

Rationalist Experiments on War*

KAI QUEK

sessions are conducted by different sets of people.
there are ten rounds in each session.
players are only informed the results of their strategies in each round after round 10.
rounds are made to be quasi-independent from each other.
any decline in the average number of wars between sessions is purely incidental
(tested subjects are different).
That said, the possibility of development of a feel for the game and its effects on
subject behavior is not eliminated. (material for future experimentation?)

Private information and the commitment problem are central to the rationalist theory of war, but causal evidence is scarce, as rationalist explanations for war are difficult to test with observational data. I design laboratory experiments to isolate the effects of private information and the commitment problem on the risk of conflict. I find that the commitment problem sharply increases the incidence of conflict, but there is no significant difference in conflict incidence with or without private information in the shadow of shifting power. I also investigate the realism of the positive experimental results with a case study of Japan's decisionmaking on the eve of the Pacific War. The permutation of formal, experimental and historical methods applies the strength of one method to compensate for the weakness of another. Convergent results from the different methods strengthen the causal inference.

Private information and the commitment problem are the fundamental causes of war in the rationalist literature (Fearon 1995). However, despite their theoretical prominence, there are few direct empirical tests of rationalist explanations for war. Do private information and the commitment problem cause conflict, and are their effects large? Do dyads blessed with both perfect information and credible commitments enjoy perpetual peace?

I offer evidence on these questions. The rationalist literature on war has made major advances theoretically, but empirical work has been severely limited by methodological challenges. It is hard to rule out the effects of unobserved confounders. It is also unclear how private information or the commitment problem may be accurately measured with observational data. Moreover, private information always exists in international politics, and we can never know every piece of private information held by each actor (as they are private). These issues do not have clear solutions. Yet it is important for scholars and policymakers to assess the empirical significance of rationalist explanations for war.

I use randomized experiments to isolate the causal effects of private information and the commitment problem on conflict. I translate the formal theory to a laboratory setting with real people, with some simplifications to make the theory experimentally testable. Experimentation enables direct manipulation of the information and commitment environments, whereas randomization controls for all observed and unobserved factors that may contaminate the causal inference. I also conduct a case study of the decisionmaking process in a historical crisis to investigate the realism of the positive experimental findings.

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I find that the commitment problem sharply increased the incidence of conflict. Despite the costs of war, decisionmakers with a commitment problem gravitated toward war. The causal effect was robust in both sequential and simultaneous-moves interactions. The effect of private information, however, was surprisingly subdued: the overall incidence of conflict in the shadow of shifting power was almost the same with or without private information.

I also design a field verification of the positive experimental results with a case study of Japan's policy deliberations leading to the Pacific War. The case includes a shock that created an unambiguous perception of an impending power shift. The United States had imposed a total oil embargo on Japan, which sharply reduced Japan's future relative power. Based on archival records of the secret deliberations before and after the embargo, I show how Japanese leaders quickly shifted to an active pro-war position against the United States when they were confronted by a commitment problem induced by the shadow of shifting power. The case study shows the realism of the commitment-problem mechanism in actual decisionmaking. It also detects important realistic complications unmodeled in the mechanism.

Together, the formal, experimental and historical findings point in the same direction: the commitment problem has a strong positive relationship with the decision for conflict. The mix of methods also shows how experiments can bridge two research traditions—formal models and historical studies—that typically have little contact.

RELATED LITERATURE

Why do rational states fight each other despite their incentives for peaceful settlements that would avoid the costs of war? Fearon (1995) highlighted two rationalist explanations: private information with the incentive to misrepresent makes it harder for states to locate a mutually acceptable deal, whereas commitment problems prevent two sides from striking a peaceful bargain in the present when one side has an incentive to renege in the future. These two explanations for war are believed to be “the full set of rationalist explanations that are both theoretically coherent and empirically plausible” (Fearon 1995, 380).¹ Since Fearon (1995), various formal models in international relations (IR) have incorporated and extended the implications of private information and commitment problems on war (e.g., Powell 1999; Wagner 2000; Filson and Werner 2002; Kydd 2003; Slantchev 2003; Smith and Stam 2004; Powell 2006; Leventoglu and Slantchev 2007). Both explanations are also prominent in the wider literature on conflict and cooperation in political science and economics.² In particular, the commitment problem appears to be pivotal in a vast variety of political phenomena, ranging from regime change and civil wars to international economic relations and the protection of human rights (e.g., Greif, Milgrom and Weingast 1994; Broz 2002; Walter 2002; Fearon 2004; Acemoglu and Robinson 2006; Jeanne 2009; Simmons 2009).

This study is the first to jointly evaluate Fearon's (1995) twin rationalist explanations in the laboratory. I focus on these two mechanisms as they are the most prominent in the rationalist literature on war, as well as the most well understood. The existing literature, however, offers no empirical guidance on what might happen when the two mechanisms interact: do these mechanisms mutually reinforce each other in reality? Is the effect of the commitment problem sharpened in the existence of private information—or does it not matter empirically? Using a

¹ A third rationalist explanation is issue indivisibility. Powell (2006) shows that the indivisibility problem is essentially one manifestation of the commitment problem.

² See survey articles in Powell (2002), Reiter (2003), Garfinkel and Skaperdas (2007), Jackson and Morelli (2011).

2×2 experimental design, I explore whether the commitment problem influences the risk of conflict in different information environments. It is useful to test the commitment-problem treatment in an idealized world of perfect information, but it is also practically relevant to understand whether and how the treatment operates in a setting with private information, which better approximates the world we live in. The 2×2 factorial design balances both considerations by testing the treatment separately under both information conditions. With the same design, we can also test whether private information has a significant effect on conflict with and without a commitment problem.

In IR, three recent game-theoretic experiments preceded this study. On the information side, Butler, Bellman and Kichiyev (2007) examined whether a status-quo-advantaged actor can achieve a better bargain under different information conditions, and found that the advantaged actor gets more when there is private information. Tingley and Wang (2010) experimentally manipulated the cost of backing down to study how players with incomplete information update their beliefs in a crisis bargaining game. These studies, while related, did not investigate the private-information mechanism proposed in Fearon (1995). On the commitment side, Tingley (2011) compared bargaining behavior at two different probabilities of repeated interaction (δ), and insightfully showed how bargaining might differ when the shadow of the future is high ($\delta = 0.7$) or low ($\delta = 0.3$). Given its specific research focus, however, the subjects' perception of a commitment problem is not fully eliminated. A subject can perceive a potential commitment problem in both experimental conditions, albeit at two different probabilities (0.3 or 0.7), as a positive probability of repeated interaction exists in both groups.

