Pirates of the Mediterranean: An empirical investigation of bargaining with asymmetric information

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We investigate the effect of delay on prices in bargaining situations using a data set containing thousands of captives ransomed from Barbary pirates between 1575 and 1692. Plausibly exogenous variation in the delay in ransoming provides evidence that negotiating delays decreased the size of ransom payments, and that much of the effect stems from the signalling value of strategic delay, in accordance with theoretical predictions. We also structurally estimate a version of the screening type bargaining model, adjusted to our context, and find that the model fits both the observed prices and acceptance probabilities well.

KEYWORDS. Bargaining, piracy, ransom.

JEL CLASSIFICATION. D23, K42, N45.

Throughout history individuals and governments have negotiated and paid ransoms to secure the release of prisoners and property. This practice remains alive and well as evidenced by the large sums of money raised by terrorists and modern-day pirates through ransoms in recent years. For example, ISIS is believed to derive 20% of its revenue from ransom payments. Similarly, the terror group al-Qaeda obtained roughly 125 million dollars from ransoms between 2008 and 2015 and Somali pirates received 360 million dollars in ransom payments between 2005 and 2012.[1]

[1]For the ISIS statistic, see http://www.newsweek.com/2014/11/14/how-does-isis-fund-its-reign-terror-282607.html; the al-Qaeda estimate can be found at http://www.cnn.com/2015/01/20/opinion/bergen-

Many of these ransom negotiations have been prolonged, imposing significant costs on the involved parties. Ransom negotiations for the release of individuals captured by Somali pirates provide one recent example of this phenomenon. Although delayed negotiations expose captives to greater mistreatment, such delays have been common, with the average duration in captivity climbing to eight months in 2011 (One Earth Future (2012)).

Why are negotiating delays common in ransoming and other bargaining environments? The theoretical bargaining literature suggests the role of asymmetric information (Sobel and Takahashi (1983), Fudenberg, Levine, and Tirole (1985), Gul, Sonnenschein, and Wilson (1986), Admati and Perry (1987)). The central idea is that the same amount of delay is more costly for buyers with a higher valuation, hence delay can credibly signal to the seller that the buyer's evaluation is low. While this explanation is intuitively appealing, it has been difficult to empirically substantiate that negotiating delays lead to lower prices (e.g., Kennan and Wilson (1989)).

In this paper, we use a historical data set on thousands of captives ransomed by Spanish ransoming teams from the North African-based "Barbary pirates" to investigate the empirical relevance of dynamic bargaining models with asymmetric information in ransoming situations. This historical setting is interesting for a few reasons. First, the large number of ransomed captives in our sample as well as detailed information on these captives—including information held only by the Spanish negotiating team—allows for a uniquely detailed empirical analysis. Second, the poor communications of the day provide plausibly exogenous variation we use to identify the effect of a delay in negotiations on a captive's ransom. Finally, the bargaining environment we analyze suggests that the results are likely to be applicable to many ransoming and bargaining situations today, which are characterized by one-sided private information.

Formally, we investigate a "screening" type dynamic bargaining game (Sobel and Takahashi (1983), Fudenberg, Levine, and Tirole (1985)) in which only the uninformed player (in our case, the seller) makes offers. We extend the most basic specification of the screening bargaining model in various dimensions, so as to fit it better to our setting. These are that (i) we assume that the time between bargaining rounds (corresponding to Spanish rescue missions) is random, according to a Poisson arrival process, (ii) we allow for a positive reservation value for the seller, (iii) we consider physical depreciation of captives, on top of standard discounting, and (iv) we allow for a positive probability that funds for rescuing a captive do not arrive in time for the first bargaining opportunity after the person was captured. These extensions do not change the qualitative implications of the screening bargaining model. In particular, as long as there is a gap between the smallest buyer valuation and the seller's reservation value, there is a unique sequential equilibrium in which the seller proposes a decreasing sequence of prices. Moreover, negotiations end in a finite number of rounds (that depends on the parameters of the model), with the last price offer being equal to the lowest buyer valuation.

Our empirical investigation is twofold. In the first part, we focus on establishing that negotiating delays caused a decrease in equilibrium prices. We do this for two reasons.

schneider-isis-ransom-new/ and the Somali statistic can be found at http://shippingwatch.com/carriers/article6194367.ece.

First, this prediction is common to all rational models of bargaining when the relevant private information is on the buyer's side, not only the specific model we propose. Second, as noted above our historic data set allows us to exploit the poor communications of the day to derive a plausibly exogenous source of delay. Specifications using this variation help address endogeneity issues that are thought to have biased estimates of the relationship between delay and prices in previous studies.

Using data on thousands of captives ransomed in Algiers, Algeria we find that on average the Spanish paid less for a captive the longer he had been in captivity (which is one of our two proxies for negotiating delay). Although this correlation is consistent with the claim that delay led to lower prices through signalling low buyer evaluation, there are clearly other possible explanations for this result. One of these is that there were multiple types of captives that the pirates could tell apart, and negotiations for types of captives with a higher value lasted a significantly different amount of time than negotiations for types with a lower value.

To address such concerns we use an instrument for delay that is rooted in the slow speed of travel in pre-industrial Spain. The family and friends of captives whose hometowns were closer to the cities where the bargaining teams were based were likely to learn about an individual's captivity with less delay—and to remit the necessary ransom funds sooner—than those whose hometowns were farther afield. A similar relationship held for individuals closer to ports commonly used to sail to Algiers. Thus, the funds to rescue a given individual were likely to reach Algiers more quickly the closer the individual's home was to these cities. We argue that the pirates could not distinguish between this distance-induced delay and strategic delay.

We use the relevant distances to construct an instrument for delay and find that a year's increase in captivity was associated with roughly an 8% decrease in a captive's ransom price. As opposed to this, we find that a year's increase in the age of a captive at the time of captivity is associated with about a 1% decrease in ransom price. Since qualitative sources suggest that the pirates were careful to preserve the value of captives they hoped to ransom, this suggests that most of the decrease in ransom price over time was due to the signalling value of delay on the part of the buyer.

The available data are consistent with the validity of the exclusion restriction underlying the instrumental variable (IV) regressions. In particular, in a subsample of the data, we observe one component of the buyer's evaluation directly: the amount of earmarked money that the captive's friends and relatives collected for rescuing the captive. Results on this subsample are similar to those in the broader sample, suggesting that systematic differences in unobserved valuations are not driving our results. Our empirical use of information that only one of the parties possessed adds to a growing empirical literature on adverse selection that aims to collect and utilize such information (Finkelstein and McGarry (2006), Finkelstein and Poterba (2014), Abramitzky (2009)). To our knowledge, ours is the first paper to empirically use such information in the context of bargaining under asymmetric information.

In the second part of the empirical analysis, we structurally estimate the dynamic bargaining model we propose. In particular, we search for the parameters of the screening model that maximize the likelihood of observing the prices in our data and the num-

ber of ransoming trips before captives were ransomed. This approach has two advantages. First, it uses more information to identify parameters than the reduced-form approach. In particular, it directly uses the information on the distribution of the number of negotiation rounds. Second, it yields estimated structural parameters, which we use to analyze the distribution of the trade surplus and to evaluate alternative trade mechanisms.

The results show that our screening model can match well with both the observed prices and the distribution of missed ransoming trips. The estimated parameters indicate that there was substantial information asymmetry between the Spaniards and the pirates, and that the first offer price was significantly lower than the median valuation. We also find that for a high share of captives (31%), ransom money was not available during the first trip, and that the pirates' reservation value of captives was much lower than the Spaniards' median valuation. Computed allocation of the trade surplus shows that the Spaniards were able to capture the bulk of the surplus (54% of the total), the pirates obtained only 32% of the surplus, and 14% of the surplus was lost due to delay in bargaining. This indicates a relative efficiency of the bargaining process.

Using the estimated parameters, we also compute the surplus allocation had the pirates sold captives in bundles of 10 or committed to a take-it-or-leave-it offer (which would have required coordination among different slave holders). The result shows that selling captives in bundles would have resulted in a higher surplus allocation to pirates and lower delay costs. Committing to a single offer would have resulted in a higher surplus allocation to pirates, but it would have implied significantly higher delay/termination costs. Given that the majority of captives in our data were not ransomed during the first ransoming trip, we interpret this result as an indication of the pirates' inability to commit to a single offer.

The remainder of the paper proceeds as follows. Section 1 discusses the related literature. Section 2 provides an historical overview, while Section 3 introduces the theoretical model. Section 4 describes the data and presents our reduced-form empirical results, while in Section 5, we structurally estimate the proposed bargaining model. A final section concludes.

1. Related literature

Our results are most closely related to the empirical literature on bargaining under asymmetric information. Much of this literature has relied on experiments (Neelin, Sonnenschein, and Spiegel (1988), Ochs and Roth (1989), Mitzkewitz and Nagel (1993), Straub and Murnighan (1995), Croson (1996), Guth, Huck, and Muller (1996), Rapoport, Sundali, and Seale (1996), Schmitt (2004)) and generally finds that play strays from equilibrium predictions. These papers compellingly argue that the main reason for this is that many subjects exhibit other-regarding preferences, and, in particular, reject offers that would give them less than what they regard as a fair share of the surplus. One important advantage of our setting is that it is reasonable to assume that the professional bargaining teams on the Spanish side and the private slave holders on the Algerian side only cared about their own physical payoffs.

