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Journal of Conflict Resolution published online 23 August 2011

DOI: 10.1177/0022002711414372

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Journal of Conflict Resolution
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DOI: 10.1177/0022002711414372
<http://jcr.sagepub.com>


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Abstract

What effect does cheap talk have on behavior in an entry-deterrence game? We shed light on this question using incentivized laboratory experiments of the strategic interaction between defenders and potential entrants. Our results suggest that cheap talk can have a substantial impact on the behavior of both the target and the speaker. By sending costless threats to potential entrants, defenders are able to deter opponents in early periods of play. Moreover, after issuing threats, defenders become more eager to fight. We offer a number of different explanations for this behavior. These results bring fresh evidence about the potential importance of costless verbal communication to the field of international relations.

Keywords

cheap talk, deterrence, experiment, reputation

Most bargaining models assume that verbal threats or promises that inflict no costs on the sender will have little or no influence on those receiving the message.¹ A state leader can claim that he or she will “fight hard” when elected, or “cut taxes once in office,” or “come to the aid of an ally” that is attacked, but in the absence of any punishment for failing to follow through, these statements are generally viewed as empty and inconsequential.²

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In reality, however, leaders engage in what could be construed as cheap talk all the time. President Kennedy promised Soviet Premier Nikita Khrushchev that he would remove American nuclear missiles from Turkey if the Soviets first removed their missiles from Cuba. France and Great Britain promised to help Poland and Czechoslovakia should the Germans attack. Secretary of State Dean Acheson claimed that the United States would not protect Korea in the 1950s. And President Clinton threatened to bomb North Korea if they continued to acquire nuclear weapon capabilities. Each of these leaders engaged in cheap talk despite the fact that there was little reason to believe that any of these pronouncements were true.

Cheap talk also appears, at times, to work. Kennedy's promise to withdraw US missiles in Turkey is widely believed to have convinced Khrushchev to withdraw his missiles from Cuba.³ Clinton's threat to bomb North Korea (together with promises of energy assistance) did appear to convince the North Koreans to stop their nuclear development program, at least temporarily.⁴ And in their study on bargaining, Farrell and Gibbons found that "[t]alk is ubiquitous and is often listened to, even where no real penalty attaches to lying, and where claims do not directly affect payoffs" (1989, 222). Verbal claims about one's intentions may be costless, but leaders use them, and they appear to influence behavior in ways we do not fully understand.⁵

This article has two goals. The first is to determine whether costless communication has any effect on behavior when used in an entry-deterrence game. If a defender is allowed to issue a verbal threat that is both costless and private, does this change the entrant's and the defender's behavior in any way? We use an incentivized laboratory experiment and find that cheap talk can influence behavior despite our subjects having opposing preferences. The second is to theorize about why such communication might be significant even if everyone knows it is costless. Here we consider the role that experience and common knowledge may play in shaping strategic decisions.

Empirically, there are at least three ways to determine whether leaders rely on costless signaling, and if they do, whether these messages are persuasive. The first is to collect and analyze observational data. One could, for example, study all verbal communication that took place between the United States and the Soviet Union over nuclear weapons during the cold war to see whether these messages influenced either party in any way. The problem with such a study is that it suffers from two difficult-to-resolve methodological problems. The first is that cheap-talk games tend to be sensitive to initial beliefs and controlling for these beliefs is hard to do in large-*N* studies. Khrushchev, for example, may have already developed a reputation for toughness when he began communicating with Kennedy. The second is that cheap talk and costly signaling often cooccur, making it difficult to isolate and identify the independent effects of the very cheapest form of communication.⁶ Observational data, therefore, can be unreliable.

A second approach would rely on qualitative case studies to trace when and how leaders engage in cheap talk and its potential effects on behavior. Studies do exist that look at relations between countries and include cheap talk as indicators, but these studies do not attempt to isolate the effect of these statements on behavior

(e.g., see Foster 2006). Even if a study did situate itself in the bargaining literature, this approach would also have disadvantages. A small number of case studies can confirm whether individuals in those cases communicated with each other in a costless way, and if those messages had any effect, but they could not confirm whether this behavior was more widespread.

A third approach—laboratory experiments—circumvents these problems. In a laboratory experiment the researcher can isolate costless signals and their consequences while controlling for confounding factors. In this way, the experiment can reveal whether threats and promises are actually used, whether they directly changed behavior, and, if they did change behavior, under what conditions. Laboratory experiments, however, are not without their own drawbacks. Since subjects tend to be undergraduate students as opposed to state leaders, the findings cannot be generalized to field settings. It is possible that state leaders use verbal communication differently from undergraduates even when placed in a similar context.⁷ Still, an empirical test of cheap talk in the laboratory will reveal whether the hypothesized relationships emerge under ideal conditions and help to advance the debate beyond the question of whether cheap talk matters, to a more constructive discussion of when, how, and why it might be used.

In what follows, we set up a simple experiment to determine whether individuals engage in cheap talk and, if they do, whether it changes behavior. The experiment compares how individuals conduct themselves in an entry-deterrence game with one-sided incomplete information when cheap talk is not allowed to one when it is. What we find is surprising. Verbal threats had significant effects on the behavior of both the sender and the target in early stages of a repeated game. Even though threats were completely costless, targets were more likely to back down if they received a threat, and senders were more likely to fight if they issued a threat. In short, when individuals engaged in cheap talk in the laboratory—and they almost always did when given the chance—it changed the behavior of everyone involved. This suggests that even the most costless form of verbal communication can be influential, at least in certain circumstances.

The remainder of the article is broken down into four sections. The first section reviews current theories and findings on cheap talk in both the international relations (IR) and economics literature. The second introduces our experimental design, presents some theoretical predictions, and explains our empirical strategy. The third section reveals the results of these experiments and offers an explanation for why cheap talk is powerful even though most bargaining models would not expect it to be. Here we highlight the potentially important roles that inexperienced and incompetent individuals can play in changing the incentives of the game. In the final section, we discuss the contributions this study makes to IR, as well as avenues for future research.

What We Know Theoretically and Empirically about Cheap Talk in IR

The IR literature has been divided between those who argue that costless verbal communication can be informative and those who argue that it provides little or

no information at all. In one camp are the constructivists who assert that things like persuasion, argumentation, and rhetoric can play a critical role in politics and diplomacy. According to Finnemore and Sikkink, "IR scholars have tended to treat speech either as 'cheap talk,' to be ignored, or as bargaining, to be folded into strategic interaction. However, speech can also persuade; it can change people's minds about what goals are valuable and about the roles they play (or should play) in social life" (2001, 402)⁸ Significant anecdotal evidence seems to support this view. Throughout history, state leaders have engaged in all sorts of verbal and symbolic communications even if, on the surface, it appears shallow.

