Promises, Reliance, and Psychological Lock-In

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ABSTRACT

We obtain experimental evidence that suggests that promisors are more likely to keep promises the more they were relied on by a promisee; that this effect is anticipated by promisees, who accordingly strategically overinvest to lock promisors into keeping their promises (psychological lock-in); and therefore that legal enforcement can reduce overinvestment as promisees do not need to make use of the extralegal mechanism of psychological lock-in when the legal regime induces promisors to perform instead. These results contrast with the central prediction of the holdup literature that underinvestment results in the absence of legal enforcement.

1. INTRODUCTION

In the absence of legal enforcement, a promisee (a recipient of a promise) may be wary of relying on a promise for fear that the promisor (the maker of the promise) will not keep it. Because classical economic theory assumes that agents are rational and self-interested, it predicts that a

promisee will underinvest in reliance on a promise whenever he believes it likely that the promisor will have a self-interested reason to break it. Legal enforcement of promises should accordingly mitigate this problem of underinvestment by giving the promisor a self-interested reason to keep her promise, thus assuring the promisee that his investment will not be wasted. A large literature on breach remedies (for example, Shavell 1980, 1984; Rogerson 1984; Cooter and Eisenberg 1985; Edlin and Reichelstein 1996; Edlin 1996; Che and Chung 1999; Schweizer 2006; Ohlendorf 2009; Stremitzer 2012) and the holdup problem (for example, Williamson 1979, 1985; Grout 1984; Grossman and Hart 1986; Hart and Moore 1988; Chung 1991; Aghion, Dewatripont, and Rey 1994; Nöldeke and Schmidt 1995; Che and Hausch 1999) studies how third-party enforcement mechanisms can mitigate this underinvestment problem.

Yet reducing underinvestment might not be the only virtue of legal enforcement if some people are intrinsically motivated to keep their promises. Casual observation, introspection, and plenty of empirical evidence tell us that many people are motivated to keep their promises, even in the absence of self-interested reasons to do so (Ellingsen and Johannesson 2004; Charness and Dufwenberg 2006, 2010; Vanberg 2008; Ostrom, Walker, and Gardner 1992; Kerr and Kaufman-Gilliland 1994; Sally 1995; Bicchieri and Lev-On 2007). There is also evidence that promisors are more likely to keep their promises the more they believe that promisees expect them to do so (Mischkowski, Stone, and Stremitzer 2019; Ederer and Stremitzer 2017). Given the close connection between expectations of performance and actions taken by a promisee in reliance on a promise, a promisor's intrinsic motivation to keep a promise is likely to also increase the more the promisee has relied on it.[1] If this is the case, then the promisee has an incentive to strategically rely on the promise to make the promisor more likely to keep it—an effect that we refer to as psychological lock-in. Indeed, in the absence of a legal regime, the promisee might overinvest to psychologically lock in the promisor. Legal enforcement of relied-on promises would then reduce a promisee's need to overinvest in reliance on a promise, because the legal regime would motivate the promisor to keep her promise instead. Legal enforcement might

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1. Consistent with this conjecture, Wilkinson-Ryan and Hoffman (2015) provide evidence based on a psychological vignette study that people are more likely to keep relied-on promises. However, in their study such reliance confers a material benefit on the promisor. Thus, they do not disentangle the effects of reliance alone from the effects of reciprocity.

therefore have the unexpected benefit of reducing overinvestment alongside the more expected benefits of reducing underinvestment and increasing rates of promise keeping.

In this paper, we experimentally investigate three questions in the laboratory. First, does a promisee's greater reliance on a promise make the promisor more likely to keep it? Second, does the promisee anticipate such an effect and so strategically increase his reliance to make the promisor more likely to keep her promise? Third, what benefits for social welfare arise from the legal enforcement of a promise? In particular, in what ways does such legal enforcement improve the promisee's investment decision? Could it be that legal enforcement reduces overinvestment by reducing the promisee's need to avail himself of the extralegal mechanism of psychological lock-in to motivate the promisor to keep her promise?

The form of legal regime that we study mimics a legal regime that is governed by the doctrine of promissory estoppel. In its canonical form, the doctrine allows a promisee to legally enforce a gratuitous promise if he detrimentally relied on the promise and this reliance was reasonably foreseeable to the promisor (*Restatement [Second] of Contracts,* sec. 90). Gratuitous promises are made without the promisor getting something in return and therefore do not satisfy the consideration requirement, the usual test for the enforceability of promises in American contract law.

The earliest applications of promissory estoppel were confined to promises that were not motivated by the prospect of commercial gain.[2] However, the domain of the doctrine has subsequently grown considerably (Holmes 1996). Courts now allow reliance to act as a consideration substitute in many situations in which the promisor is motivated by the prospect of commercial gain. For example, gratuitous firm offers, voluntary waivers, and one-sided modifications, which once could be freely repudiated for lack of consideration, are now binding obligations when relied on by the promisee (Knapp 1998, p. 1198).[3]

Moreover, contemporary courts sometimes invoke promissory es-

2. The historical roots of the doctrine of promissory estoppel can be traced to a few categories of gratuitous promises: gratuitous promises to convey land, gratuitous promises by bailees, promises to make gifts to charitable institutions, and gratuitous promises among family members (Farnsworth 2004, pp. 90–92).

3. Courts have also allowed promissory estoppel claims based on promises of at-will employment to proceed where the prospective employee has relied on such a promise by, for example, moving her family a long distance or giving up other sources of income or employment. See, for example, *Grouse v. Group Health Plan, Inc.* (306 N.W.2d 114 [1981]); *Ravelo v. County of Hawaii* (658 P.2d 883 [1983]).

toppel to enforce promises that are supported by consideration when those promises would be rendered unenforceable for some other reason, for example, a lack of definiteness or a failure to satisfy the Statute of Frauds. Thus, in *Wheeler v. White* (398 S.W.2d 93 [1966]), White promised to obtain a loan for Wheeler to enable Wheeler to construct a commercial building on a tract of land owned by Wheeler. In return, Wheeler promised future payments including commissions on rentals received from any tenants of the new building. Following reassurances by White that the loan would be forthcoming, Wheeler proceeded to raze the existing building and prepare the land for the new structure. White then informed Wheeler that no loan would be forthcoming. In the subsequent litigation, Wheeler's contract claim was denied on the grounds that the agreement was too indefinite to be enforceable. But the Supreme Court of Texas ultimately enforced White's promise on promissory estoppel grounds (for other examples of cases invoking promissory estoppel to enforce contracts that would be unenforceable on grounds of definiteness, see *Rosnick v. Dinsmore,* 457 N.W.2d 793 [1990]; *Hoffman v. Red Owl Stores, Inc.,* 133 N.W.2d 267 [1965]). In a similar vein, in *Alaska Airlines, Inc. v. Stephenson* (217 F.2d 295 [1954]), Stephenson gave up his job with Western Airlines after securing an oral agreement on a 2-year employment contract with Alaska Airlines. Alaska Airlines then fired him before the 2 years were up and argued that the agreement was unenforceable because it had not been formalized in a writing signed by Alaska Airlines and therefore failed to satisfy the requirements of the Statute of Frauds. The court, however, found Alaska Airlines' promise of employment enforceable because of Stephenson's foreseeable reliance on it (for other examples of cases invoking promissory estoppel to enforce contracts that would be unenforceable under the Statute of Frauds, see *Kolkman v. Roth,* 656 N.W.2d 148, 153 [2003]; *Olympic Holding Co. v. ACE Ltd.,* 909 N.E.2d 93, 94 [2009]; *Jamestown Terminal Elevator, Inc. v. Hieb,* 246 N.W.2d 736 [1976]; see also *Restatement [Second] of Contracts,* sec. 139[1]).

Our experiment mimics the structure of cases like *Alaska Airlines* and *Wheeler*. At the outset of our experiment, pairs of subjects were given the opportunity to exchange promises to cooperate in a subsequent experimental stage. That is, they could exchange promises to do something valuable for each other—as Alaska Airlines and Stephenson did in entering into their employment agreement and White and Wheeler did in forming their agreement concerning the financing of Wheeler's building

project. Some of our subjects were then assigned to a treatment in which they faced no sanctions for breaking their promises, while others were assigned to a treatment that models a promissory estoppel regime: subjects who broke their promises had to compensate the promisee, but only if the promisee had relied on the promise—just as Stephenson's resignation from his job with Western Airlines and Wheeler's preparation of his land for the new building served as the grounds for enforcing Alaska Airlines' and White's promises.

Although canonical promissory estoppel cases involve purely gratuitous promises, we designed our experiment to model exchanges of promises because cases like *Alaska Airlines* and *Wheeler* are likely to have greater commercial importance and remain controversial as a doctrinal matter, thus increasing their significance from a policy standpoint (on the controversy surrounding the application of promissory estoppel to indefinite agreements, see, for example, *Keil v. Glacier Park, Inc.,* 614 P.2d 502, 506 [1980]; *Black Canyon Racquetball Club, Inc. v. Idaho First National Bank,* 804 P.2d 900, 907 [1991]; *Forstmann v. Culp,* 648 F. Supp. 1379 [1986]; see also Scott [2007]; for skepticism about the application of the doctrine in the Statute of Frauds context, see *Tanenbaum v. Biscayne Osteopathic Hospital, Inc.,* 190 So.2d 777, 779 [1966]; *Mullins v. Southern Pacific Transportation Co.,* 851 P.2d 839 [1992]; *Genin, Trudeau, and Co. v. Integra Development International,* 845 F. Supp. 622 [1994]). Thus, our experimental results do not directly speak to applications of the doctrine to purely gratuitous promises. However, our results should generalize to such settings so long as people do not distinguish among promises according to the motive with which they are made.