I construct the treatment condition under which players confront the commitment problem with certainty. For the control group, the key challenge is to shut down the commitment problem but keep the shadow of shifting power the same as in the treatment group. This is achieved through the removal of the proposer's ability to change its first-period promise in the second period, which constructs a perfect-enforcement device that eliminates all possibilities of renegeing.

In economics, the most relevant studies involve bargaining experiments such as the ultimatum-game experiment (e.g., Roth et al. 1991; Cameron 1999; Costa-Gomes and Zauner 2001). There is a sizeable literature on the effects of information asymmetry on bargaining. In general, there are more bargaining breakdowns when information is asymmetric, but the support for particular equilibrium predictions is sometimes weak (see Forsythe, Kennan and Sopher 1991). The significance of asymmetric information appears to depend on the particularities of the information environment and the specific form of interaction. The experiments here focus specifically on private information on the cost of war in the shadow of shifting power.³

In contrast, there are few experiments that focus on the commitment problem in bargaining games. The closest strands of literature in experimental economics are three. The first consists of experiments that focus on the communication of commitment, particularly the making of a promise and its generally positive effect on cooperation (e.g., Ellingsen and Johannesson 2004; Charness and Dufwenberg 2006; Vanberg 2008). These experiments are mostly silent on the commitment problem. The second strand focuses on experimental manipulations of the "shadow of the future" (e.g., Dal Bó 2005; Duffy and Ochs 2009; Tingley 2011). These manipulations can induce a commitment problem, although different in nature from that created

³ This allows us to jointly evaluate the effect of asymmetric information and the commitment problem with the same experimental design and model, as discussed earlier. Future work may test the effect of asymmetric information in other forms. For surveys of bargaining experiments conducted across information environments with different specific features, see Kennan and Wilson (1993), Roth (1995), Plott and Smith (2008).

by non-enforcement in the shadow of shifting power.⁴ The third strand of literature involves experimental manipulations of bargaining power, which involve variations in the payoffs for conflict between players that translate to variations in their bargaining strength (Binmore et al. 1991; Fischer, Güth and Pull 2007; Anbarci and Feltovich 2011). Although my experiments also involve variations in bargaining power, I focus on commitment elicitation, enforcement conditions and intertemporal power shifts, which are not emphasized in this literature.

I focus on a commitment problem in the shadow of shifting power, which scholars believe to be a potent cause of many wars in history (see Vagts 1956, Ch. 8; Powell 1999; Van Evera 1999, Ch. 4; Powell 2006). To my knowledge, there is no preceding work in experimental economics or political science that eliminates the perception of a commitment problem under shifting power to isolate its causal effect, nor is there any existing study that combines and jointly evaluates Fearon's (1995) twin explanations experimentally. This study attempts to fill these gaps in the literature.

The strengths and limitations of experiments with regard to internal and external validity are well rehearsed in political science. Appendix 1 summarizes and evaluates the debate in the context of IR experiments.

MODEL PREDICTIONS

I combine the information problem and the commitment problem in a joint model by integrating Fearon's (1995) models.⁵ The goal is to facilitate experimental testing based on the simplest possible model that combines both the information problem and the commitment problem under one structure. The results and intuitions of the joint model are similar to those in Fearon's (1995) separate models.

The joint model has three key features. First, it combines the information and commitment problems in a two-period model, instead of a single-period model for the information problem and an infinite-period model for the commitment problem, as in Fearon's work. A two-period model offers the simplest possible structure that generates the commitment problem, but avoids the practical difficulties of implementing infinite-period bargaining with human subjects. The second feature is the use of "perfect enforcement." The enforcement condition is defined such that Player A cannot make any change to its Stage 1 demand in Stage 2. Hence, the commitment problem is completely shut down in the enforcement condition. The use of a perfect-enforcement device eliminates any noise that might arise from the expectation of even a minuscule possibility of reneging. Third, I black box the war technology. Players end up with an absolute payoff if war is chosen, without having to deal with expected value calculations involving the probabilities of winning wars. With these changes, the experiment becomes cleaner in design as well as simpler for the players to play.

Consider a two-stage game in which two players bargain over a prize with the value v . In Stage 1, Player A makes a demand $x_1 \in [0, v]$. Player B observes x_1 and decides to agree or to fight. If B fights, the game ends with payoff $(w_{1A} - c_A, w_{1B} - c_B)$, with $w_{1A} = w_{1B}$, where w_{1i} and c_i are Player i 's war payoff in Stage 1 and cost of war, respectively. If B agrees, the game enters Stage 2 in which A confirms its demand $x_2 \in [0, v]$. B observes x_2 and decides to

⁴ A related work is by McBride and Skaperdas (2009), in which the winner of a Period 1 conflict would receive additional payoffs in future periods of interaction. The study found that conflict propensities increase as the continuation probability increases.

⁵ There are other bargaining models that can also be reformulated to incorporate both private information and the commitment problem (e.g., Garfinkel and Skaperdas 2000). I use Fearon's work partly because it is simpler and more general relative to other models, and partly because of its centrality in the rationalist literature on war.

agree or to fight. If B agrees to x_2 , the game ends with payoff $(x_2, v - x_2)$. If B fights, the game ends with payoff $(w_{2A} - c_A, w_{2B} - c_B)$, with $v = w_{1A} + w_{1B} = w_{2A} + w_{2B}$ and $w_{2A} > w_{1A} > w_{2B} > c$. Thus, A and B have equal bargaining power in Stage 1, but the bargaining power shifts in A's favor in Stage 2.

In the experiment, v is fixed at 10 and c_i is fixed at 2. For the war payoffs, $w_{1A} = w_{1B} = 5$, $w_{2A} = 7$ and $w_{2B} = 3$. These parameter values are chosen to minimize noise in the experiment. Payoffs are set within the integer range of $[0, 10]$ to simplify the mathematics for subjects and to reduce the likelihood of calculation error. Parameter c_i is set at 2 as it is the highest integer value by which all payoffs in the model remain in positive integer domain, given $w_{2A} = 7$ and $w_{2B} = 3$. The values of w_{2A} and w_{2B} are chosen to achieve equidistance from the midpoint. Note that other combinations of integer values will complicate the experiment by either breaking the equidistance or generating potential payoffs that are 0 or negative given the requirements of the model.⁶

I analyze the incentive structures implemented in the experiment condition by condition. The formal analysis is described in Appendix 2. It yields four predictions for experimental testing:

PREDICTION 1: War will be avoided if there is enforcement in the public-information condition.

PREDICTION 2: War will occur with certainty if there is no enforcement in the public-information condition.