The non-experimental empirical literature has also faced challenges. In particular, existing studies have struggled to establish a negative relationship between the length of negotiations and prices. For example, Card (1990) found virtually no relationship between agreed upon wage and the length of negotiations in an analysis of Canadian employment contract data for the period 1964–1985. Although McConnell (1989) finds a statistically significant negative relationship between average wage settlements and average strike duration using US contract data for the period 1970–1981, this relationship is sensitive to model specification. Our results robustly suggest that delay had a causal effect on prices, thus providing evidence consistent with one of the central predictions of the theoretical literature.

Our work also contributes to a recent string of papers that structurally estimate dynamic bargaining models with asymmetric information: Sieg (2000), Keniston (2011), and Larsen (2014).[2] Similarly to our paper, Sieg (2000) investigates a situation with one-sided private information, but in a setting in which the uninformed party can only make one offer and rejection leads to a court case decided by a jury. Keniston (2011) and Larsen (2014) investigate situations with two-sided private information. Because of the complexity of dynamic bargaining games with two-sided asymmetric information, these papers do not estimate equilibrium strategies; instead they try to recover the basic parameters of the bargaining games in more indirect ways.

More distantly, our work is related to studies of the determinants of bribes and extortion payments (Hsieh and Moretti (2006), Olken and Barron (2009), Rose-Ackerman (2010)). Although ransom payments are believed to stimulate predation in weakly institutionalized polities with significant welfare impacts (Besley, Fetzer, and Mueller (2015)), their determinants are poorly understood. The evidence presented in this paper suggests the relevance of bargaining theory in explaining ransoming outcomes.[3]

2. Historical background

Between the 16th and 19th centuries, the Barbary pirates preyed on commerce and coastal populations in the Mediterranean and Atlantic. These pirates derived important revenues from the sale of captured cargoes and captives, affecting both trade and coastal settlement patterns for centuries (Tenenti (1967), North (1968), Friedman (1983)). Recent scholarship estimates that the pirates captured and enslaved over one million individuals between 1530 and 1780 (Davis (2001, 2003)).[4]

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[2]Less related are the works of Watanabe (2009) and Tang and Merlo (2012), which estimate complete information bargaining games. There is also an earlier literature that computes point estimates of parameters of dynamic bargaining models based on US data on wage negotiations: see Fudenberg, Levine, and Ruud (1985) and Kennan and Wilson (1993). See also Merlo, Ortalo-Magne, and Rust (2015), who estimate a dynamic model with asymmetric information and adopt a reduced-form assumption about bargaining behavior.

[3]In a broader sense, our results speak to a growing literature that investigates piracy from an economic standpoint (Leeson (2007, 2009), Hillmann and Gathmann (2011)). Like these studies, our paper suggests the relevance of economic theory in explaining the actions of pirates.

[4]Since the Barbary pirates operated with the support of their local governments we should technically refer to these pirates as corsairs. For expositional simplicity, however, we follow popular convention and use the term pirates. For a detailed treatment of the history of the Barbary pirates, see Julien (1970), Abun-Nasr (1977), Bono (1998), Davis (2003), Panzac (2005), and Weiss (2011).

The city of Algiers (in modern-day Algeria) was an important center of pirate activity on the North African coast. Following its establishment as a center of piracy in the early 16th century, it was home to thousands of individuals who had been captured by pirates and subsequently sold into slavery.

Two primary factors determined the price of captured individuals in the Algerian slave market. The first of these was related to the present value of a captive's marginal product. Older captives were valued less and captives with special skills (such as carpentry) commanded higher prices. The second factor was a slave's potential for ransom. As this potential increased with a slave's social status, slave traders and potential buyers examined both the possessions and bodies of the captives in detail in an attempt to ascertain their social status. The Algerians also provided incentives to fellow captives to correctly identify high-ranking captives.

Once a captive had been sold into slavery, his captors encouraged him—or a fellow captive on his behalf if he was illiterate—to write home to secure ransom payments. Merchants, ransomed captives, and returning Spanish ransoming expeditions carried these letters to Spain (Hershenzon (2011, pp. 64, 65)).

How long did it take for this information to reach a captive's home? Although it is impossible to exactly measure, delay increased with the distance from the captive's home to what we refer to as the *bargaining bases*. These cities were the three ports commonly used to travel from Spain to Algiers (Alicante, Cartagena, and Valencia) and the two cities (Madrid and Seville) in which the Spanish bargaining teams were based (e.g., Martínez Torres (2004, pp. 106–107)). The distance-induced delay in the arrival of news of a loved one's capture could be significant. For example, even if the bearer of the letter went directly from the bargaining base to a captive's home by land, he would have on average covered about 13 kilometers per day (Grafe (2012, p. 110)). In practice, this speed is likely an upper bound on the speed with which the news of an individual's capture traveled.[5]

Once the news of an individual's capture had reached home, the local community had various means to raise ransom funds. For the most part, the brunt of the financial burden for an individual's ransom lay with his family. To raise the necessary funds, family members resorted to a variety of strategies such as selling property, taking out loans, or using the dowries of unwed daughters. Those who were unable to raise the necessary funds could beg or directly petition the government for aid.[6]

Most families entrusted their ransom funds to one of the two Catholic religious orders who transported the funds to Algiers and negotiated the ransom payments on a family's behalf (Martínez Torres (2004, p. 79)). As with the news of an individual's capture, the time required to transport ransom funds to these religious orders seems to have increased with the distance of a captive's home from the bargaining bases (e.g., Anaya Hernández (2001)).

In sum, after a captive had been captured and sold in the Algerian slave market, the distance to the bargaining bases affected the delay with which his ransom money

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[5]For example, it is probable that distance also increased the likelihood of a letter being lost. The loss of letters also contributed to overall delay as captives routinely had to write many times before letters reached their destination (Hershenzon (2011, pp. 63–64)).

[6]The "government" in this case was primarily the *consejo de cruzada*, which was centered in Madrid.

reached Algiers in two ways. First, it increased the delay with which his family learned of his captivity. Second, it increased the time necessary to transfer funds to the religious orders that negotiated ransoms in North Africa.

2.1 *Negotiations in Algiers*

After arriving in Algiers, the Spanish ransoming teams focused on ransoming two groups of individuals. The first group included those "earmarked captives" whose families and friends had raised funds for their ransom. Funds for the ransom of these captives on average accounted for 40% of all ransom funds (Friedman (1983, p. 115)). The second group of captives were ransomed using the remaining funds, which came from alms and bequests. Some of these funds could be used at the discretion of the religious orders, although a portion were to be used for the ransom of specific types of captives such as women, children, clerics, or soldiers.[7]

Before the ransom negotiations began, the ransoming team was instructed to "visit the dungeons where the miserable captives live [...] and identify all the Christian vassals of the King [of Spain...] their home towns, names [and] the names of their parents" (mss2974, f. 4) and to note those captives they wished to ransom.[8] The Spanish seem to have done this for every captive possible, in part to obscure the identity of the captives they wanted to ransom.[9]

> At the start of the negotiations, the Algerian government required the Spanish to ransom some of its slaves at inflated prices. After this, the Spanish were generally free to negotiate ransoms with private Algerian slave owners. When an agreement was reached, the Spanish recorded the relevant information in a book and gave the slave owner a signed piece of paper. At the end of the negotiations, the Spanish paid the slave owners and the ransomed slaves returned with the negotiating team to Spain (mss2974, f. 6).

Although the Algerians knew that the Spanish preferred to ransom certain types of captives and could often identify the highest-ranking individuals (Friedman (1983, p. 151)), there is evidence that they faced uncertainty regarding which captives the Spanish wanted to ransom and how much the Spanish were willing to pay. For example, surviving instructions to the ransoming teams consistently advise the negotiators to "delay the ransom [...] and pretend to not be interested in the captives that they most want to

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[7]We have not found much information regarding the incentives faced by the bargaining team, although it is clear that "good" performance on the ransoming expeditions could lead to promotion (e.g., Martínez Torres (2004, p. 94)). Very broadly speaking, the bargaining team appears to have attempted to maximize the number of "desirable" captives ransomed given the available funds. These desirable captives were broadly speaking both those who had earmarked funds as well as those who belonged to a desired captive type (i.e., women, children, etc.).

[8]Throughout, archival entries prefaced with l are from the Archivo Històric Nacional, còdices. The number after l details the legajo. Archive entries prefaced with mss are from the Biblioteca Nacional de Madrid. The number after mss gives the manuscript number. For details, see the Appendix, available in supplementary files on the journal website, http://qeconomics.org/supp/655/supplement.pdf and http://qeconomics.org/supp/655/code_and_data.zip.

[9]Lists with the physical descriptions of earmarked captives further helped the negotiating team correctly identify these captives (Martínez Torres (2004, p. 41)).

rescue [...since the Algerians after this delay] will often sell their slaves for less than they thought they were worth" (mss2974, f. 5). These instructions seem to have been followed in practice as evidence has survived of the ransoming teams leaving captives in captivity for longer to obtain lower prices. For example, in the record of one ransoming mission from the end of the 16th century, the scribe notes that some earmarked captives were not ransomed in that trip because their prices were "too high" (l. 122, f. 159r).