The second camp is composed mostly of formal models where costless communication or "cheap talk" should not matter.⁹ If two states have completely opposing preferences and incomplete information about each other's payoffs, costless messages provide no additional information about what the sender is likely to do. This is because all players have incentives to make similar claims whether they are true or not. It is only when real costs are attached to the messages that sincere senders can be distinguished from those who are just bluffing. This perspective drew on an earlier economics literature which also showed that cheap talk can be effective and could influence strategy choices when players have some commonality in preferences (Morrow 1994; V. Crawford and Sobel 1982).¹⁰

Existing empirical studies suggest some support for thinking cheap talk will not have its desired effect when there are conflicting preferences. In a study of militarized disputes between 1816 and 1993, Sartori (2005) found that verbal communication in the form of diplomacy could change an adversary's mind about its desire to fight, but only if the sender had already invested heavily in the credibility of these messages through the costly use of force. Thyne (2006) found that cheap signals could actually have negative consequences. In a study of civil wars, he found that negotiations were more likely to fail if one of the disputants used hostile costless signals.¹¹

Consistent with earlier theoretical results (V. Crawford and Sobel 1982), the main evidence in favor of cheap talk being influential comes from laboratory experiments where the preferences of the sender and the target are aligned. Cooper, DeJong, and Forsythe (1989) and J. Crawford (1998) found that in a battle of the sexes game, costless communication allowed players to coordinate on an outcome, making successful cooperation possible.¹² Similarly, in a public goods game with incomplete information about private endowments, Palfrey and Rosenthal (1991) found that subjects regularly conditioned their behavior on the cheap-talk message they received but did not obtain more efficient outcomes as a result.¹³ Majeski and Fricks (1995) found that cooperation was more likely in a prisoner's dilemma game if cheap talk was allowed. In all these cases, cheap talk worked to some extent, but only because each side already had an interest in cooperating.

The problem is that many interactions in the world of IR occur under conditions where players have conflicting preferences. World leaders often benefit from deceiving and misleading each other and frequently do not want to cooperate. The few experiments that have been run to assess cheap talk under conditions of

conflict have found limited or no effect. Forsyth and colleagues found that in a bargaining game with one-sided incomplete information individuals did not behave much differently if they were allowed to communicate cheaply versus if they were not allowed to communicate at all (Forsythe, Kennan, and Sopher 1991).¹⁴ Similarly, Croson and colleagues found that in an ultimatum game with incomplete information about outside options, cheap talk affected behavior but only temporarily. Subjects could increase their short-term bargaining outcomes using cheap talk but would be punished in the long term if they chose to lie (Croson, Boles, and Murnighan 2003).¹⁵ Finally, in an n -person market entry game, Sundali and Seale (2004) found that entrants exaggerated their intention to enter when given the chance, but that this did not influence how others played the game. The balance of experimental results, therefore, suggests that cheap talk will have very little influence on behavior in more conflict-prone settings.

In what follows, we investigate the effects of cheap talk in an experiment that more closely models a wider range of IR interactions using a repeated entry-deterrence game.¹⁶ We believe that by examining the role of cheap talk in a common strategic situation in IR, we can begin to understand the puzzle state leaders present for our theoretical models. If cheap talk really serves little positive purpose in most conflict situations, why do world leaders continue to use it?

Cheap Talk and Entry Deterrence

In this section, we introduce a game in IR that allows us to study the effects of cheap talk in situations where players have strong incentives to deceive each other, especially in early periods of the game. We have chosen an entry-deterrence game for three reasons. First, it is relatively common in international affairs for governments to use verbal threats as part of an attempt to deter potential challengers. China's verbal pronouncements against any move by Taiwan to declare independence are made, in part, to deter other ethnic groups such as the Uyghurs or Tibetans from seeking independence. Second, an entry-deterrence game has a simple sequential structure which allows us to observe when defenders choose to issue threats, and how different entrants react to any threat they may receive. Finally, the experimental economics literature on reputation building is surprisingly quiet on the role of cheap talk in this type of repeated bargaining environment. Thus, there are good substantive and methodological reasons for choosing this particular game.

We begin by presenting the simple game of one-sided incomplete information. We then characterize the sequential equilibrium of a repeated version of the game with no communication (and hence no cheap talk) and then consider how we would expect cheap talk to influence behavior.

The Structure of the Game

The game is straightforward. In it, a defender faces a series of potential entrants who must decide whether to challenge the defender or stay quiet. The defender, in turn,

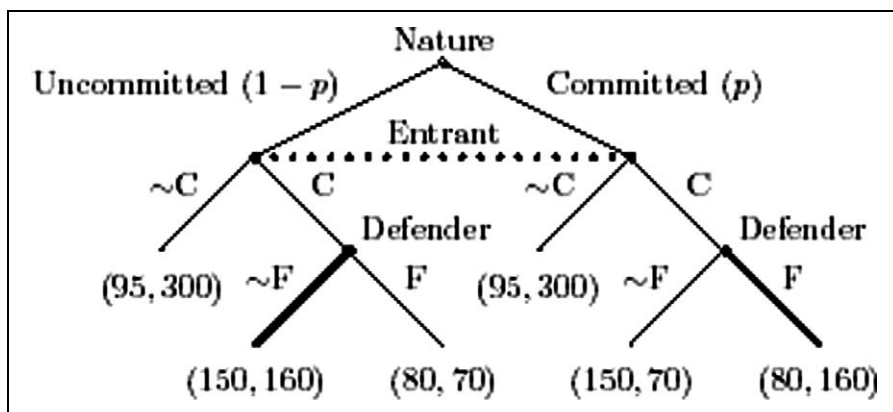


Figure 1. Structure of the entry deterrence stage game

must decide whether to fight entry or allow the challenger to enter. Figure 1 reveals the structure of the stage game as well as the payoffs each of the players knows it will receive for the different outcomes.¹⁷

The game begins with nature randomly choosing whether the defender is committed (strong) or uncommitted (weak) to fighting a challenge with probability p . This introduces the element of uncertainty necessary for reputation building to occur. If the defender is committed, it will always prefer to fight entry rather than acquiesce since this will always deliver better payoffs (see Figure 1). If it is uncommitted, it would prefer to acquiesce rather than pay the costs of war.¹⁸ Once nature has chosen the defender's type, the entrant must decide whether to challenge (C) or not challenge ($\sim C$).

The key to the game is that the entrant does not know whether it is facing a defender who is committed (in which case the entrant would prefer not to challenge) or a defender who is uncommitted (in which case the entrant would prefer to challenge). If the entrant decides to challenge, the defender then chooses whether to fight (F) or concede ($\sim F$).

The game is then repeated with the defender's type remaining fixed. Once the defender makes his or her choice, a second entrant then chooses whether to challenge, after which the defender again decides whether to fight or accommodate that entrant. As each entrant plays, they obtain information about how previous entrants played against the defender they are currently matched with, and how the defender played *if* the previous entrant decided to challenge. Thus, they are able to update their beliefs about what type of defender they are likely to face. The game continues until the defender has been pitted against a commonly known number of entrants.¹⁹ How the defender behaves toward an early entrant, therefore, can be interpreted by later entrants as important information about how the defender is likely to behave toward them.