PREDICTION 3 There is always a positive risk of war in the private-information condition with enforcement.

PREDICTION 4: War will occur with certainty in the private-information condition without enforcement.

EXPERIMENTAL DESIGN

I apply a 2×2 factorial design with controlled variation in information and enforcement. The first treatment variable captures the difference in information conditions; the second captures the difference in enforcement conditions.

Random Assignment

Subjects are randomly divided into *public-information* and *private-information* groups. Subjects in each group are then randomly allocated to either the *enforcement* condition or the *no-enforcement* condition. In each round, subjects are randomly assigned as decisionmakers for either Country A or B and they play with a randomly assigned opponent within the same information/enforcement conditions. To control for reciprocity effects, the matching of anonymous players is subject to a "stranger-matching" rule that no two players be matched with each other twice in the same scenario.

Experimental Conditions

- *Enforcement*: Player A cannot change its Stage 1 demand (x_1) in Stage 2. The computer will directly equate $x_1 = x_2$. This is perfect enforcement and it eliminates the commitment problem.

⁶ The exceptions are parameter values with $w_{2A} = 6$ and $w_{2B} = 4$, which create a minimal shift in bargaining power across the two stages; and with c_i set at 0 or 1, which would render war costless or extremely cheap. Future work may investigate the special cases not addressed in this experiment.

- *No enforcement*: Player A can change its Stage 1 demand in Stage 2. Hence, the commitment problem remains.
- *Public information*: The cost of war $c_A = c_B = 2$ is known to all players. Each player is told: “You know your Opponent’s cost of war. Your Opponent knows your cost of war.”
- *Private information*: Each player knows its own cost of war ($c_i = 2$), but does not know its opponent’s cost of war.⁷ Each player is told: “You do NOT know your Opponent’s cost of war. Your Opponent does NOT know your cost of war.”⁸ Players are told that the cost will be assigned once in Round 1 and thereafter fixed for all subsequent rounds in the game.

There are two important details to highlight for the private-information condition. First, players are explicitly told that the computer will assign the cost *once and only once* in Round 1, and thereafter the cost will be fixed for all rounds in the game.⁹ This is important to support the belief that costs are drawn from a discrete uniform distribution. With the present design, we can hold constant the effect of the cost of war, while making it natural for subjects to play with the same cost in different rounds. Second, in the private-information condition, there is an *incentive* for misrepresentation but no channels for communication. This is to ensure a clean experiment consistent with the model in the earlier section and with Fearon’s (1995) original model, in which there is an incentive to misrepresent but no opportunity to communicate. As Fearon (1995) emphasized, the private-information argument is *not* driven by the lack of opportunities to communicate.¹⁰ Future research may explore the effectiveness of different types of communication channels in dealing with private information under shifting power.

Experimental Sequence

Participants begin the experiment by reading the instructions on their computer screens. They are placed in the role of a decisionmaker bargaining with another country for a valuable prize worth 10 points. I frame the situation as one involving crisis bargaining and war to enhance the relevance of the experiment, as my research question deals directly with rationalist explanations for war. The war framing also helps to strengthen the inducement of utilities by reducing the potential other-regarding behavior widely documented in many bargaining experiments.¹¹ This is important as rationalist explanations for war do not assume other-regarding behavior.

⁷ Players are told that “[t]o generate the costs of war, the computer will assign one of the values {0, 1, 2, 3, 4} to you. Then, it will assign one of the values {0, 1, 2, 3, 4} to your Opponent.” In other words, all they know is that the c_i of their opponent will be one of the integers in the uniform distribution [0, 4]. Hence, the experiment evaluates the case of private information on non-catastrophic costs of war.

⁸ The instructions mention explicitly that the computer will assign the c_i parameter. No deception is involved, but the parameter is assigned at $c_i = 2$ rather than randomly assigned. The purpose is to control for the effect of c_i by design with c_i constant for all players, whereas sustaining the belief that the opponent’s c_i will be drawn from a uniform distribution based on the model in the Model Predictions section. Subsequently, subjects are asked to state their assessed probabilities of particular values of their opponent’s cost of war (Test Question 4 in Appendix 3). Their responses confirm that they were indeed thinking in terms of a uniform distribution.

⁹ In other words, when a player gets “cost = 2” in Round 1, he/she will expect a 100 percent chance of getting the same cost in all rounds. He/she, however, believes that there is only a 20 percent chance for all subsequent opponents to get a particular cost in the integer range [0, 4]. This belief was empirically verified in a subsequent test question.

¹⁰ Fearon’s original model excludes communication between A and B. Later, Fearon (1995, 412–4) considered a case where B can make an announcement to A, and showed that as the announcement has no payoff relevance, “in any equilibrium in which state A does not choose randomly among demands, A will make the same demand regardless of what state B says, and the ex-ante risk of war will remain the same as in the game without communication by state B” (Fearon 1995, 396).

¹¹ I thank an anonymous reviewer for highlighting this point.

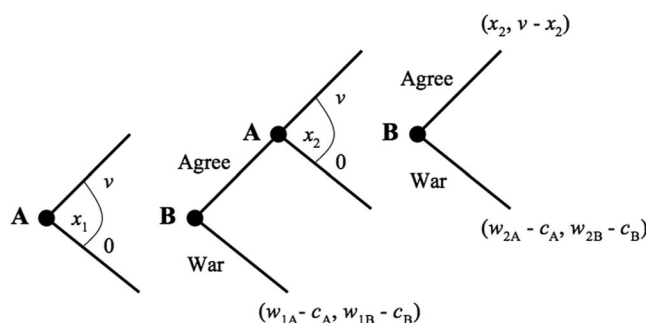


Fig. 1. Crisis bargaining game

Participants are informed that they will be randomly divided into dyads. They will be randomly assigned as a decisionmaker for either Country A or B in each round, and hence their paired opponent will also change at random after every round. The two-stage bargaining game (Figure 1) is carefully explained to the subjects, with the payoff difference between Stages 1 and 2 highlighted. In Stage 1, both countries are equally powerful: if they fight a war, each country will seize 50 percent of the prize (5 points) for itself. In Stage 2, Country A becomes more powerful, and if they fight a war, A will seize 70 percent of the prize (7 points). As war is costly, each country will lose points given the cost of war. The cost of war is known to all subjects in the public-information condition, whereas subjects in the private-information condition know their own cost but not their opponent's cost. If a peaceful bargain is reached, both sides avoid the costs of war.