Judges have discretion to determine the remedy once they have found a promisor liable on promissory estoppel grounds. In particular, they can choose between expectation damages—the standard remedy for breach of contract, which is designed to make the promisee as well off as he would have been had the promise been kept—and reliance damages—a remedy designed to ensure that the promisee is no worse off than he would have been had he never received and relied on the promise (see *Restatement [Second] of Contracts,* sec. 90). But the scholarly consensus seems to be that judges usually award expectation damages in promissory estoppel cases (Farber and Matheson 1985; Yorio and Thel 1991). Thus, in this paper, we study the effects of introducing a promissory estoppel regime with expectation damages (Expectation Damages, hereafter ED). We do

so by contrasting it with a regime in which legal enforcement is absent (No Regime, hereafter NR).[4]

Consistent with the predictions of a simple model, we find that in the absence of legal enforcement, promisors are more likely to cooperate with promisees the more promisees relied on their promises and that promisees believe that promisors will act in this way. And we find that the introduction of a promissory estoppel regime with expectation damages decreases overinvestment. These findings constitute evidence of a psychological lock-in effect.

The remainder of the paper is organized as follows. Section 2 reviews related literature. Section 3 describes the experimental design and procedures, outlines the model of agents' preferences that forms the basis of our predictions, and formulates the hypotheses that we test. Section 4 presents our results, which are largely in line with our theoretical predictions. We discuss potential limitations of our results in Section 5 and implications for legal theory in Section 6. Section 7 concludes.

2. RELATED LITERATURE

Two theories explaining why many promisors are intrinsically motivated to keep their promises have dominated the recent experimental literature. Proponents of the expectation-based theory argue that promisors keep their promises to avoid failing to meet the expectations of performance that their promises have created in the promisee (Dufwenberg and Gneezy 2000; Charness and Dufwenberg 2006). In contrast, proponents of the commitment-based theory claim that promisors have a preference for keeping their word regardless of the promisee's expectations (Ostrom, Walker, and Gardner 1992; Braver 1995; Ellingsen and Johannesson 2004; Vanberg 2008; Potters and Ismayilov 2012).

It is, of course, possible that both a promisee's expectations of performance and a promisor's sense that she is duty bound to keep her promise regardless contribute to her willingness to keep her promise. Vanberg (2008) argues that his results show that a sense of being duty bound regardless is the only determinant of promise keeping. Similarly, Ellingsen et al. (2010) find no significant relationship between expectations and

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4. In *Alaska Airlines v. Stephenson,* the court affirmed the lower court's award of expectation damages, while the court in *Wheeler v. White* held that Wheeler was entitled to reliance damages.

contributions in dictator and trust games in which they elicited expectations from recipients and communicated those expectations to the dictators and trustees. However, in both of these studies, the findings that expectations play no role arise under conditions in which the dictator (or trustee) had not made a promise to the recipient (or trustor).[5]

In the experiment in Charness and Dufwenberg (2006), by contrast, there were such promises, and the results suggest that expectations matter. But those results are based on correlations between second-order beliefs and actions that allow for alternative explanations. Ederer and Stremitzer (2017) get around this problem by exploiting exogenous variation in promisors' second-order beliefs. They show that promisees' expectations affect promise keeping in cases in which expectations are supported by direct promises. And they postulate that motivations to keep promises may depend on an interaction effect: a recipient's expectations matter if and only if he received a promise. Using a vignette design, Mischkowski, Stone, and Stremitzer (2019) find evidence of a weaker interaction effect, showing that promising makes promisors more sensitive to promisees' expectations.

We assume that a promisor's motivation to keep her promise increases as the promisee's reliance on the promise increases. This might be because a promisee's reliance is an indicator of his expectations that the promisor will perform, and the promisor dislikes disappointing the promisee's expectations. But it also might be because the promisor simply does not like to make the promisee worse off than he would have been had the promise been kept. This implies that the more the promisee relied on the promise, the more willing the promisor will be to keep it.[6]

Our paper is also related to the experimental literature that examines the effects of legal enforcement on investment incentives. Sloof et al. (2003, 2006) obtain experimental evidence that, consistent with the predictions of classical economic theory (for example, Shavell 1980), shows

5. Vanberg (2008) finds that exogenous variation of promisors' second-order expectations—that is, their beliefs about promisees' beliefs—does not generate a statistically significant difference in promise keeping. But in his design, subjects were rematched after a round of promises was made, so that promisors ultimately faced promisees to whom they made no promises. In Ellingsen et al. (2010), no promises were ever made.

6. We focus on material harms that promisees suffer as a result of their reliance on promises as opposed to the psychic harms that may be associated with disappointed expectations. Thus, for simplicity, we do not assume that a promisor's second-order beliefs about the promisee's expectations influence her preferences. This means that, unlike the expectation theorists, we do not need to employ the apparatus of the psychological game theory model in Battigalli and Dufwenberg (2007, 2009).

that enforcement of contracts with expectation damages protects a promisee's investment too well by encouraging overinvestment, both when renegotiation is not possible (Sloof et al. 2003) and when renegotiation is possible (Sloof et al. 2006). In those experiments, there is a chance that performance of the contract might be inefficient. Overinvestment occurs because expectation damages perfectly insure the promisee against the risk of breach, even in states of the world in which performance of the contract is inefficient. The promisee disregards this possibility when choosing his investment level and so invests the amount that would be desirable if performance were always efficient.[7] Our design abstracts from this reason to overinvest by ensuring that promise keeping is always efficient.[8]

3. DESIGN AND PROCEDURE

In this section, we describe our experimental design and procedures. We also introduce the hypotheses that arise from our simple model.

3.1. Experimental Design

Subjects played a modified dictator game in which a dictator decides whether to cooperate with a recipient by sending money to him. The game differs from a standard dictator game in two ways. First, at the outset of the game players do not know whether they will be assigned the role of dictator or recipient. Behind this veil of ignorance, they decide whether to exchange promises to cooperate if assigned the role of dictator during a communication stage. Second, once roles have been assigned, but before the dictator decides whether to cooperate ($\alpha = 1$) or not ($\alpha = 0$), the recipient chooses a level of investment $i \in \{0, 1, 2, 3, 4, 5, 6\}$.[9]

In NR, the scenario without enforcement, which is depicted in Figure 1, the recipient's monetary payoff first increases and then decreases in his

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7. Sloof et al. (2003, 2006) also show that, consistent with theoretical predictions, reliance damages cause even more overinvestment. Reliance damages, like expectation damages, perfectly insure the investment decision against the possibility of breach. But reliance damages give the investor an additional incentive to invest to reduce the likelihood of breach.

8. This is not an unrealistic assumption in a relevant subset of cases, as we explain in Section 6.

9. Here we describe the version of the game we implement in our experiment. For full analysis of the more general model on which the game is based, see Online Appendix OA.
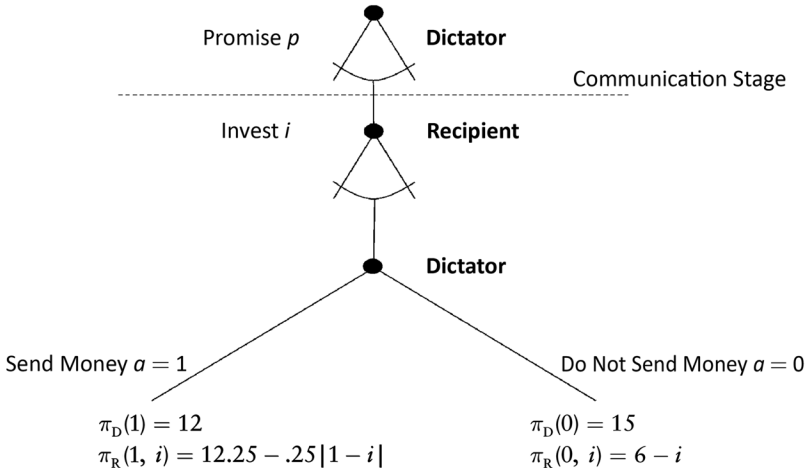
Figure 1. No Regime

investment if the dictator cooperates, while it always decreases his payoff and does so at a faster rate if the dictator does not cooperate. Regardless of the dictator's action, the recipient's investment has no effect on the monetary payoff of the dictator. Thus, the dictator has no reciprocity-based reason to reward the recipient for a positive investment choice. The dictator's monetary payoff is given by

$$\pi_D(a,\ i) = \begin{cases} 15 & \text{if } a = 0 \\ 12 & \text{if } a = 1 \end{cases}, \tag{1}$$

and the recipient's is given by

$$\pi_R(a,\ i) = \begin{cases} 6 - i & \text{if } a = 0 \\ 12.25 - .25|1 - i| & \text{if } a = 1 \end{cases}. \tag{2}$$

If the dictator cooperates, the recipient maximizes his monetary payoff by investing 1. If the dictator does not cooperate, the recipient maximizes his payoff by investing 0. As in a standard dictator game, the dictator maximizes her monetary payoff by choosing not to cooperate.