3. The theoretical model

We model ransom negotiations between Spanish rescue teams and captive holders as dynamic bargaining games with asymmetric information. In particular, the relevant private information is the exact value of a given captive for the rescuers. Our motivation here is that the value of a particular captive for the Spaniards always had a component not known by the slave owners: the amount of earmarked money that was collected for a given captive. Over time, the captors could learn the distribution of this private value conditional on observables of a captive, but not the exact value for individual captives. In contrast, other important parameters of the bargaining process, such as the parties' time preferences and transaction costs, or reservation values of different types of captives for the holders, could either be observed by the parties through public information (such as interest rates charged by money lenders or the price that a certain type of captive could be sold for at slave markets) or learned over time.[10]

For now, we also assume that the negotiation for every captive is a separate game and is independent of all other negotiations. This is motivated by the fact that the captives in our data set were held by many different slave owners, who negotiated with the rescuers separately. In Section 5, where we structurally estimate the model, we investigate how much slave owners could gain by bundling their captives and negotiating for their collective release.

To keep the analysis tractable, we consider the simplest modeling framework for dynamic bargaining with one-sided asymmetric information, in which only the player with no private information (the seller) makes offers, standardly referred to as a screening type bargaining model.[11] Accepting an offer ends the game, while rejection implies that the game moves to the next period, where periods represent ransoming trips. We note that the negative relationship between the length of the negotiations and the agreed upon price, which we focus on testing in the reduced-form analysis of Section 4, is an implication of not only the model described here, but of any rational model of dynamic bargaining with one-sided private information on the buyer side.[12]

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[10]Captive-holders might have a privately known individual-specific evaluation for a certain type of captive, exceeding market price. However, for common type captives, the thickness of the market implies that they could purchase additional captives of the same kind until the marginal benefit became equal to the market price.

[11]Sobel and Takahashi (1983) introduced a finite version of the model, while Fudenberg, Levine, and Tirole (1985) and Gul, Sonnenschein, and Wilson (1986) extended the analysis to infinite horizon. These models are incomplete-information extensions of the dynamic bargaining models proposed by Stahl (1972) and Rubinstein (1982).

[12]For discussions of this point, see, for example, Card (1990) and Kennan and Wilson (1993).

Motivated by specific features of the bargaining environment we investigate, and to facilitate structural empirical investigation, we extend/modify the most basic specification of the screening model, described, for example, in Section 5.1 of Kennan and Wilson (1993), in four directions. First, instead of a fixed time lapse between bargaining periods, we assume that bargaining opportunities come stochastically, according to a Poisson arrival process. Second, we allow the seller's outside option to be strictly positive. Third, we allow for physical depreciation of the captives over time (besides standard discounting). Last, we allow for the possibility of a liquidity constraint in that the funds for rescuing a captive arrive after a delay, in which case the buyer cannot accept any first period proposal. Here we assume that the arrival of funds is private information; hence the seller does not know whether rejection of a first period offer is due to a temporary lack of funds or a low valuation for the captive. The first extension essentially does not affect the analysis, as a game with random bargaining opportunities can be translated to an expected payoff-equivalent standard deterministic discrete-time bargaining game. In fact, in the theoretical analysis we work with the notationally simpler discrete-time version of the model, but in the structural analysis we use stochastic bargaining opportunities, as the time between bargaining trips varied and was influenced by random events. The second and third extensions are standard, and given the parameter restrictions below, they do not affect the qualitative predictions of the model. However, they are important for the validity of the structural estimations and for the resulting welfare analysis. The extension to the possibility of a liquidity constraint complicates the calculation of the initial offer of the seller in equilibrium, but continuation games after the first bargaining period are equivalent to bargaining games with no liquidity constraint (with an appropriately updated distribution of types).

Formally, our general model is a continuous-time bargaining game, starting with a bargaining opportunity at time 0 (time is normalized to 0 at the first bargaining opportunity). Subsequent bargaining opportunities arise randomly, according to a Poisson arrival process with arrival rate λ. If at time s there is a bargaining opportunity, the seller makes a price offer y_s, immediately followed by an acceptance or rejection response by the buyer. Let $v \geq 0$ be the seller's flow reservation utility, let $r > 0$ be the common discount rate, let $x \geq 0$ be the common depreciation rate, and let b denote the buyer's privately known time-zero valuation. We assume that b is distributed according to a cumulative distribution function $F(\cdot)$ with support $[\underline{b}, \overline{b}]$, where $f(b) = F'(b)$ is the associated probability density function. We impose $v/(r + x) < \underline{b}$, implying that the buyer's valuation always strictly exceeds the seller's outside option.[13] Finally, we assume that the buyer is liquidity constrained and unable to accept the offer at time 0 with some probability $\pi \in [0, 1]$.

This continuous-time game can be mapped into a discrete-time game with equivalent expected payoffs, in which bargaining opportunities arise deterministically, at $t = 0, 1, 2, \ldots$, with common discount factor $\delta = \frac{\lambda}{\lambda+r}$ and depreciation factor $\beta = \frac{\lambda}{\lambda+x}$. For ease of exposition, and given the payoff equivalence, below we focus on this discrete-time representation.

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[13]This assumption makes the analysis simpler, and it is also plausible for the type of captives we focus on in the empirical analysis.

The game has a unique sequential equilibrium, analogous to a similar result in Gul, Sonnenschein, and Wilson (1986).[14] The equilibrium has the feature that negotiations end at some finite period T, determined endogenously by the parameters of the model. In periods $1, \ldots, T$, the seller proposes a strictly decreasing sequence of prices $p_1, \ldots, p_T$, such that p_T is exactly equal to the lowest buyer valuation at time T. Buyers are partitioned into T intervals, where the kth highest interval corresponds to buyers who accept the seller's offer in the kth period. Relative to a basic screening model, the extensions we introduce do not change the qualitative conclusions of the model.[15] The possibility of a liquidity constraint on the buyer side changes the initial price offer of the seller, and hence all subsequent offers, but in a way that corresponds to strategies in an out-of-equilibrium continuation game in the unique sequential equilibrium of the game with no liquidity constraint.

Below we demonstrate the above results by analytically solving for the unique sequential equilibrium when $\beta = 1$ (no depreciation) and the buyer's valuation is uniformly distributed on $[\underline{b}, \overline{b}]$. For a general characterization of sequential equilibrium, with positive depreciation and a general distribution of buyer valuations, see the Appendix.

First consider the case of $\pi = 0$ (no liquidity constraint). Since $p = \underline{b}$ in the final bargaining period, we can compute the upper bound on the remaining types such that $p = \underline{b}$ is optimal for the seller. From the first-order condition for the optimality of charging p in the last round,

$$p \geq \delta \underline{b} + \frac{1}{2}\big(v + (1 - \delta)X\big); \tag{1}$$

hence the upper bound on remaining types before the final round, for the optimality of $p = \underline{b}$, is $X = 2\underline{b} - \frac{v}{1-\delta}$.

Let b_t^* denote the threshold valuation such that the buyer is indifferent between accepting and rejecting in period t. The price in the next-to-last period, p_{T-1} must be such that b_{T-1}^* is indifferent between accepting this price in period $T - 1$ and waiting until the last period, which leads to $p_{T-1} = (1 - \delta)b_{T-1}^* + \delta \underline{b}$.

Continuing in a similar fashion, types $b_2^*, \ldots, b_{T-1}^*$ and prices $p_1, \ldots, p_{T-2}$ can be determined recursively:

$$b_{t+1}^* = \frac{1}{2}\left(b_t^* + \frac{v}{1 - \delta}\right), \quad t = 1, 2, \ldots, T - 2, \tag{2}$$

$$p_t = b_t^*(1 - \delta) + \delta p_{t+1}, \quad t = 1, 2, \ldots, T - 1. \tag{3}$$

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[14] Our assumptions correspond to what they label the "gap case" in their paper. Note that while the basic model of Gul, Sonnenschein, and Wilson (1986) analyzes subgame perfect Nash equilibria of a game in which a durable goods monopolist is selling its product to a continuum of consumers, as discussed on p. 170 of their paper, the same analysis applies to sequential equilibria of a bilateral bargaining game between a buyer and a seller, where the buyer's evaluation is private information.

[15] In particular, the proof of Theorem 1 in Gul, Sonnenschein, and Wilson (1986) can be extended to our setting. Since the steps of the proof are completely analogous to those in the original proof, they are omitted.

