Pathways of Persuasion*

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Abstract

While economic theories of persuasion emphasize self-interest, others posit an important role for other-regard. For example, a salesperson might describe product features but also try to build rapport. We study these two mechanisms within a simple but rich experimental framework in which sellers, in a free-form conversation, try to convince buyers to raise their valuations for objects. We find that sellers benefit from communication despite their material conflict of interest. Communication affects both buyers self-interest and their other-regard. Changes in other-regard are mean zero, but interestingly a minority of sellers target other-regard and substantially outperform their peers. More generally, however, who is buying is actually a better predictor of persuasion than who is selling. Buyer-seller homophily also strongly predicts persuasion: gender-match, for example, more than doubles the sellers expected gain.

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1 Introduction

People often use communication to persuade others to do what they want: politicians and voters, job candidates and employers, salespeople and customers, fundraisers and donors, CEOs and investors, and so on. In these interactions, the “sender” might persuade the “receiver” in different ways. The approach emphasized within economics is to convince the receiver that a given action is in her own self-interest.\textsuperscript{1} For example, a salesperson might describe appealing features of a product, or a politician may specify how a policy would affect a voter. Writers outside of economics, on the other hand, often emphasize the importance of being likeable, building rapport, and so on. The cover of an early edition of Dale Carnegie’s self-help classic “How to Win Friends and Influence People” promised, for example, to reveal “the six ways of making people like you” (Carnegie, 1936). Evidence from social psychology suggests that “people prefer to say yes to individuals they know and like” (Cialdini, 1993). In short, persuasion could also work through manipulation of other-regard.

In this paper we bring these two mechanisms – self-interest and other-regard – together to quantify their roles in persuasion. Which pathways do successful (and unsuccessful) persuaders use? How do the answers depend on who is in the conversation, what they are persuading them to do, and what each of them knows or does not know? To examine such questions we create naturalistic experimental situations in which there is scope for one party to persuade another. For concreteness we focus on a buyer-seller paradigm: buyers have a telephone call with sellers who are incentivized to convince them to pay more for an object such as an iPad.\textsuperscript{2} We let them chat however they wish: we do not suggest approaches or train the sellers in any way other than providing material incentive to persuade. We then capture as much as possible about the interaction, from seller strategies, buyer perceptions, and the content of the conversations themselves, to their effects on buyer valuations.

Our design lets us decompose the effects of communication into changes in other-regard and (perceived) self-interest. We elicit buyers’ willingness to pay for objects before and after conversations (and also benchmark these changes against the effects of a placebo treatment in which buyers simply had time to think). We then decompose willingness-to-pay into two components: the amount they are willing to pay for the object when the seller does not receive a commission, and the additional (possibly negative) amount they are willing to pay when she does.\textsuperscript{3} Changes in the first component capture effects on self-interest, while changes in the second capture effects on other-regard.\textsuperscript{4}

\textsuperscript{1}See Crawford and Sobel (1982), Olszewski (2004), Mullainathan et al. (2008), Kamenica and Gentzkow (2011), Dziuda (2011), and Che et al. (2013), among many others, for theoretical results on when and how such persuasion is possible.

\textsuperscript{2}The sellers incentives are common knowledge, so that disclosure is not a salient strategy dimension. See Loewenstein et al. (2011) for evidence on the effects of disclosing conflicts of interest.

\textsuperscript{3}This difference can be thought of as a measure of altruism. See Yamamori et al. (2008), Andreoni and Rao (2011), and Greiner et al. (2012) for evidence on the effects of communication in a pure dictator game. See Charness and Gneezy (2008) for effects of removing anonymity, Landry et al. (2006) on face-to-face interaction, and Leider et al. (2009) on social distance.

\textsuperscript{4}Our distinction between self-interest and other-regard is thus related to but distinct from the (helpful) distinction drawn in the survey by DellaVigna and Gentzkow (2009) between belief-based and preference-based persuasion. To highlight the difference, beliefs could affect either perceived self-interest (“it seems like a great
We complement our revealed preference measures with two additional data sources. First, we collect self-reported data from buyers and sellers before and after conversations on their plans for and interpretation of the interaction. Second, we transcribe and code all conversations. To discipline this content analysis we use a novel competitive mechanism: we asked each of seven leading researchers on persuasion across economics and psychology to propose the five metrics they thought would best predict persuasion, and promised to reveal the winner, and the winner only.\(^5\) The set of items provided by the experts arguably gives us a snapshot of the state-of-the-art in forecasting while abating the usual concerns about data mining such a wide data set. To address the concern that coding is inevitably subjective we draw our coders from the same subject pool as the original buyers in our experiment, ensuring that their subjective interpretations are statistically the same as those of actual participants.

One advantage of our design is its adaptability to many contexts – one could vary the participants, the decision problem, the communication channel, and so on. Examining all of these permutations is of course beyond the scope of the paper, but we take a few first steps in order to suggest hypotheses for future work. We repeat the experimental procedure four times within each session, rematching buyers and sellers to discuss four different objects for sale. This structure gives us panel data to estimate individual effects, and also lets us examine the effects of variation in the object being sold. We chose objects to vary the degree of information asymmetry between buyers and sellers. Two are consumer electronics devices with which sellers had (by design) more familiarity than buyers. The other two are simpler and fully specified: a lottery ticket with known payoff distribution and a post-dated check. We think of the latter two objects as especially hard to sell since buyers and sellers have symmetric information about them; they effectively test whether sellers can influence risk and time preferences.

We find that communication does affect buyer behavior, despite sellers conflicts of interest. Buyers change their valuations by an (absolute) average of $49 following conversations, significantly higher than the corresponding $24 figure for placebo treatments. On the extensive margin, 56\% of valuations change after conversations compared to 31\% in placebo. Communication thus matters despite having no effect in a sizeable minority of pairs. Communication also benefits sellers: the mean change in buyers’ valuations with commissions is $14 and significantly different from 0, compared to an insignificant $3 in placebo. These are large effects in context though phone calls only lasted 3.5 minutes on average, sellers were able to increase buyer valuations by almost 5\%. Extrapolating linearly (which is unrealistic, but illustrative), our amateur sellers increased revenue by about $235 per hour.\(^6\)

How does this persuasion work? We find sizeable effects of communication on both self-interest and other-regard. Changes in self-interest are larger: conditioning on other-regard reduces the overall variation of persuasion by 16\%, compared to 29\% for (perceived) self-

\(^5\)We congratulate Joel Sobel, whose measures predicted 18\% of the variation in persuasion.

\(^6\)This calculation is based on 17 phone calls per hour, with an average increase in willingness to pay of $13.86. This number is not meant to be compared directly to telemarketers who do not have the luxury of a captive audience with every phone call.
Interestingly, though we observe some change in other-regard in 53% of conversations, these changes tend to cancel each other out across conversations, so that on average other-regard is unaffected by conversation (insignificant mean change of $-3) while perceived private valuations jump up (significant mean change of $20).

One candidate interpretation of these results is that changes in other-regard are less a result of proactive persuasion than a by-product of social interaction. Some sellers are more likeable than expected, others less, and on average no different. To test this we examine sellers self-reported strategies, coding them based on whether they emphasize self-interest or other-regard (or were simply unclear). A minority (19%) of sellers do explicitly plan to manipulate other-regard, with strategies such as “get the customer on my side,” “build a [sic] rapport,” or “remember the person’s name! People love when others remember their names.” Consistent with their strategies, these sellers are significantly more likely to have tangential conversations that do not address the decision at hand, and significantly more likely to use tactics thought to affect other-regard.

Strikingly, sellers who target other-regard out-perform their peers by a wide margin. They succeed in their stated objective, shifting buyers other-regard by $26 more on average than their peers. At the same time they do no worse than their peers at shifting buyers’ perceived self-interest. The upshot is that they benefit nearly three times as much from communication as other sellers. Even comparing conversations within-seller we find that the same seller does significantly better when targeting other-regard than when not. In short, manipulating other-regard appears to be a highly effective strategy for those who use it, though not widely used.

This result highlights one dimension along which some sellers are better than others. We might also wish to understand what characterizes the best sellers more generally. The natural first step is to use our panel data to estimate seller fixed effects, and study their correlates. Yet to our surprise we cannot reject the null that all seller effects are identical. Instead it is buyer effects that significantly predict persuasion. In fact, buyer fixed effects explain more of the variation in our data than do seller fixed effects, product fixed effects, and order effects combined, and nearly as much as the measures suggested by our most successful forecasting contestant. From a forecasting perspective, understanding what makes someone persuasive may thus be less important than understanding what makes someone persuadable.

We also examine how the identities of the participants interact, and in particular whether homophily (i.e. similarity between buyer and seller) matters. We examine two specific traits in our data – gender and nationality – as well as an omnibus measure of buyers’ perceptions of similarity with the seller. We find strong evidence that homophily facilitates persuasion: a 1 s.d. increase in perceived similarity is associated with a $18 (or 101% of the mean) increase in persuasion. Interestingly, this is driven by significant increases not only in other-regard but also in self-interest, suggesting that homophily both directly affects liking and may also make

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Note that these figures do not sum to 100% because self-interest and other-regard are not statistically independent, so that some portion of persuasion cannot be exclusively attributed to either.

This might be important, for example, if the most persuasive people tend to select into persuasion-intensive roles such as sales or negotiations.