For our analysis of the role of cheap talk, we had our subjects play the game in two different ways. In one version, they engage in the game exactly as we described it without any communication between the defender and entrants. In the second version, defenders experienced with the first version are given the opportunity to issue a costless threat. Our test of cheap talk, therefore, entailed a simple addition to the game. Each defender sent a signal to each potential entrant. They could either issue a message that said they would fight if faced with entry or they could send a message that said that they would not fight.²⁰

A critical feature of this communication is that it is private. No other player besides the current entrant was able to see the message. This allowed us to observe the messages in their most costless form. Since none of the messages could be observed by any other player—a fact that was made clear both in our instructional period and during the experiment—there were no incentives for the sender to follow through with threats for reputational reasons. This is especially true since defenders knew that they would interact with each entrant only once. Thus, senders could not build a reputation for following through with threats (or not following through with such threats) and gained no additional deterrent value by publicly honoring them. This created a situation where talk was truly “cheap” and no costs could be imposed on the sender for not following through. By structuring communication this way, we are able to isolate most cleanly the influence of cheap talk. However, because the use of cheap talk in the real world can be public as well, future research should consider differences in behavior depending on the private or public nature of the messages.

Theoretical Predictions about Cheap Talk

The standard entry-deterrence model—as outlined by Kreps and Wilson (1982) and Milgrom and Roberts (1982)—makes three predictions about how the defender should play if no cheap talk is allowed in the repeated game setting we study.²¹ First, strong-type defenders should always fight no matter what period they are in. Second, weak defenders should play a strategy that depends on how many entrants remain. Weak defenders know that if they acquiesce to the first challenger, this will immediately reveal their type and this information will trigger a wave of additional challenges. Weak defenders, therefore, have the incentive to bluff in early periods—fighting early entrants—and then acquiescing with increasing probability as the number of remaining entrants declines. The third prediction is that entrants should base their strategy on information they can glean about the type of defender they are facing and the incentives this defender has to build a tough reputation over time. If a defender backed down in an earlier period, entrants know they are facing a weak or uncommitted opponent, and they should always enter. If the defender never backed down, entrants should never enter in the early periods, since both weak and strong defenders will fight in early periods. They should then be more likely to enter during the middle and latter periods (knowing that weak defenders will be increasingly likely to back down over time).

Importantly, allowing cheap talk alters none of these predictions because of the sufficiently opposing preferences of entrants and defenders (V. Crawford and Sobel 1982). The sequential equilibrium of the formal model indicates that the defender and the entrants should not change their behavior if cheap talk is possible. As shown in the article's Supplemental Appendix, only a pooling equilibrium exists where all defenders should always issue a threat; no separating or semiseparating equilibria exist because cheap talk does not alter the defender's or the entrant's payoffs in any way.²² Furthermore, cheap talk provides no new information about the defender's type because, in equilibrium, all defenders should pool on sending a threat.

The model, therefore, makes two predictions about the effect of cheap talk:

Hypothesis 1: Defenders who issue a threat to fight will not deter any more entrants than those who do not issue a threat.

Hypothesis 2: Defenders who threaten to fight should not be more likely to fight than those who did not.

Experimental Design

Again, our experimental design had two separate parts that all subjects experienced.²³ The first did not allow communication, the second did. The two parts were identical in every other way. At the beginning of the experiment, subjects were randomly assigned to two separate positions, entrants and defenders, which were referred to simply as first movers and second movers. These neutral terms were used in order to avoid leading the subjects in any way. Defenders were then assigned a type, either weak or strong, which were called "type 1" or "type 2." Strong types preferred to fight if challenged, whereas weak types did not. Entrants were not told who was a strong or weak type—only that there was a one-third chance that any defender was strong.

All defenders faced a sequence of eight entrants with each repetition, and this number was known to everyone.²⁴ Entrants were given information on how the defender played against all other previous entrants. If a previous entrant had chosen to challenge the defender, all subsequent entrants would see whether the defender had backed down or stayed tough. If an entrant chose not to challenge, the defender would not need to respond and no information about the defender's choice would be produced.

The experiment proceeded as follows. Entrants faced the defenders sequentially. Once paired with a defender, entrants were asked to choose between entering the game and thus challenging the defender, or not entering. We elicited defender choices using the strategy method: defenders were asked to select a strategy based on what an entrant might do "if the first mover enters I will choose B1 or B2 (not fight or fight)."²⁵ Each entrant made one decision with no available history (in the first period), one decision with a previous period's history against a different defender (in the second period), and so on.²⁶ At the end of each repetition (after each

entrant had played each defender once), subjects saw a screen with their decision history, the decisions of the subject they were paired within each period, and their own payoffs. Subjects knew that these payoffs would be translated into US dollars at the end of the experiment. Subjects then repeated the experiment.²⁷ Each repetition was done five times in order to account for the effects of learning and to generate sufficient data for the analysis.²⁸

After completing the five repetitions, subjects were told that we were making a slight change to the experiment. We explained that defenders would now be able to communicate to entrants whether they would fight or not. Defenders could do this by sending the following message through the computer: “if you choose enter, I will [fight, not fight].”²⁹ This message was seen only by the immediate entrant and not by later entrants. This is important because we did not want reputations about the history of using cheap talk to contaminate our results, as this is outside the private cheap-talk model we explore. Everything else in the experiment was the same as our baseline design.³⁰ Hence, our cheap-talk experiment was run on a set of subjects with experience in the strategic environment of the repeated entry-deterrence game, but no prior experience with cheap talk and no prior knowledge that cheap talk would be allowed.³¹ An Supplemental Appendix provides additional details and full subject instructions.

Results and Interpretation

Our goal in running the experiment was to collect data on how subjects played when no cheap talk was allowed versus how they played when it was.³² We did this to answer two questions. First, would entrants be deterred by cheap-talk threats or would they be equally likely to challenge in the face of a threat (Hypothesis 1)? Second, would defenders who issued a cheap-talk threat be more likely to fight than those who did not, or would it have no effect at all (Hypothesis 2)? The results, which we discuss below, are striking.