We compare NR with ED, in which promise keeping is enforced with expectation damages so long as the recipient has invested something in reliance on any promise to cooperate that the dictator made during the communication stage. The monetary payoffs of the two players in ED, π_D^L and π_R^L, are the same as in NR if the dictator keeps her promise or made

no promise, but if she breaks a promise and the recipient invested more than zero, then the dictator must pay the recipient an amount of money $l(i)$ that ensures that his monetary payment is the same as if the dictator had kept her promise. In this way, as we explained in Section 1, we model applications of the doctrine of promissory estoppel to agreements that are supported by consideration but unenforceable for some other reason like lack of definiteness or a failure to satisfy the Statute of Frauds. Thus,

$$l(i) = \begin{cases} 0 & \text{if } i = 0 \\ \pi_R(1, i) - \pi_R(0, i) = 6.5 - .75i & \text{if } i > 0 \end{cases} \tag{3}$$

and when the dictator breaks a promise to cooperate, the recipient's payoffs are given by

$$\pi_R^L(0, i) = \pi_R(0, i) + l(i) = \begin{cases} 6 & \text{if } i = 0 \\ 12.50 - .25i & \text{if } i > 0 \end{cases} \tag{4}$$

and the dictator's are given by

$$\pi_D^L(0, i) = 15 - l(i) = \begin{cases} 15 & \text{if } i = 0 \\ 8.50 - .75i & \text{if } i > 0 \end{cases} \tag{5}$$

Predictions of the Standard Model. The standard self-interested rational actor model assumes that each subject maximizes his monetary payoff. It therefore predicts that the dictator will never cooperate in NR (because $\pi_D(0) = 15 > 12 = \pi_D(1)$) and that, anticipating this choice by the dictator, the recipient will invest 0. By contrast, in ED, the dictator will always cooperate if and only if the recipient invested at least 1 (because $\pi_D(1) = 12 > 8.5 - .75i = \pi_D^L(0, i)$). Thus, the recipient will invest 1, as this maximizes his payoff in the event that the dictator cooperates, which guarantees the socially optimal outcome.

Intrinsic Motivation to Keep Promises. Consistent with prior evidence, we assume, contrary to the standard model, that subjects are intrinsically motivated to keep their promises. Thus, we suppose that the dictator cares about both her material payoff and keeping any promise she made to the recipient and that she cares more about keeping a promise the more the recipient would lose should she break it. We therefore posit that, if the dictator makes a promise, $p = 1$, and subsequently breaks it, $a = 0$, her utility is reduced by an amount that depends on the resulting reduction of the recipient's material payoff.[10] In line with the

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10. Because our focus is on the effects of reliance on promising, we abstract from other considerations that may drive agents to keep their promises like a desire not to dis-

economics literature on promising, we refer to the latter component of the utility as the dictator's guilt (Charness and Dufwenberg 2006; Battigalli and Dufwenberg 2007, 2009; Ederer and Stremitzer 2017).[11] Formally, we suppose that the dictator's utility is given by

$$u_D(a, p) = \pi_D(a) - p\gamma g[\pi_R(1, i) - \pi_R(a, i)], \tag{6}$$

where $\gamma \geq 0$ is a parameter that represents the weight she places on her guilt relative to her material payoff and $g(x)$ is the guilt function with $g(0) = 0$ and $g'(x) > 0$. Guilt is therefore 0 whenever the dictator made no promise, $p = 0$, or whenever she cooperates, $\alpha = 1$: $u_D(a, 0) = \pi_D(a)$ and $u_D(1, p) = \pi_D(1)$. Because the recipient made no promise to choose a particular investment level, such a motivation has no relevance to his decision-making, and so we assume that his utility function is simply his payoff function: $u_R(a, i) = \pi_R(a, i)$.

Our design is a particular instantiation of a more general model that we solve fully by backward induction in Online Appendix OA. Here we highlight the features of the equilibrium that form the basis of our hypotheses starting with NR. The central observation is that in the absence of a material penalty for breaking a promise, the guilt function may provide the dictator with sufficient inducement to keep her promises. A dictator who made no promise to cooperate will never find it in her interests to cooperate because $\pi_D(0) = 15 > 12 = \pi_D(1)$. But a dictator who made a promise will cooperate whenever

$$\pi_D(1) \geq \pi_D(0) - \gamma g[\pi_R(1, i) - \pi_R(0, i)], \tag{7}$$

that is, whenever the sensitivity to guilt γ exceeds a critical value $\gamma^c(i)$, which depends on the recipient's investment level:

$$\Leftrightarrow \gamma \geq \gamma^c(i) = \frac{\pi_D(0) - \pi_D(1)}{g[\pi_R(1, i) - \pi_R(0, i)]}. \tag{8}$$

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appoint promisees' expectations or a simple desire to do as one promised (Mischkowski, Stone, and Stremitzer 2019).

11. Although we follow the literature in using the terminology of "guilt," we are agnostic over whether the guilt function should be given a literal interpretation. The use of the term "guilt" might be taken to suggest that the dictator's psychological well-being is reduced when she breaks a promise, thus giving her a self-interested reason to keep it. In this case, "guilt" should enter the social welfare function, as it would represent a component of the dictator's welfare. But we could instead regard it simply as capturing the strength of the non-self-interested considerations that the dictator believes give her reasons to keep her promises. If so, then "guilt" would not represent a component of the dictator's welfare and so ought not to be included in the social welfare function.

Because the guilt function $\pi_R(1, i) - \pi_R(0, i)$ is increasing in i, the guilt that the dictator experiences when she breaks a promise increases in investment, and the critical value $\gamma^c(i)$ that determines whether she will cooperate decreases.

It follows that when γ is so low that even the highest possible investment level will not trigger enough guilt to induce the dictator to cooperate—that is, $\gamma < \gamma^c(6)$—then the dictator will never cooperate, and a recipient, if he knows γ, will accordingly invest 0. Conversely, when γ is so high that investing the efficient amount is sufficient to induce the dictator to cooperate, $\gamma > \gamma^c(1)$, then the recipient will choose the efficient investment level, the dictator will cooperate, and social welfare is maximized. Finally, when γ is in an intermediate range, whether the dictator will be motivated to keep her promises depends on the recipient's investment level, but investing the efficient amount is not sufficient to induce the dictator to cooperate. Thus, the recipient will choose a level of investment greater than 1 to make the dictator's guilt from breaking the promise sufficient to induce the dictator to keep it, that is, to psychologically lock in the dictator.[12] Accordingly, guilt can also produce a new kind of inefficiency: overinvestment. The amount the recipient needs to overinvest in this intermediate range is decreasing in γ, as the higher is the dictator's internal motivation to keep her promise, the less the recipient needs to increase the guilt function, $\pi_R(1, i) - \pi_R(0, i)$, to motivate the dictator to keep it. Figure 2 shows how equilibrium investment changes assuming a linear guilt function and a recipient who knows γ.

In ED, the enforcement regime ensures that the recipient is always as well off when the dictator cooperates as when he does not, so long as the dictator made a promise to cooperate and the recipient invests at least 1. Thus, although the dictator will not experience guilt from breaking a promise in this case, by investing at least 1 the recipient can ensure that any dictator, regardless of her guilt parameter, will prefer to cooperate. And so in equilibrium the recipient will invest 1 so long as the dictator made a promise, leading to the socially optimal result. Hence, the equilibrium prediction of our model is the same as that of the standard model.

Working backward, we can now analyze the communication stage. Under ED, both parties always exchange promises in equilibrium. This is

12. This result obtains because when the recipient invests the amount required to induce the dictator to cooperate, his payoff is greater than the payoff he would receive were he to invest 0. If this condition did not obtain, then the recipient would choose an investment level of 0, and the dictator would not cooperate.
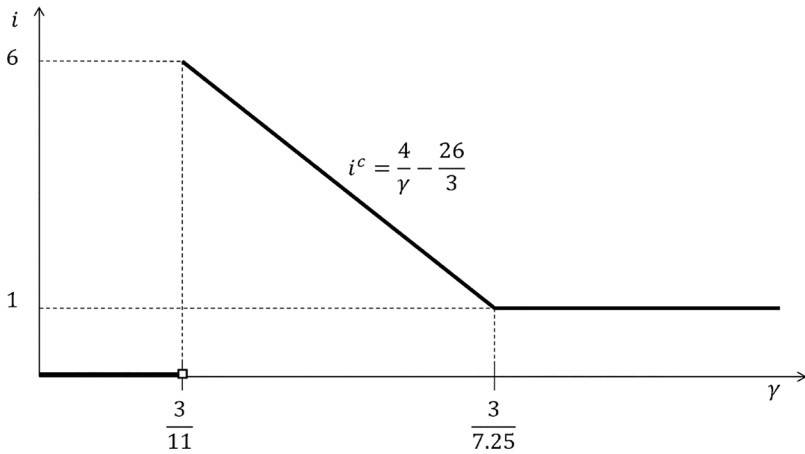
Figure 2. Equilibrium investment

because, behind the veil of ignorance about the parties' roles in the subsequent subgame, the benefit of receiving a promise in return outweighs the costs of making one. The same applies in NR so long as each party believes that the other has a guilt parameter large enough to induce her to keep her promise in the subsequent subgame. The communication stage is analyzed in more detail in Online Appendix OA.

We also compare NR to a third scenario, Control (C), which is identical to NR except there is no communication stage. Because promising is therefore not possible in C, the comparison enables us to assess the extent to which subjects' promises are driving their behavior in NR.

3.2. Experimental Procedure

Subjects played several rounds of one of the games described above. At the outset of each round, each subject was randomly and anonymously matched with another subject. The round then consisted of four steps.

Step 1. During the communication stage, subjects exchanged precoded messages with one another sequentially in a randomly determined order. The subject who was selected to make the first communication decision (participant 1) had to decide whether to send message 1 to the subject with whom she had been paired (participant 2): "I promise to send you money if I am chosen to be player A so long as you make me a return promise." If participant 1 decided not to send message 1 to participant 2, she instead sent a message that said "I do not promise." After learning of

participant 1's decision, participant 2 then had to decide whether to send message 2 to participant 1: "I promise to send you money if I am chosen to be player A." If participant 2 decided not to send message 2 to participant 1, he instead sent her a message that said "I do not promise." Notice that the conditional form of message 1 meant that when participant 1 sent message 1, she ended up promising to send participant 2 money only if participant 2 made her a promise in return.[13] Figure A1 shows which subjects ended up making promises depending on the messages they sent one another during the communication stage. Given this message structure, our model predicts that subjects make only nongratuitous promises in equilibrium. That is, they make promises to cooperate only when they anticipate that doing so will elicit a promise in return (see Section 3.1 and Online Appendix OA).[14]

Step 2. Once the communication stage was over, subjects' roles for the round were selected, and subjects learned whether they would be the dictator (player A) or the recipient (player B). Roles were randomly assigned anew in each round.