To the best of our knowledge, little is known about the correlates or consequences of persuadability. One interesting study by Cialdini et al. (1974) finds that more easily persuaded (confederate) subjects were perceived as more intelligent by the persuaders and less intelligent by observers.
communication more credible. This is particularly evident when we focus on gender: sellers raise valuations by $22 more on average when matched with buyers of the same gender, and this effect is driven entirely by self-interest. Buyer-seller pairs from the same country see a mean positive, though insignificant increase in valuations.\footnote{These results are broadly consistent with work in psychology on similarity and persuasion, though this literature does not measure revealed preferences or decompose effects into self-interest and other-regard. Early work found robust correlations between similarity and likeability (Byrne, 1971) as well as a causal effect of similarity (in appearance) on willingness to do small favors (Emswiller et al., 1971) while more recent work has also shown incidental similarity can be persuasive: (Deceptively) being told you have the same birthday as someone increases willingness to do a favor for them (Burger et al., 2004), cooperation in a prisoner’s dilemma with them (Miller et al., 1998), and hypothetically buying a gym membership from them (Jiang et al., 2010).} We also find evidence that endogenous seller behaviors can influence perceptions of homophily. In a given interaction, sellers who use conversational tactics in their other conversations predictive of perceived similarity are perceived as more similar.

Finally, we examine how persuasion differs with the good being sold. Comparing consumer electronics goods to the abstract lottery and post-dated check, we find that sellers are $15 more effective at persuading buyers to increase their valuations for the electronics. This is consistent with the notion that information asymmetry is an important input into persuasion. Yet we also find that mean persuasion is significantly greater than zero for the time and risk questions, even though information about these is entirely symmetric. And while sellers induce much greater gains in self-interest for tablets ($28), these are substantially offset by a $13 lower change in other-regard. These patterns are consistent with the idea that the self-interest pathway is more relevant when information is asymmetric, but that the use of this pathway may substitute for or even come at the cost of other-regard.\footnote{One caveat to this last result is that we find similar (albeit insignificant) patterns in the placebo treatment. Given this, we view the last result as suggestive only.}

Our results have a number of implications for the literature on persuasion. The extensive theoretical canon on persuasion through self-interest would benefit from complementary work formalizing persuasion through other-regard, which remains under-theorized. Similarly, persuadability seems under-conceptualized, with room to better understand why some people are more receptive to persuasive messages than others, what consequences this has for them (gullible or open-minded?) and what it means for the performance of markets. The results also beg a number of obvious empirical follow-up questions – for example, how and why do experts perform better than amateurs? What are the returns to face-to-face interaction (and do they justify business travel budgets)?

Our paper also contributes to the literature on the determinants of other-regard. A number of studies have documented the role of environmental factors.\footnote{Such factors include proximity (Marmaros and Sacerdote, 2006), social distance (Leider et al., 2009), group interaction (Feigenberg et al., 2010; Shue, 2013), identification of partner (e.g. Bohnet and Frey (1999) and Small and Loewenstein (2003)), or the availability of communication (e.g. Andreoni and Rao (2011), Yamamori et al. (2008), and Greiner et al. (2012)).} Our paper adds to a smaller literature showing how economic incentives affect social preferences conditional on such factors.\footnote{See for example Glaeser et al. (2002) who document evidence of social capital investment.}
Golub and Jackson, 2012), and network formation (Curra\-\-ri\-ni et al., 2009).

The rest of the paper is organized as follows. Section 2 describes the experimental design; Section 3 describes recruitment and participant characteristics; Section 4 presents the main results; and Section 5 summarizes and offers concluding thoughts.

2 Experimental Design

Our goal in designing the experiment was to create relatively natural and relevant settings for persuasion that also enabled us to capture rich data (including conversations themselves). We focus on a buyer-seller context because of its familiarity to subjects (as well as general economic important). We organize the experiment as a series of interactions in each of which (1) a buyer submits an initial valuation for a good, (2) the buyer is randomly matched to a seller, (3) the buyer and seller have a free-form telephone conversation lasting at most 10 minutes, and (4) the buyer then submits an updated valuation. It is common knowledge that the seller’s (expected) payoff increases in the buyers’ willingness to pay, creating an incentive for her to persuade the buyer.

We define the difference between a buyers valuation before and after the phone call as persuasion. Identifying persuasion within-subject in this way maximizes our statistical power, which will facilitate analyzing specific determinants of persuasion. To address the concern that valuations might change over time even without conversation we also conduct a placebo arm, described below. Note that sellers were not given any information about buyers, including their ex ante or ex post valuations.

To decompose persuasion into effects working through self-interest and other-regard, we elicit buyers’ valuations in two different ways: once for the object alone and once including a $500 commission for the seller. To isolate self-interest, we elicit the buyers’ valuations for the good alone. The buyer makes a series of incentivized decisions, choosing between $500 or the object plus $X. As the instructions explain to the subjects, the price of the good is thus $500-X for that decision. We vary X from $0 to $480 in increments of $20. (See Table 1.) We define the buyers valuation for the good as $500 less the smallest X such that the buyer chooses the good plus $X over $500, and define changes in this quantity due to conversation as persuasion through the self-interest channel.

To measure the buyer’s altruism towards the seller, we also elicit the buyer’s valuation for the good with the seller receiving a $500 commission. Immediately following the valuation elicitation for the product alone, the buyer makes a sequence of decisions between either $500 for herself and $0 for the seller, or the object and $X for herself and $500 for the seller. (See Table 2.) The only change from the first elicitation is that the seller now receives $500 if a purchase is made; thus, how much more (or less) the buyer is willing to pay under commission captures how much she values the seller earning $500.

We incentivize the valuation decisions in the following way: For every fifty buyers that participated in the experiment, a computer lottery held roughly one week later randomly chooses one to win $500. The lottery winner also has one of her decisions across the entire experimental session randomly chosen to be implemented. For example, if a commission
Table 1: Eliciting Willingness to Pay, Flat Wage

<table>
<thead>
<tr>
<th>Please select which alternative you would prefer</th>
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<tbody>
<tr>
<td>⊙ I would prefer the new iPad</td>
</tr>
<tr>
<td>⊙ I would prefer $500</td>
</tr>
<tr>
<td>⊙ I would prefer $20 and the new iPad</td>
</tr>
<tr>
<td>⊙ I would prefer $500</td>
</tr>
<tr>
<td>⊙ I would prefer $480 and the new iPad</td>
</tr>
<tr>
<td>⊙ I would prefer $500</td>
</tr>
</tbody>
</table>

Each row is a separate question, and buyers answered all questions before proceeding with the experiment.

question is chosen where she chooses the good, she is given the good and $X, and the seller is paid $500. Since any question might be implemented, the buyer’s incentive is to report truthfully her preferences to any one question. The seller’s material incentive is to increase the buyer’s WTP for the commission questions. The higher is the buyer’s WTP, the more questions the buyer will select the alternative in which the seller receives a commission. Since we randomly choose one question for payment, a higher WTP for the bonus questions increases the likelihood the seller is paid her bonus. This is explained to the seller in her instructions (All experimental instructions and questionnaires available on request).

Our basic design is highly flexible, with scope to accommodate different communication technologies, different participant types, and different objects for sale (or even alternative decision problems). While we certainly do not fully exploit this potential, we do introduce some variation in participant matching and in the set of objects for sale. Specifically, every subject participates in four buyer-seller interactions, always in the same role; every interaction is with a new counterpart and with a different object on sale.

We chose objects (and our recruitment strategy – see below) to create variation in the degree of information asymmetry between buyers and sellers. Two of the objects were new, popular tablet computers: the “New Apple iPad” and the “Microsoft Surface”. These two products have a number of features that create rich context for discussion, and since many of these features are likely not known to potential buyers they also create uncertainty about the products’ value. Moreover, the fact that a meaningful proportion of our subject pool already owned one of these two products enabled us to recruit a cohort of sellers with more experience using them than the typical buyer. The other two objects were completely-defined abstract goods: a $500 check post-dated for January 1st, 2014, and a lottery that paid $1,000 with 50% chance and $0 with 50% chance. In contrast to the tablets, there was no uncertainty about the objective attributes of these goods. Buyer valuations for these should in theory depend only on time and risk preferences and hence should be difficult for sellers to influence, at least through the self-regard channel. The order in which subjects discussed these four products was randomized at the session level, and subjects did not learn which product they would be discussing in each conversation until after buyer-seller pairings had been made.

Buyers and sellers were recruited to different buildings on campus. They were not made aware of the other room full of participants until the instructions were read. Buyers sat in the Economics experimental laboratory. Their instructions were read by subjects on the
Table 2: Eliciting Willingness to Pay, Commission

<table>
<thead>
<tr>
<th>Please select which alternative you would prefer</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>⊗ I would prefer the new iPad for me and $500 for the seller.</td>
<td>⊗ I would prefer $500 for me and $0 for the seller.</td>
</tr>
<tr>
<td>⊗ I would prefer $20 and the new iPad for me and $500 for the seller.</td>
<td>⊗ I would prefer $500 for me and $0 for the seller.</td>
</tr>
<tr>
<td>⊗ I would prefer $480 and the new iPad for me and $500 for the seller.</td>
<td>⊗ I would prefer $500 for me and $0 for the seller.</td>
</tr>
</tbody>
</table>

Each row is a separate question, and buyers answered all questions before proceeding with the experiment.

computer (using the Qualtrics survey platform) with only clarifying questions answered. Their valuation elicitations were done in private at their computer terminal once they confirmed the instructions were clear. They then dialed into calls with sellers using cordless landlines. Sellers sat in individual music practice rooms, preventing them from overhearing the conversations other sellers were having and (potentially) learning from their approaches. Sellers read their instructions off of hard copy slide shows, with only clarifying questions answered. They then dialed into calls with buyers using their cell phones and headsets provided by the researchers. Calls were made and recorded using a third party conference call service. Calls would end at the seller’s discretion or after ten minutes had passed.