Hypothesis 1: Entrants Should Not Be Deterred by Cheap-Talk Threats

Contrary to the implications of the formal model, cheap talk had a significant effect in our experiment. Potential entrants were more likely to be deterred in early periods when we allowed cheap talk than when we did not allow cheap talk.³³ Pooling participants together, Figure 2 shows the entry rates of entrants across the two different experimental manipulations at each period in the game conditional on the defender not having backed down in a previous time period. Later we consider cases where the defender had backed down. The figure reveals a dramatic difference in entry rates in the early periods—particularly in the first period. When communication was not allowed, fully 83 percent of entrants entered in the first round. However, when defenders were allowed to issue a verbal threat and chose to do so, the high rates of entry in the first period disappeared.³⁴ Only 38 percent of potential challengers

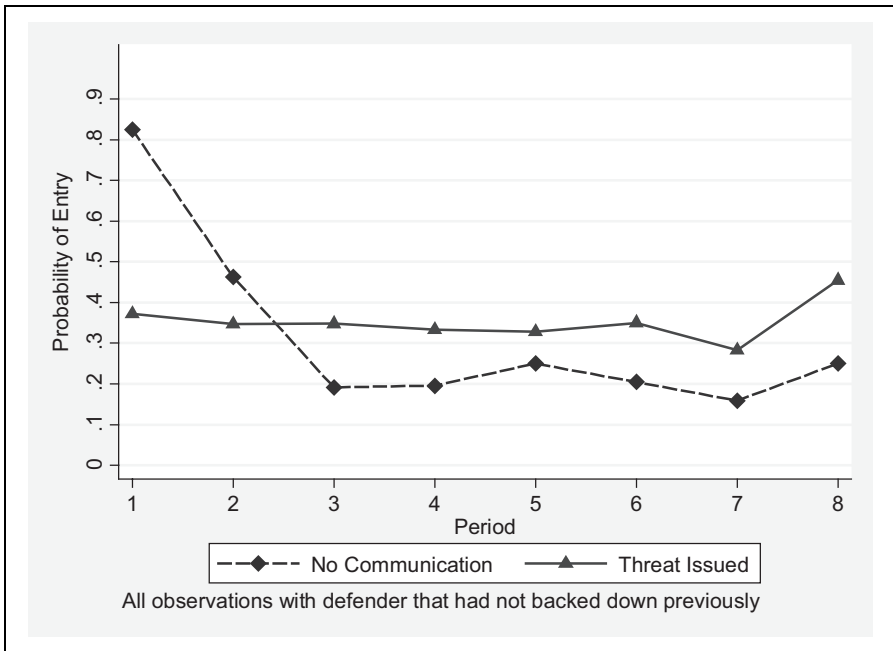


Figure 2. Challenger behavior with and without cheap talk: entry probability

entered in the first round when a costless threat was issued. That represents a striking 45 percent decrease in the probability of entry and is significant at $p < .01$.³⁵ This difference occurred despite the fact that the very same entrants were making these decisions.

Cheap-talk effects persist into the second period. As can be seen in Figure 2, there is a perceptible but not quite statistically significant advantage to issuing a threat in the second period. The small size of the difference in period two is, in part, due to combining two different groups of observations: entrants that faced a defender who had previously faced entry, and entrants who faced a defender who had not faced entry in the first period. Once we consider this difference, it is clear that cheap talk still has a large effect in the second period. In period two, when entrants faced a defender who had previously faced entry, the entry rate was 26 percent if the defender chose not to issue a threat. But if the defender chose to threaten, the entry rate was a much lower than 12 percent.³⁶

The effect of cheap talk was even larger when entrants faced a defender who had not been challenged in period one. In this case, the entry rate was 100 percent when communication was not possible and 52 percent when a threat was sent.³⁷ Importantly, since these results are conditioning on no previous backing down by the defender, the effect of cheap talk is a pure one. These results are unexpected.

When entrants had little to no information about the type of defender they were facing, they were significantly more likely to be influenced by the messages they received even though these messages were known to be costless.

Not surprisingly, the more information entrants were able to gather across periods, the less influential cheap talk appeared to become. As can be seen in Figure 2, costless threats did not continue to deter after the second time period, and by later rounds these threats actually caused entrants to be slightly *more* likely to challenge. We believe cheap talk's influence declines over time because entrants are able to obtain more reliable information about defender behavior simply by observing what the defender had done in the past. Rather than having to rely solely on verbal promises, entrants could observe how a defender behaved against other challengers in previous rounds and tie their strategies to these more dependable data. These results are similar to those found elsewhere (Bracht and Feltovich 2009).

The results so far suggest that cheap talk can work when little observable information is available on which entrants can make decisions. But what if entrants already have information that strongly suggests that the defender is weak? About 46 percent of the time, weak defenders chose to back down against an entrant, whereas less than 1 percent of strong defenders chose to back down. This means that entrants who observe a defender backing down can be fairly certain that they are facing a weak defender since strong defenders so rarely acquiesce. Can threats in this case—where presumably the defender's reputation for resolve has been lost—still make a difference?

Figure 3 suggests they can. In Figure 3, we compare the relative probability of entry in the cheap-talk versus the noncheap-talk experiment for each time period in cases where the defender had already backed down. Even in this extreme case, cheap talk still mattered. Entrants were still less likely to challenge in every period that a threat was issued, even if the defender had already revealed herself to be uncommitted.³⁸ Without communication, 95 percent of entrants chose to enter when their opponent had backed down previously, whereas only 85 percent entered after receiving a cheap-talk threat.³⁹ This suggests that even a costless threat by a non-credible player has some deterrent value.

Hypothesis 2: Defenders That Threaten Should Not Be More Likely to Fight

Costless verbal threats clearly influence whether entrants choose to fight in early periods of the game. But did it affect how defenders played? According to the logic of our formal model, defenders should not be more likely to fight after issuing a threat since there is no punishment for not following through. Did defenders who were allowed to threaten change their behavior in any way?

Our experiment reveals that weak defenders, at least early on, were more likely to fight if they said they would fight.⁴⁰ Figure 4 illustrates the rate of fighting across the two versions of the experiment by period and reveals that this difference is most pronounced in period 1, after which it disappears. Defenders are more likely to follow through in the very first period and then taper off after that. Interestingly, this

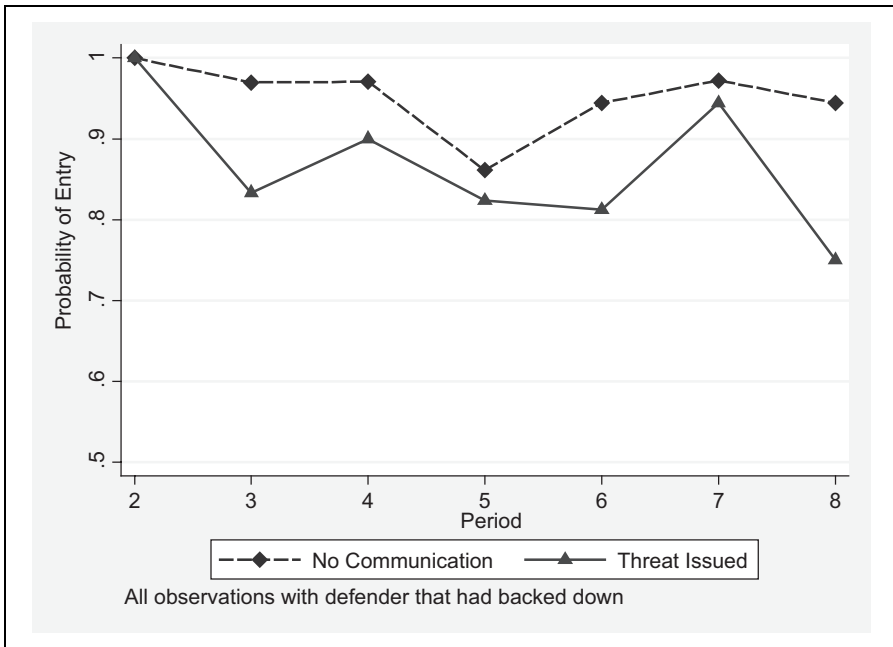


Figure 3. Challenger behavior with and without cheap talk: entry probability after defender has backed down