Now consider $\pi \in (0, 1]$. In this case, the posterior in the second period is the prior up to the cutoff for acceptance in the potentially constrained period (where there is a kink), and is a "flattened" version of the prior from the kink to $\bar{b}$. Suppose now that there is a cutoff of $\tilde{b}_1$ in the first period in the original liquidity-unconstrained problem, such that the posterior with $\tilde{b}_1$ is the same as in the liquidity-constrained problem with b_1^* for all $b \in [\underline{b}, b_1^*]$ (i.e., for any valuation below the kink). Since the marginal return below the kink is the same in the two problems, the optimum b_2^* is that which corresponds to $\tilde{b}_1$ (and will be below the kink). Therefore, since the game will resemble the original case from $t = 2$ on, we can express the future prices and cutoffs as

$$b_t^* = \frac{1}{2^{t-1}}\tilde{b}_1 + \left(1 - \frac{1}{2^{t-1}}\right)\frac{v}{1-\delta},$$

$$p_t = \delta^{T-t}\underline{b} + \frac{(1-\delta)(1-(\delta/2)^{T-t})}{2^{t-1}(1-\delta/2)}\tilde{b}_1 + \left(\frac{1-\delta^{T-t}}{1-\delta} - \frac{1-(\delta/2)^{T-t}}{2^{t-1}(1-\delta/2)}\right)v,$$

where the "effective" cutoff in period 1 is

$$\tilde{b}_1 \equiv \left(\pi\bar{b} + (1-\pi)b_1^*\right) = \pi\bar{b} + \frac{1-\pi}{1-\delta}(p_1 - \delta p_2).$$

The seller's payoff if the game ends in t is $(1-\delta^{t-1})\frac{v}{1-\delta} + \delta^{t-1}p_t$, so the objective function can be given as

$$\max_{b_1^* \in [\frac{1}{2}(\bar{b}+\frac{v}{1-\delta}),\bar{b}]} \left\{(1-\pi)(\bar{b}-b_1^*)p_1 + \sum_{t=2}^{T-1} \frac{\tilde{b}_1 - \frac{v}{1-\delta}}{2^{t-1}}\left[\frac{(1-\delta^{t-1})v}{1-\delta} + \delta^{t-1}p_t\right]\right.$$

$$\left. + \left[\frac{\tilde{b}_1 - \frac{(1-2^{T-2})v}{1-\delta}}{2^{T-2}} - \underline{b}\right]\left(\frac{(1-\delta^{T-1})v}{1-\delta} + \delta^{T-1}\underline{b}\right)\right\}.$$

The optimal choice of b_1^* can be derived by taking the first-order condition and algebraically manipulating it (we omit these steps here to save space). With b_1^* known, the remaining $b_2^*, \ldots, b_{T-1}^*$ can be calculated as in the case without the liquidity constraint from (2), and the prices $\underline{b} = p_T, p_{T-1}, \ldots, p_2$ can likewise be calculated as before from (3). Then the initial price offer can be computed from $p_1 = (1-\delta)b_1^* + \delta p_2$.[16]

Last, the above solution is only valid if the correct T is used. Hence, the full solution is that which simultaneously satisfies the expressions above as well as

$$T = \arg_t b_{t-1}^* \in \left(\underline{b}, 2\underline{b} - \frac{v}{1-\delta}\right)$$

for the computed b_{t-1}^* given T.

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[16]In the Appendix, we also show that this price sequence is decreasing.

4. Reduced-form estimates

Our data come from surviving records of the notaries who accompanied 22 ransoming missions to Algiers between 1575 and 1692.[17] The Spanish crown appointed these notaries, who was responsible for keeping detailed records of all financial transactions and verifying their accuracy. These records are believed to be accurate and have been described as "extremely thorough" (Friedman (1983, p. 107)).

The ransom record of Juan Antonio Sandier from the year 1667 is a representative ransom entry. It reads, "Juan Antonio Sandier son of Juan de la Peña and of Luisa Rodríguez from Valladolid of 41 years of age and 15 months of captivity [...] his ransom cost 160 pesos of which 50 pesos came from earmarked money [...] the remainder came from the alms of the holy cathedral of Valladolid" (mss3586, f. 62). In this entry we learn that Juan Antonio Sandier was ransomed after 15 months of captivity for the price of 160 pesos.[18] In addition, his family (or friends) had sent 50 pesos for his ransom. The remaining funds came from alms collected in the cathedral of his hometown of Valladolid.

Using thousands of similar entries, we have identified 4680 individuals ransomed in 22 ransoming expeditions. The Appendix provides a detailed description of the data construction along with a list of summary statistics and correlations.[19]

To investigate the effect of delay on ransom prices, we estimate an equation of the form

$$\ln(ransom_{ib}) = \alpha_b + \beta timecaptive_{ib} + \boldsymbol{\gamma}'\mathbf{x}_{ib} + \varepsilon_{ib}, \tag{4}$$

where i indexes individuals and b indexes ransoming trips. The variable $\ln(ransom)_{ib}$ denotes the natural logarithm of a captive's ransom price, and α_b denotes ransoming trip dummies that we include to account for trip-specific unobservables such as the possibility that some negotiating teams were more skilled than others. The variable $timecaptive_{ib}$ is the time an individual spent in captivity before he was ransomed and is a proxy for negotiating delay that is used in this section.[20] The vector $\mathbf{x}_{ib}$ contains a set of individual-level covariates. These variables are explained in the Appendix.

It is important to stress that although only some captives were ransomed, there is no selection bias in our context, as we are interested in the effect of delay on ransom prices conditional on being ransomed.[21] We begin our regression analysis in panel A of Table 1 by comparing ransomed captives within trips. Throughout this section coefficients in

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[17]We omit ransoming missions after 1700 because after this date the ransoming missions are thought to have had different procedures, expenditures, and goals than those prior to this date (Martínez Torres (2004, p. 34)). These changes may have been related to a decline in the military power of the pirates toward the end of the 17th century as documented in Chaney (2015).

[18]The silver peso (also known as the *real de a ocho*, piece of eight, or Spanish dollar) was a currency unit in the Spanish empire.

[19]Throughout, we limit the sample to the 4378 captives for whom a full ransom was paid. See the Appendix for details.

[20]We use this metric instead of the number of missed ransoming trips in this section to directly test the hypothesis that the coefficient on time in captivity is distinct from the affect of aging. Results are qualitatively similar, however, if the missed trips metric is used.

[21]Of course, this means that the results may not tell us much regarding the effect of delay on hypothetical ransom prices for captives that were never ransomed.

equation (4) are multiplied by 100 for ease of exposition. In column 1, we present results from a regression that omits all covariates with the exception of an individual's age at capture and trip dummies.[22] The point estimate implies that a year increase in captivity is associated with a 1.18% decrease in the ransom prices. This is significantly different from the coefficient on age at capture, which implies that a year increase in an individual's age is associated with a 0.63% decrease in that individual's ransom. In column 2, we add the additional controls (these include profession dummies as well as female and child indicator variables) and note that the results are qualitatively similar. Throughout, we report standard errors clustered by year of capture.[23]

While these results provide evidence of a negative correlation between time in captivity and the size of the ransom, there are many reasons to doubt this correlation is causal. Perhaps the most obvious possibility is that the Spanish simply waited longer to ransom less valuable captives. Fortunately, we have been able to identify the amount of money sent from Spain for 908 captives. Although historical evidence suggests that this represents roughly half of all the earmarked captives, the subsample of captives that we have identified as earmarked provides a useful check on the general results for at least two reasons. First, inasmuch as the omission of earmarked money in the sources was random, these results will be representative of the entire earmarked subpopulation. Second, in this earmarked sample, we are able to directly control for the quantity of money sent to ransom each earmarked individual. This information was only held by the Spanish, and we consider it as an additive term to the rescuers' valuations for the given captives.

In Figure 1, we provide a plot of the logarithm of ransom prices against the logarithm of the amount of money sent for each captive (we have partialled out trip fixed effects for both variables). As the figure shows, a 1% increase in earmarked money on average increased ransom price by 0.5%, increasing the relative share of the surplus that the Spanish could keep. While this figure provides evidence for information asymmetries, the strong correlation between earmarked funds and ransom price also suggests that these earmarked funds are a reasonable proxy for the ransoming team's private valuation. This is because qualitative evidence suggests that the pirates could extract a significant amount of this private valuation (from the condition in which an individual was captured, from information provided by other captives, etc.); thus the positive correlation is both expected and encouraging.

In column 3 we restrict the sample to these earmarked captives and control for a quadratic function of the logarithm of earmarked funds (as the relationship between the two appears to be approximately quadratic). When we do this, the standard errors increase. In columns 4–6, we restrict the sample to individuals from within mainland

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[22]In addition, we omit individuals who have missing distances to the bargaining bases for comparability between the IV and ordinary least squares (OLS) estimates.

[23]Given that we always include trip fixed effects, we are most worried about within-year correlation as many individuals caught in the exact same circumstances were ransomed in different trips. However, we have also experimented with double clustering by both this dimension and by trips (Cameron, Gelbach, and Miller (2011)). A drawback of this approach is that we only have 22 trip clusters and we are not aware of work addressing situations in which there is multi-way clustering and few clusters.

TABLE 1. Time in captivity, distance to bargaining bases, and ransom prices.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Panel A: OLS			
Years captive	−1.18	−1.05	−0.93	−1.23	−1.07	−0.82	−1.05
	(0.14)	(0.13)	(0.37)	(0.17)	(0.18)	(0.47)	(0.18)
Age at capture	−0.63	−0.59	−0.54	−0.79	−0.71	−0.59	−0.70
	(0.08)	(0.07)	(0.12)	(0.09)	(0.09)	(0.14)	(0.10)
ln(earmarked)			−94.33			−99.56	
			(20.06)			(19.62)	
ln²(earmarked)			9.59			10.00	
			(1.48)			(1.44)	
p-value	[0.00]	[0.00]	[0.29]	[0.03]	[0.06]	[0.63]	[0.07]
				Panel B: IV			
Years captive	−7.43	−6.66	−7.87	−16.24	−10.59	−7.50	−9.86
	(2.39)	(1.91)	(2.40)	(6.71)	(4.85)	(4.21)	(5.18)
	[−14.16, −3.55]	[−9.46, −2.64]	[−14.23, −3.03]	[−42.53, −6.94]	[−14.62, −1.56]	[−17.33, 8.98]	[−29.35, −0.63]
Age at capture	−0.91	−0.94	−0.85	−1.39	−1.21	−0.81	−1.17
	(0.15)	(0.15)	(0.17)	(0.30)	(0.26)	(0.23)	(0.25)
ln(earmarked)			−93.57			−101.66	
			(18.70)			(18.69)	
ln²(earmarked)			9.46			10.07	
			(1.35)			(1.35)	
p-value	[0.00]	[0.00]	[0.00]	[0.02]	[0.04]	[0.10]	[0.08]
				Panel C: First Stage			
Ldis	0.25	0.31	0.34	0.15	0.18	0.23	0.29
	(0.06)	(0.06)	(0.07)	(0.05)	(0.05)	(0.08)	(0.13)
N	4220	4220	876	2474	2474	564	2474
Clusters	127	127	100	120	120	78	120
Controls?	No	Yes	Yes	No	Yes	Yes	Yes, cities
Sample	All	All	All	Castile	Castile	Castile	Castile