After each conversation and after the ensuing WTP elicitation, buyers filled out an unincentivized survey about the conversation they had just had. For example, buyers wrote down as many factual statements the seller made as they could recall (up to ten) and noted for each whether whether that statement would be more likely to make someone pay more, less, or about the same for the product, and whether it was more likely to be true or false. The question we make most use of in the analysis below asked buyers to rate how similar the seller was to them on a scale from 0-10.

2.1 Researcher Predictions and Conversation Coding

Besides valuations and survey responses, we also viewed the communications between buyers and sellers themselves as data that potentially shed light on how persuasion works. Recent analyses of such content have demonstrated surprising predictive power and insights into the nature of communication (Pennebaker, 2011). We therefore recorded and later transcribed all

14 rondee.com
15 The other questions included were: What credible sources did the seller cite and how often? Did they seller make any statements of intent? Did they seller make any promises? (Charness and Dufwenberg, 2006) How surprised would your friends be if they knew how much/little you paid for this product? What percent of OSU students own the product? How likeable was the seller? How trustworthy was the seller? What did the seller think of you? Did the seller listen well? Was the interaction adversarial/cooperative? Did you know the seller beforehand? How clear were the instructions? How confusing was the study? Could anything be improved? Were you given enough time? What was the point of the conversation? And what was the other participant’s strategy?
buyer-seller conversations.\textsuperscript{16}

In coding these transcripts for econometric use we were cognizant of two common concerns
with content analysis. First, researchers often worry that coding is inherently subjective. We
agree, but do not view this as a (necessary) weakness; rather, we take the view that because
communication is inherently at least somewhat subjective it is important to capture the “right”
subjective interpretation, i.e. the one that is relevant for the research question posed. To do
this we recruited coders from the same subject pool as the buyers from our original experiment.
This ensures that our coders interpretation of messages are (statistically) the same as those of
the audience for which they were originally intended. We also incentivized thoughtful, honest
answers by our coders using the coordination game structure proposed by Houser and Xiao
(2011): two subjects were anonymously paired with each other, read the same transcript and
were asked the same question, e.g. “How likeable did you find this seller?” They were paid
every time they their answers matched.

Second, researchers often worry that rich textual data-sets invite data-mining. We therefore
sought to discipline our analysis by committing ex ante to a fixed set of informed, theory-
relevant predictions. Rather than selecting these ourselves from the literature and attempting
to map them into our context, we solicited proposals directly from leading researchers in
the field using a novel competitive mechanism. We contacted eight researchers from across
economics and psychology and asked them to describe five variables each that could be obtained
from transcripts using either computer or human coding and that would best predict changes
in buyer valuations in our experiment.\textsuperscript{17} We provided each researcher with the instructions
for our experiment and informed them that we would estimate regression models interacting
their predictors with product fixed effects, identify the submission with the highest $R^2$, and
publish the name of the winner (but keep the names of other participants anonymous). This
scheme was meant to make the challenge fun and to elicit thoughtful predictions; identifying
the “winner” is not central to our results.

\subsection*{2.2 Placebo Condition}

Unlike many experimental designs in which the obvious counterfactual to “treatment”
is “no treatment,” there are several outcomes to which we could meaningfully compare post-
conversation valuations. If we want to compare these choices to decisions made after not having
given any previous thought to the problem, then pre-conversation valuations are arguably the
best counterfactual. At the same time it seems plausible that part of the effect of a conversation
is simply the effect of having additional time to consider the decision. In this case it is useful
to benchmark buyers’ valuations after conversations with valuations after they have additional
time to reflect.

To estimate this benchmark we also ran a placebo condition that was structurally identical
to the one above except without conversations: a buyer was matched with a seller who could

\textsuperscript{16}Due to technical difficulties with the teleconferencing service, the phones themselves, and the online recording and hosting service, we either failed to record or lost the recordings for seven conversations, leaving 257 conversations for the content analysis.

\textsuperscript{17}We do not claim, nor did we intend, for the eight researchers to be the “top” eight in the field of persuasion. Rather, they are all well published and recognized in the field.
potentially earn a commission, but they were not allowed to communicate. These buyers instead waited silently for about the length of a phone call before submitting ex post valuations. We ran this placebo treatment during the last three sessions.

3 Recruitment and Data Description

3.1 Recruitment

We recruited subjects via an email to the entire Ohio State Economics experiment subject pool inviting them to take a survey, which would qualify them for our experiment. This two-minute “intake survey” asked a variety of questions about ownership, use, and familiarity of various products. In order to identify subjects with more or less experience with the tablet goods in our experiment, we asked survey respondents (among many filler questions) about their ownership of and familiarity with the iPad (any version) and the Surface.18 We then assigned anyone who filled out the intake survey and said they owned the iPad or the Surface to be sellers, and assigned anyone who participated in the intake survey and said they owned neither the iPad or the Surface to be a buyer. We did not tell subjects what role they would be playing, so that the only perceptible difference was the location on campus to which subjects were invited.

We held 15 experimental sessions between January and April of 2013, during which we observed 264 buyer-seller conversations and 68 placebo interactions. Appendix Table A-2 summarizes participation by session. In sessions 1 through 11, equal numbers of buyers and sellers participated and all buyers spoke to sellers four times each, once per product. In sessions 12 through 15 more buyers than sellers participated and buyer-product pairs were randomly assigned to one of two conditions: conversations with a seller, as in sessions 1 through 11, or a placebo condition in which the buyer did nothing for ten minutes. The analysis that follows refers to data from conversations in the first 11 sessions except where otherwise noted.19 Overall, sixty-six sellers participated in four conversations each. Fifty-five buyers participated in sessions in which they spoke with four sellers, while 28 buyers participated in sessions where, randomly from product-to-product, they might have conversations, or they might be in the placebo condition.20 Subjects were paid $15 for participation plus their payments from buyers’ choices and the lottery the following week. No session lasted longer than two hours.

18 In addition, the intake survey only asked about ownership, use, and familiarity of cars, bicycles, iPhones, iPods, Android tablets, e-book readers (eg Kindle), Windows 7 phones, Android smart phones, paper shredders, scanner/copiers, digital cameras, food processors, and air purifiers in addition to whether they voted in the 2012 presidential election (with one answer identifying non-American citizenship), the number of Economics experiments they have participated in, the number of Psychology experiments they have participated in, what their major is, what their weekly consumption is (in $), their age, and their gender.

19 We focus on the first 11 sessions as there could potentially be an interaction between the treatment and control in the latter sessions. The results below do not qualitatively change when conversation data from sessions 12-15 are included (available on request).

20 Our sample size was not limited by an ex ante power calculation but rather by how many tablet owners we were able to recruit to take our intake and subsequently to participate in the experiment.
3.2 Data Description

Table A-1 in the appendix summarizes participant characteristics. Buyers and sellers are similar demographically but differ, as expected, in their product ownership. These differences are most stark for the iPad and Microsoft Surface as – by design – no buyer owned either of these devices while all sellers owned at least one. Buyers are not entirely unfamiliar with these products, however, as 86% and 16%, respectively, indicate they have previously used one. Differences for other consumer electronics are less stark but point in the same direction. Sellers also spend more than buyers in a typical week.

Figure A-2 displays average initial valuations for the four products. The light gray region shows how much of the total valuation is for the product alone, and the dark gray is the increase in their valuation when the seller receives a commission. Over all of the goods for sale, initial valuations for the product alone is $279 (significantly different than zero, $p < 0.001$), which increases by $14 when a commission is added significantly larger than zero ($p = 0.06$). We find this altruism noteworthy: while it is of course common to find altruistic behavior in laboratory environments like dictator games, here we find it despite having framed the buyer-seller interaction in a more adversarial manner.

Buyers exhibit a high degree of impatience, valuing a $500 check nine to twelve months from now at $390. As one would expect, given that they have chosen not to purchase tablets, buyer valuations of the tablets are well below their $500 market prices at the time. On average, buyers are willing to pay $282 for the iPad and $236 for the Surface. Finally, buyers are quite risk averse, valuing a 50/50 chance of winning $1,000 at $211.

The recorded conversations themselves vary widely. In some, sellers quickly capitulate:

Seller: Yeah, if I were--yeah if I were in your position, I would probably think like only get like, 200, \$300 profit. That would be a very nice deal already.

Buyer: Yeah, definitely.

Seller: [long pause] Well...[long pause] I don’t know. I’m not a good sales person.

Buyer: It’s okay.

Seller: We can talk about whatever.

Buyer: So what year are you?

... As a result of the conversation, the buyer decreased her valuation of the post-dated check from $480 to $300, perhaps at the seller’s suggestion to target $200 profit; however, the buyer’s net valuation of the check with the seller’s commission increased from $300 to $500, suggesting that though her value of the product decreased her altruism towards the seller increased even more dramatically.
Other sellers seem far more comfortable, even to the point of monopolizing the conversation:

**Seller:** And uh I know judging by your lack of gambling experience and nervousness with that, I think you would probably agree with me there. But at this point, I would---I would say you might as well go for it. I would do it. Cause you made fifteen bucks here. So, you're positive. You're not going to lose any money. I'd say—I'd say it's not a safe risk, but fifty-fifty shot on winning a $1000 in addition to the big one five that you're pulling in right now as your [inaudible]. I don't ---I don’t--I don’t think I could turn that down. I think I would have to go for it.

**Buyer:** Alright. Well, I'll keep that in mind.

... Both the buyer's valuation of the lottery alone as well as her net valuation of the lottery plus the seller's commission were unchanged as a result of the conversation; in fact, she revealed no altruism before the conversation, valuing the seller's commission at $0 additional, and this was not affected by the seller-dominated conversation.

Some conversations focus on the attributes and value of the object for sale:

**Seller:** Haha, I'm pretty good. So, I'd like to talk to you a little bit more about the iPad as far as, do you own an iPad or anything? Er, do you own an iPhone?