follow-through brings the behavior of weak defenders closer to the first period equilibrium predictions of the formal model with no communication.⁴¹

We were also able to see whether the same subject changed his or her behavior when cheap talk was allowed, permitting us to test our hypothesis within subjects. We found that 90 percent of our defenders increased the percentage of times they fought in the first period if they were allowed to issue a threat.⁴² Moreover, the change was usually large. On average, the same defenders were 16 percent more likely to fight after a threat than when they did not have the opportunity to issue a threat—a rate that is significantly different than 0 ($N = 16$, $t = 2.05$, $p = .06$).⁴³ Whether measured in the aggregate or at the individual level, cheap talk has a real effect on the behavior of individuals who engage in it.

These results indicate that cheap talk affects the behavior of defenders who have not previously backed down. But what happens after defenders have already signaled their type by backing down? Presumably, there is even less reason to follow through with threats in these cases.

Remarkably, costless communication still matters. As Figure 5 shows, cheap-talk affects the behavior of defenders even after they had backed down. In each time period, the probability of fighting is higher when cheap talk is allowed than when it is not. If we pool across periods 2 through 8, 17 percent of subjects in the

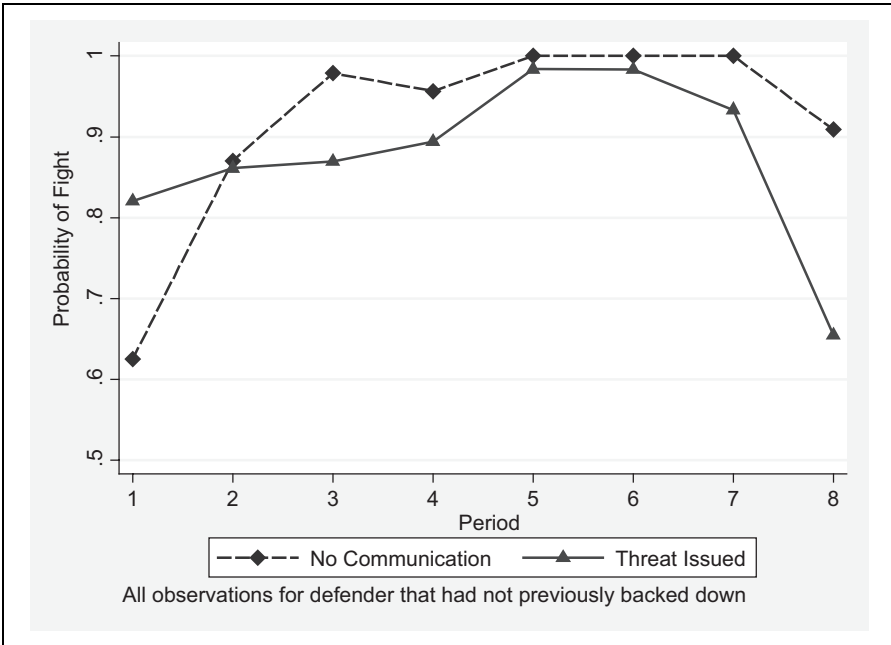


Figure 4. Weak defender fighting probability with and without cheap talk and no previous backing down

cheap-talk treatment decided to fight, whereas less than 3 percent decided to fight when no cheap talk was allowed.⁴⁴

Explaining the Power of Cheap Talk

Our experiment investigated the role of cheap talk in a repeated entry-deterrence game and revealed that verbal communication can influence behavior in ways not captured by much of the formal and empirical literature in IR. These results are striking. Verbal threats not only decreased an entrant's eagerness to challenge early in a game but also increased a defender's willingness to fight. Cheap talk may be costless, but in early rounds of a repeated game it successfully deterred entrants and made defenders more willing to fight.

These findings bring us back to our original puzzle. If promises and threats are not worth the paper they are written on, as Samuel Goldwyn once said, why would anyone believe them, and even more puzzling, why would anyone follow-through?

The fact that cheap talk was most influential in early rounds of play helps explain why it is influential. The first round of play (and to a lesser extent the second) is different from other rounds. During the first round, defenders and challengers are operating in an information-poor environment; no information exists about the defender

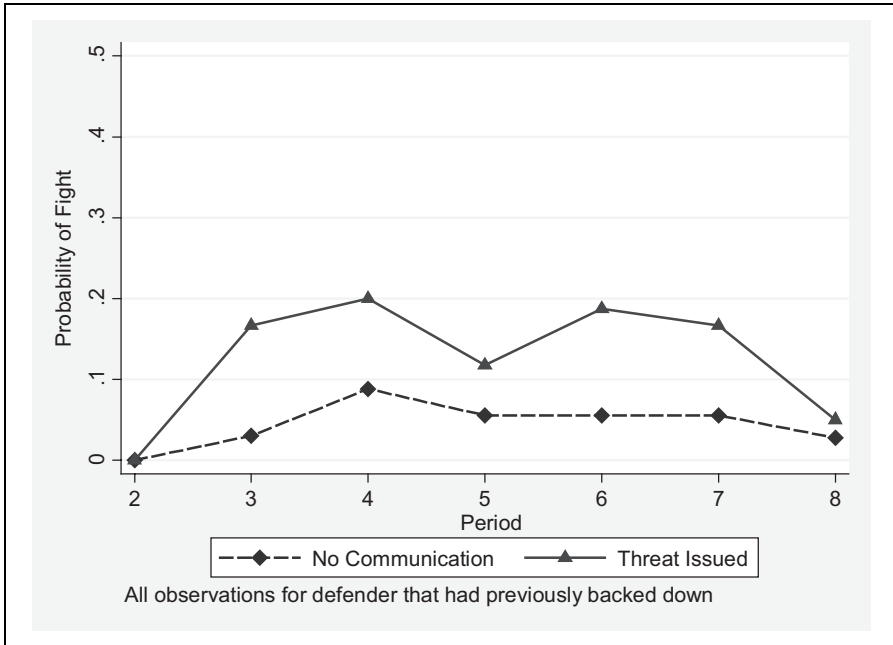


Figure 5. Defender behavior with and without cheap talk: fighting probability after defender has backed down

and how he or she played against other challengers and decisions must be made with little information about the defender's type. Verbal threats, even costless verbal threats, may be the only source of information challengers have at this point in time. It may be because information about the defender is so incomplete in early stages of a repeated game, that any hint about type may be useful. Knowing this, defenders may have incentives to issue threats, and challengers may have incentives to listen.