Step 3. The recipient then indicated his level of confidence that the dictator would cooperate with him for each possible investment level. He then chose how much to invest by selecting a number between 0 and 6.

Step 4. Once the recipient had made his investment decision, the dictator had to decide whether to cooperate with the recipient by sending him money. We used the strategy method to elicit the dictator's cooperation decision (Selten 1967). That is, the dictator made her decision in ignorance of the recipient's choice by indicating whether she wanted to cooperate for every possible investment level that the recipient might have chosen. The dictator's choice, and therefore the final payoffs of both

13. Vanberg (2008) and Hoppe and Schmitz (2011) also use endogenously generated promises, while Sloof et al. (2003, 2006) simply tell their subjects to imagine that they had signed a contract. Vanberg (2008) uses free-form messages, allowing subjects to exchange a sequence of instant messages, instead of computer-coded messages. Free-form messages may be more natural, which possibly explains why they produce larger effects sizes (see, for example, Charness and Dufwenberg 2010). But they have the disadvantage that subjects may make promises without using promissory language, which makes it difficult to disentangle promises from mere statements of intent.

14. Thus, our design gives subjects good (self-interested) reasons to make promises, and most end up making promises, which is important given that our aim is to study determinants of promise keeping. Furthermore, had subjects been free to negotiate the terms of their future interaction, we think it likely that they would have devised mutually beneficial (rather than one-sided) arrangements resembling the equilibrium prediction of our model.

players, was given by the dictator's choice for the recipient's chosen investment level. If, for example, the recipient invested 3, then the dictator's action was the action that she indicated that she wanted to choose in the event that the recipient chose an investment of 3. At the very end of the round, the subjects learned the payoffs that each would earn during that round if it was randomly selected for payment on the basis of their actions or the recipient's stated beliefs.[15]

We conducted 13 experimental sessions with a total of 210 student subjects. We used a between-subject design, so subjects participated in only one treatment: 70 subjects participated in ED, 70 subjects participated in NR, and 70 subjects participated in C.

The experimental sessions were conducted at the Experimental Social Science Laboratory (XLab) at the University of California, Berkeley, and the Experimental and Behavioral Economics Laboratory (EBEL) at the University of California, Santa Barbara. The XLab subject pool consisted of undergraduate students at the University of California, Berkeley, and the EBEL subject pool consisted of undergraduate students at the University of California, Santa Barbara.[16]

Subjects were assigned to visually isolated computer terminals. Beside each terminal they found paper instructions, which are reproduced in Online Appendix OD. Instructions were read aloud to subjects, and questions were answered individually and confidentially at the subjects' seats. The experiment was programmed and conducted using the software z-Tree (Fischbacher 2007).[17]

Each session consisted of two unpaid practice rounds followed by eight rounds, one of which was randomly chosen at the end of the experiment for payment on the basis of the subjects' decisions in that round and

15. Instead of our modified dictator game in which subjects exchange promises to cooperate conditional on being selected to be the dictator, we might instead have used a trust game with payoffs similar to those of our modified dictator game (as in Mischkowski, Stone, and Stremitzer 2019). But the trust game involves two reliance decisions: the recipient's opt-in decision and his subsequent investment decision. So had we used such a game, we would not have been able to observe dictator behavior when recipients decide not to rely on the promise at all (by opting out). And we would have observed only the effects of reliance on promising among recipients who opt into the game, who may not be representative of the population.

16. At the University of California, Santa Barbara, 30 subjects participated in ED, 30 subjects participated in NR, and 40 subjects participated in C. At the University of California, Berkeley, 40 subjects participated in ED, 40 subjects participated in NR, and 30 subjects participated in C.

17. Sample screenshots are shown in Online Appendix OE.

another of which was selected for payment on the basis of the recipient's stated beliefs at step 3 about the likelihood that the dictator would cooperate with him for each possible investment level. Recipients were paid for stating their beliefs in accordance with Table A1, which was structured to give them incentives to report their beliefs truthfully (Vanberg 2008). We selected a different round from the round selected for payment on the basis of subjects' decisions so that subjects had no reason to change their behavior in any of the rounds to hedge their bets.

No subject interacted with the same subject more than once in the latter eight rounds. We achieved this by creating matching groups of exactly 10 subjects and having each subject play against the same subject during the two practice rounds. Thus, after the practice round, each subject was paired with eight different subjects in the matching group in a random order.

Subjects were paid an average of $11.20, ranging between $0 and $15, for their decisions, and they were paid an average of $1.88, ranging between $0 and $4.55, for stating their beliefs about the dictator's actions. Subjects also received a fixed fee of $5 for showing up on time, $5 for completing a postexperiment survey, and an additional sum ranging between $0 and $1.50 depending on how well they did on a postexperiment cognitive reflection test.

No time limit was placed on any of the subjects' decisions. The software ensured that the experiment could not continue to the next round until every subject had made the decisions he was required to make in the relevant round.

In light of the complexity of the experiment, we took various steps to ensure that subjects understood the game. First, after reading the instructions aloud, subjects had to complete two preliminary questions that tested their understanding of the relationship between the players' actions and their payoffs. Subjects' answers were checked, and wrong answers were corrected to make sure that they understood how to arrive at the correct answer. Second, the two practice rounds, which were designed so that subjects would experience the roles of both player A and player B, gave subjects the opportunity to familiarize themselves with the game and the program interface before they played the game for money. Finally, in the postexperiment survey, we asked subjects whether they felt that they understood the payoff consequences of their actions. They could answer "yes," "no," or "kind of." We introduced the "kind of" category, as we thought that forcing subjects to choose between "yes" and "no" was too

crude, and we wanted to identify subjects whose understanding was only partial. We excluded subjects who answered "kind of" and "no" from our data. This left us with 54 subjects in NR, 54 subjects in C, and 57 subjects in ED.

3.3. Hypotheses

We are now in a position to formulate the hypotheses that flow from our theoretical model. First, we predict, contrary to the predictions of the standard model, that the rate of cooperation in NR will exceed 0 and that, anticipating cooperative behavior on the part of dictators, recipients will be willing to invest.

> Hypothesis 1
> 1) Cooperation in NR will exceed 0.
> 2) The rate of investment in NR will exceed 0.

Explanation. We predict that many dictators will make promises and that, of those dictators, those with high guilt parameters will cooperate regardless, while those with intermediate guilt parameters will cooperate so long as recipients invest enough. Thus, hypothesis 1.2 follows so long as some recipients believe it likely that the dictator has a sufficiently high guilt parameter. Hypothesis 1.1 follows so long as there are in fact some dictators with sufficiently large guilt parameters.

Second, we predict that recipients will strategically overinvest in NR to psychologically lock in the dictator, while they will not do so in ED. We also predict that we will see less underinvestment in ED and more efficient investment, because in ED recipients who received a promise can give dictators a sufficient self-interested reason to cooperate simply by investing 1, the efficient level of investment.

> Hypothesis 2
> 1) The incidence and magnitude of overinvestment will be higher in NR than in ED.
> 2) There will be less underinvestment and more efficient investment in ED than in no NR.

Explanation. Hypothesis 2.1 follows so long as some recipients hold beliefs about the dictator that make overinvestment rational. That is, some recipients must believe that there is a high chance the dictator has an intermediate guilt parameter that makes her willing to cooperate only if investment is greater than 1. Hypothesis 2.2 follows from our model so

long as similar numbers of promises are made in ED as in NR, and some recipients are not too confident that the dictator is a high-guilt type who will cooperate regardless of the investment level or an intermediate-guilt type who will cooperate only if investment is greater than 1.

Third, we predict that dictators will become more willing to cooperate as investment increases in NR and that recipients will anticipate this effect.

Hypothesis 3
1) Recipients exhibit a higher degree of confidence that dictators will cooperate as investment increases in NR.
2) Contingent cooperation rates will increase in NR as investment increases.

Explanation. Hypothesis 3 follows from our prediction that many dictators will make promises in NR and so will experience guilt that is increasing in a recipient's investment.

Fourth, we predict that there will be higher rates of cooperation in ED than in NR.

Hypothesis 4. The cooperation rate will be higher in ED than in NR.

Explanation. Hypothesis 4 follows from our model so long as there are some low-guilt dictators who will not cooperate in NR regardless of the investment level and enough promises are made in ED compared with NR.

Finally, we expect that the above will all entail that ED will do better than NR in terms of overall payoffs.

Hypothesis 5
1) Joint payoffs will be higher under ED than under NR.
2) Average payoff differentials will be lower under ED than under NR.

Explanation. Hypothesis 5.1 and hypothesis 5.2 follow if our other predictions are confirmed. This is because these predictions entail that investment decisions will be superior and rates of cooperation higher under ED than under NR.

4. RESULTS

The data from our two main treatments consist of 14 matching groups and 111 subjects—54 subjects in NR and 57 subjects in ED—after excluding subjects who reported that they did not fully understand the game. Each session lasted for eight rounds. Thus, a total of 432 decisions were made in NR, and 456 decisions were made in ED. However, the decisions made by subjects in each matching group were not independent of one another and taken together constitute a single independent observation. Thus, while the results we report are based on data from all sessions and all rounds, our nonparametric tests are based on matching-group averages of the relevant variables.