**Buyer:** Uh, I have only, I’m using an apple right now.

**Seller:** Ok so, the iPad obviously, to me, I personally have an iPad, and I, I love it cause, it seems great for taking notes and everything like that and it's very handy cause it's much lighter than a laptop. So that's kinda nice.

**Buyer:** Uh, huh.

**Seller:** Uh, I would definitely recommend getting one. I know they are on the pricier side, but as far as I know they are much better iPad than like android tablets that have broken like within a week. So, I would definitely recommend it. As far as quality goes, they are definitely worth the price. Plus, when you buy it you automatically get apple care and everything like that. And you can take it in to any apple store, and if anything is wrong with it they'll pay, they'll fix it for free, you just have to pay for any parts. And normally there's not any parts that break. Um, it’s great for mail and everything like that. Yeah, and as far as facebook, any social media like that. And then games to kill time between class is kinda nice.
Focusing on the object seemed to work: the buyer increased her willingness to pay for the iPad by $20 (up from $360 to $380), but this strategy came at a cost: this increase was perfectly offset by a $20 decrease in her valuation of the seller’s $500 commission (down from $20 to $0), leaving a flat net valuation of the product and commission.

Other conversations are largely tangential to the purchasing decision. In some cases this is strategic; before the following conversation, for example, the seller had told us that her plan was to “chat with them and try to get them to connect with me so they will spend more:”

Seller: Oh yeah, sorry, I don’t know about it, bad reception in the building or something. Yeah, so the whole thing of it is that, willing to see how much you would or how much you would be willing to spend to buy a basically a future which is a $500 check for January 1st 2014.

Buyer: Yeah, yeah and I know a little bit about the time value of money, so.

Seller: No, I understand, so do I. Do you have a job?

Buyer: Yeah, I work part time for Ohio high school athletic association.

Seller: Oh, really thats cool. What do you do for them?

Buyer: Yeah, I work part time for Ohio high school athletic association.

Seller: So, what’s your, basically, your bottom line, like, how much would you be willing to pay for this?

Buyer: Well, how much does it, I guess, retail for?

Seller: It retails for 500.

Buyer: Okay, 500 dollars? Yeah. I’m thinking, probably then, my bottom would be [long pause] I mean I would be willing to pay 250 dollars for it.
Seller: Well, yeah, but, [inaudible]

Buyer: Ok, well, the highest I'd be willing to go, would probably be about 325.

Seller: Ok, well, see there's another thing. If we sell it for, we can't make anything if we sell it for less than 360.

... 

This seller continues through the rest of this conversation to assert (falsely) that “we can’t sell it for less than $360,” a price just above the buyer’s announced willingness to pay of $325. She ultimately persuades the buyer to agree to this price on the phone, and, after the phone call, the buyer indeed is willing to pay $400 for the iPad when the seller gets a commission (up from $340 before the phone call).

Clearly, reducing the nuance of these transcripts to a set of quantitative metrics is a difficult task. Seven researchers (of eight we invited) agreed to tackle it as part of our “prediction challenge,” and we coded the 35 questions they proposed (see Appendix B for a complete list).

Together the researcher’s questions provide a snapshot view into the “state-of-the-art” in research on persuasion: how social scientists currently understand its inner workings. Several features are noteworthy.

First, congruence across researchers is low. To the human eye they appear to propose very different measures, with only three items appearing more than once: the number of words in the conversation (3×), the number of times the seller uses the buyer’s name (2×), and how likeable the seller is (2×). Of course, variables could appear different to the eye and yet capture similar statistical information. We therefore calculate how well a randomly chosen variable proposed by one research can be predicted by the variables proposed by a second, randomly chosen researcher. This metric would be 100% if all researchers were in fact proposing the same underlying model, and 2% in expectation if their variables were completely unrelated. The value if this statistic in our data is 26%, implying at most modest agreement. This suggests to us a relatively young field of research that has yet to converge on a consensus view of how persuasion works.

Second, the researchers collectively appear to see roles for both self-interest and other-
regard. Of the thirty-five items, twenty one were coded by at least two of three research assistants as attempting to capture appeals to other-regard, and fifteen items were coded as intending to capture other-regard. Researcher 3, for example, focused on self-interest, proposing a series of questions about product attributes (e.g. “How many statements of the form ‘you can use it for X’ does the seller make?”) and their interaction with a measure of trustworthiness. Several other researchers, meanwhile, asked questions about the likeability (or dis-likeability) of the seller, or about features like language style matching which are thought to promote liking. Yet other questions, such as word count, are not obviously exclusive to either channel.

Third, there is variation along an axis from relatively subjective questions that rely heavily on coders’ intuition or “fuzzy logic,” to relatively objective ones that focus on machine-readable features of the transcripts. At one extreme, Researcher 1 asked coders to simply estimate themselves how much the conversation changed the buyers’ valuation. This approach to forecasting should be effective if lay coders have a better intuitive understanding of persuasion than social scientists can easily distil into a short set of predictors. At another extreme, Researcher 2 lists a number of objective features such as the number of words, the number of times the word “no” is spoken, and the number of times the participants speak at the same time (overlap). This approach puts greater confidence in our theoretical understanding of the mechanics of persuasion.

4 Results

We lay out methodology and then analyze the data in four stages. First, we examine whether communication changes valuations and whether these changes tend to benefit sellers, in spite of their known conflict of interest. Second, we assess the relative importance of appeals to self-interest versus other-regard. Third, we investigate the roles of sellers strategies, the relative importance of the buyer and seller, and of buyer-seller homophily in determining the extent and mechanics of persuasion. Finally, we analyze how persuasion and its pathways are predicted by product characteristics.

4.1 Methods

Recall that for the sake of analysis we define subjects’ valuations for objects as the highest value $X$ for which they indicated that they preferred to receive the object and $500 - X$ to receiving $500$. Because we did not impose monotonicity on these responses it was possible for subjects to buy at one price $X$ but refuse to buy at a lower price $X' < X$. In practice, of the 1,328 valuations we elicited, 1,306 (all but 2%) are monotonic in the expected direction.25

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23 Among the researchers, we had both economists and psychologists. The economists’ items did not differ meaningfully from the predictions of the psychologists in their focus on self-interest or other-regard, nor did either set outperform the other in predicting variance in outcomes for either pathway of persuasion (unreported).

24 The coding was not mutually exclusive, so two items were coded as intending to capture both pathways. No item was coded as intending to capture neither channel.

25 2% is a low number relative to other experiments. Holt and Laury (2002), for example, find non-monotonicity rates of 5.5% and 6.6% in their high- and low-stakes risk preference treatments.
We define the change in buyers’ valuation for a product under the commission condition as “persuasion”. Let $WTPC_{bsp}^1$ be buyer b’s WTP for product p under the commission condition after a conversation (or waiting period) with seller s, and $WTPC_{bsp}^0$ be her pre-conversation (or pre-waiting period) WTP. Let $WTPF_{bsp}^1$ be the analogous valuations in the fixed (non-commission) condition. Persuasion is then $\Delta WTPC_{bsp} = WTPC_{bsp}^1 - WTPC_{bsp}^0$. To see how this contains changes due both to self-interest and to other-regard, note that we can write

$$\Delta WTPC_{bsp} = \Delta WTPF_{bsp} + \Delta (WTPC_{bsp} - WTPF_{bsp})$$

Note that this is an accounting identity: it says that any change in overall valuation must (mechanically) equal the sum of changes in that valuation’s two components. It does not rule out the possibility of complex interactions between self-interest and other-regard. For example, manipulating other-regard might be an effective strategy largely because it builds trust, which then enables sellers to persuade the buyer to increase the self-interested component $WTPF_{bsp}$ of their valuations.

Where possible we operationalize hypothesis tests non-parametrically using clustered rank-sum tests. Where we need to condition on additional variables to improve precision or remove bias we use regression models that extend the the following baseline specification:

$$Y_{bsp}^t = \alpha + \beta Y_{bsp}^0 + \gamma_p + \epsilon_{bsp}$$

where $b$ indexes buyers, $s$ indexes sellers, $p$ indexes products, and $Y_{bsp}^t$ is a valuation for a product at time $t \in \{0, 1\}$. We include product fixed effects to account for systematic differences between products. The advantage of this approach relative to moving $Y_{bsp}^0$ to the left-hand side and treating the change $Y_{bsp}^1 - Y_{bsp}^0$ as the dependent variable is that it does not impose $\beta = 1$, which our data easily reject ($p < 0.001$).

4.2 The Mechanics of Persuasion

Result 1. Communication affects buyer valuations, with effects through both self-interest and other-regard.

Our first result is that communication altered buyers’ choices. Figure 1 shows the distributions of absolute changes in valuations with commission for both the communication and control treatments. Visually, the control distribution has much less spread in the change in valuations. Indeed, absolute changes of valuations for communication conditions is $\$49$ (for all 15 sessions), significantly and substantially larger compared to $\$24$ for the control (clustered rank-sum $p < 0.01$). Further, 56% of buyers change their valuation in the control group and 31% in the placebo. Thus, a sizeable minority of buyers are entirely unaffected by communication, but the majority are and some are affected substantially.