This, however, does not explain why challengers would be more likely to back down after a threat was issued in the first and second round of play, or why defenders would be more likely to follow through. If every defender had incentives to issue a threat early in a game, why would any of these threats be viewed as credible? And why would defenders be more willing to follow through?

A second possible explanation has to do with signaling behavior between defenders and early challengers. One of the assumptions formal models make is that defenders and entrants interact under conditions of common knowledge. That means everyone operates under the assumption that everyone understands the game and will play optimally as a result all the time. It is possible, however, that common knowledge does not exist either in the laboratory or in the real world. Mistakes may be made because some players misconstrue how the game should be played, misinterpret the instructions, or simply play irrationally for different idiosyncratic

reasons. If this were true, cheap talk could be used by savvy subjects to signal to each other that they understand the game so that a higher proportion of efficient decisions are made.⁴⁵ This would be especially important in early rounds of a game when challengers have no additional information about what type of defender they may be facing. In this case, it would be rational for challengers to respond to a threat that has been made—however costless that threat may be.

This explanation helps illuminate why defenders would be more likely to follow through with an early threat, and why challengers would be more likely to listen. Given that a threat is costless, a defender who threatens early in the game is playing exactly as one would expect him or her to play. Likewise, a player who does not issue a threat may be indicating that he or she does not fully understand the game. Thus, sending a threat or not sending a threat signals to the challenger something about the sophistication of the defender.

Alternative explanations might stress psychological variables more. One explanation relates to the willingness to lie. It is possible that some players do not engage in cheap talk because they prefer to be honest even if this means fewer payoffs. If this were true, a separating equilibrium would emerge where “honest” defenders would never threaten, and those who threatened would be more likely to follow through.⁴⁶ Another possibility is that individuals hold preferences for being consistent and hence following through on their messages. A long literature provides evidence for this (e.g., Cialdini, Trost, and Newsom 1995), but our design is unable to explore this further and so we leave it for future research.

Conclusion

IR has been skeptical about whether costless verbal communication has any influence on behavior despite the fact that state leaders engage in threats and promises all the time. In this article, we put cheap talk to a particularly hard test: a situation in which the preferences of the players are opposed and threats are private and costless. We expected that in an entry deterrence game where entrants were uncertain about whether defenders would fight, the use of costless threats would not change entrant and defender behavior in any way.

Controlling for confounding factors, our laboratory experiment revealed just the opposite. If a defender threatened to fight an entrant, that entrant was significantly less likely to challenge, and the defender was significantly more likely to fight. This occurred despite the fact that defenders suffered no punishment for failing to follow through with a threat; no additional entrants would know about the bluff and no other costs would be incurred. Cheap-talk threats were also significantly more likely to be used and more likely to deter in early rounds of play, when little additional information existed about the sender. These findings bring academic research closer to what we have been observing anecdotally in the real world. State leaders routinely issue verbal promises and threats, both publicly and privately, and sometimes these threats influence behavior.

Why did formal models miss these effects? We believe it has to do with at least one incorrect assumption underlying the models. Standard models assume that all players involved in a game will fully understand how to play the game and will play optimally all the time as a result. Our laboratory experiment suggests that this assumption is not true, at least among the population of undergraduates we studied. Even among this relatively homogeneous subject pool, there appeared to be significantly more heterogeneity of ability and understanding than the models took into account. Some subjects were quite good at playing the game while others were not, and savvy players appeared to incorporate this into their calculations about how best to play the game.

The most important difference in terms of its effect on cheap talk was the existence of individuals who were not particularly skilled at playing the game, took longer to figure out the game, or never fully comprehended the game. This created an opening for more adept defenders to signal their understanding of the game by issuing and then following through with a verbal threat in early rounds of play. Thus, the existence of less-talented players in a given population may make costless verbal communication in low-information environments a rational and effective strategy to pursue.

Our laboratory experiment, therefore, reveals a potentially important modification to our understanding of bargaining and communication in low-information environments. In situations where disputants have almost no information about each other's type, costless communication can have value. As this experiment showed, cheap-talk threats can be completely costless and still provide some information about the sender and their likely future behavior. This may matter only in the first and second rounds of play, and only between players with no previous experience with each other, but it does mean that in very low-information environments cheap talk could play a role that current theories have not captured.

The same heterogeneity of skill may not exist among state leaders engaged in their own entry-deterrence games. State leaders may be far more experienced and far more adept at navigating a complex strategic game than the undergraduates we studied. If this were true, then early threats would provide less information since all players would play optimally from the start. We strongly suspect, however, that the heterogeneity of skill we found in the laboratory is not significantly different from what we are likely to find among leaders playing similar games in the wider world. Some state leaders will be highly competent, others will not, and this will create a similar opportunity for cheap talk to be useful, especially when facing an opponent for the very first time.

Still, much more needs to be done. Our research is only the beginning of a much longer agenda aimed at understanding why cheap talk matters and why it seems to be so influential in low information environments. We do not know, for example, why savvy players would know to coordinate their behavior in early rounds, or what biases, beliefs, or mental handicaps our subjects brought to the laboratory. We have some quotes from the subjects themselves, but their responses are unreliable and in need of additional analysis.

More work also needs to be done to understand differences across subjects. More extensive pre- and postexperiment psychological tests could reveal

correlations between certain types of behavior (e.g., deceitful behavior) and behavior in the game. Our analyses could also be extended to more targeted subject pools such as military officers and diplomatic officials (Mintz 2004). Finally, the ecological validity of the experiment could be increased (e.g., by making the decision context more concrete instead of abstractly described). All of these represent opportunities for future research that could build on the results reported in this article.

Still, laboratory experiments have rarely been used in IR (Mintz 2004; McDermott, Cowden, and Koopman 2002), especially with a game theoretic model to structure the design and empirical analysis (Tingley and Wang, 2010; Tingley, 2011; Tingley and Walter, 2011; Butler, Bellman, and Kichiyev 2007). Our article shows how this can be done with potentially important results. The experiment presented in this article is a first step in explaining how and why individuals use verbal communication to influence each other's behavior in a repeated entry-deterrence game, but it is far from complete. We hope the striking findings presented in this article encourage other scholars to push this research even further and in the process further reveal why costless communication appears to be influential in some contexts but not others.