Note: The dependent variable in panels A and B is the logarithm of captive's ransom whereas that in panel C is years in captivity before ransom. The row *p*-value in panels A and B presents the *p*-value for the null hypothesis that the coefficient on years in captivity is the same as that on age at capture. *L*dis is the logarithm of 1 plus the minimum distance from the captive's home to the bargaining bases. Standard errors are clustered by year of capture. Coefficients in panels A and B are multiplied by 100 for ease of exposition.
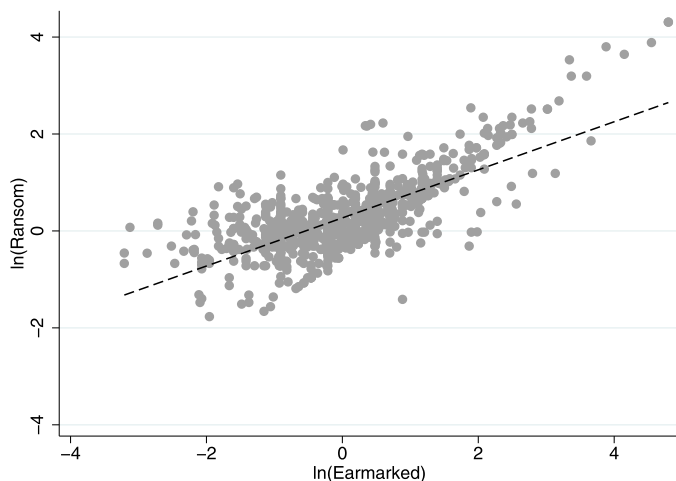
FIGURE 1. Observed ransoms and earmarked funds. The dashed line provides the fitted values of the regression of ransom prices on earmarked funds, which implies that a 1% increase in earmarked funds is associated with a 0.5% increase in ransom price.

Castile as a robustness check, given that the ransoming missions concentrated on freeing Castilian captives.[24] Here we simply note that these results are qualitatively similar to those in columns 1–3. Thus, the results in panel A of Table 1 provide evidence of a negative correlation between time in captivity and ransom prices, although in some specifications we cannot reject the null hypothesis that this correlation is simply due to the effects of aging.

Despite our ability to control for the ransoming team's private valuations in the earmarked sample, there are still reasons to doubt that the results reflect the causal effect of negotiating delay on ransom prices. First, there is the obvious issue of measurement error. We are using time in captivity as a proxy for bargaining delay, when in reality many captives who were ransomed after longer delays were sent to regions where the ransoming teams did not travel and were only ransomed when they were sold to owners in Algiers (Friedman (1983, p. 45)). Inasmuch as this noise is random, it will attenuate the coefficient on time in captivity. Second, there is the issue of reverse causality. We conjecture that this simultaneity bias is likely to bias the results upward. This is because, conditional on the ransoming team's valuations, individuals whom the pirates initially overvalued should be ransomed later than those whom they did not overvalue.[25]

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[24]The mainland of the former Kingdom of Castile is located in the western two-thirds of modern-day mainland Spain. The boundaries of the kingdom are shaded grey in Figure 2 (here we use modern-day boundaries to define the kingdom and thus abstract from the small number of municipal changes in and out of Castile over the centuries; thus Gibraltar is not considered part of Castile because it is a British territory today).

[25]To see this most easily, suppose that there are two types of captives with an identical amount of earmarked funds sent and that the pirates undervalue one group and make the initial ransom offer R, which is immediately accepted. They initially overvalue the other group, which is ransomed after some delay for the

To address these concerns, we develop an instrumental variables strategy rooted in the poor communications of the pre-industrial world. This strategy relies on historical evidence that both the information regarding a captive's capture and the time required to remit the funds to Algiers increased for earmarked captives whose homes were further from the bargaining bases.

Our identifying assumption is that—conditional on covariates—the Algerians treated this distance-induced delay as strategic negotiating delay. In other words, we assume that the only reason that captives from further afield were ransomed for less was because they had been left in captivity for longer. The Algerians, in turn, interpreted this delay as a credible signal that the Spanish valued these captives less.

We faced two practical difficulties implementing this IV strategy. First, note that our distance metric should only affect the delay with which earmarked captives are ransomed.[26] Consequently, in the ideal world we would separate the sample by earmarked and non-earmarked captives. As noted above, unfortunately we are unable to identify all of the earmarked captives. However, since only earmarked captives will be "compliers," inasmuch as the exclusion restriction holds, we expect the IV results in the entire sample to be similar to those in the complete (unobserved) earmarked subsample.

Second, the historical evidence suggests the distance from the bargaining bases affected the delay with which ransom funds reached Algiers in two steps. In the first step, this distance increased the time it took news of a captive's ransom to reach his home. In the second, the distance increased the time it took to transport the ransom funds to the negotiating orders. We would have liked to construct the delays induced by each step for each hometown. Unfortunately, the information necessary to do this is not available. As a proxy for this quantity, we use the minimum great circle distance of the captive's hometown to the bargaining bases.[27] Throughout we use 1 plus the natural logarithm of this distance as it ensures that captives from distant locations such as the Americas do not play a disproportionate role.[28]

In panel B of Table 1, we present the IV coefficients, whereas in panel C we present the first stage. The samples and control vector included are the same as in the corresponding columns of panel A. Below the IV coefficients, we present 95% confidence intervals that are robust to both weak instruments and arbitrary correlations within year of capture (Finlay and Magnusson (2009)).

Columns 1–3 show that there is a reasonably strong first stage in the entire sample (implying that a 1% increase in distance increases a captive's time in captivity by roughly

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price $R + g$, where $g > 0$. If we plotted observed ransoms against time in captivity, we would find a positive slope even if the "ransom price schedules" are declining in time in captivity for both groups of captives.

[26]This is because non-earmarked captives were ransomed with general funds and thus the distance of their home to the bargaining bases did not affect the delay with which these general funds were available.

[27]It is worth noting here that prior to the ransoming expedition, the bargaining teams often traveled to collect ransom funds. As we usually do not observe the exact places they went, we ignore this fact and note that these places seem to have been in relatively close proximity to the bargaining bases.

[28]It is useful to note that in two stage least squares "consistency of the second-stage estimates does not turn on getting the first-stage functional form right" (Angrist and Krueger (2001, p. 80)). Thus we generally find very similar results to those presented when we use our distance metric untransformed and drop a handful of captives from distant locations.

1 day), and the corresponding IV coefficients imply that a year in captivity resulted in a decrease in the ransom price of between roughly 6 and 8%. These point estimates are generally larger in columns 4–6, but are approximately similar in magnitude, especially in the specification that holds constant the amount of earmarked funds sent for a captive. In column 7 we provide evidence that the bargaining bases are not driving the results by including in the specification of column 5 a dummy equal to 1 if the captive's home was within 50 km of the bargaining bases.[29]

We only claim to identify the effect of an increased year in captivity on ransom prices. Within this effect, we believe that there is both (i) the effect on the price of an additional year of aging in captivity and (ii) the signalling effect of delaying a captive's ransom for an additional year. We do not claim to be able to precisely separate the two components of the effect. However, the row labeled p-value in panel B provides some evidence that the effect of aging is unlikely to account for the entire estimated decline in prices. In this row, we provide the p-value corresponding to the hypothesis test that the coefficient on years in captivity is the same as that on age at capture, which is our proxy for the independent effect of an additional year in captivity on ransom payments. We can generally reject the null hypothesis that the effect of an additional year in captivity on prices is the same as that of an additional year of age at capture at the 5% level. While one might worry that this difference reflects a greater rate of "depreciation" of captives while in captivity, there is abundant historical evidence that, in general, the Algerians took good care of their captives. For example, Friedman (1983, p. 76) notes that the Algerians "recognized that their captives were a valuable commodity [. . .] and in the vast majority of cases acted to protect their investments." Although such qualitative evidence does not completely rule out the possibility that the IV results are a reflection of the greater hardship faced by captives in captivity, it casts some doubt on this possibility.[30]

Is our instrument valid? The exclusion restriction will fail if distance affects prices through a channel other than an increase in the time in captivity. Although there is little reason to expect systematic differences in incomes across Spain in the Malthusian era, the very logic of our instrument suggests that it may not be valid, at least in the general sample.

We are claiming that earmarked captives whose homes were located at a greater distance from the bargaining bases were ransomed with a greater delay because the difficulty of getting the relevant funds to the bargaining bases increased with distance. A complementary prediction is that a greater distance from the bargaining bases also increases the probability that either the notification of captivity never reaches the captive's family or that the collected money never reaches the bargaining base. Furthermore, a longer delay in the earmarked money reaching a bargaining base also increases the probability that the captive is dead by the time the earmarked fund reaches Algiers. All these factors suggest that we should find a smaller proportion of earmarked captives

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[29]We have also experimented with restricting the geographic region within mainland Castile. While the point estimates generally remain similar, we lose statistical power as we drop observations.