One potential concern is that the observed variation is driven by subjects who are perhaps confused, lazy, or not paying attention and providing answers that are somewhat random

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26We are underpowered to perform the analysis within the last three sessions alone.
that naturally, and unmeaningfully, vary from before to after the phone call. For example, if a buyer feels under-incentivized, they might randomize their responses rather than think carefully and provide honest answers. Or perhaps a buyer had an incorrect interpretation of the instructions that was corrected by talking to the seller, and with this new clarity of the game, she provided different valuations. That absolute changes are larger in the treatment compared to the placebo suggests communication is indeed the cause of the absolute changes rather than subjects answering randomly. Further, we can control for subject confusion in three ways. First, after each conversation buyers self-report how clear they thought the instructions were on a scale of one to three. Second, later subjects who read the transcriptions code for how confused each of the buyer and seller seemed about the rules of the game. Third, we can consider buyers who provide any non-monotonic response in the willingness to pay elicitation (e.g. willing to pay $100, but not willing to pay $80) as confused or not paying attention. Restricting our sample to only those who self-report no confusion, or to pairs whom coders view as understanding the rules, or to buyers who provide only monotonic responses in that matching does not qualitatively change any of the results above, and the absolute changes in the treatment are always significantly greater than those in the placebo (unreported).

The impacts of conversation work through changes in both self-interest and other-regard. Figure 2 illustrates this, plotting the histograms of absolute changes for the two components of persuasion. The mean absolute change in self-interest is $60 following a conversation compared to $24 in the placebo (clustered rank-sum $p < 0.01$), and the mean absolute change in other-regard is $50 if the buyer and the seller speak compared to $26 if they do not (clustered rank-
Figure 2: Changes in self-interest and other-regard driven by communication

Histogram of absolute change in self-interest, by treatment.

Histogram of absolute change in other-regard, by treatment.

sum $p < 0.01$). The evidence indicates that changes in valuations observed in our baseline is largely a result of communication rather than other artefacts of our design (e.g. time delay or asking twice).

To quantify the relative contributions of self-interest and other-regard to variation in persuasion, we ask what proportion of the overall variation is eliminated by conditioning on each component. Specifically, we estimate

$$1 - \frac{\mathbb{E}[V(\Delta WTPC|\Delta WTPF)]}{V(\Delta WTPC)}$$

(3)

to measure the proportion of total variance attributable solely to self-interest, and

$$1 - \frac{\mathbb{E}[V(\Delta WTPC|\Delta (WTPC - WTPF))]}{V(\Delta WTPC)}$$

(4)

to measure that attributable solely to other-regard. We find that conditioning on self-interest reduces variance more than conditioning on other-regard, but the difference is not dramatic: 16% of the variance can be explained by other-regard alone, and 29% can be explained by self-interest alone.\textsuperscript{27,28}

**Result 2.** Sellers benefit from communication, and on average this is driven by changes in buyers (perceived) self-interest.

Of course, communication could affect the distribution of buyers’ valuations without shifting its mean. Generically, if we think of communication as the revelation of a noisy signal that buyers use to update their beliefs, there is no reason to expect mean valuations to shift in ei-

\textsuperscript{27}Note that simpler variance decomposition procedures are hard to interpret meaningfully here given the non-independence of the two components.

\textsuperscript{28}Restricting the sample to subjects who self-report no confusion, or to pairs where both were coded as understanding the rules of the game, or to buyers who only gave monotonic responses does not alter these conditional variances substantially. In all cases, 15-19% of variance can be attributable to other-regard alone, and 28-36% can be explained by self-interest alone.
For communication to be valuable to sellers, on the other hand, we would need to see that at least on average valuations increase. This is what we find. In the full 15-session sample, the average change in buyers valuation with commissions following communication is $14 (all 15 sessions), significantly different from the mean change in placebo of $3 (clustered rank-sum $p = 0.01$) (See left-most bars in Figure 3). This overall difference reflects smaller differences in both self-interest and other-regard, both of which are directionally positive but statistically insignificant. The average change of perceived self-interest is $18 for the treatment and $11 for the placebo (clustered rank-sum $p = 0.2$) (See middle bars in Figure 3). The average change in other-regard was -$4 in the communication arm compared to -$8 in the placebo arm (clustered rank-sum $p = 0.2$) (See right-most bars in Figure 3). Though insignificant for the two channels, despite the short time frame and known misaligned incentives, sellers systematically increased how much buyers were willing to pay for the goods. Given this, we focus henceforth on changes under the communication treatment and (conservatively) on data from the first 11 sessions with only communication interactions.

Which component of persuasion explains sellers’ average gains? We find that mean effects are driven entirely by self-interest. Figure 3 shows that in the full 15-session sample they are driven primarily by self-interest. The same is true in the restricted sample: changes in self-interest net to a mean change of $20, significantly different from zero (clustered t-test $p < 0.01$), comprising 115% of total persuasion, while the changes in other-regard roughly cancel each other out, netting a mean change of -$2.64, insignificantly different than zero (clustered t-test $p = 0.7$). In sum, interacting with sellers affects both buyers self-interest and their other-regard, but only the former effect is mean positive in the full sample of participants.
Table 3: Sellers who target other-regard outperform

<table>
<thead>
<tr>
<th></th>
<th>Persuasion</th>
<th>Self-Interest</th>
<th>Other-regard</th>
<th>Persuasion</th>
<th>Self-Interest</th>
<th>Other-regard</th>
</tr>
</thead>
<tbody>
<tr>
<td>Targets Other-Regard</td>
<td>25.83</td>
<td>-1.38</td>
<td>27.21</td>
<td>34.46</td>
<td>-8.58</td>
<td>43.05</td>
</tr>
<tr>
<td></td>
<td>(13.21)*</td>
<td>(14.60)</td>
<td>(13.80)**</td>
<td>(20.55)*</td>
<td>(23.78)</td>
<td>(23.56)*</td>
</tr>
<tr>
<td>Targets Self-Interest</td>
<td>1.81</td>
<td>-5.78</td>
<td>7.59</td>
<td>9.42</td>
<td>-10.30</td>
<td>19.72</td>
</tr>
<tr>
<td></td>
<td>(17.46)</td>
<td>(20.01)</td>
<td>(17.46)</td>
<td>(15.00)</td>
<td>(21.48)</td>
<td>(20.99)</td>
</tr>
<tr>
<td>Has Any Strategy</td>
<td>11.73</td>
<td>14.10</td>
<td>-2.37</td>
<td>-0.26</td>
<td>1.14</td>
<td>-1.40</td>
</tr>
<tr>
<td></td>
<td>(19.58)</td>
<td>(20.28)</td>
<td>(15.22)</td>
<td>(20.43)</td>
<td>(27.20)</td>
<td>(21.41)</td>
</tr>
<tr>
<td>Seller FEs</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>N</td>
<td>220</td>
<td>220</td>
<td>220</td>
<td>220</td>
<td>220</td>
<td>220</td>
</tr>
<tr>
<td>R²</td>
<td>0.04</td>
<td>0.03</td>
<td>0.02</td>
<td>0.22</td>
<td>0.25</td>
<td>0.27</td>
</tr>
</tbody>
</table>

Each column is a separate regression. Each regression includes product fixed effects. “Targets other-regard”, “Targets self-interest”, and “Any strategy” are indicators equal to one if at least two of three coders agreed that the seller’s self-reported strategy involves manipulating other-regard, manipulating self-interest, or influencing the buyer in any way respectively. Standard errors clustered by buyer and by seller in parenthesis. Statistical significance is denoted as: *p < 0.10, **p < 0.05, ***p < 0.01.

Result 3. A minority of sellers plan to target other-regard and outperform those who do not.

One possible interpretation of the results thus far is that while other-regard does affect valuations, sellers have no ability (or intent) to strategically manipulate it. Instead, buyers’ other-regard adjust upwards or downwards to fixed characteristics of sellers which they discover through conversation.

To test this hypothesis we examine the strategies sellers report to us before each call and their relationship with persuasive performance. Some sellers do unambiguously plan to target the buyer’s self-interest. For example, sellers planned to “talk about how awesome the iPad is,” “tell them the specs and convince them the Surface is worth full retail price,” “give extra information,” and “sell benefits – USB, keyboard for notes, fast internet, touch screen capabilities, no stylus required to take notes.” Yet other sellers describe strategies that explicitly target other-regard. In this group are sellers who planned to “get the customer on my side,” to “build a rapport,” and to “get on buyer’s good side.” Some sellers even articulated specific other-regarding tactics from the literature, as for example the seller who planned to “remember the person’s name! People love when others remember their names.” Motivated by these qualitative comments, we coded seller strategies to identify those that target other-regard, those that target self-interest, and those that seem to contain any intention of influencing the buyer. Three undergraduate research assistants independently assessed whether each seller strategy referred to other-regard. The data they coded was stripped down to subject identifier and seller strategy. We code a seller strategy if at least two coders identified it as such: 19% of strategies were coded as targeting other-regard, 30% were coded for self-interest, and 55% were coded for having any strategy. Those who target seem to follow through on their plans: These sellers directionally use the buyer’s first name more frequently (n.s.), have more personal conversations (p = 0.01), and 157% more likely to have a tangential conversation, not focused on the product (p = 0.03) (All p-values from OLS regressions controlling for product fixed effects, clustering at seller level).

We analyze the efficacy of each strategy in Table 3. The third column shows that sellers
who use strategies that target other-regard are successful in their stated aim, raising buyers’ other-regard by significantly more than their peers. Interestingly, the second column shows that this does not come at a penalty with respect to their impact on buyers’ self-interest: the difference is small and insignificant. The first column shows the upshot of these results: sellers who target other-regard do significantly better than their peers overall. Moreover, the estimated effect size represents a sizeable 149% of average persuasion in our conversations. Apparently a sizeable minority of sellers do exert influence through other-regard, and these sellers also do better than average.29,30

This result is important in its own right as it suggests that other-regard may be important for understanding the careers of workers who go on to specialize in persuasion-heavy tasks such as sales or negotiation. Of course, it also raises the question whether these (or other) sellers would have performed better or worse had they used different strategies. While we cannot answer this question conclusively without experimentally manipulating strategies, we can provide some indication by exploiting the fact that some of our sellers changed their strategies between interactions. The last three columns of Table 3 report the results of specifications that include seller effects and thus estimate the within-seller effect of strategy choice. While the estimates are slightly less precise, they are qualitatively similar and if anything more pronounced than the pooled estimates. In particular, sellers have a larger effect on other-regard when using strategies that target other-regard and do better overall as a result. Note, moreover, that these specifications include product fixed effects, so that the result cannot be explained by variation in the underlying decision problem. It thus appears that other-regarding strategies are more successful than alternatives for the subset of sellers who use them.