Appendix A

Given the number of entrants, the payoffs in the game, and the distribution of defender types (in our experiment one-third were strong and two-third were weak types) we can derive a sequential equilibrium as done in Jung, Kagel, and Levin (1994). Assuming risk neutrality, Figure A1 graphs the probabilities of entry and fight where there has been no previous backing down. As we noted earlier, when a weak defender has previously backed down, the formal model indicates that challengers should always enter and the defender should always back down. Permitting cheap talk does not alter the predictions because there is a strict incentive for all defenders to signal that they will fight and hence the signals are uninformative. Details presented in Supplemental Appendix.

Authors' Note

All mistakes are our own. Replication and supplementary materials available at <http://hdl.handle.net/1902.1/15353>.

Acknowledgment

We would like to thank Ernesto Reuben for assistance with z-Tree programming, and Zoli Hajnal, Alice Hsiaw, Tom Palfrey, and Kristopher Ramsay for discussions.

Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The author(s) received no financial support for the research, authorship, and/or publication of this article.

Notes

1. See, for example, Fearon (1994, 1995) and Schultz (1998). For models that consider how cheap talk might be made costly through domestic institutions and/or reputational concerns, see Ramsay (2004), Guisinger and Smith (2002), and Sartori (2002). For models where cheap talk can influence behavior, see (V. Crawford and Sobel (1982), V. Crawford (2003), Sobel (1985), Farrell and Gibbons (1989), and Farrell (1995).
2. The literature does, however, argue that cheap talk could be effective if there is adequate overlap in the interests of the signaler and signalee (e.g., Kydd 2003).
3. For the counterview that Khrushchev had already decided to back down when news arrived that Kennedy had accepted the trade, see Fursenko and Naftali (1998).
4. For more examples, see J. Davis (2000).
5. In most of these cases, we do not believe that vocal leaders would have faced significant costs for not following through, but we admit that it is impossible to assess this counterfactual to determine whether talk really was cheap.
6. Thyne (2006) is one of the few article that attempts to isolate cheap talk in a large-*N* study. Schrodtt (1993) presents a time series showing the dynamic of US–Soviet relations, US–China relations and Israeli–Palestinian relations using the Conflict and Peace Data Bank (COPDAB) and World Event/Interaction Survey (WEIS) data sets. However, cheap-talk events are mixed with other events and are, therefore, difficult to evaluate.
7. This possibility cannot be ruled out in the absence of additional testing. Ours is a first step in that direction.
8. See also Risse (2000).
9. See especially Fearon (1995, 414).
10. Of course, the economics literature has developed these points with substantial nuance. Farrell and Gibbons show how cheap talk relating to the desire to negotiate can be effective in a double auction game, allowing parties to trade-off bargaining position against the likelihood of continued bargaining (1989). And even when preferences are aligned there can exist babbling equilibria where cheap talk is not informative (J. Crawford 1998).
11. Thyne 2006 theorized that this was in part because parties were more likely to make excessive demands when costless communication was used (957).
12. The way costless communication is transmitted may also matter. Isaac and Walker (1988) and Ostrom, Walker, and Gardner (1992) found that in a public goods provision game, subjects were more likely to contribute larger sums of money if verbal pledges were made face-to-face rather than anonymously.
13. However, subjects did not ultimately obtain more efficient outcomes as a result. In other experiments, Wilson and Sell found that subjects contributed more in a public good game when preplay communication was allowed *and* information existed about past behavior (1997). Wilson and Sell, however, surprisingly found that subjects contributed the most when they could *not* communicate with each other and had *no* information about past behavior.
14. Uncertainty in this game was over the size of a resource to be divided as opposed to preferences.

15. Our interest is in line with that of Croson and colleagues in that we are interested in the role of cheap talk in bargaining environments. Our investigation differs from theirs in several important ways. They looked at the role of reputation between a pair of actors who repeatedly interacted with each other. Our study looks at behavior where a single “defender” faces a series of different challengers. The strategic game we study also differs. They use a repeated ultimatum game with outside options, whereas we use a repeated entry-deterrence game. Both reflect a bargaining framework except that an entry-deterrence game follows a protocol where by following entrance there is only a dichotomous choice over the division of resources.
16. Our experiment differs from Sundali and Seale (2004)—the most closely related experiment—in two respects. First, our defenders faced a sequence of entrants. In their experiment, everyone played the same role (entrant) and decided whether or not to enter a market. Second, our experiment included incomplete information and the chance for reputation building. Theirs did not. Other experiments discussed above looked more at situations with bargaining over a continuum of potential divisions. Our work hews more closely to the deterrence literature prominent in IR (Huth 1999), though entry-deterrence and many bargaining situations share important similarities such as their sequential nature and in this case the existence of opposing preferences. One difference compared to Sundali and Seale is the presence of incomplete information in the game we study, whereas this is less common in many bargaining experiments.
17. Payoff parameters are from Jung, Kagel, and Levin (1994) and used in a companion paper (Tingley and Walter, 2011).
18. In this case, the payoffs are 160 for not fighting a challenger and 70 for fighting a challenger.
19. In order to keep the framework consistent with earlier work on the entry-deterrence game, we only analyze repeated play between different opponents (see Walter 2006). The game could also be played repeatedly between a defender and a single entrant. This would be similar to a situation where a government engaged in a series of continuing disputes with a single ethnic group, where the ethnic group demanded greater and greater concessions over time (e.g., the Canadian government’s relationship with the Parti Quebecois). This would be an interesting extension but the theoretical predictions remain the same.
20. We considered a number of other options for the message space. We could have allowed subjects to select not sending a message at all. Or we could have allowed the subjects to choose a costly signal. While each of these additions would have improved the correspondence between the communication options available to real decision makers and those in our experiment, each would have introduced their own complications both theoretically (cheap-talk models typically assume that *some* message is sent) and empirically (conditioning our analysis on three or four message options instead of two would exacerbate sample size problems). Other scholars using experiments share our approach (Palfrey and Rosenthal 1991, 188). For an interesting study of open communication and threats in an IR simulation experiment, see McDermott, Cowden, and Koopman (2002).
21. These predictions are based on a sequential equilibrium solution assuming risk-neutral utility functions (Jung, Kagel, and Levin 1994). Our assumption about risk neutrality could affect both defender and entrant behavior (Camerer and Weigelt 1988), though a model allowing for nonrisk-neutral utility functions for all players is needed to make more definitive equilibrium predictions. We use risk-neutral utility functions because it

substantially simplifies the analysis. We also include them because there is no reason to believe that risk tastes will differ across our experimental treatments and that the relatively small stakes mean that the effects of any concavities in the utility function will be moderate. Finally, procedures designed to induce complete risk neutrality can be problematic (Cox and Oaxaca 1995; D. Davis and Holt 1992, 476). In the analyses below, we compare behavior in two similar situations using the same subjects. Any nonrisk-neutral utility functions should not bias our results in a particular direction.

