***	-0.002
	(-6.20e+10)	(-0.40)	(-1.68)	(-3.93)	(-3.70)	(-0.15)
group decision	0.025***	-0.128*	0.079	-0.014	0.056+	—
	(4.77e+09)	(-2.03)	(1.45)	(-0.55)	(1.93)	
identification	0.166***	0.105***	-0.160**	0.046+	0.087**	0.002
	(4.10e+10)	(3.60)	(-3.06)	(1.88)	(2.99)	(0.15)
social cue	0.119***	-0.075	0.079	-0.015	0.074*	-0.015
	(3.46e+10)	(-1.54)	(1.15)	(-0.36)	(2.43)	(-0.94)
concealment	-0.271***	-0.084*	0.072**	0.004	0.014	-0.069**
	(-2.24e+10)	(-2.41)	(2.64)	(0.13)	(0.57)	(-3.00)
double blind	0.032***	-0.094**	0.065**	-0.018	-0.009	-0.053*
	(3.01e+09)	(-3.27)	(2.59)	(-1.08)	(-0.40)	(-2.11)
take option	-0.039***	-0.086+	0.029	-0.112*	-0.186*	—
	(-2.58e+09)	(-1.77)	(0.63)	(-2.08)	(-2.08)	
deserving recipient	-0.081***	0.268***	-0.169***	0.050+	-0.085**	0.089***
	(-7.14e+09)	(4.99)	(-4.06)	(1.88)	(-2.99)	(5.86)
recipient earned	0.083***	0.259***	-0.133*	0.195***	0.132***	0.066**
	(7.14e+09)	(5.07)	(-2.42)	(4.61)	(3.36)	(3.02)
efficiency recipient	0.024*	0.034**	-0.044*	-0.003	-0.007	0.016***
	(2.51)	(2.78)	(-2.49)	(-0.28)	(-0.82)	(4.85)
multiple recipients	0.079***	-0.121+	0.213*	0.138*	0.107*	-0.088**
	(2.84e+10)	(-1.78)	(2.11)	(2.31)	(1.97)	(-3.02)
dictator earned	-0.053***	-0.329***	0.270***	-0.212***	-0.291**	-0.080+
	(-1.16e+10)	(-7.59)	(6.44)	(-3.96)	(-2.78)	(-1.68)
real money	0.301***	0.062	0.010	0.005	0.011	0.098**
	(2.25e+10)	(1.41)	(0.31)	(0.30)	(0.50)	(3.22)
degree of social proximity	0.503***	0.039	-0.036	-0.009	-0.023	0.083
	(2.08e+10)	(1.39)	(-0.83)	(-0.38)	(-0.91)	(1.25)
student	-0.244***	-0.274**	0.085	-0.087**	0.139+	-0.076***
	(-2.40e+10)	(-2.81)	(0.54)	(-3.13)	(1.76)	(-6.07)

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child	-0.426***	-0.220*	-0.007	-0.004	0.172+	-0.219***
	(-1.91e+10)	(-2.10)	(-0.04)	(-0.09)	(1.93)	(-3.74)
middle age	-0.237***	0.018	-0.307	-0.007	0.333***	-0.041
	(-1.14e+10)	(0.16)	(-1.60)	(-0.20)	(3.85)	(-0.94)
old age	0.194***	0.166	—	0.203***	0.257**	-0.055
	(1.42e+10)	(1.60)		(4.56)	(2.83)	(-1.45)
developing country	-0.385***	0.023	-0.124***	-0.058	0.015	-0.019
	(-1.58e+10)	(0.74)	(-3.67)	(-1.30)	(0.46)	(-0.94)
indigenous society	-0.691***	-0.114	-0.241	-0.024	0.166+	-0.166***
	(-2.93e+10)	(-1.17)	(-1.52)	(-0.80)	(1.94)	(-4.23)
adj. R2/ pseudo R2	0.311	0.176	0.086		0.081	0.338
N	18,708	18,708	18,521	12,854	18,708	16,457

+ $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$

Our dependent variable for all the non-logit regressions are the reconstructed share of the total endowment that the dictator allocates to the recipient. The dependent variable for "Logit 0" (5) is a dummy for whether the dictator gives nothing to the recipient; the dependent variable for "Logit 50" (7) is a dummy for whether the dictator allocates equal amount to the recipient and herself; and the dependent variable for "Logit 100" (8) is a dummy for whether the dictator allocates the maximum amount to the recipient.