While this result highlights one dimension along which seller performance varies, one might also want to understand what differentiates good from bad sellers more generally. The panel structure of our data allows us to perform this kind of unstructured analysis by estimating the distribution of seller (and buyer) fixed effects and then ask (a) how much better the best sellers are than their peers, and (b) what traits or behaviors correlated with being good at persuasion. The tradeoff is that by virtue of its flexibility this approach is also less powerful statistically, as it is an omnibus test and thus weak against specific alternative hypotheses. For example, if a small minority of sellers have a particular trait that makes them especially influential we will not be well-powered to detect this.

Result 4. Persuasion is significantly predicted by who is buying, but not by who is selling.

To our surprise, we find that seller effects are poor predictors of persuasion. Figure 4 represents the adjusted $R^2$s from a series of regressions, one per column, of persuasion

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29 This result holds when restricting the sample to buyers who self-report no confusion, or to pairs who are coded as both understanding the rules of the game, or to pairs whose buyer only submits monotonic willingnesses to pay (unreported).

30 We find mixed evidence for another seller tactic: Building credibility by saying something negative about the product (e.g. Dziuda (2011)). Using the coded conversation data, saying something negative about the products has a direct, significant negative effect on willingness to pay, but doing so also significantly and substantially increases the positive effect of saying good things about the product. However, we cannot substantiate these findings with the self-reported data from the buyers on what the seller said (both unreported). This could be because the data elicited from the buyer is unincentivized and potentially noisy, or it could be that the findings from the conversation data are a false positive.
Figure 4: Buyer identity predicts influence better than seller identity

Plots the adjusted R-squared for OLS regressions, predicting post-conversation commission valuation using initial valuation and covariate(s) listed. Researcher predictions for “All Predictions” and “Prediction Challenge Winner” interacted with product fixed effects.

\((WTPC_{bxp}^1 - WTPC_{bxp}^0)\) on a particular battery of fixed effects. Recall that the “adjustment” here lets us compare the explanatory power of a given set of variables to that of an equal number of randomly generated variables, and thus allows us to make meaningful comparisons across models with different numbers of predictors. We find that order and product effects are, unsurprisingly, unimportant for explaining the change in valuations. Seller effects are even less predictive, performing worse than an equally sized random set of covariates (adjusted \(R^2 = -0.07\)). They are all noticeably less predictive than buyer effects (adjusted \(R^2 = 0.07\)). In fact, only the prediction challenge winner was able to explain more variance than buyer fixed effects alone. Researchers as a group did not perform well, predicting a statistically insignificant XX% of the variation (F-test \(p = 0.15\)).

Figure A-1 in the data appendix show the distributions of seller (panel a) and buyer (panel b) fixed effects. Other than three outliers in amongst the sellers, the distribution appears flat relative to the buyer distribution, which is more steadily increasing from left to right. To make inferences of these effects, we estimate models that begin with the base specification in equation 2, augmenting it with various sets of fixed effects. For each regression we conduct an F-test of the restriction that the included fixed effects are jointly zero. While we cannot reject the null that seller effects are jointly zero (\(p = 0.87\)), we easily reject the null that buyer effects are zero (\(p < 0.01\)). Figure A-1 plots the distributions of these individual effects.

\(^{31}\)Standard asymptotic inference for this statistic requires that observations per participant approach infinity. In light of our short panel, we also calculated alternative \(p\)-values via randomization inference. Under the null hypothesis that changes in valuation are independent of the individuals involved, the distribution of our data is unaffected by permuting individual identifiers. We therefore draw 1,000 permutations of individual identifiers, re-estimate the \(F\)-statistic after each, and use the resulting distribution to estimate the sampling distribution.
for reference. The data do not make it clear whether the buyer’s differ in propensity to be persuaded to like a product or a person. Utilizing the same F-test to test for buyer effects on change in self-interest is insignificant \( (p = 0.19) \) as is the same test for other-regard \( (p = 0.53) \) (Seller F-test for self-interest \( p = 0.64 \), other-regard \( p = 0.38 \)). In short, the evidence for persuasive types is much weaker than the evidence for persuadable types.\(^{32}\)

**Result 5.** *Persuasion increases in buyer-seller homophily, and in particular gender match.*

We also explore whether the match between buyer and seller matters, conditional on their individual characteristics. We measure homophily, or buyer-seller similarity, in two ways. First, we use buyers’ unincentivized self-reports of their beliefs regarding how similar the seller was to themselves. The benefit of this measure is that it is comprehensive; the drawback is that buyers might feel a need to answer in a way that rationalized their valuation responses. The evidence generally points towards subjects reporting honestly: report are distributed fairly uniformly across the eleven bins from 1 (“not at all”) to 11 (“very much so”), with no score occurring less than 5% of the time or any score occurring more than 14% of the time. The average is 5.8, and the median is 6. Buyers who were gender-matched with their seller reported a significantly higher similarity score, 1.0 point higher (rank-sum \( p < 0.01 \)). Similarly, buyers matched on nationality with their seller reported they were more similar, 1.5 points higher (rank-sum \( p < 0.01 \)). This suggests the buyers were reporting thoughtfully. As a robustness check, however, we also test directly whether match on the observable characteristics of gender and nationality predict persuasion.

We find that homophily strongly predicts persuasion.\(^{33}\) Figure 5 illustrates this graphically for self-reported similarity, while Table 4 reports regression coefficients for all three measures. In Column 1 we estimate a $5.7 increase in persuasion for every one point increase in similarity score \( (p < 0.01) \). To provide some perspective, this implies that a one standard deviation increase in similarity would produce an $17.7 increase in persuasion, more than doubling average persuasion ($17.4). This result is even stronger when we focus on match on gender specifically: while neither buyer or seller gender predicts persuasion or changes in self-interest or other-regard, the interaction of their gender does. The fourth column shows that pairs that are matched on gender have increases of $22 more than the gender-mismatched pairs \( (p = 0.04) \).\(^{34}\) Only for nationality is this pattern weaker; persuasion is higher by $45.4 for same-nationality pairs but this is not statistically significant.\(^{35}\)

It seems reasonable that the homophily effect could be driven by either channel of persuasion of our original \( F \)-statistic (Lehmann and Romano, 2005). As before we cannot reject the null that valuation changes are independent of seller identifies \( (p = 0.87) \) but reject the null that they are independent of buyer identifies \( (p < 0.01) \).

\(^{32}\)Both results, the significance of buyer fixed effects and the insignificance of seller fixed effects, hold when restricting the sample to self-reportedly unconfused buyers, or to pairs who are coded to understand the experimental rules, or to pairs whose buyer only gives monotonic responses.

\(^{33}\)All of the results that follow do not qualitatively change when restricting our sample to any of the “unconfused” subject criteria used throughout the results section.

\(^{34}\)When we break down “match” further we estimate similar effects of male-male and female-female match.

\(^{35}\)One might wonder whether two students who have in common that they are not US citizens are well-described as “matched.” Anecdotally there is a very high probability that such students are both East Asian, and in any case such non-American to non-American matches are too rare (4%) to be driving our results.
Plots the mean change in persuasion by how similar the seller was to the buyer according to the buyer, where 11 is most similar. Vertical bars depict standard errors allowing for clustering by buyer and by seller.

Discussion: similarity could beget altruism, build trust, or given the seller insight into the buyer’s preferences, for example.\textsuperscript{36} We find evidence for both channels, but somewhat stronger evidence for self-interest. A one point increase in reported similarity increases changes in self-interest by $4.2$ ($p = 0.06$) and other-regard by $3.0$ ($p = 0.04$). With gender-matched pairs this gap is starker: gender-matched pairs see a large $29.4$ increase in valuations via self-interest ($p = 0.02$) but only an insignificant $3.3$ increase via other-regard ($p = 0.6$). Neither channel is significant for nationality match.

Result 6. **Conversation can increase buyer-seller homophily.**

One intriguing question is whether perceptions of homophily simply reflect exogenous, perhaps shared, characteristics of the participants, or are also in part shaped by the endogenous tactics of the seller (or buyer). Can the seller nurture feelings of homophily through conversation? A natural approach to this question is to use our coded conversation data and ask whether specific conversation characteristics predict perceived homophily. Indeed, when we do this we find that six of the thirty-four items we coded do significantly predict homophily, controlling for the sheer number of words spoken in the conversation (Table 5 Columns 1 and 2).\textsuperscript{37} Moreover, they are items that seem intuitively likely to be associated with similarity –

\textsuperscript{36}Though similarity and a measure of trust coded from the transcripts are significantly, positively correlated, we do not have evidence that similarity operates simply by engendering trust. Both measures predict valuation increases simultaneously (unreported).