Even though we are primarily interested in what happened when a dictator made a promise, our main results rely on aggregate data—that is, all the data including data generated when a dictator made no promise—though we sometimes also report differences focusing only on dictators who made a promise or recipients who received one. We focus on aggregate data because the dictators who made promises in NR may differ systematically from those who made promises in ED. Thus, if we looked only at behavior of dictators who made promises or recipients who received promises, our results might be driven by selection effects. A concern about selection effects was also the motivation for running C, which is identical to NR except for the absence of a communication stage in which subjects can exchange promises. Comparing aggregate results in C with those in NR provides us with a cleaner way of assessing the effects of promising than comparing subjects who did or did not make or receive promises in NR, because by relying on aggregate data we eliminate concerns about selection effects.[18]

4.1. Prevalence of Opportunism

First, consistent with much other empirical evidence, we find that the self-interested rational actor model overestimates the importance of opportunism and so overstates the need for legal enforcement of relied-on promises. Dictators cooperate 40.3 percent of the time in NR, despite the fact that they lack a self-interested reason to do so, and this rate of cooperation is significantly different from 0 ($p = .02$, Wilcoxon signed-rank test; hypothesis 1.1). Recipients, moreover, seem to anticipate this

18. We compare NR and C in Section 5. The data from C consisted of seven matching groups and 54 subjects after excluding those who reported that they did not fully understand the game. A total of 432 decisions were made in C.
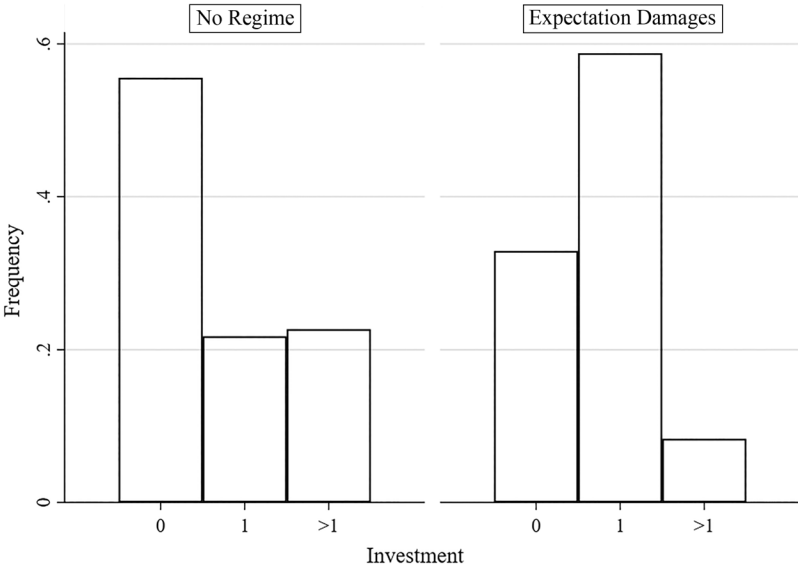
Figure 3. Investment in No Regime and Expectation Damages

cooperative behavior. Average investment is equal to 1.00 in NR, and this is significantly different from 0 ($p = .02$, Wilcoxon signed-rank test; hypothesis 1.2).

4.2. Recipients' Investment Decisions

More interesting are our findings that, consistent with the existence of psychological lock-in, enforcing relied-on promises with expectation damages induces unambiguously superior investment decisions (see Figure 3 and Table 1). Unsurprisingly, there is less underinvestment in ED than in NR—33 percent compared with 56 percent—and the difference is statistically significant ($p = .047$, Wilcoxon rank-sum test; hypothesis 2.2). There is also more efficient investment in ED—59 percent compared with 22 percent—and the difference is statistically significant ($p < .01$, Wilcoxon rank-sum test; hypothesis 2.2). Crucially, however, there is also less overinvestment in ED—8 percent compared with 23 percent—and the difference is statistically significant ($p < .01$, Wilcoxon rank-sum test). Moreover, average investment conditional on overinvesting is also lower in ED—2.42 compared with 3.45—and the difference is statistically significant ($p < .02$, Wilcoxon rank-sum test). These results suggest that both the magnitude and the incidence of overinvestment are reduced

Table 1. Incidence of Efficient Investment, Overinvestment, and Underinvestment

	No Regime ($N = 216$)	Expectation Damages ($N = 228$)	Z-Statistic
Efficient	.22	.59	3.0**
	(.41)	(.49)	[$p < .01$]
Underinvest	.56	.33	2.0*
	(.50)	(.47)	[$p < .01$]
Overinvest	.23	.08	2.6**
	(.42)	(.28)	[$p < .05$]

Note. The results are based on pooled data from all sessions and all rounds. Standard deviations are in parentheses. The Z-statistic reflects Wilcoxon rank tests using matching group averages.
 * Significant at the 5 percent level.
 ** Significant at the 1 percent level.

by the introduction of a legal regime with expectation damages: more people overinvest and they overinvest more on average in the absence of a legal regime (hypothesis 2.1). Indeed, the average increment of overinvestment—a measure that combines both the incidence and the magnitude of overinvestment—is also considerably lower in ED—.12 compared with .56—and the difference is again statistically significant ($p <$ .01, Wilcoxon rank-sum test).[19]

Thus, we find, consistent with our theory, that some dictators are more willing to cooperate in NR the more the recipient has relied on a promise and that recipients anticipate this by overinvesting to induce more cooperation. In ED, by contrast, there is no need to overinvest to motivate a dictator to keep her promise, because the legal regime ensures that a dictator has a sufficient self-interested reason to keep it, so long as the recipient chooses to invest at least the first-best amount of 1.

4.3. Dictators' Willingness to Cooperate and Recipients' Confidence

We also find evidence that directly supports our theory. In line with hypothesis 3, dictators become more willing to cooperate and recipients become more confident that dictators will cooperate when recipients overinvest, that is, invest more than 1 (the socially optimal level of investment). We have data to test this hypothesis because we employed the strategy

19. We compute this measure by taking the mean of the increment by which investment exceeds the efficient level of 1.

method to elicit dictators' preferences for all possible investment levels, and we elicited recipients' beliefs about the likelihood that dictators will cooperate for all possible investment levels.

Our model of promisor preferences suggests that some dictators will prefer to keep their promises regardless of the investment level, others will prefer to keep their promises only when investment exceeds a certain level, while others will never cooperate regardless of the investment level. The phenomenon of psychological lock-in—overinvestment by recipients in order to lock in a dictator—depends on the existence of this intermediate group of dictators whose preferences depend on their investment level.

If dictators have guilt parameters varying over a continuum, our model predicts that as investment increases, the number of dictators who are willing to keep their promises is nondecreasing. This is because higher investment levels induce dictators with lower guilt parameters to keep their promises while having no effect on the decisions of those with higher guilt parameters who were willing to keep their promises at lower investment levels. Thus, to test hypothesis 3, we compare recipients' average confidence in dictators' cooperation for investment of 0 in NR with the corresponding average for investment of 6. We also compare average contingent cooperation rates in NR over the same range.

In line with our hypothesis, we find that recipients' confidence increases from 1.8 to 2.2, where 0 describes the confidence of a recipient who thinks it certain that the dictator will not cooperate and 4 is the confidence of a recipient who thinks it certain that the dictator will cooperate (see the line representing NR in Figure 4). This difference is statistically significant ($p = .03$, Wilcoxon signed-rank test; hypothesis 3.1). Importantly, the effect is not driven only by the difference between investment of 0 and the efficient level of 1. The difference in average confidence for investment of 1 (1.9) and that at investment of 6 is also statistically significant, albeit only at the 10 percent level for the Wilcoxon signed-rank test ($p = .06$) and at the 5 percent level for the one-sided t-test ($p = .03$).[20]

We also observe that the average contingent cooperation rate increases from 41 percent for investment of 0 to 49 percent for investment of 6 (see the line representing NR in Figure 5).[21] This difference is, however, sta-

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20. The one-sided t-test is appropriate where, as here, we have a directed hypothesis.

21. Contrary to our model's prediction, we see a negative slope for average contingent cooperation in NR over some ranges of investment, especially when moving from $i = 2$ to $i = 3$, though the effect is not significant ($p = .13$, Wilcoxon signed-rank test; $p = .14$, two-sided t-test). A speculative possibility is that some dictators punish recipients for their
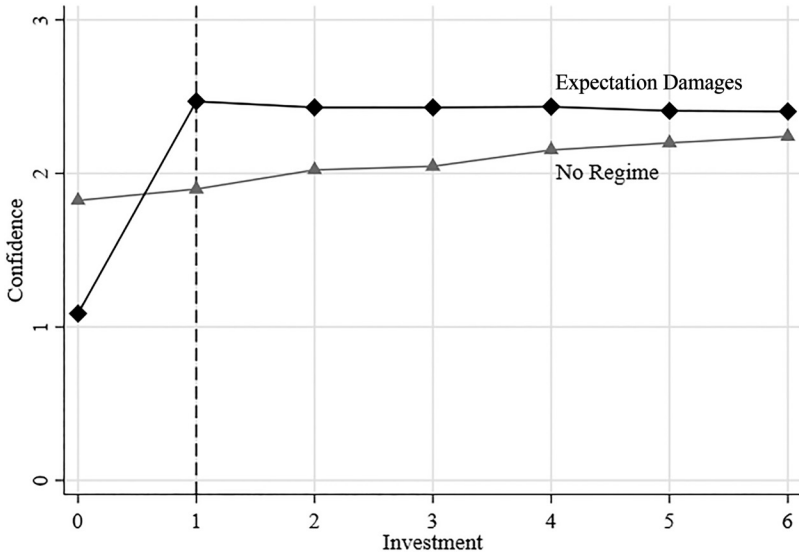
Figure 4. Belief profile

tistically significant at only the 10 percent level for the one-sided *t*-test ($p = .18$, Wilcoxon signed-rank test; $p = .07$, one-sided *t*-test; hypothesis 3.2).[22] But focusing on the difference between cooperation rates at an investment level of 1 (39 percent) and an investment level of 6—that is, focusing specifically on the effects of overinvestment—we find statistical significance at very close to the 5 percent level for the Wilcoxon signed-rank test ($p = .051$) and at the 5 percent level for the one-sided *t*-test ($p = .02$).