[30]In the next section, we also structurally estimate the depreciation rate of captives during captivity, for those in our sample. Our estimate for the rate is 2%, which is much smaller than the estimated effect of one additional year in captivity on release price using our IV approach.

in the ransomed population the further a captive's home lies from the bargaining bases. The data are consistent with this prediction.[31]

On the one hand, this finding is encouraging, as it lends additional support to our claim that the distance of a captive's home from the bargaining bases increased the difficultly of getting funds to Algiers. On the other hand, the finding suggests caution when interpreting the general IV results. This is because, in addition to affecting the delay with which a captive was ransomed, distance affects whether or not a captive was ransomed with earmarked funds, potentially violating the exclusion restriction. Fortunately, in the earmarked sample this concern largely disappears, as we are able to directly control for the ransoming team's valuations. We are reassured by the fact that the results in the earmarked sample are qualitatively similar to those using all observations, as it is consistent with the claim that any violation of the exclusion restriction in the general sample is not significantly biasing the results.

Of course, one might worry that even conditional on being earmarked, earmarked captives from further afield are systematically different from those whose homes lay in close proximity to the bargaining bases. As noted above, the fact that we are able to directly control for the amount of earmarked funds in the earmarked sample helps address such concerns. Yet perhaps the most convincing evidence in support of the exclusion restriction conditional on being earmarked comes from the regression of earmarked funds on distance to the bargaining bases, which are presented in columns 1 and 2 of Table 2. These results show that within the earmarked sample, there is no relationship between the amount of earmarked money sent for a captive and the distance of his home from the bargaining bases. Thus, we find no evidence that our instrument is correlated with the ransoming team's valuation, which to our minds is the main threat to our identification strategy.

Our IV strategy relies on the assumption that the Algerians systematically ransomed captives from further afield for less because they treated distance-induced delay as negotiating delay. In our view, this is a plausible assumption. First, even if the Algerians were perfectly aware of the distance-induced component of delay and they could observe distances of a captive's family from the nearest bargaining bases, they could not observe which captives would end up receiving earmarked funds. The majority of captives did not receive such funds; hence no agreement in the first round purely indicated relatively low valuation for the rescuers. Even for earmarked captives, with some probability, the funds reached the rescuers by the time of the first rescuing trip after being captured, and for these captives, negotiating delay was again informative about valuation. To summarize, the captors could not distinguish between strategic delay related to the captive's valuation versus delay caused by earmarked funds not arriving in time, and the high probability of the first type of delay would have lead the captors to update their expectation negatively after a failed negotiation, even in the above scenario. We claim

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[31]Regressing an indicator for earmarked captives on 1 plus the logarithm of the distance from a captive's home to the bargaining base yields a constant of 0.30 (standard error of 0.03) and a slope of -0.017 (standard error of 0.004). When introducing trip dummies, the slope is -0.014 (standard error of 0.003). We thank an anonymous referee for suggesting this check.

TABLE 2. Earmarked funds and distance to Algiers.

	ln(*earmarked*)		Captive		ln(*price*)	
	(1)	(2)	(3)	(4)	(5)	(6)
*L*dis	1.19	−2.11				
	(1.88)	(2.72)				
*L*disalg			0.16	0.51	0.51	−3.49
			(0.12)	(0.45)	(1.54)	(3.65)
N	876	564	4220	2474	4220	2474
Clusters	100	78	127	120	127	120
Sample	All	Castile	All	Castile	All	Castile
Controls?	Yes	Yes	Yes	Yes	Yes	Yes

Note: The dependent variable in columns 1 and 2 is the logarithm of ear-marked funds. The dependent variable in columns 3 and 4 is the time a captive was in captivity prior to ransom, whereas in columns 5 and 6 it is the logarithm of a captive's ransom. *L*disalg is the logarithm of 1 plus the distance of a captive's home to Algiers. *L*dis is the logarithm of 1 plus the minimum distance from the captive's home to the bargaining bases. Controls include age at capture, and profession, child, and female dummies. Standard errors are clustered by year of capture. Co-efficients in columns 1, 2, 5, and 6 are multiplied by 100 for ease of exposition.

that this fact coupled with the difficulty of exactly measuring the distance from the bargaining bases to a captive's home implies that knowledge about the geographic location of the captive's family had a very limited effect on the captors' bargaining strategy.

How difficult would it have been for the pirates to calculate the relevant distances? Although it is impossible to conclusively answer this question, here we provide evidence that the relevant distance metric was harder to calculate than simply measuring the distance of a captive's town to Algiers. The differences between these two metrics is demonstrated visually in Figure 2 where we provide a map of the homes of captives in the sample. Algiers is labeled and the bargaining bases are denoted by the remaining black dots.

Empirically, we investigate this question by estimating both the first stage and the reduced form using the "placebo" instrument that measures the distance of a captive's hometown to Algiers. As we show in columns 3–6 of Table 2, there is no relationship between distance from Algiers and time in captivity or ransom prices. This result shows that the relevant part of the distance from the bargaining bases was more difficult to observe than simply noting that certain towns were further from Algiers than others. Indeed, even with access to the ransoming records and using modern software, the relevant first stages are at times weak enough to trigger the usual concerns around weak instruments. Thus, the results are consistent with the claim that the relevant distances would have been difficult for the pirates to calculate.

As a final check on the extent to which the pirates observed/used information on the location of a captive's home, we have gathered data on the exact place of capture for 2109 of the ransomed captives from the original ransom records (we were not able to locate this information for the remainder of the captives).[32] It seems reasonable to assume that the pirates had more information on the location of the homes of captives

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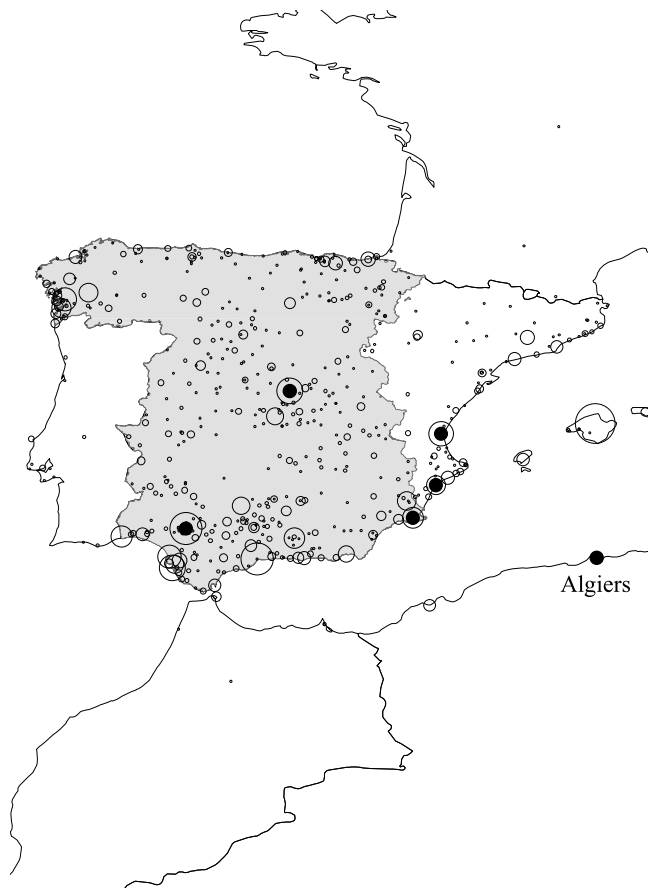[32]A map of these locations is provided in the Appendix.

FIGURE 2. Homes of ransomed captives. Larger circles denote a larger number of ransomed captives. Algiers is denoted by the black circle labeled Algiers. The remaining black circles denote the bargaining bases. The Kingdom of Castile is shaded grey.

captured close to home. Consequently, if the pirates were using this information, one would expect the interaction term to be positive in the "reduced-form specification" $\ln(ransom_{ib}) = \alpha_b + \beta_1 ldis_{ib} + \beta_2 ldiscap_{ib} + \beta_3 ldis_{ib} \cdot ldiscap_{ib} + \varepsilon_{ib}$, where $ldis_{ib}$ is the natural logarithm of 1 plus the distance from the captive's home to the bargaining bases, and $ldiscap_{ib}$ is the natural logarithm of 1 plus the distance between where a captive was captured and his home. Yet in this regression, only the coefficient β_1 is statistically significant.[33] This result casts doubt on the claim that the pirates were using information on the location of a captive's home to predict the delay with which he would be ransomed. Finally, and for completeness, in the Appendix we show that including the distance between where a captive was captured and his home as a control in equation (4) yields similar results to those presented above.

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[33]The point estimate of β_1 is -3.14 with a standard error of 1.00, the estimate of β_2 is 0.53 with a standard error of 1.38, and the estimated β on the interaction term is 0.20 with a standard error of 0.23.

5. STRUCTURAL ESTIMATION

While the reduced-form section provides evidence that negotiating delay had a causal effect on ransom prices, this analysis is limited in its ability to address other relevant dimensions of the negotiations. In this section, we provide a structural estimation of the bargaining model described in Section 2. The goal of this estimation is to evaluate how well the model fits our data, discuss the estimated structural parameters, and evaluate the distribution of the surplus between buyers and sellers. To introduce our structural estimation, we provide a mapping between the data set and the theoretical model, and a parametrization of the model. The buyer in our model is a Spanish team that was sent to ransom captives from slave owners in North Africa, while slave owners are the sellers.