\textsuperscript{37}We use total number of words as a control and do not consider it in the analysis. This decreases the total
Table 4: Buyer-Seller homophily predicts persuasion, change in self-interest, and change in other-regard

<table>
<thead>
<tr>
<th></th>
<th>Persuasion</th>
<th>Self-Interest</th>
<th>Other-Regard</th>
<th>Persuasion</th>
<th>Self-Interest</th>
<th>Other-Regard</th>
<th>Persuasion</th>
<th>Self-Interest</th>
<th>Other-Regard</th>
</tr>
</thead>
<tbody>
<tr>
<td>Similarity Score</td>
<td>5.71</td>
<td>4.22</td>
<td>3.00</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.53)**</td>
<td>(2.23)*</td>
<td>(1.41)**</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Gender Match</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Buyer Female</td>
<td>22.01</td>
<td>29.36</td>
<td>3.28</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(10.33)**</td>
<td>(12.41)**</td>
<td>(6.68)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>1.53</td>
<td>-5.16</td>
<td>-3.46</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(12.46)</td>
<td>(13.56)</td>
<td>(8.79)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Seller Female</td>
<td>-1.26</td>
<td>-2.54</td>
<td>-14.90</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(10.29)</td>
<td>(13.02)</td>
<td>(7.68)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Citizenship Match</td>
<td>5.39</td>
<td>0.44</td>
<td>13.45</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(7.12)</td>
<td>(20.42)</td>
<td>(17.09)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Buyer Non-American</td>
<td>15.69</td>
<td>1.13</td>
<td>19.79</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(9.46)*</td>
<td>(16.33)</td>
<td>(19.97)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Seller Non-American</td>
<td>-11.61</td>
<td>-30.16</td>
<td>16.91</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(7.42)</td>
<td>(21.93)</td>
<td>(15.16)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>210</td>
<td>210</td>
<td>210</td>
<td>220</td>
<td>220</td>
<td>220</td>
<td>216</td>
<td>216</td>
<td>216</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.74</td>
<td>0.61</td>
<td>0.08</td>
<td>0.74</td>
<td>0.62</td>
<td>0.08</td>
<td>0.74</td>
<td>0.63</td>
<td>0.08</td>
</tr>
</tbody>
</table>

Each column is a separate regression. Each regression includes product fixed effects. Standard errors clustered by buyer and by seller in parenthesis. Statistical significance is denoted as: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. 
Table 5: Conversation tactics can increase perceived homophily

<table>
<thead>
<tr>
<th>Value of indept. variable:</th>
<th>From same conversation as reported similarity</th>
<th>Avg from seller’s other three conversations</th>
<th>Avg from buyer’s other three conversations</th>
</tr>
</thead>
<tbody>
<tr>
<td>Times seller uses buyer’s first name</td>
<td>0.61 (0.29)**</td>
<td>0.77 (0.30)**</td>
<td>–</td>
</tr>
<tr>
<td>How personal does buyer get</td>
<td>0.60 (0.25)**</td>
<td>0.84 (0.25)**</td>
<td>–</td>
</tr>
<tr>
<td>How warm or likeable is pair</td>
<td>0.89 (0.39)**</td>
<td>1.11 (0.48)**</td>
<td>0.45 (0.45)</td>
</tr>
<tr>
<td>Fraction words spoken by buyer</td>
<td>2.66 (1.58)*</td>
<td>3.21 (1.84)*</td>
<td>-1.07 (3.05)</td>
</tr>
<tr>
<td>Number overlaps/interruptions</td>
<td>0.07 (0.25)***</td>
<td>0.06 (0.24)***</td>
<td>0.29***</td>
</tr>
<tr>
<td>First name, “you”, or “your”</td>
<td>0.89 (0.25)***</td>
<td>0.77 (0.33)***</td>
<td>0.95 (0.33)***</td>
</tr>
<tr>
<td>Gender-match control?</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Citizenship-match control?</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
</tr>
</tbody>
</table>

Each cell is a separate OLS regression. We regress buyer’s post-conversation, self-reported similarity with the seller after the conversation on one variable from transcription codings (noted in the far left column), and number of words in the conversation. Columns 1 and 2 report coefficients from regressing reported similarity on the transcription variable value from the same conversation; Columns 3 and 4 report coefficients from regressions using the mean value of that variable for the seller’s other three conversations; Columns 5 and 6 report coefficients from regressions using the mean value of the item for the buyer’s other three conversations. We include every coded transcript item (i) that significantly predicts reported similarity while controlling for number of words spoken and (ii) that is positively, significantly correlated with the mean value of that same item for the other three conversations (for the seller for columns 1-4 and for the buyer for the final two columns). Standard errors clustered by buyer and by seller in parenthesis. Statistical significance is denoted as: *p < 0.10, **p < 0.05, ***p < 0.01.

...for example, frequently using the buyer’s name, getting personal, and so on.

While this pattern is consistent with the idea that conversation can endogenously shape perceptions of homophily, there are two potential confounds. First, it might be that conversations are more likely to have these features precisely when the participants have similar exogenous traits. For example, a buyer-seller pair who both grew up in Southern Ohio might be more likely to get personal. To attempt to rule out such a confound, we examine whether a seller (or buyer’s) typical behavior in their other three conversations predicts perceived homophily in the fourth. We find that, for all six items, average characteristics of other conversations do predict homophily in the conversation at hand. Interestingly, this holds for sellers but not for buyers, suggesting that it is seller behavior that is generating these conversational features.\(^{38}\)

Second, it could be that some sellers are both more likely to have conversations with these number of items elicited from the persuasion researchers from thirty-five to thirty-four.\(^{38}\)

Likewise, a seller’s mean number of words spoken in three conversations is predictive of perceived similarity in the fourth, but this does not hold for buyers (unreported).
features and to have traits that lead buyers to perceive them as similar. For example, if men were over half the subject pool and also more likely to refer to each other by name, we might erroneously conclude that the use of first names increases perceptions of homophily when in fact gender does so. We cannot control for all such potentially confounding traits, of course, but do show in Columns 2, 4, and 6 of Table 5 that controlling for the major covariates we do observe (gender and nationality) has little impact on the results. While not fully dispositive, this strongly suggests that it is endogenous tactics and not exogenous traits that are driving the result.

**Result 7.** The nature of the good being sold affects both the extent and the mechanics of persuasion.

Our last set of results examine the extent to which context – and in particular the nature of the good being sold – affect the degree and the pathway of persuasion.\(^39\) As a reminder, we chose objects for sale in order to create as much variation as possible in the degree of information asymmetry between buyers and sellers. We now compare outcomes for the two tablet computers of which every seller owned at least one and buyers owned neither, with outcomes for the lottery and time preference questions ("abstract goods").\(^40,41\)

We find that sellers were noticeably more successful overall at persuading buyers to pay more for tablets. Figure 6 breaks down average valuation changes, overall and for the two pathways, for the tablets and the abstract objects. The left-most bars indicate that sellers are more successful overall at persuading buyers with the tablets. Valuations increase by $9.6 for the abstract goods as opposed $25.1 for the tablets (rank-sum, clustered at buyer and seller level, \(p = 0.03\)). This is consistent with the view that information asymmetry creates greater scope for persuasion.

The middle bars in Figure 6 help explain why: Changes in self-interest for the abstract goods is $6 versus $34 for the tablet computers (rank-sum, clustered at buyer and seller level, \(p = 0.05\)). Directionally, this came at a cost as other-regard decreases by $8.9 for the tablets while it increases by $3.6 for the abstract goods, but this difference is not statistically significant (rank-sum, clustered at buyer and seller level, \(p = 0.22\)). We interpret these results to mean that the relative importance of other-regard and self-interest in persuasion is conditional on the nature of the decision facing the buyer. They also suggest that self-interest plays a larger role when information is more asymmetric.\(^42\)

One caveat to these results is that we cannot reject the hypothesis that the same patterns

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\(^39\)Again, the results in this section hold when removing “confused” subjects (unreported).

\(^40\)We focus on the delineation of tablets versus abstract goods rather than individual products because the recruiting procedures naturally create this bifurcation (sellers own at least one tablet while buyers own neither). Further, an F-test rejects the hypothesis that individual product fixed effects explain variance for persuasion (\(p = 0.11\)), increases in self-interest (\(p = 0.22\)), or gains in other-regard (\(p = 0.49\)).

\(^41\)We find no direct evidence that information asymmetry drives persuasion in our context. Conversations where the seller independently owns the good have directionally lowered valuations compared to other pairings with a tablet as the object. Further, how familiar the buyer reports being with the tablet before the session has a very small, positive, insignificant effect (both unreported).

\(^42\)We do not find evidence that sellers who vary their strategy are more successful: Variance in composite measures of conversational tactics of either increasing perceived self-interest or other-regard is not predictive of outcomes, either overall persuasion, or changes in either channel.
5 Conclusion

In this paper we define and apply an experimental framework for studying the mechanics of persuasion: in particular, whether and when persuasion works through appeals to self-interest or to other-regard. We find that both mechanisms matter to a degree, with substantial variation depending on context and individuals. The “average” seller in our experiment benefits from communication primarily because communication convinces buyers that it is in their own best interest to pay more for objects. At the same time, a minority of sellers make a strategic choice to focus on getting buyers to like them, and these sellers outperform their peers significantly both at cultivating other-regard and at persuasion overall. Sellers gains are driven more by buyer self-interest when they discuss tangible products about which sellers were better-informed, but more by other-regard in settings of symmetric information. Finally (and surprisingly), variation in persuasion is driven less by who is selling than by who is buying.

Our experimental framework can be adapted to study a wide range of additional questions about persuasion. We close by highlighting three. First, future work could compare the effects on buyers of conversations with expert as opposed to amateur persuaders. Many roles in an
economy require specialized persuasive skills – salespeople, CEOs politicians, and so forth – and it would be valuable to understand both what it is that sets such expert persuaders apart and where these skills come from – whether innate or acquired. This seems especially interesting given that we do not find significant variation in seller performance in our sample of novices.

Second, future work could compare the effectiveness of persuasion with and without face-to-face interaction. Firms spend huge sums of money flying their employees (and especially their salespeople) around the world in the belief that face-to-face interactions yield higher returns than telephone or even videoconferenced interactions, yet there is little rigorous evidence on those returns. Such experiments could also shed light on the evolution of urban form; as scholars have noted, rapid ongoing urbanization seems hard to reconcile with dramatic improvements in communication technology unless face-to-face interactions are particularly valuable.