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overinvestment, perhaps because they do not like the idea, suggested by our theory, that a recipient invests beyond the productive level purely to make them more likely to keep a promise. We have not explicitly allowed for such backlash effects in our model. But notice that our experiments made the efficient level of reliance very transparent to subjects. Thus, it was relatively easy for our subjects to distinguish between efficient and excessive reliance. In the real world it will often be hard for a promisor to make this distinction because of imperfect information about the preferences and costs of the promisor. In this respect, our results likely overestimate a possible backlash effect.

22. The fact that the difference in contingent cooperation is barely significant while the difference in beliefs is highly significant is likely due to the fact that beliefs are measured on a scale from 0 to 4 while the cooperation decisions are binary. This binary structure adds noise to the data. Moreover, about 88 percent of recipients invest 2 or less, and the difference between average contingent cooperation for investment of 0 and 2 is highly significant ($p < .01$, Wilcoxon signed-rank test).
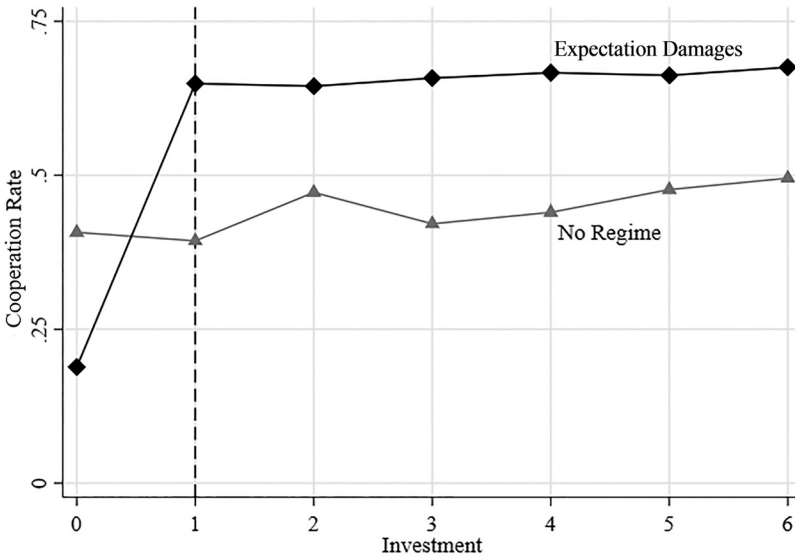
Figure 5. Cooperation profile

4.4. Overall Rates of Cooperation

Cooperation rates are higher in ED than NR: 62.3 percent compared with 40.3 percent. The difference is statistically significant ($p = .02$, Wilcoxon rank-sum test; hypothesis 4). This result is not particularly surprising. So long as dictators make promises, thus ensuring that there is legal enforcement of relied-on promises in ED, the result is predicted by both the standard self-interested rational actor model and our model in which dictators experience guilt from breaking their promises. Indeed, the difference is more stark, and again statistically significant, when we focus only on dictators who made promises: 93.2 percent cooperate in ED, while 47.5 percent cooperate in NR ($p < .01$, Wilcoxon rank-sum test). By contrast, there is no statistically significant difference in cooperation rates among dictators who did not make promises: 7.7 percent cooperate under NR compared with 7.3 percent under ED.

4.5. Crowding Out of Cooperation by Enforcement

The difference in cooperation rates between NR and ED, however, masks one interesting difference in dictators' behavior that is not predicted by our model of promisor preferences and so is not reflected in our hypotheses. When recipients invest 0, dictators face no pecuniary penalties if they

break their promises to cooperate regardless of the regime. In NR, dictators are never required to compensate recipients for promise breaking, while in ED they must do so only once recipients have invested at least 1. Thus, from the standpoint of parties' monetary incentives, ED and NR are indistinguishable when recipients invest 0, and thus both the standard rational actor model and our alternative model predict no difference in cooperation rates contingent on 0 investment across the regimes.

In fact, however, as Figure 5 shows, we find that the average cooperation rate contingent on 0 investment is considerably lower in ED than in NR: 18.9 percent compared with 40.7 percent when we look at the behavior of all dictators and 24.7 percent compared with 46.9 percent when we look only at dictators who made promises. Both differences are statistically significant ($p = .02$ and $p = .048$, respectively, Wilcoxon rank-sum test). (Among dictators who did not make promises, the difference is not statistically significant.)

Recipients, moreover, anticipate this effect, as shown in Figure 4. Average recipient confidence that the dictator will cooperate for 0 investment is higher in NR than ED: 1.8 compared with 1.1 in aggregate and 2.1 compared with 1.4 when we look only at recipients who received promises. Both differences are statistically significant ($p = .03$ for both, Wilcoxon rank-sum test).

These results suggest that the presence of a legal regime crowds out voluntary promise keeping when the recipient fails to trigger its penalties by investing 0 (see Frey and Jegen [2001] for a survey of experimental evidence of crowding out in economic settings). To capture this effect in our model of promisor preferences, we would have to assume that the legal regime reduces the guilt a dictator experiences as a result of promise breaking holding the recipient's payoffs constant (perhaps because the dictator interprets the recipient's decision to invest 0, thereby failing to trigger legal penalties for promise breaking, as releasing the dictator from her obligation to keep her promises). That is, we would need to assume that the unrealized possibility of legal consequences for promise breaking crowds out the dictator's internal motivation to keep her promises by reducing her guilt parameter whenever the recipient decides not to trigger those consequences by investing 0.

This crowding out seems unlikely to have a large effect on overall cooperation rates, however, because recipients can prevent it from occurring, and guarantee the optimal result, by investing 1 if they receive promises. Indeed, the data confirm that this crowding out largely occurs

off the equilibrium path. In ED, only 4.0 percent of recipients invest 0 if they receive promises (compared with 52.2 percent in NR and 89.6 percent in ED if there were no promises).

With the exception of this crowding out, ED performs exactly as standard theory predicts. And our results give us no reason to expect crowding out to occur at any level of investment in a regime that enforces promises regardless of the recipient's reliance, because in such a regime a promisee would be legally protected against breach regardless of his investment decision. In short, our results suggest that a reliance-independent expectation-damages regime would conform to the predictions of standard theory over the entire investment range. Thus, we see no reason to suppose that our main finding that legal enforcement prevents overinvestment is confined to a setting where legal enforcement is contingent on the recipient relying on a promise. Rather, it seems to be the enforcement of promise breaking with expectation damages that is driving this result.

4.6. Joint Payoffs and Payoff Differences

The improved investment decisions and higher cooperation rates that result from introducing a legal regime result in higher joint payoffs. Average joint payoffs are $22.86 in ED compared with $21.68 in NR, and the difference is significant ($p = .01$, Wilcoxon rank-sum test). The difference is even starker when we look only at pairs of subjects with a dictator who made a promise: $23.92 compared with $21.87 ($p < .01$, Wilcoxon rank-sum test). There is no statistically significant difference in average joint payoffs among pairs of subjects with a dictator who did not make a promise (hypothesis 5.1).

The introduction of a legal regime also reduces average payoff differences between dictators and recipients from $5.90 to $3.15, a statistically significant reduction ($p = .02$, Wilcoxon rank-sum test). The difference is even more stark when we focus on pairs of subjects with a dictator who made a promise: $5.28 compared with $.08 ($p < .01$, Wilcoxon rank-sum test). Again, there is no statistically significant difference in average joint payoffs among pairs of subjects with a dictator who did not make a promise.

Average payoff differences are a measure of risk resulting from opportunism. Before subjects were assigned their roles, they did not know whether they would be assigned the role of dictator or recipient, and so they did not know whether they would be in a position to exploit the other subject or instead be exploited. Our results suggest that behind this

veil of ignorance, a risk-averse subject would prefer ED to NR, because it reduces the risk that he will be exploited by the other ex post (hypothesis 5.2).[23]

4.7. Subjects' Understanding of the Game

As we explained above, we went to considerable lengths to try to make sure that subjects understood the game. Of course, the question remains whether they did in fact understand the game. Figure 5 shows that average contingent cooperation in ED rises sharply at an investment level of 1 when a promise was made and remains high for higher investment levels. And Figure 4 shows that recipients become close to completely confident that a dictator will cooperate as soon as investment is greater than or equal to 1 when a promise was made. Both suggest that subjects understood their payoffs in ED. In NR, average contingent cooperation rates and average confidence levels rise more steadily as investment increases, which is consistent with our posited psychological lock-in effect.

5. DISCUSSION

5.1. Real-World Significance

Our results suggest that ordinary people are more willing to keep their promises the more they have been relied on by promisees and that promisees anticipate this effect of reliance and overinvest accordingly. But what about promises made in high-stakes transactions by sophisticated self-interested actors? What about promises made by collective entities like corporations? It is perhaps less plausible to suppose that such agents exhibit the guilt-averse preferences of the promisor in our model that gives promisees strategic reasons to overinvest in reliance on promises.