Subjects stay in the same information condition for Rounds 1–10. For the enforcement manipulation, I use a crossover design with a sudden switch in the enforcement condition in Round 6. Hence, half the subject pool plays five rounds with enforcement before switching to five rounds with no enforcement; the other half plays in the reverse sequence. The crossover design is used often in clinical trials but rarely in political science. It offers two benefits in this study: the collection of both within-subjects and between-subjects data on the enforcement variable; and a replication test of the results with a sudden exchange of treatments. Findings that survive the crossover test are more robust than those that do not.

To summarize the experimental sequence:

- *Main experiment:* Participants play ten rounds. They play five rounds with enforcement and five without. They stay in the same information treatment as randomly assigned in Round 1.
- *Extensions:* After Round 10, participants play five more rounds under the public-information condition with an option for simultaneous moves.¹² This is an extension to test whether the effect of enforcement is generalizable and robust beyond pure sequential bargaining. After Round 15, participants play one round of the same game in the no-enforcement condition with timer treatments that induce differential time pressures (see Part 4 of Appendix 5 for details).
- *End:* The session ends with a risk-aversion game in the final round that measures individual risk preferences.

The full experimental instructions are reproduced in Appendix 3.

¹² Subjects in Session 1 played three rounds with the simultaneous-moves structure followed by three rounds with timer treatments. This feature facilitates a robustness check for Finding 4 in Appendix 5.

Implementation

70 students from Harvard University were recruited through the Harvard Decision Science Laboratory. The experiment was programmed on z-Tree, which allowed subjects to interact anonymously through computers (Fischbacher 2007). The experiment was conducted at Harvard University in February 2011. There were three sessions in total. Each session had 22–24 subjects. Each subject participated in only one session.

Special attention was paid to ensure that subjects fully understood the game. The instructions were written in neutral language, with questions at the end to check subject understanding. No practice rounds were implemented before the experiment. Participants were paid solely based on their performance in the game. The computer paid US \$0.50 for each point earned in nine randomly chosen rounds. On average, each participant earned US \$19.87 in the 1-hour session.

RESULTS

RESULT 1: The existence of a commitment problem causes a large increase in the incidence of war.

The incidence of war (war outcomes as a percentage of all outcomes) is 59 percent in the no-enforcement condition (with commitment problem) compared with 20 percent in the enforcement condition (without commitment problem) (two-tailed test of proportion, $p < 0.0001$, $n = 347$). Table 1 shows the incidence of war across the four experimental conditions in the first ten rounds. Table 2 shows the average number of wars per player in each condition. In the public-information condition, the average number of wars is more than thrice as high in the no-enforcement group than in the enforcement group. In the private-information condition, the average in the no-enforcement group is more than twice as high as the average in the enforcement group. Both differences are statistically significant at $p < 0.001$ (two-tailed t -test: $n = 72$ for public-information condition; $n = 62$ for private-information condition).¹³ Player average is used as the unit of observation as round outcomes may not be independent. Round outcomes will be analyzed with different model specifications later in this section.

Together, the data show strong tendencies toward war when there is a commitment problem in both public and private-information conditions (Predictions 2 and 4), and toward a peaceful bargain when there is enforcement in the public-information condition (Prediction 1).

Next we turn to the crossover test. If the causal effects are robust, we should expect an *increase* in the incidence of war when players switch from the enforcement condition to no-enforcement, as well as a *decrease* when they switch from no-enforcement to enforcement.

This is indeed what we see: the sudden introduction of a commitment problem caused a jump in the incidence of war, whereas the war incidence slumped when the commitment problem was removed. Figure 2 shows a sharp difference across Rounds 5 and 6 when a sudden exchange of treatments was introduced. The crossover test reversed the direction of the treatment effects and revealed a striking symmetry in the effects measured: the incidence of war increased from 22 percent (Round 5) to 67 percent (Round 6) when a commitment problem was introduced, and fell from 63 to 25 percent when the commitment problem was removed (two-tailed tests of proportion: $p = 0.0073$, $n = 36$ dyadic outcomes; $p = 0.0325$, $n = 32$,

¹³ The differences remain significant at $p < 0.01$ (two-tailed t -test: $n = 36$ for public-information condition; $n = 31$ for private-information condition) if the observations are restricted to only those before the crossover at Round 6.

TABLE 1 *Incidence of War Across Conditions*

	Public Information	Private Information
Enforcement	0.17	0.23
No Enforcement	0.63	0.55

Note: maximum possible value for the incidence of war is 1.00 (100 percent).

TABLE 2 *Average Number of Wars Per Player*

Session	Public-Information Condition		Private-Information Condition	
	Enforcement	No-Enforcement	Enforcement	No-Enforcement
1	1.00	2.17	1.33	2.67
2	0.83	3.67	1.00	3.20
3	0.67	3.67	1.33	2.33
Overall	0.83	3.17	1.23	2.74

Note: maximum possible value is 5.00 for each player.

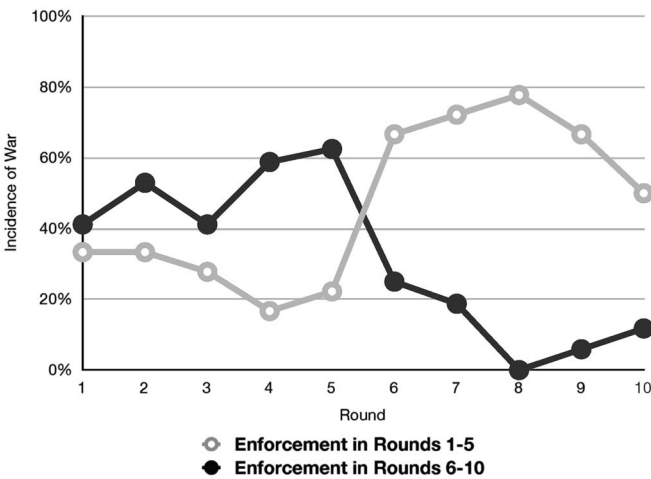


Fig. 2. Incidence of war in Rounds 1–10

respectively). Splitting the sample by information conditions, introducing a commitment problem in the public-information condition increased the incidence of war from 33 percent (Round 5) to 67 percent (Round 6), whereas the removal of the commitment problem caused it to fall from 56 to 11 percent.¹⁴ In the private-information condition, war incidence increased from 11 to 67 percent when the commitment problem was introduced, and fell from 71 to 43 percent

¹⁴ Within-subject difference in player war outcome (two-tailed paired *t*-test, $p = 0.0017$, $n = 36$). Between-period change in dyadic outcome with a switch from enforcement to no-enforcement (two-tailed test of proportion, $p = 0.1573$, $n = 18$). Between-period change with a switch from no-enforcement to enforcement (two-tailed test of proportion, $p = 0.0455$, $n = 18$).