We assume that the buyer's valuation of captive i with time in captivity t has the form

$$v_{it}^b(t) = e^{\mu_i - rt_i - xt_i} e^{\sigma Z_i}, \tag{5}$$

where $\mu_i = \boldsymbol{\alpha} X_i$ is a commonly known component of the valuation and is assumed to be a linear function of observed personal characteristics X_i (same as the ones we used as control variables in our reduced-form estimations), r is the common real interest rate, x is the depreciation of the captive's value with time,[34] and σZ_i is a valuation component privately known by the buyer. We normalize Z_i to have zero mean and unit variance. Thus, σ captures the uncertainty in the private valuation component. We further assume that the distribution of Z_i is truncated normal. The truncation level $Z_{\min}$ determines the minimal buyer's valuation (for $\mu_i = 0$ and $t = 0$):

$$v_{\min} = e^{\sigma Z_{\min}} > 0. \tag{6}$$

This specification implies that the minimal buyer valuation is strictly positive. Given that $e^{\sigma Z_i} \approx 1 + \sigma Z_i$, $\sigma Z_i * 100$ is as a percent deviation from the median valuation. For example, $\sigma Z_i = 0.3$ means that the buyer's valuation is 30% higher than the median valuation.

The seller's valuation has a similar structure,

$$v_{it}^s(t) = e^{\mu_i - rt_i - xt_i} v_{\text{res}}, \tag{7}$$

where $v_{\text{res}} < v_{\min}$ to make the trade always efficient.

As the duration of the rescue trips was a small fraction of the time elapsing between trips, for simplicity we assume that slave owners were able to make one offer each time the Spaniards visited their market. We assume the timing of the rescuing trips is distributed Poisson with intensity parameter λ. We estimate this parameter from the data. On the interval [1575, 1692], we have found evidence of 40 trips, suggesting that the average time between trips is 2.95 years.[35] As one can easily verify, the maximum likelihood estimate of λ is the inverse of the average time between the trips $\overline{\triangle t}$,

$$\hat{\lambda} = (\overline{\triangle t})^{-1}. \tag{8}$$

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[34]This depreciation rate is specific to the person being in captivity, because bargaining takes place in captivity, and everywhere in this section, t stands for time in captivity. The effect of normal aging is captured by controlling for the captive's age when captured (this variable is present in X_i).

[35]See the Appendix for the sources we used to identify ransoming trips.

This corresponds to $\hat{\lambda} = 0.34$ in our sample, which we use as our estimate of λ throughout this section.

As we argued in the previous section, the relevant information had not reached the friends and family of many captives prior to the departure of the first ransoming trip to Algiers following their capture. For some of these captives, this would result in the rejection of the first offer simply because the relevant earmarked funds were not yet available. We incorporate this into our estimation by assuming that with probability π, the first offer was rejected for exogenous reasons, and we estimate this parameter with the others.

Using the equilibrium of the screening model described in the previous section, for any set of parameter values we can compute the equilibrium price $p(i, n, t)$, where n is the number of the offer. For example, $p(i, 3, 6)$ would be the equilibrium price in the third offer for captive i who spent 6 years in captivity. For the functional forms of the buyer's and seller's valuations, the equilibrium price has the convenient multiplicative form

$$p(i, n, t) = p_n e^{\alpha X_i - x t_i}, \tag{9}$$

where p_n is just a function of the offer number.

The actual offer price could be different from the computed equilibrium price for many reasons (such as our model not being a perfect description of reality, measurement errors, etc.). To incorporate these errors, we assume that the actual offer prices differed from the equilibrium prices by an independent multiplicative error term

$$\log P(i, n, t) = \log p(i, n, t) + \varepsilon_i, \tag{10}$$

where $\varepsilon_i \sim N(0, \theta)$ and is independent and identically distributed (i.i.d.).

We estimate the parameters of our model by maximum likelihood (ML). For our model the log-likelihood function can be expressed in the manner[36]

$$(\hat{\alpha}, \hat{r}, \hat{x}, \hat{\sigma}, \hat{\pi}, \hat{v}_{\text{res}}, \hat{v}_{\text{min}}) = \underset{\alpha, r, x, \sigma, \pi, v_{\text{res}}, v_{\text{min}}}{\arg\max} [\log L], \tag{11}$$

$$\log L = -\frac{N}{2} \log\left(\frac{1}{N} \sum_i \left(\log P(i, n_i, t_i) + x t_i - \alpha X_i - \log p_{n_i}\right)^2\right) + \sum_i \log \text{Prob}[n_i], \tag{12}$$

where N is the number of observations, n_i is the number of missed trips plus 1 or our proxy for the number of rejected offers, t_i is the time in captivity of captive i, and $\text{Prob}[n_i]$ is the predicted probability that offer n_i will be accepted. Intuitively, the likelihood of each observation consists of two parts. The first part is the likelihood of the observed price; the second part is the likelihood of observing the corresponding number of missed trips before the captive was ransomed. Thus, in our structural estimation we match both observed prices and observed numbers of missed trips (unlike the reduced-form estimations where we only match prices).

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[36]This concentrated likelihood function can be derived from the original likelihood function by solving for and substituting in the variance of the error term, θ. The details of the derivation of the likelihood function are provided in the Appendix.

The functional form of the log-likelihood function allows us to perform maximization in two steps.[37] In the first step, for each parameter value $(r, x, \sigma, \pi, v_{\text{res}}, v_{\text{min}})$, we minimize the sum of squared errors $\sum_i (\log P(i, n_i, t_i) + xt_i - \boldsymbol{\alpha} X_i - \log p_{n_i})^2$. This can be simply done by regressing $\log P(i, n_i, t_i)$ on t_i, X_i, and $\log p_{n_i}$. The residual sum of squared errors (RSS) in this regression is denoted RSS_{OLS}. Substituting the resulting RSS_{OLS} in the original likelihood function yields the simplified expression

$$\log L = -\frac{N}{2} \log\left(\frac{1}{N}\text{RSS}_{\text{OLS}}\right) + \sum_i \log \text{Prob}[n_i]. \tag{13}$$

In the second step, we maximize the expression above with respect to the nonlinear parameters $(r, x, \sigma, \pi, v_{\text{res}}, v_{\text{min}}/v_{\text{res}})$. Estimating the vector parameter $\boldsymbol{\alpha}$ in a separate step is necessary because this vector has more than 10 elements (personal characteristics, trip fixed effects), which would make a one-step procedure very challenging.

The sample and the set of captive characteristics X_i used in our estimation coincide almost exactly with those used in the reduced-form analysis. Hence, we do not describe them here. The only difference between the samples is that in this section, we drop outliers in the number of missed trips.[38] The estimated parameters are reported in Table 3. The estimate of $\sigma = 0.35$ implies that 95% of captives had values from 50 to 200% of the median value (controlling for personal characteristics); $v_{\text{res}} = 0.26$ means that the reservation value of the slave owners was about 26% of the median valuation by the Spaniards. The depreciation rate of $x = 0.02$ means that each year in captivity decreases the value of captives by 2%; $\pi = 0.32$ indicates that the Spaniards did not have money for 32% of captives when these captives' first ransoming mission following their capture arrived in Algiers; $v_{\text{min}}/v_{\text{res}} = 1.6$ implies that the minimal buyer's valuation was 42% of the median valuation. Most parameter values are significantly different from zero.[39]

Using the estimated parameters, we first analyze how well our model fits the data. To do so, we compute the normalized transaction prices, removing the effects of personal characteristics and time in captivity:

$$\widetilde{P}(n_i) = P(n_i, t_i)/e^{\hat{\boldsymbol{\alpha}} X_i - \hat{x} t_i}. \tag{14}$$

This normalization allows us to see directly how the offer prices depend on the number of rejected offers. We compare these normalized observed prices with their predicted values p_{n_i}. Figure 3(a) plots the average of $\widetilde{P}(n_i)$ for each n and p_n as functions of n. This figure shows that overall the model matches well the observed decline in the average price with the number of rejected offers. Consistent with our screening model, both functions are decreasing in n. The rate of decline is substantial. While the first offer

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[37] We can maximize in two steps due to the fact that $\log p_{n_i}$ and $\log \text{Prob}[n_i]$ do not depend on $\boldsymbol{\alpha}$.

[38] Outliers can affect our structural estimation significantly because we do not allow for errors in the number of rejected offers and our structural model would have to rationalize the existence of negotiations with many rejected offers. We report results where we drop negotiations with the number of rejected offers exceeding 5. However, results do not change significantly if we drop only negotiations with the number of rejected offers exceeding 10.

[39] Throughout, we bootstrap the standard errors.

TABLE 3. Structural parameters of the screening model.

Structural Parameters		Linear Parameters	
σ	0.4419	Age when captured	−0.0056
	(0.0398)		(0.0006)
v_{res}	0.2328	Female	0.1145
	(0.2760)		(0.0423)
x	0.0275	Child	0.0199
	(0.0035)		(0.0409)
r	0.0657	Profession controls	Yes
	(0.0071)		
π	0.3061	Trip fixed effects	Yes
	(0.0478)		
v_{min}/v_{res}	1.593	N_{obs}	3885
	(0.2822)		

Note: This table presents maximum likelihood estimates of the structural parameters of the screening model. The structural parameters are σ, a measure of information asymmetry; v_{res}, seller's reservation value; x, depreciation rate of a captive; r, the same interest rate for pirates and Spaniards; π, the probability of the funds arriving only with the second ransom team; v_{min}/v_{res}, the minimal valuation by the buyer over the seller's reservation value. Standard errors are given in parentheses.

is close to 65% of the median value, the third offer is about 50% of the median value. Thus, the estimated price drops by 15% after two offers are rejected. The sixth offer price predicted by the model is 40% of the median valuation. Hence, our model predicts that all captives were eventually rescued by the Spaniards after six trips. This prediction matches the fact that in our data, almost all captives were rescued after six trips.