There are, however, two reasons to believe that our results may be more significant than they first appear. First, the agency relationships that pervade collective entities may boost the effects of psychological lock-in. This is because a corporation's agents may feel morally bound by the promises they make on behalf of the corporation. This would make it psychologically costly for them to break such promises, while the cost of keeping such promises will be borne by the corporation rather than its

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23. This result is likely to generalize because it arises from the fact that expectation damages insure the promisee against breach. In the absence of a legal regime, the promisee cannot avail himself of this kind of insurance.

agents (so long as the corporation has not introduced an incentive scheme for its agents that forces them to internalize those costs).

Second, if the underlying driver of a promisor's guilt tracks general beliefs about moral reasons to keep promises, we should expect third-party observers to judge a promisor more harshly when she breaks a promise that has been relied on more by the promisee. And if those third parties have the opportunity to punish such a promisor even at some cost to themselves, then, given theory and evidence that suggests that persons are willing to altruistically enforce social norms, we should expect that those third parties will punish the promisor more harshly the greater was the promisee's reliance.[24] Reputational concerns that track agents' moral beliefs would then make even self-interested promisors act as if they had guilt-averse preferences.

In Stone and Stremitzer (2019), we provide experimental evidence that confirms this hypothesis. Subjects were asked to imagine that they had observed various scenarios emerging from a game that was similar in crucial respects to that played by subjects in the current experiment.[25] The scenarios differed along two dimensions: whether the party making the cooperation decision (player A) promised the other (player B) that she would cooperate with B or merely stated an intention to do so while making explicit that she was making no promise and B's level of investment. After being presented with each scenario, subjects were asked to report the likelihood that they would choose to inflict a punishment on A for not cooperating with B at some small but not insignificant cost to themselves. Subjects saw the scenarios in a randomized order, which allowed us to generate both between-subject data (subjects' responses to the first condition) and within-subject data (subjects' responses in their entirety). The results suggest that third-party observers are more willing to punish a noncooperative player who has made a promise than one who has not, that their willingness to punish increases with the recipient's investment, and that there is something distinctively promissory about

24. Gintis et al. (2005) provide an overview of the theory and evidence that supports the existence of dispositions toward strong reciprocity. Fehr and Fischbacher (2004) provide experimental evidence of subjects' propensities to punish third parties at a cost to themselves.

25. We used a trust game instead of the more complicated exchange of conditional promises we used in the current experiment to simplify explanation of the scenario to participants. Otherwise the game was the same: one player, who may or may not have received a promise to cooperate from the other, made an investment decision prior to learning whether the other would in fact cooperate with him, and payoffs from investment and cooperation were the same as in the current experiment.

the latter effect, in that the willingness to punish increases more with the recipient's reliance if he received a promise. Thus, lock-in may arise even when promisors are self-interested if promisees increase their reliance investments to make it more likely that promise breaking will be punished by a third party.[26]

5.2. Competing Hypotheses

Our results suggest that, even in the absence of a legal regime that enforces relied-on promises, a promisee's reliance on a promise makes the promisor more likely to keep it. We have offered a theory of promisors' preferences that can account for this result. But there are other explanations that might lead to similar predictions even in the absence of any promising. In particular, the experimental literature on cooperative behavior in dictator games, ultimatum games, and public goods games suggests that cooperative behavior can arise from efficiency or equity preferences. Such preferences could cause the dictator to cooperate as investment increases and the recipient to overinvest in the absence of the promissory mechanism that we theorize is driving subjects' behavior.[27]

One reason to believe that our posited promissory mechanism is driving our results can be found in the existing literature on the effect of promisees' expectations on promise keeping. While the evidence as to whether a counterparty's expectations alone increase a person's willingness to cooperate is mixed (Vanberg [2008] and Ellingsen et al. [2010] find that they do not matter; Reuben et al. [2009] and Mischkowski, Stone, and Stremitzer [2019] find that they do), there is evidence that expectations certainly do increase a promisor's willingness to keep her promises when those expectations were induced by her promise (Ederer and Stremitzer 2017; Mischkowski, Stone, and Stremitzer 2019). To the extent that a promisee's reliance on a promise to perform has a similar effect on a promisor's willingness to cooperate as a promisee's expectation

26. Note that the purpose of this experiment was not to provide evidence that people engage in altruistic punishment but rather to provide evidence for the claim that to the extent people do exhibit such a propensity (as the literature suggests that they do), they will exhibit a greater willingness to punish promise breaking the more the promisee relied on the promise. That the costs of punishing were purely hypothetical may well weaken conclusions that can be drawn about the magnitude of subjects' willingness to altruistically punish others in the face of real stakes, but it is less likely to weaken conclusions that can be drawn about the relative strength of such a willingness as a function of our variables of interest.

27. See Online Appendix OB for a detailed description of how efficiency and equity preferences play out in our setting.

of performance, our model's assumptions are supported by these findings.[28]

The comparison between NR and C also provides evidence that there is something distinctively promissory about our lock-in effect. Control was identical to NR with the exception that participants were not able to send messages to each other. Thus, promising was entirely absent in C, and therefore comparing C and NR allows us to cleanly evaluate the influence of promising.[29] Consistent with the conjecture that there is something distinctively promissory about the lock-in effect, more recipients overinvest in NR compared with C: 22.7 percent compared with 13.9 percent. This effect is statistically significant ($p = .08$, Wilcoxon rank-sum test; $p = .03$, one-sided t-test). For a more detailed comparison of the two treatments that makes use of the belief and contingent cooperation data, see Online Appendix OB.

Finally, in Stone and Stremitzer (2019) we find not only that third parties are more willing to punish, at some cost to themselves, noncooperative promisors when their promises were relied on by promisees but also that the effect of reliance on the willingness of third parties to punish noncooperative action is greater when that noncooperation breaches a promise to cooperate as opposed to simply running counter to a prior statement of intent. To the extent that such altruistic punishment is driven by the same forces that drive promisors to keep their promises in the first place, this serves as further evidence that lock-in has promissory underpinnings.

5.3. Incidence of Promising

For reasons explained in Section 3.1 and Online Appendix OA, our model predicts that subjects will always make promises in ED, while they

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28. Reliance and expectations have a similar flavor, but there are important differences. Expectations do not have direct monetary payoff consequences. Expectations are also not as easily manipulable by the holder of those expectations. It is difficult to see how a recipient could strategically increase his expectations of cooperation to induce a dictator to cooperate, though a message by a recipient about his expectations could be used to strategically overstate his expectations (and, indeed, Ellingsen et al. [2010] allow subjects to send such messages).

29. In NR not all dictators made promises (and therefore not all recipients received them), so we have variability in data from this treatment that might seem to allow us to test whether there is something distinctively promissory about our lock-in effect. But because promises are generated endogenously, the unobserved characteristics of dictators who promise and those who do not may systematically differ, which prevents us from cleanly testing this hypothesis using these data.

will do so in NR only if they believe that the subject they are paired with has a sufficiently high guilt parameter. Therefore, we should expect no more promising under NR than under ED.

Contrary to this prediction, enforcement of relied-on promises with expectation damages causes a reduction in the number of promises that get made: 81.7 percent of subjects make promises in NR compared with 63.6 percent in ED. The difference is significant, although only at the 10 percent level ($p = .06$, Wilcoxon rank-sum test). This suggests that factors outside our model are driving promise-making behavior. This is unsurprising given that anticipating the full game at the beginning of the game requires a lot of sophistication on the part of subjects. If subjects myopically focus on the material costs of promising without weighing those costs against the potential benefits, rates of promising will be lower under ED in line with what we find.

The fact that we observe this difference between the regimes, however, does not undermine our central results: ED produces higher cooperation rates in aggregate than NR despite the lower incidence of promising. And the average increment of overinvestment when dictators made promises is significantly higher in NR than in ED: .57 compared with .16 ($p = .02$, Wilcoxon rank-sum test), which suggests that psychological lock-in is driving the greater average overinvestment in NR. Moreover, as explained previously, the comparison between C and NR suggests that promissory lock-in explains our results.

5.4. Strategy Method and Demand Effects

It is possible that some features of our design may have conveyed to subjects that dictators were expected to alter their responses in response to recipients' investment decisions, which might have induced subjects to behave in a manner conforming to our hypothesis. In particular, because we employed the strategy method to determine dictators' choices, we elicited dictators' preferences for all possible investment levels, including those not chosen by recipients. We also asked recipients to predict what dictators would do for all possible investment levels. And each subject experienced the roles of both dictator and recipient over the course of the experiment.[30]

30. It is inherently difficult to simulate the real-world salience of a variable in a lab experiment. Our design may have made a promisee's reliance more salient to subjects than it would have been in the real world. But it is also likely that observable reliance by the promisee will be highly salient to the promisor in the real world. Indeed, our the-

Yet nothing about our design suggested to subjects that they were expected to respond to the investment variable in a manner conforming to our hypotheses.[31] Thus, we think that our results still tell us something about the existence of a real-world effect, even though we cannot make inferences about its magnitude.[32]

6. IMPLICATIONS FOR LEGAL THEORY

6.1. Implications for Enforcement Theory

We find that in the absence of a legal regime promisors are more likely to keep relied-on promises. This result, in conjunction with our finding in Stone and Stremitzer (2019) that third parties also punish relied-on promises more harshly, suggests that people believe that it is morally worse to break a promise the more the promisee relied on it. Furthermore, promisees anticipate the lock-in effect of reliance on promisors and so overinvest to induce their promisors to keep their promises.

To the extent that one believes that law tracks prevailing moral intuitions, our results help to explain the emergence of legal doctrines that create liability for relied-on promises. The best example of such a doctrine in the common law is promissory estoppel, which gives significance to a promisee's reliance by treating it as a ground of enforcement.