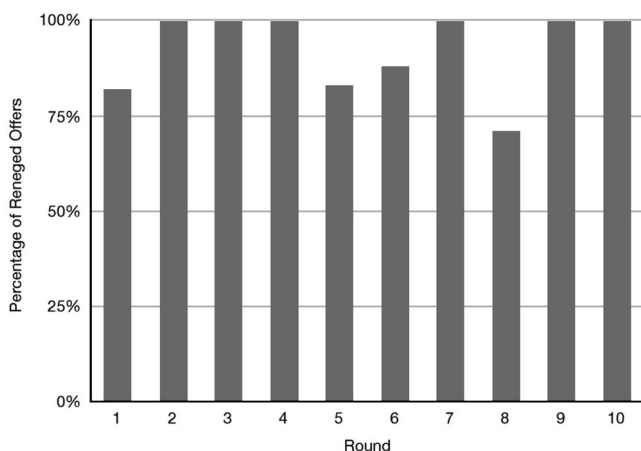


Fig. 3. Percentage of reneged offers (no enforcement)

when it was removed.¹⁵ Figure 2 also shows that the average incidence of war in the final rounds (Rounds 8–10) converged to 6 percent with an extremely low range of 0–12 percent when there is enforcement. By contrast, without enforcement, the incidence of war in the final rounds is 65 percent with a high range of 50–78 percent. Again the same story emerges: with enforcement, the dominant behavior revolved around the peaceful bargain; without enforcement, the dominant behavior was war.

Although the observations agree with the model predictions, Result 1 does not tell us whether the predictions were observationally correct because of the equilibria-generating calculations underlying the model, or because of other systematic factors that were not captured by the model. Hence, I test two observable implications derived from the strategic calculations behind Predictions 1, 2 and 4. If the hypothesized mechanisms are correct, we should expect that in the no-enforcement condition (1) players with a relative power shift *against* their favor will choose to fight in Stage 1 and (2) players with a relative power shift *in* their favor will renege on their Stage 1 agreement in Stage 2. These are indeed what happened.

RESULT 2: In the no-enforcement condition, wars in Stage 1 comprised 89 percent of all wars fought, whereas players with a power shift in their favor reneged on their promises 93 percent of the time.

Figure 3 shows the percentage of players who reneged on their promises across ten rounds in the no-enforcement condition.¹⁶ A player is coded as a “reneger” if he or she made a Stage 2 offer lower than his or her Stage 1 offer. Figure 4 shows that wars in Stage 1 comprised 89 percent of all wars fought in the no-enforcement condition.¹⁷ Taken together, the data

¹⁵ Within-subject difference in player war outcome (two-tailed paired *t*-test, $p = 0.0008$, $n = 32$). Between-period change in dyadic outcome with a switch from enforcement to no-enforcement (two-tailed test of proportion, $p = 0.0156$, $n = 18$). Between-period change with a switch from no-enforcement to enforcement (two-tailed test of proportion, $p = 0.2801$, $n = 14$).

¹⁶ In all, 82 dyadic observations with no war in Stage 1 in the no-enforcement treatment. In each round, there was a random matching of the participants into different conflict dyads (within the same treatment condition) subject to the “stranger-matching” requirement (see Experimental Design section).

¹⁷ In all, 103 dyadic observations with a war outcome in the no-enforcement treatment.

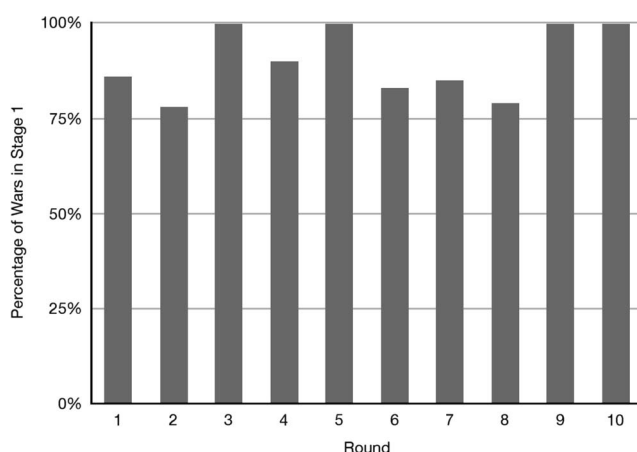


Fig. 4. Percentage of wars in Stage 1 (no enforcement)

suggest that the observed behaviors supporting Predictions 1, 2 and 4 were very likely motivated by the strategic calculations underlying the model.¹⁸

RESULT 3: The existence of private information has no significant effect on the incidence of war in the shadow of shifting power.

Pooling the data across enforcement conditions, the incidence of war in the private-information condition (39 percent) is almost identical to that in the public-information condition (40 percent) (two-tailed test of proportion, $p = 0.8374$, $n = 347$). From Table 1 earlier, war incidence is slightly higher under private information than under public information in the enforcement condition, and lower in the no-enforcement condition. Both differences are insignificant (two-tailed test of proportion: $p = 0.3033$, $n = 173$; $p = 0.2503$, $n = 174$, respectively).¹⁹ Figure 5 also indicates that differences in information conditions have no systematic association with the incidence of war across ten rounds.

For robustness check, I fit logit models to analyze the round outcomes. Each model uses one dyadic observation per round and session, with robust standard errors corrected for clustering at the subject level. The dependent variable is the decision for war in each round. The treatment variables are binary variables with the value 1 if there is enforcement (for the enforcement treatment) or public information (for the information treatment), and 0 if otherwise. I use different model specifications with and without round and session fixed effects, and with and without an interaction term for the treatment variables, a control for the size of Player A's initial offer and a control for individual risk preference. Risk preference is measured on a summed

¹⁸ Player responses to reneged agreements in Stage 2 also provide an indicative probe of player rationality. Faced with an opponent who reneged on the earlier agreement, players could choose to fight in Stage 2 out of emotion or miscalculation, or they could rationally assess the Stage 2 offer and accept it if it provided a higher payoff than their war payoff (1 point) in Stage 2. Player responses could thus be separated into the non-rationality category in the former and the rationality category in the latter; 89 percent of player responses fell into the rationality category, based on the percentage of peaceful responses to reneged offers paying more than the war payoff across ten rounds.

¹⁹ The differences calculated with player averages as observations remain conventionally insignificant under a two-tailed t -test ($p = 0.0847$ and $p = 0.1970$, respectively; $n = 67$ in each condition).