Third, future work could compare persuasion about private goods to persuasion about public ones. Consider for example persuading a neighbor to vote in favor of environmental reforms: this problem has the interesting features that (i) it may never be in the neighbors narrow self-interest to do so, and (ii) voting for the reform likely benefits the persuader if and only if it also benefits the neighbor, creating a direct link between self-interest and other-regard that is absent in the private goods case.

Finally, our results suggest a new perspective on bargaining. We view our design as a natural complement to standard bargaining designs: while these typically fix an objective value for the object being transacted and let participants negotiate price, we fix price (stochastically) and let participants discuss the subjective value of the object. Of course, many real-world situations combine elements of both designs. It would therefore be interesting to see how canonical results from the bargaining literature (e.g. the importance of fairness) interact with our results in a hybrid setting.
References


A Data Appendix

Table A-1: Participant characteristics

<table>
<thead>
<tr>
<th>Variable</th>
<th>Buyers</th>
<th></th>
<th>Sellers</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>SD</td>
<td>Mean</td>
<td>SD</td>
</tr>
<tr>
<td>Age</td>
<td>21.3</td>
<td>1.9</td>
<td>21.2</td>
<td>1.8</td>
</tr>
<tr>
<td>Gender = Male</td>
<td>0.55</td>
<td>0.50</td>
<td>0.55</td>
<td>0.50</td>
</tr>
<tr>
<td>Voted</td>
<td>0.57</td>
<td>0.50</td>
<td>0.67</td>
<td>0.47</td>
</tr>
<tr>
<td>Owns an iPad</td>
<td>0.00</td>
<td>0.00</td>
<td>0.74</td>
<td>0.44</td>
</tr>
<tr>
<td>Used an iPad</td>
<td>0.86</td>
<td>0.35</td>
<td>0.97</td>
<td>0.17</td>
</tr>
<tr>
<td>Owns a Windows Tablet</td>
<td>0.00</td>
<td>0.00</td>
<td>0.20</td>
<td>0.43</td>
</tr>
<tr>
<td>Used a Windows Tablet</td>
<td>0.16</td>
<td>0.37</td>
<td>0.53</td>
<td>0.50</td>
</tr>
<tr>
<td>Owns an iPhone</td>
<td>0.49</td>
<td>0.50</td>
<td>0.71</td>
<td>0.45</td>
</tr>
<tr>
<td>Owns an iPod</td>
<td>0.72</td>
<td>0.47</td>
<td>0.76</td>
<td>0.43</td>
</tr>
<tr>
<td>Owns an Android Tablet</td>
<td>0.02</td>
<td>0.15</td>
<td>0.11</td>
<td>0.31</td>
</tr>
</tbody>
</table>

Reports means and standard variations of age (top row) and indicator variables for whether a given statement is true (all other rows). By sample construction, no buyers owned an iPad or a Windows tablet.

Table A-2: Participation by Session

<table>
<thead>
<tr>
<th>Session</th>
<th>Date</th>
<th>Buyers</th>
<th>Sellers</th>
<th>Conversations</th>
<th>Placebos</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>January 15</td>
<td>4</td>
<td>4</td>
<td>16</td>
<td>0</td>
</tr>
<tr>
<td>2</td>
<td>January 22</td>
<td>6</td>
<td>6</td>
<td>24</td>
<td>0</td>
</tr>
<tr>
<td>3</td>
<td>January 24</td>
<td>5</td>
<td>5</td>
<td>20</td>
<td>0</td>
</tr>
<tr>
<td>4</td>
<td>January 29</td>
<td>5</td>
<td>5</td>
<td>20</td>
<td>0</td>
</tr>
<tr>
<td>5</td>
<td>January 30</td>
<td>7</td>
<td>7</td>
<td>28</td>
<td>0</td>
</tr>
<tr>
<td>6</td>
<td>February 6</td>
<td>4</td>
<td>4</td>
<td>16</td>
<td>0</td>
</tr>
<tr>
<td>7</td>
<td>February 7</td>
<td>5</td>
<td>5</td>
<td>20</td>
<td>0</td>
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<tr>
<td>8</td>
<td>February 14</td>
<td>5</td>
<td>5</td>
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</tr>
<tr>
<td>9</td>
<td>March 6</td>
<td>5</td>
<td>5</td>
<td>20</td>
<td>0</td>
</tr>
<tr>
<td>10</td>
<td>March 7</td>
<td>5</td>
<td>5</td>
<td>20</td>
<td>0</td>
</tr>
<tr>
<td>11</td>
<td>March 28</td>
<td>4</td>
<td>4</td>
<td>16</td>
<td>0</td>
</tr>
<tr>
<td>12</td>
<td>April 4</td>
<td>10</td>
<td>6</td>
<td>20</td>
<td>20</td>
</tr>
<tr>
<td>13</td>
<td>April 10</td>
<td>7</td>
<td>3</td>
<td>8</td>
<td>20</td>
</tr>
<tr>
<td>14</td>
<td>April 17</td>
<td>6</td>
<td>2</td>
<td>4</td>
<td>20</td>
</tr>
<tr>
<td>15</td>
<td>April 18</td>
<td>5</td>
<td>4</td>
<td>12</td>
<td>8</td>
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<tr>
<td>Totals</td>
<td></td>
<td>83</td>
<td>70</td>
<td>264</td>
<td>68</td>
</tr>
</tbody>
</table>
Figure A-1: Distribution of participant fixed effects

(a) Sellers

(b) Buyers

Plots the sorted mean change in persuasion individually for each seller and each buyer, respectively.
Figure A-2: Initial valuations show moderate altruism

Plots buyers’ mean initial valuations, in dollars, for each of the four products studied. Valuations have been decomposed into the mean willingness to pay absent any commission for the seller (“self-regarding”) and the mean incremental willingness to pay when commissions are added (“other-regarding”).
B Researcher Predictions for Correlates of Persuasion

Here, we present the predictions from the persuasion researchers who participated in our Prediction Challenge. The goal of the contest was to choose five features of the conversations that would best predict variance in net influence. We present them in no particular order.

Researcher 1
1. On a scale of 1 to 5, how much do you think this conversation increased the buyer’s valuation for the product? (1=not at all, 5=a lot)
2. On a scale of 1 to 5, how much do you trust this seller? (1=not at all, 5=a lot)
3. On a scale of 1 to 5, how much do you like this seller? (1=not at all, 5=a lot)
4. Is the buyer a man?
5. Is the buyer a man and the seller a woman?

Researcher 2
1. The length (in words) of the conversation.
2. The number of times that the seller says “I” (first person singular).
3. The number of times the seller calls the buyer by first name.
4. The total number of times “no” is used.
5. The number of “overlaps/interruptions” (but people talking at the same time).

Researcher 3
1. How many statements of the form “I’ve used it and really like X” does seller make?
2. How many statements of the form “someone I know has used it and really liked X” does the seller make?
3. How many statements of the form “you can use it for X” does the seller make?
4. How many statements of the form “I’m not telling you this because of my incentives” does the seller make?
5. The interaction (4) x [(1) + (2) + (3)]

Researcher 4
1. Was the conversation about pros and cons of the object/the decision of buying, or was it was tangential?
2. Did the seller appear impartial by giving also cons of buying the object?
3. The duration (in words) of the conversation (though not certain in which direction this will go)
4. The fraction (of words) of the conversation during the buyer spoke.
5. How many questions RELATED to the buying decision (not questions about jobs or weather) did the seller ask?
Researcher 5

1. Look for the mere number of words used for buyer and seller
2. How many times does the seller say the buyer’s name, the word “you”, or the word “your”?
3. On a scale from 1 to 5, how many and how strong do you think the sellers arguments are? (1 = no arguments or only very weak arguments, 5 = many strong arguments)
4. On a scale from 1 to 5, how warm or likeable do you find the participants (1 = not likeable at all, 5 = very likeable)
5. On a scale from 1 to 5, how competent or expert do you find the participants (1 = not at all competent or expert., 5 = very competent or expert)

Researcher 6

1. How many times did the seller lie about or exaggerate the product attributes?
2. Does the seller making a ‘panhandler’ kind of plea for help?
3. Does the seller describe details about the products (for the two products)?
4. Does the seller provide the actuarial value of the items (for the check and lottery)?
5. Does the seller explain the incentive scheme to the buyer?

Researcher 7

1. Word count (the more words in the conversation, but especially the buyer)
2. Language style matching plus positive emotion words (LSM – this uses LIWC and calculates the relative use of the following function words: personal pronouns, impersonal pronouns, articles, prepositions, auxiliary verbs, negations, adverbs, conjunctions). In other words, high LSM and high positive emotion words will result in the highest amount.
3. What did the buyer say they were willing to pay at the beginning of the conversation – or if (s)he did not say, what do you think (s)he was willing to pay at the beginning of the conversation?
4. On a scale from 1 to 5, how personal does the buyer get? (Personal is defined by the buyer saying something about his background, emotions, or personal concerns. Also, more likely to say his/her name.) (1 = not at all personal, 5 = very personal)
5. One a scale from 1 to 5, how would you rate the buyer’s initial desire or interest in the product?

Items added by authors

1. Do the buyer and seller make a side deal (agreed to payments outside of the experiment) and/or agree to meet afterwards?
2. On a scale from 1 to 3, how well does the buyer appear to understand the rules of the game? (1 = not at all, 2 = somewhat, 3 = very well)
3. On a scale from 1 to 3, how well does the seller appear to understand the rules of the game? (1 = not at all, 2 = somewhat, 3 = very well)