Our findings can also be viewed as adding to the economic theory of legal enforcement. The standard view is that legal enforcement serves only to mitigate underinvestment that arises when a promisee fears that the promisor will not perform. Our findings point to a surprising addi-

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ory suggests that the promisee has a strong incentive to make any reliance known to the promisor. And other experimental work has shown that a promisee's expectations of performance are important determinants of promise keeping (Charness and Dufwenberg 2006; Ederer and Stremitzer 2017; Mischkowski, Stone, and Stremitzer 2019). A promisee's reliance and expectations are closely related phenomena insofar as the promisee's willingness is likely dependent on his expectations of performance.

31. Control made the investment level salient to subjects in exactly the same ways as NR did. Despite this similarity, the comparison between the two suggests that there is something distinctively promissory about the lock-in effect as explained above. Also of note is Brandts and Charness (2011, p. 387), which surveys experimental studies that employ both the strategy method and direct-response method (in which subjects respond only to realized values of variables of interest) and finds that "there are significantly more studies that find no difference across elicitation methods than studies that find a difference."

32. It is generally true of lab experiments that, while they can point to the existence of effects, ineliminable concerns about external validity prevent inferences being drawn about effect sizes. Reliable results about effect sizes require field experiments.

tional benefit of legal enforcement—a reduction of overinvestment that arises from a promisee's incentive to psychologically lock in a promisor. Thus, legal enforcement may reduce overinvestment at the same time as it also reduces underinvestment.

Of course, the recipient of a promise that is supported by consideration but unenforceable because of a lack of definiteness or a failure to satisfy the Statute of Frauds could ensure enforcement of the promise by persuading the promisor to make his promise more definite or ensuring that the Statute of Frauds is satisfied rather than by investing to psychologically lock in the promisor. But there are often significant transaction costs of making an agreement more definite and formal: formalizing details takes time, the parties' interests may diverge in the interim, and insisting on formality or further detail may be viewed by the promisor as a signal of the promisee's future litigiousness or some other negative characteristic, which could unravel the agreement.[33] By contrast, reliance on a promise is a unilateral act that is less likely to be viewed as an aggressive act at least if such reliance is reasonably foreseeable.

6.2. Enforcement-Induced Overinvestment

It might seem that the claim by Shavell (1980, 1984) that legal enforcement of promises with expectation damages produces overinvestment undermines the significance of our claim that such enforcement mitigates an overinvestment problem.[34] But Shavell's prediction is premised on there being a positive probability that performance will turn out to be inefficient ex post. This was not a feature of our experimental design, and in many real-world situations performance is always, or nearly always, the efficient choice. Shavell also assumes that the performance decision is binary. But when the performance variable is instead continuous, parties who know that their agreement will be legally enforced with expectation damages can set the performance term to ensure that the promisee has no incentive to over- or underinvest (Edlin and Reichelstein 1996).[35] Finally, as Cooter and Eisenberg (1985) point out, even when the perfor-

33. In the spirit of this suggestion, Spier (1992) argues that asymmetric information about a contracting party's type can drive contractual incompleteness.

34. Intuitively, a self-interested promisee invests the amount that would be optimal if performance were always efficient, because the promisee is fully compensated for breach, and so will fail to internalize the likelihood that performance will be inefficient when making his investment decision.

35. Intuitively, the promisee would have an incentive to underinvest if the agreed level of performance were 0 (equivalent to no enforcement) and would have an incentive to

mance variable is binary, the promisee's incentive to overinvest can be eliminated by limiting damages to the promisee's expectation had she not overinvested—something courts would arguably have the discretion to do in the promissory estoppel context where remedies may be "limited as justice requires" (*Restatement [Second] of Contracts,* sec. 90).

6.3. Mitigating Underinvestment

When a promisee can motivate the promisor to perform in the absence of legal enforcement simply by investing at the efficient level, the forces of psychological lock-in will mitigate underinvestment rather than produce overinvestment. Whether an enforcement regime's potential to reduce overinvestment is more or less significant than its potential to reduce underinvestment depends on the distribution of types of contracting parties and their beliefs about their contracting partners—specifically their guilt parameters. Notice, however, that enforcement should be able simultaneously to reduce the underinvestment of some and the overinvestment of others. Suppose, for example, that there are three agents: one who invests 0 in the absence of legal enforcement because he believes that his partner has a low guilt parameter, another who invests 1 because he believes that his partner has a large guilt parameter, and another who invests 2 because he believes that his partner's guilt parameter is in the intermediate range. Our findings suggest that legal enforcement can correct both problems simultaneously by encouraging the underinvesting agent to invest more and the overinvesting agent to invest less.

7. CONCLUSION

We show that reliance on promises matters, because many promisors are more likely to keep relied-on promises. Promisees, moreover, anticipate this, and accordingly some overinvest to motivate promisors to keep their promises. Thus, we show that there is a heretofore unappreciated benefit of legally enforcing relied-on promises with expectation damages: legal enforcement reduces the overinvestment that results when promisees invest to psychologically lock in promisors.

Our results also have a specific doctrinal implication. While the doctrine of promissory estoppel tracks our subjects' moral intuitions by

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overinvest if the agreed level of performance exceeded a certain level. The parties can balance the two effects by agreeing to an intermediate level of performance.

according legal significance to the promisee's reliance, the unexpected benefit of legal enforcement that we identify—the reduction in overinvestment—is less likely to be realized when a showing of reliance is made a condition for legal enforcement. This is because the tendency to overinvest is eliminated only if the promise is enforced, but if a promisee must invest to ensure the promise is enforced, then he may have to overinvest just to get the benefit of the legal regime. In *Maryland National Bank v. United Jewish Appeals Federation* (407 A.2d 1130 [Md. 1979]), for example, the court finds that the promisee charity did not have a viable promissory estoppel claim against the promisor's estate (the promisor having died before he had honored his promise to the charity), because it had engaged in prudent budgeting, failing to pledge any unpaid portions of the promised donation to its charitable projects, with the result that it had not in fact detrimentally relied on the promise. Had it instead pledged some of the promised funds to specific projects, even imprudently or prematurely, it might well have prevailed. Accordingly, it will often make sense for lawyers to advise their clients to engage in detrimental reliance after they have received a promise so that they can invoke promissory estoppel in the event that the promise is broken. This suggests that enforcement pursuant to the doctrine of promissory estoppel may create its own incentives for promisees to overinvest (see, for example, George and Korobkin 2016). Can these incentives to overinvest be eliminated by getting rid of promissory estoppel? Our findings suggest not: incentives to overinvest exist and might even be higher in the absence of legal enforcement. Therefore, reducing the scope of promissory estoppel will not solve and could well aggravate the overinvestment problem.

The solution lies instead in reducing the amount of reliance that is required to trigger enforcement of a promise or eliminating the requirement that the promisee rely on the promise entirely. Enforcing more promises eliminates a promisee's incentive to overinvest in order to psychologically lock in the promisor, and reducing the amount of reliance required to trigger enforcement makes it less likely that a promisee will have to overinvest to get the protection of the doctrine.

In line with this prescription, the doctrine appears to be moving away from requiring a showing of reliance. The second *Restatement of Contracts* dropped the first *Restatement*'s requirement that the reliance on the promise be of a "definite and substantial character" (*Restatement of Contracts,* sec. 90 [1932]). And in light of their survey of the case law, Yorio and Thel (1991, pp. 156–57) argue that courts do not in fact do a

searching inquiry for actual reliance in promissory estoppel cases, focusing instead on the extent to which reliance was reasonably foreseeable to the promisor as a marker of the promisor's seriousness when he made the promise. Farber and Matheson (1985, p. 910) likewise conclude that the "requirement of an identifiable detriment no longer defines the boundary of enforceability" when courts are applying the doctrine of promissory estoppel.

Thus, our results point to a novel reason why courts should be more willing to recognize promissory estoppel claims and less insistent on requiring reliance. But our results likely have wider applicability. We designed our ED treatment to mimic the operation of a promissory estoppel regime. Our main result, however, would likely generalize to a legal regime that enforced promises in the absence of any reliance. In this sense our results shed light on the benefits of contract enforcement more generally.
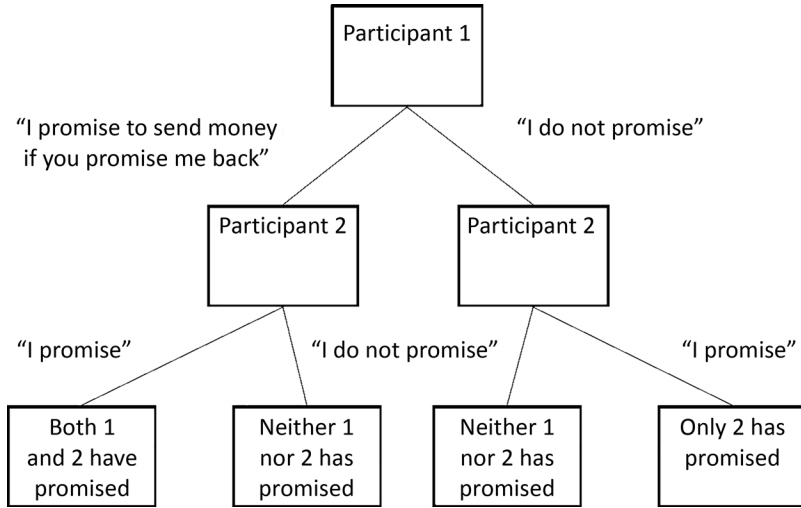
Figure A1. Communication stage

Table A1. How Player B's Payoffs from Stated Beliefs Were Determined

	Player A Will Choose to Send Money			Player A Will Choose Not to Send Money	
	Certainly	Probably	Unsure	Probably	Certainly
Player B's earnings if player A chooses to send money ($)	.65	.60	.50	.35	.15
Player B's earnings if player A chooses not to send money ($)	.15	.35	.50	.60	.65

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