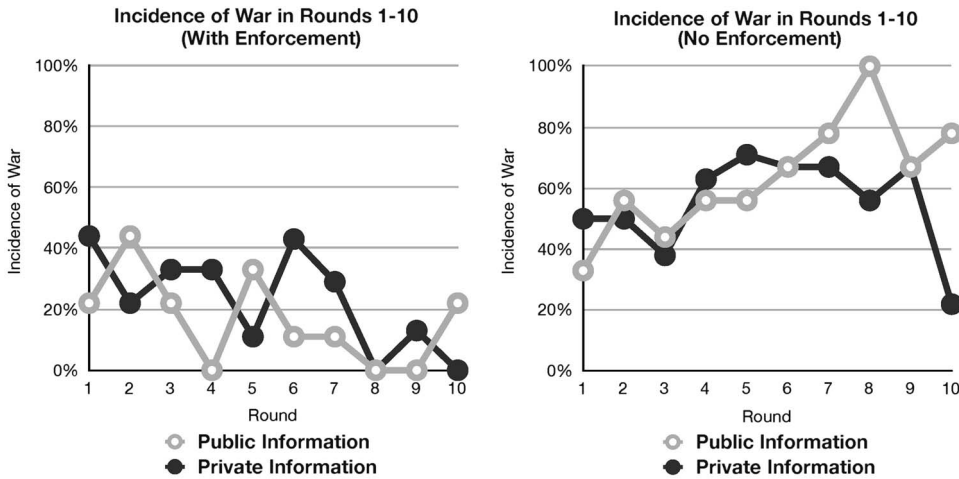


Fig. 5. Incidence of war (by enforcement conditions)

TABLE 3 *Logit Estimates of Determinants for the Decision for War (Rounds 1–10)*

	(1)	(2)	(3)	(4)
Enforcement	–1.809 (0.296)***	–1.422 (0.395)***	–2.434 (0.333)***	–2.088 (0.491)***
Information	0.044 (0.294)	0.350 (0.397)	0.001 (0.325)	0.270 (0.451)
Enforcement × information		–0.772 (0.601)		–0.653 (0.620)
Initial offer			–0.587 (0.216)**	–0.573 (0.216)**
Risk preference			0.078 (0.094)	0.075 (0.091)
Constant	0.088 (0.548)	–0.076 (0.567)	2.318 (1.195)	2.125 (1.214)
Round and session fixed effects	Yes	Yes	Yes	Yes
Probability > χ^2	0.000	0.000	0.000	0.000
Log-likelihood	–200.600	–199.394	–191.151	–190.333
Pseudo R^2	0.138	0.143	0.179	0.182
Observations	347	347	347	347

Note: In parentheses are heteroskedasticity-robust standard errors corrected for clustering at the subject level. Round and session dummies are used to control for round and session fixed effects.

*** $p \leq 0.001$, ** $p \leq 0.01$, * $p \leq 0.05$.

score based on the decisions made in the risk-aversion game (see Appendix 3) at the end of the experiment: the higher the score, the greater the individual willingness to take risk.

The information treatment has no significant relationship with the decision for war across all model specifications ($p > 0.30$). In contrast, the enforcement coefficient is negative and highly significant in all specifications ($p < 0.001$). Meanwhile, the negative and significant coefficient ($p < 0.01$) for the initial-offer variable suggests that Player B is less likely to decide for war when Player A makes a higher initial offer. The interaction term for the treatment variables is insignificant ($p \geq 0.20$) across all models. Table 3 shows the logit estimates with different combinations of control variables.

Result 3 suggests the relative non-importance of private information in the shadow of shifting power. The pertinent question is why. One possibility is that when players were placed in the two-stage game with shifting power, their attention was captivated by the power shift at the

expense of the information asymmetry. As the impending change in power gripped the minds of the players, their mental resources were diverted away from thinking about what the opponent's cost of war might be, given the limited working memory capacity of the human mind and the ease of attentional capture by a more dynamic and immediate stimulus (Yantis and Jonides 1984; Fukuda and Vogel 2009). This neural tendency might have thus weakened the effect of private information in the players' calculations. Of course, one can also interpret the result without appealing to human neuroscience. One may observe that in the game-theoretic literature the commitment problem often produces a sharp deterministic prediction of conflict, whereas private information does not. Indeed, Prediction 3 in my model is probabilistic, whereas Predictions 1, 2 and 4 are deterministic. Although the model clearly predicts war with certainty in the case of the commitment problem, it predicts only a positive risk of war in the private-information condition with enforcement. This is not unique to my model. Comparable private-information models of war, including that in Fearon (1995), generate similar probabilistic predictions. Based on differences in the nature of their model predictions, one might expect the effect of private information to be less decisive than the effect of the commitment problem. Hence, all we conclude is that the behavioral data provide no clear support for the causal impact of private information, based on private information on non-catastrophic costs of war in the shadow of shifting power.²⁰ This result applies only to this particular form of private information. Future work may investigate if a similar result holds for other types of private information.

RESULT 4: The treatment effect of the commitment problem is robust with interactions that allow for simultaneous moves.

After Round 10, I modified the game to allow for simultaneous moves. This extension study serves two purposes. Methodologically, it provides a stress test by moving beyond pure sequential bargaining. Substantively, it is motivated by the fact that, in reality, states can always choose to forego the bargaining process if they believe that diplomacy is futile. The modified game is more complex but provides a closer approximation to reality. The game is similar to the public-information variant of the original game, but with the option for simultaneous moves: Player A decides its demand, whereas B decides at the same time whether to wait for the demand (and subsequently accept or reject it) or to wage a war. The model predictions remain unchanged.²¹ Subjects were randomly assigned to either the enforcement or the no-enforcement condition. All subjects played five rounds under public information (i.e., everyone knows that $c_i = 2$).²²

The incidence of war in the no-enforcement condition is 73 percent, which far exceeds the incidence of 8 percent in the enforcement condition. The difference in the average incidence of war across players is statistically significant at $p < 0.001$ (two-tailed t -test, $n = 70$). Regression models

²⁰ In the private-information condition, Player A does not know c_B and Player B does not know c_A , but they know that the c_i of their opponent exists within a non-catastrophic range represented by the discrete uniform distribution $[0, 4]$.

²¹ In this game, Player B has three options in Stage 1: agree to go to Stage 2 after seeing Player A's offer; go to war after seeing A's offer; or go to war without seeing A's offer. With enforcement, B is always better off seeing A's offer, which eliminates the rationality of the third option. With no enforcement, A's offer is not credible, and hence the option to see A's offer becomes immaterial to B's decision. Given the elimination of options, the modified model can be solved in the same way as the original model to yield predictions convergent to Predictions 1 and 2.