The second dimension we examine is the probability of offer acceptance. To check how well the model performs in this dimension, we plot the observed distribution of the number of accepted offers (distribution of n_i) with the predicted probability of offer acceptance (Prob[n_i]). Figure 3(b) shows the results. Overall, the model matches the data well in this dimension. The first offer is accepted with probability 0.43 and this probability declines as the number of offers increases.

Figures 3(a) and 3(b) reveal that even though the pirates started with a relatively low price (65% of the median valuation), only in 43% of the cases was the offer accepted. Some of the offer rejections are explained by the first period liquidity constraint. Our estimates show that only in 69% of the cases $(1 - \pi)$ would the Spaniards have accepted any first offer. Thus, out of the 69% that did not have the first period liquidity constraint, 26% decided to wait for a better price and 43% accepted the offer.

In our structural estimation above, we assume that we measure the number of rejected offers perfectly. However, due to potential transfers of captives between different places, the number of missed trips may not coincide with the number of opportunities for the slave owners to make an offer to the Spaniards. This measurement error and that from other sources may bias our estimates. To analyze this bias, we note that assuming we observe all ransom trips to Algiers, the true number of offers can only be lower than the number of missed trips. This means that the true distribution of the offers is shifted to the left of the observed distribution of missed trips. Similarly, the true price schedule
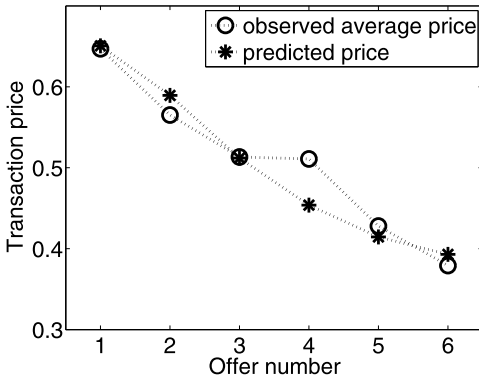
(a) Observed and Predicted Average Prices
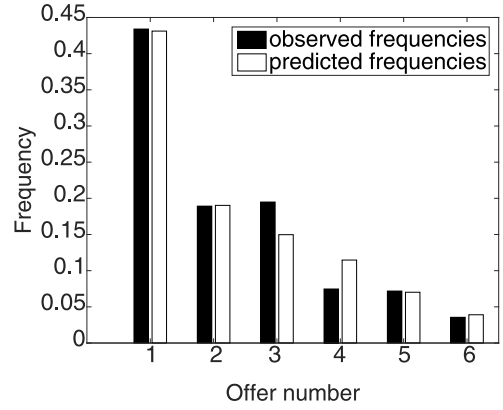
(b) Observed and Predicted Distributions of the Number of the Accepted Offer

FIGURE 3. Model fit. The observed average price is the average price paid for a captive with the observed median valuation normalized to 1, as defined in equation (14). The predicted price p_n is the optimal price offer for a captive with median valuation 1, for whom $n - 1$ offers have been rejected. Both prices are computed based on the parameter estimates reported in Table 3. The observed distribution is the distribution of the number of missed trips plus 1. The predicted distribution is the probability of offer number n being accepted. The predicted distribution is computed based on the parameter estimates reported in Table 3.

is steeper than the one reported in Figure 3(a). Numerical simulations of our screening model show that such effects are associated with higher depreciation, higher discount factors, and more uncertainty about the value of the captives, σ. Hence, if the measurement error is severe, we expect our estimates of the interest rate, the discount factor, and σ to be lower than their true values.

One of the benefits of our structural estimation is that we can use the estimated parameters to evaluate the distribution of trade surpluses and the delay costs. To introduce the notation for these surpluses, let $n(Z)$ denote the number of the accepted offer as a function of the private valuation parameter and let $t(Z)$ denote the random acceptance time that corresponds to this equilibrium. We define the seller's surplus as the expected discounted price net of the reservation value:

$$V^s = E\big[(p_{n(Z)} - v_{\text{res}})e^{-t(Z)(x+r)}\big]. \tag{15}$$

Respectively, the buyer's surplus is defined as the expected value of the captive minus the price paid, discounted for the interest rate and depreciation:

$$V^b = E\big[(v(Z) - p_{n(Z)})e^{-t(Z)(x+r)}\big]. \tag{16}$$

The total trade surplus is defined as the expected value of a captive minus the seller's reservation value of the captive:

$$V^{\text{total}} = E\big[v(Z)\big] - v_{\text{res}}. \tag{17}$$

TABLE 4. Estimated distribution of trade surplus.

	Screening	Bundles of 10	Take It or Leave It
	Percent of Total	Percent of Total	Percent of Total
Seller's surplus	31.5	43.4	45.1
Buyer's surplus	54.3	47.0	36.3
Delay/termination	14.2	9.6	18.6
Total surplus	100.0	100.0	100.0

Note: This table shows the expected trade surpluses for the parameter estimates reported in Table 3. The seller's surplus is the expected discounted price minus the reservation value. The buyer's surplus is the expected discounted valuation minus the price paid to the seller. The delay costs denote the expected depreciation of a captive during the negotiation process net of the services he produces to the seller. The total surplus is the difference between the expected valuation by the buyer and the seller's reservation value. The termination costs are applicable to the take-it-or-leave-it strategy and are defined as the difference between the total surplus and the surpluses of the agents. The first data column shows the distribution of surpluses if the original trading mechanism is used. The second data column shows the distribution of surpluses if 10 random captives with the same observable characteristics are sold as a bundle. The last column shows the distribution of expected gains from trade if the seller can commit to make only one offer.

Finally, since delay costs is the only source of inefficiency in our model, one can calculate these costs as the difference between the total surplus and the surpluses of the buyer and the seller:

$$C^{\text{delay}} = V^{\text{total}} - V^s - V^b. \tag{18}$$

The simulated surpluses are reported in the first data column of Table 4. These estimates show that the Spaniards were able to keep the bulk of the total surplus, 54%; the pirates' share is estimated at 32% of the total trade surplus. The estimated delay costs are relatively low, about 14% of the surplus.[40]

In addition to the welfare analysis, we perform two counterfactual tests. The first assumes that instead of selling captives separately, the slave owners could bundle a number of captives together. By using this strategy, the pirates could have reduced the amount of information asymmetry between themselves and the Spaniards. To show the effect of this strategy, we assume that a bundle the seller could offer consists of 10 randomly picked captives. Keeping all other conditions of the trade the same, this would result in a significant redistribution of surplus from the buyer to the seller and would reduce the costs of delay. This result is reported in the second data column of Table 4. According to our estimation, about 10% of the total trade surplus would shift from the buyer to the pirates.

The second counterfactual experiment assumes that instead of screening, the seller could commit to make one take-it-or-leave-it offer to the buyer. In this case, the values of the seller and the buyer, and the total surplus can be computed using formulas (15)–(17), but instead of the delay costs, equation (18) defines the negotiation termination

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[40]We show comparative statics of the distribution of gains from trade with respect to λ and x in Table A.5 in the Appendix. Changes in λ have a strong effect on this distribution: higher λ (more frequent trips) are associated with lower seller's value and higher buyer's value. Changes in x have a much smaller effect. Higher x are associated with lower seller's value and lower buyer's value.

costs. Our results, reported in the third data column of Table 4, show that being able to commit to a take-it-or-leave-it offer could increase the seller's surplus by 14% relative to the no-commitment case and decrease the buyer's surplus by 18%. The resulting 4% difference is the difference between the termination and delay costs.

6. Conclusion

Using a historical data set containing detailed information on thousands of captives ransomed from the Barbary pirates, we documented a robust negative relationship between negotiating delays (as proxied by time in captivity) and ransom prices. This result is both consistent with qualitative evidence from contemporary bargaining instructions and with the predictions of all rational models of bargaining when the relevant private information is regarding the buyer's evaluation. To address potential endogeneity concerns, we developed an instrumental variable strategy rooted in the slow speed of travel in pre-industrial Spain. We also performed a structural estimation of a dynamic screening type bargaining model, extended with features motivated by the historical setting. We showed that the model fits the observed prices and acceptance probabilities well. We used the estimated structural parameters to analyze the trade surplus distribution and compute how this distribution would have changed under different trading mechanisms.

It is worth noting that the results are likely most relevant to ransoming and bargaining situations today, which are characterized by one-sided private information. Thus, the results seem more relevant to bargaining with Somali pirates (whose actions appear to be more aimed toward extracting rents) than to negotiations with ISIS (where the possible propaganda value of executing a captive likely makes the information asymmetry more two-sided).

In closing, we note that the historical response of many European powers to the Barbary pirates may provide insights into negotiating with Somali pirates (and possibly other criminal groups). For example, the historical preference for centralized ransoming organizations suggests that such institutions might aid negotiations with pirates today by both enabling negotiations for multiple cargoes at once and by reducing transaction costs (which, besides saving costs directly, improves the bargaining power of the negotiating team).

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