22. This is most easily seen in analyzing a single-shot version of the game, but this holds in the repeated setting we focus on as well. Our particular payoffs ensure there are no semi-separating equilibria because preferences are sufficiently in opposition.
23. Subjects were recruited through a university social science laboratory using an e-mail solicitation to all students who had signed up with the lab. Those who responded were accepted until all positions were filled. Subjects were only allowed to participate in the experiment one time. Students entered the laboratory one by one and were seated at computer workstations that were separated by pull-out dividers to prevent interaction between subjects. Instructions were then read to all participants. During this process, subjects were given the opportunity to make practice decisions and review a set of questions and answers about the experiment. Any questions from subjects were repeated and answered so that all subjects could hear. This ensured that all aspects of the experiment design were common knowledge. All matching was entirely anonymous, with subjects seated at separate, partitioned computer terminals. Subjects were paid one by one at the end of the experiment with money earned in the experiment and a guaranteed \$10 "show-up" fee. The experiment was programmed and conducted with the software z-Tree (Fischbacher 1999). Our design, instructions, and computer interface went through a lengthy piloting period in order to obtain the best possible experimental protocol.
24. Each entrant and defender were paired together once in each part. When all the subjects had played each other once, a "repetition" of the experiment was complete.
25. We did this to observe the decision of a defender even when their opponent did not choose to enter. There is some possibility that the strategy solicitation method could influence choices. In fact, there is considerable debate on this in the literature (Brandts and Charness 2000; McLeish and Oxoby 2004; Bosig, Weimann, and Yang 2003). We do not believe this is likely to be a problem in our experiment for three reasons. First, behavior in our no communication treatment is very similar to that observed by (Bolton and Ockenfels 2007) who elicited strategies sequentially. Second, pilot tests that compared the strategy solicitation procedure and sequential play revealed no differences in behavior. And third, we explicitly described the game in sequential terms and did not provide defenders a choice option when there was no entry in order to distinguish as much as possible from the normal form version of the entry-deterrence game.
26. This design allowed us to keep all subjects engaged throughout the experiment and maximizes the amount of data we could collect within an experimental session.
27. Across repetitions of the experiment all positions (first mover/second mover) stayed the same, entrants were randomly assigned when they would move against each defender, and defender types (strong/weak) were randomly reassigned according to the commonly known distribution of types at the beginning of each repetition. Random reassignment and anonymity prevent subjects from forming strategies based on playing again against a specific subject.

28. The precise number of repetitions was unknown to subjects; they were simply told that the experiment “may or may not be repeated” in order to limit attempts to build reputations across repetitions.
29. Our experiment used neutral descriptions, and thus subjects actually chose between “I will (not) choose B1 if you choose A1.” We did not allow subjects to avoid sending a message.
30. All subjects kept either their entrant or their defender roles.
31. One might argue that changes in behavior between the first noncheap-talk experiment and the second cheap-talk experiment are due to learning. While some degree of learning might be at work, the stark differences we explore below suggest that this cannot drive our results. Furthermore, introducing cheap talk produced large differences in behavior in additional analyses that only used behavior in the later parts of the noncheap-talk experiment. Future experiments could run sessions using only the cheap-talk treatment or complete a full A-B/B-A experimental design.
32. Our empirical strategy for all of our hypotheses is to break defenders out by those who had already backed down and those that had not. We also break out entrants into those that faced a defender who had not yet backed down, and those that faced a defender who had. We do this because the equilibrium model we discuss above makes this important distinction, and we do not want to conflate reputational effects with the effect of cheap talk. Next, we calculate either the mean rate of a behavior (e.g., taking the average of cases where entry = 1 and no entry = 0) or the test statistics using standard difference in mean tests. Tests using differences in proportions produce nearly identical results.
33. All entrants prior to participating in the cheap-talk section had also gone through the section with no cheap talk. Supplemental analysis showed very little changes in behavior over repetitions of the noncheap-talk section. Hence a cumulative learning effect does not explain this stark difference in behavior after allowing for cheap talk.
34. The vast majority of defenders chose to issue a threat (91 percent of all cases). When the defender indicated that they would back down upon entry, entrants not surprisingly decided to enter.
35. The test statistic was computed using 1,251 observations with 16 subjects. Standard errors are clustered at the subject level. Within-subject tests show a similar story. All except two subjects had higher rates of entry in the first period under the no communication treatment. The average within-subject decrease in entry from no communication to communication condition was 47 percent.
36. The test statistic for mean difference based on 418 observations and clustering on 16 subjects was $t = -1.64$, $p = .122$ (two-sided test).
37. $N = 348$, 15 individuals, $t = -5.51$, $p < .01$.
38. Due to the small number of cases, the difference is not significant in every individual time period. However, when we pool across all periods a significant difference does exist between the cheap and noncheap-talk versions of the game.
39. Without clustering on individuals, this difference was significant ($t = 2.56$, $p < .01$). Clustering on 13 individuals with 327 observations yields smaller tests statistics ($t = 1.2$, $p = .25$).
40. We focus on the behavior of weak defenders because strong defenders should always fight (and almost always do). We exclude the first repetition of each treatment because

behavior of defenders changed significantly from the first to the second repetition in the design where defenders were not allowed to communicate. Excluding the first repetition decreases the extent to which our results are driven only by a cumulative learning pattern.

41. As we will note later, this may be because defenders felt it would be dishonest to not follow-through on that threat.
42. Here we calculated the total number of times that a subject chose to fight in the first period when they were assigned a weak defender role. We then divided this by the total number of times a subject was a weak defender in the first period (recall that *type* was randomly assigned and hence subjects might have different number of times that they played a weak defender role). This gives us a value between 0 and 1. We calculated this value separately for each treatment and each subject, and took the difference between these values for our subjects in the defender role.
43. This suggests that across treatments, differences at the aggregate level move in the same direction as differences we observe within individual subjects; our aggregate differences are not driven by a single subject radically changing their behavior.
44. There are no observations in period 1 because there were no previous periods in which a player could back down. The difference in fight rates was significantly different from 0 (108 observations with standard errors clustered on 8 individuals yield a $t = 2.8$ and $p < .05$). Unpooling across periods radically reduces the sample size for which to conduct statistical tests. Thus, it is not surprising that unpooling our analysis generates less-significant test statistics for each period (results available from authors).
45. An argument similar to this was made by V. Crawford in an attempt to explain why deception might work (2003). Instead of having a distribution of honest types, as we argue may be the case, he considered that some people may be more easily “fooled” than others. This creates a similar separation of types where rational players know that costless verbal communication will deceive those individuals who are less rational, making cheap talk sensible. This again suggests that certain types of individuals will behave quite differently from what existing models would expect.
46. Jervis (1970) was one of the first to discuss the restraints that decision makers might face regarding lying. This includes moral restraints and the degree to which individuals may or may not value honesty.

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