²² Subjects in Session 1 played three rounds instead of five (see footnote 12). The treatment effect based on the incidence of war across players remains significant at $p < 0.001$ (two-tailed t -test, $n = 70$) with data based on the three rounds in common (Rounds 11–13).

TABLE 4 *Incidence of War (Rounds 11–15)*

Session	Public-Information Condition	
	Enforcement	No-Enforcement
1	0.00	0.67
2	0.17	0.88
3	0.03	0.63
Overall	0.08	0.73

Note: the maximum value for the incidence of war is 1.00 (100 percent). In Rounds 11–15, subjects played the crisis bargaining game with an option for simultaneous moves.

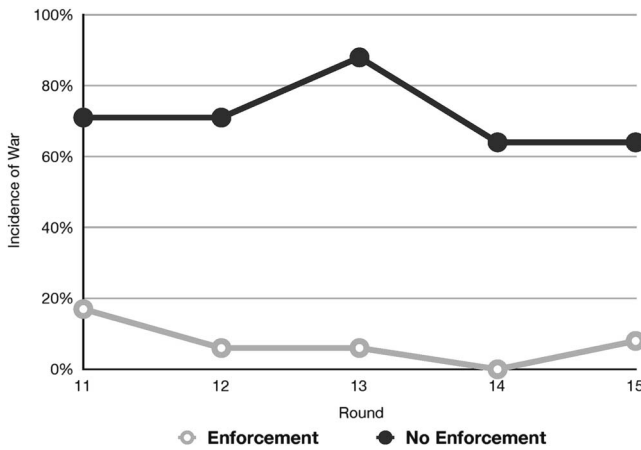


Fig. 6. *Incidence of war (Rounds 11–15)*

analyzing the round outcomes confirm the same result. I fit logit models with the war decision as the dependent variable and the enforcement variable as the regressor. Different model specifications are used, with and without round and session fixed effects, and with and without a control for the size of the initial offer and a control for risk preference. Table A1 in Appendix 4 displays the logit estimates conditioned on different sets of controls, showing that enforcement has a strong negative relationship with war outcomes across all specifications ($p < 0.001$).

Table 4 summarizes the incidence of war in each session. Figure 6 visualizes the large and persistent difference in the incidence of war across the two conditions.

Taking this result with the earlier observations, we can conclude with confidence that the commitment problem has a strong positive effect on the decision for conflict. The treatment effect is large even in an experiment without large stakes. In real-world crises, the stakes are dramatically increased. All else being equal, it is plausible that decisionmakers will be more payoff sensitive and will try to behave more (rather than less) rationally in a high-stake environment (see Appendix 1). If this is the case, the gravitation to the rational equilibrium—and war—may be even more pronounced.

CASE STUDY DESIGN

A laboratory experiment allows the researcher to construct the *ceteris paribus* condition often assumed in a scientific theory but rarely achievable in the real world, and is thus a powerful tool

for testing mechanisms. Yet, the scientific virtue of the lab also raises a reasonable concern on whether the identified effect—and the mechanism that predicts it—actually *matters* in the real world when a *ceteris paribus* setting does *not* exist. For instance, a mechanism may be valid but its causal effect sensitive or weak. The effect may be significant in a tightly controlled environment, but canceled out, reconditioned or interacted away by the circumstances found in a naturalistic setting. In this case, the experimental result may be theoretically validating but also practically misleading.

This concern may be addressed with *field experiments* (Fisher 1926; Campbell 1998; Levitt and List 2009; Gerber and Green 2012). In international politics—where field experiments are practically and ethically difficult—a feasible alternative is to seek *field verifications* of the experimental results. Specifically, in identifying causes of war—a rare event that can often only be retrospectively investigated—the feasible alternative may be to study the relevant historical processes with the most reliable and high-resolution evidence that we can find. If a decision-based mechanism is being investigated, evidence on the historical decisionmaking process should be gathered, and consistency between the predicted and revealed behaviors carefully evaluated. This is the logic behind the field verification design described in this section.

In the last section, I have shown that the commitment problem increases the risks of conflict in the laboratory. To what extent does the commitment problem shape decisionmaking in a real-world crisis?²³ I focus on three questions: (1) Does the commitment-problem logic operate in a real-world crisis triggered by an impending power shift? (2) How does the logic affect the decision process leading to war? (3) Does the case reveal important omissions in our theoretical model? Questions (1) and (2) help to direct the field verification exercise, whereas (3) extracts new information from the field verification.

I address the three questions with a case study of Japan's deliberations on the eve of the Pacific War, which involves a sharp power shift. I analyze the meeting records from the four imperial conferences preceding the Pacific War and the 38th–74th liaison conferences between the imperial conferences (translated in Ike 1967). Owing to space constraints, methodological considerations and case analysis are contained in Appendix 5.

The case analysis can be encapsulated in three key points.²⁴ First, Japanese leaders did not focus on war with the United States before the latter's imposition of a total oil embargo starting in August 1941. That focus quickly changed after the US embargo. The evidence shows a clear difference in the strategic calculations articulated at the imperial conferences *before* and *after* the embargo. In the July conference, the focus was on plans to establish the Greater East Asia Co-Prosperity Sphere and settle the war in China expeditiously, and the question of war with the Soviet Union. Japan's belligerence did not center on the United States; the conference heard no specific plan to attack the United States; and there was no clear statement of the commitment-problem logic that would be repeatedly applied against the United States in later conferences.

Second, the oil embargo triggered Japanese perceptions that its relative power would deteriorate, and that the United States cannot credibly commit not to exploit Japan in the future. The embargo left Japan with no doubt about its impending decline as Japan depended on foreign oil imports to sustain its war machine. The reference materials for the September imperial conference emphasized: "We need not repeat that at present oil is the weak point of our

²³ I focus on the commitment problem to test whether the positive finding in the experiment also holds in historical crises. It is difficult to isolate the private-information effect with observational data: all historical crises have occurred in environments with private information, and it is hard to compare and measure variations across different dimensions of the information environment with historical data.

²⁴ I thank an anonymous referee for suggesting the succinct synthesis.

Empire's national strength and fighting power.... As time passes, our capacity to carry on war will decline, and our Empire will become powerless militarily" (Ike 1967, 155). Stemming the relative decline was crucial, because "[e]ven if we should make concessions to the United States by giving up part of our national policy for the sake of a temporary peace, the United States, its military position strengthened, is sure to demand more and more concessions on our part; and ultimately our Empire will have to lie prostrate at the feet of the United States" (Ike 1967, 152). Hence, the September conference was warned that "military force should be used promptly if there is no prospect of diplomatic success. It is expected that the United States and Great Britain will try to delay us with diplomatic negotiations. We must be careful not to be inveigled into this trap" (Ike 1967, 155).