Table F.4. Multiple Regressions with Moral Reference Points

	(2) OLS treat dummies	(3) Tobit	(5) Logit 0	(6) Truncated OLS	(7) Logit 50	(8) Logit 100
r_1	0.102	-0.239	-0.264	-0.474***	-0.190	-0.413***
	(0.84)	(-1.17)	(-1.48)	(-3.32)	(-1.21)	(-3.37)
r_2	0.040	-0.387**	0.020	-0.520***	-0.674**	—
	(0.66)	(-3.13)	(0.17)	(-3.80)	(-2.68)	
limited action space	0.119***	-0.077	0.192***	0.103**	-0.018	-0.107*
	(1.19e+10)	(-1.28)	(3.96)	(2.76)	(-0.55)	(-2.47)
degree of uncertainty	-0.289***	-0.237*	0.347*	-0.064	0.005	—
	(-4.81e+10)	(-2.40)	(2.01)	(-0.71)	(0.09)	
incentive	-0.006***	-0.005	-0.040	-0.044**	-0.032+	-0.009
	(-1.27e+09)	(-0.22)	(-1.46)	(-3.08)	(-1.67)	(-1.00)
repeated game	-0.175***	-0.009	-0.055+	-0.074***	-0.084***	-0.002
	(-2.15e+11)	(-0.37)	(-1.67)	(-3.91)	(-3.69)	(-0.13)
group decision	0.025***	-0.128*	0.079	-0.014	0.056+	—
	(5.78e+09)	(-2.03)	(1.45)	(-0.54)	(1.93)	
identification	0.166***	0.105***	-0.159**	0.047+	0.088**	0.002
	(5.73e+10)	(3.62)	(-3.06)	(1.92)	(3.01)	(0.18)
social cue	0.119***	-0.075	0.079	-0.015	0.074*	-0.014
	(3.45e+10)	(-1.53)	(1.15)	(-0.34)	(2.46)	(-0.93)
concealment	-0.271***	-0.084*	0.072**	0.004	0.014	-0.066**
	(-2.36e+10)	(-2.40)	(2.66)	(0.15)	(0.59)	(-3.00)
double blind	0.032***	-0.096***	0.064*	-0.021	-0.011	-0.051*
	(3.13e+09)	(-3.30)	(2.53)	(-1.28)	(-0.46)	(-2.17)
take option	-0.250***	-0.123*	-0.010	-0.181***	-0.219*	—
	(-2.28e+10)	(-2.32)	(-0.17)	(-3.47)	(-2.20)	
deserving recipient	-0.081***	0.270***	-0.168***	0.053*	-0.084**	0.085***
	(-7.62e+09)	(5.04)	(-4.04)	(1.96)	(-2.96)	(5.93)
recipient earned	0.083***	0.260***	-0.134*	0.197***	0.134***	0.064**
	(7.63e+09)	(5.03)	(-2.46)	(4.67)	(3.39)	(3.08)
efficiency recipient	0.024*	0.034**	-0.043*	-0.003	-0.007	0.016***
	(2.51)	(2.78)	(-2.50)	(-0.27)	(-0.81)	(4.85)
multiple recipients	0.079***	-0.124+	0.209*	0.135*	0.104+	-0.085**
	(3.10e+10)	(-1.82)	(2.08)	(2.24)	(1.93)	(-3.07)
dictator earned	-0.053***	-0.330***	0.269***	-0.214***	-0.291**	-0.076+
	(-1.52e+10)	(-7.56)	(6.44)	(-4.02)	(-2.77)	(-1.67)
real money	0.301***	0.062	0.011	0.006	0.011	0.095**
	(2.28e+10)	(1.42)	(0.34)	(0.36)	(0.52)	(3.29)
degree of social proximity	0.503***	0.040	-0.035	-0.007	-0.023	0.081
	(2.18e+10)	(1.42)	(-0.81)	(-0.31)	(-0.88)	(1.26)
student	-0.244***	-0.273**	0.086	-0.086**	0.140+	-0.073***
	(-2.49e+10)	(-2.80)	(0.54)	(-3.08)	(1.77)	(-6.07)

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child	-0.426***	-0.218*	-0.006	-0.002	0.173+	-0.210***
	(-1.96e+10)	(-2.10)	(-0.04)	(-0.04)	(1.94)	(-3.77)
middle age	-0.237***	0.028	-0.299	0.004	0.340***	-0.038
	(-1.18e+10)	(0.24)	(-1.58)	(0.11)	(3.93)	(-0.92)
old age	0.194***	0.166	—	0.203***	0.257**	-0.053
	(1.46e+10)	(1.60)		(4.56)	(2.83)	(-1.48)
developing country	-0.385***	0.023	-0.124***	-0.057	0.015	-0.018
	(-1.66e+10)	(0.76)	(-3.67)	(-1.28)	(0.47)	(-0.92)
indigenous society	-0.691***	-0.113	-0.241	-0.022	0.167+	-0.159***
	(-3.08e+10)	(-1.16)	(-1.52)	(-0.74)	(1.94)	(-4.23)
adj. R2/ pseudo R2	0.311	0.176	0.086		0.081	0.345
N	18,708	18,708	18,521	12,854	18,708	17,173

+ $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$

Our dependent variable for all the non-logit regressions are the reconstructed share of the total endowment that the dictator allocates to the recipient. The dependent variable for “Logit 0” (5) is a dummy for whether the dictator gives nothing to the recipient; the dependent variable for “Logit 50” (7) is a dummy for whether the dictator allocates equal amount to the recipient and herself; and the dependent variable for “Logit 100” (8) is a dummy for whether the dictator allocates the maximum amount to the recipient.

APPENDIX G – LIST OF PAPERS IN DICTATOR GAME METADATA

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