Third, the oil embargo created a strong time pressure that made Japanese leaders push for a quick decision for war. This led to a tight deadline for war and a truncated timeframe for negotiations. As Foreign Minister Togo concluded at the November imperial conference, "negotiations with the United States are very much restricted by the time element; consequently, to our regret, there is little room left for diplomatic maneuvering The prospects of achieving an amicable settlement in the negotiations are, to our deepest regret, dim" (Ike 1967, 214).

Taken together, three key findings emerge from the archival evidence: (1) Japan's decision for war involved strategic calculations that repeatedly applied the commitment-problem logic. (2) The perceived *speed of shift* in power influenced Japan's calculations under the commitment problem. (3) The speed of power shift made Japan truncate the bargaining timeframe, making it more difficult to achieve a diplomatic settlement. Finding 1 demonstrates the realism of the commitment-problem explanation. Findings 2 and 3 highlight a historically important factor—the "truncation effect" arising from a rapid shift in power—that was omitted in my model and experiment.

These findings may provide a useful additional interpretation on why Japanese leaders rushed into the gamble that was Pearl Harbor. Although Findings 1–3 are closely consistent with the archival evidence, traditional historical narratives of the Pacific War often do not develop the theoretical connections explicated in these findings. Meanwhile, historical analyses of the war by IR scholars emphasize the role of shifting power (Sagan 1989; Van Evera 1999; Trachtenberg 2006), but they do not explicate it within the framework of the commitment problem or highlight how it was the perceived *speed* of shift in power that made Japan push for a tight deadline, which truncated the bargaining timeframe, which in turn dampened the prospect of a diplomatic settlement (truncation effect).

It is important to be cautious about the causal mechanism suggested by the case findings, as constraints in the historical data do not allow the truncation effect to be cleanly identified. To probe further, I replicated the mechanism in a laboratory setting to test its significance. The experiment offers suggestive evidence that the truncation effect exists. The experimental design and results are summarized under Finding 4 in Appendix 5.

CONCLUSION

Private information and the commitment problem are central to the rationalist theory of war. The causal evidence, however, is scarce. I use an experiment to show that the commitment problem causes a large increase in conflict incidence in environments both with and without private information. The crossover design embedded in the experiment shows that a sudden introduction of a commitment problem causes a sharp rise in conflict incidence, whereas its sudden removal causes a sharp fall. Furthermore, in the absence of enforcement, players with a power shift in their favor renege on their agreements 90 percent of the time. The results also

show that despite the theoretical prominence of the private-information argument, private information on the cost of war has no significant effect on conflict in the shadow of shifting power. This raises the general question of whether and how the private-information mechanism exerts causal impact on war decisions, an important question that remains for further investigation. It would also be useful for future research to explore the efficacy of the private-information mechanism empirically in different forms and across different conditions.

The experiment shows that the commitment problem has a decisive effect on the incidence of conflict. A case study focused on the commitment problem investigates the historical realism of the experimental results. It shows how Japan's policy deliberations leading to the Pacific War were shaped by calculations based on the commitment-problem logic. The case study also reveals a mechanism for the Pacific War that remains untheorized in the literature. The mechanism highlights how the perceived speed of shift in power made Japanese leaders push for a tight deadline, which truncated the bargaining timeframe and made diplomatic settlement more difficult. A supplementary experimental probe offers suggestive evidence for the mechanism.

Convergent results from the formal, experimental and historical investigations strengthen the causal inference: The commitment problem has a strong and positive relationship with conflict incidence. Despite the costs, decisionmakers with a commitment problem gravitate toward conflict. Thus, impending power shifts can be very dangerous. In general, power shifts are perilous when there is no enforcement structure to suppress the incentive to renege on prior agreements. The existence of this incentive dissolves the willingness to trust and makes costly conflict more likely. I have focused specifically on interstate conflict, but the strategic form of the commitment problem isolated here is extremely general. As a consequence, the findings may have potential implications for other forms of conflict—ranging from civil conflict to contentious politics—that involve the commitment problem in a similar form.

A key policy implication is that enforcement can calm the commitment problem and reduce the risk of costly conflict. In IR—where there is no world government to enforce bargains—the lack of external enforcement is often seen as the root of international anarchic violence. Yet, external enforcement can also occur without a world government, insofar as there is a third-party actor willing and able to play the role. Several cases in history have shown how external intervention can help to calm civil wars and bring about peace in violent situations (e.g., Regan 2002; Walter 2002). Nevertheless, the nature of international enforcement and its multiple possible forms have yet to be fully theorized and rigorously tested in the existing literature.²⁵ It is hoped that future research can bridge the gap and shed light on how commitment problems may be cured in theory and in practice.

A key methodological implication is the importance of crossing methodological lines by designing an integrated hybrid of different methods to investigate the same phenomenon. This study uses the experimental method to bridge two major research traditions—formal models and historical studies—that usually have little real contact. Methods can be combined in different ways, and this study shows the utility of a particular permutation (models–experiments–cases) that has received very little attention in political science. First, formal models clarify the strategic logic and generate equilibrium predictions. Next, experiments isolate the causal effects and detect systematic deviations from the predictions. Finally, case studies verify the substantive significance of the experimental results and highlight important realistic omissions

²⁵ External enforcement can take different forms (Quek 2013): for instance, a “hard” form when costs are imposed through military force, or a “soft” form when costs are imposed through economic and diplomatic measures. Theoretically, both forms would promote peace by calming the commitment problem, but scholars of international security typically focus more on the former.

in the modeled mechanism. Theoretical insights are compared and cumulated as we iterate between formal models, randomized experiments and historical cases.

This article demonstrates how this particular permutation may strengthen causal inference in the study of war and peace.²⁶ Formal models are often criticized for their deviations from observed behavior, laboratory experiments for their lack of realism and case studies for their causal confounds. A combinatorial hybrid of the three can apply the strength of one to compensate for the weakness of another, and thus make a more robust contribution to our scientific knowledge. Such multi-method integration, however, remains scarce in political science. It is hoped that more would recognize the scientific value of interrogating the same phenomenon with different and independent methods, and offer encouragement to those who cross the methodological lines that divide our discipline.

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²⁶ Although I do not know of any earlier work with the same permutation of methods in international security, the civil-conflict literature offers an example of a similar combination in Walter's (2009) study on reputation.

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