

## Overview

If the 1970s are remembered in international finance as the halcyon days of international lending, the 1980s and 1990s will be recalled as the turbulent years of international debt rescheduling. From the Philippines to Brazil to the Ivory Coast, the rescheduling of sovereign loans has become the major occupation – and preoccupation – of international bankers. But this pattern of boom and bust in lending is not unique to this epoch; borrowing cycles have occurred in both the nineteenth and twentieth centuries.

The financial crises that the world faced in the 1820s and 1830s, for example, resemble the entanglements of today in many respects. After the French successfully floated indemnity loans in 1817 and 1818, other countries rushed into the borrowing market. Brokers for these loans earned huge profits, which immediately made lending exceptionally attractive. As lenders scrambled to offer funds, many countries in the Americas and Europe leapt to secure loans. In the ensuing melee, countries often misrepresented the intended use of their newly acquired monies. For example, the Greek government easily secured loans by claiming that they would be used to bolster military defenses and keep the country free from Turkish rule. Yet only a portion of the proceeds were used for this claimed purpose:

The greater part went to various intermediaries [such] as the Greek commissioners to pay them for their arduous labors in negotiating the loan, several well-known English Philhellenes, to compensate them for their losses in the falling securities market, Admiral Cochrane to enable him to spend the rest of his life at ease and several Brooklyn ship-builders to pay them for frigates, most of which never saw service in the cause. The Greek government did not pay interest on these bonds for over half a century.<sup>1</sup>

Conversely, unscrupulous adventurers offered bogus bonds for the reckless investor. One house even floated a bond issue for a nonexistent “Kingdom of Poyais.”<sup>2</sup> Even when countries were straightforward and lenders were legitimate, few of the investment houses made decisions based on sound economic, political, and financial information. A lack of prudent financial sense characterized the era.

As with the 1820s, our current debt problem has been exacerbated by unwise lending, uninformed borrowers, and the misdirection of funds to private coffers where they would not be used to stimulate the economies of debtor countries. Disproportionate profits in both the 1820s and the 1970s encouraged loans that might not have proliferated had bondholders correctly assessed the risks attached to lending. Bankers in both eras assumed that because the sovereign debt was

<sup>1</sup> Edwards (1938), p. 18.

<sup>2</sup> Jenks (1927/1973), p. 45. See the list of loans in Hobson (1914), p. 10.

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backed by governments, and thus had a sharply different character than domestic loans, it would always be safe. Yet historical experience suggests the opposite: the fact that sovereign debt lacks collateral makes it risky because enforcing norms of repayment is very difficult.

International debt rescheduling, both in earlier epochs and our present one, has been marked by a flurry of bargaining. In this process, significant variation has emerged over time and across cases in the extent to which debtors have undertaken economic adjustment, banks or bondholders have written down debts, and creditor governments and international organizations have intervened in negotiations. My focus in this book is on explaining this complex rescheduling process, rather than on seeking to account for why countries and creditors have faced the same loan problems time and again. To predict the results of negotiations, my central objective is to develop and apply a “situational theory of bargaining” – focusing on an actor’s basic bargaining constraints – to explain why bargaining outcomes have varied.<sup>3</sup> I then use game theory to predict the likely outcome of their strategic interaction. My theory, which involves both a static and a dynamic component, allows us to steer a course between the Scylla of purely aggregate-level analysis and the Charybdis of unique historical analysis of individual cases. This approach enables us to generalize about the debt rescheduling process and to show how it varies with the traits of the participants. Why, for example, in the 1940s and 1950s did Mexico secure high concessions from bondholders for debt going back to the turn of the century while Peru secured a less favorable agreement in 1953? And why did Mexico undertake high adjustment in the early 1980s, whereas Argentina and Brazil often found themselves in deadlock with the banks?

The static portion of my theory, the “situational theory of payoffs,” allows us to construct bargaining games between debtors and lenders based on fundamental political and economic variables.<sup>4</sup> I use the term “situation” to refer to the three variables of domestic coalitional stability, issue capabilities, and overall capabilities that I assume will constrain actors’ basic goals. By solving these games for equilibria using standard game theoretic techniques, and by taking into account the implications of potential intervention by third parties, I am thus able to predict debt bargaining outcomes.

<sup>3</sup> I am not using the term “bargaining” here in the technical sense developed by John Nash (1950) and others. Whereas I consider all strategic interaction situations as bargaining situations, Nash (1950), Harsanyi (1977), and others restrict it to cases where both parties to a negotiation are able to gain from their interaction. This would rule out a game such as Deadlock (see Chapter 2) since it does not have a “bargaining range” in utility terms. A bargaining range requires that “there be a possibility of negotiated compromise that both parties would prefer to no agreement at all” (Snyder and Diesing, 1977, p. 52). On this point, see Harsanyi (1977), p. 12, and Rapoport and Guyer (1966), p. 203. This issue is discussed at length in Chapter 2.

<sup>4</sup> The word “situation” is sometimes associated with a pure microeconomic approach with an emphasis on constraints generated by an actor’s position in a system (Latsis, 1976). While some have used this notion to focus only on international constraints (Keohane, 1984, pp. 26–28), I use the word “situation” to refer to constraints on decisionmakers emanating from both the international and domestic realm.

The dynamic element of my approach, a “situational theory of change,” helps to show how bargaining outcomes might vary over time as a result of actors’ efforts to change their bargaining situations or serendipitous events. Intended situational change efforts include attempts by bondholders to secure allies, secret debt buybacks by Peru and Mexico, and appeals to international law. Serendipitous events or shocks include such things as the discovery of guano in Peru and oil in Mexico, the rise of particular leaders, earthquakes, or a sudden fall in oil prices.

### The approach and organization of the book

My investigation of international debt rescheduling focuses on two key elements: the development of theoretical tools to understand debt bargaining outcomes, and the exploration of the utility of these tools to understand debt rescheduling from an empirical perspective in a large number of current and historical cases.

I present the theoretical portion in Part I of the book, which consists of four chapters. Chapter 1 begins by characterizing debt bargaining outcomes by the degree to which debtors agree to undertake economic adjustment that may further debt repayment, and the extent to which lenders agree to grant lending concessions. I then focus on the structural characteristics of four commonly identified time spans over the last 170 years (1820s–1860s, 1860s–1914, 1920s–1950s, and 1970s–1990s) to investigate debt rescheduling outcomes. The nested systems approach I use to examine these four time periods (or what I term “epochs”)<sup>5</sup> focuses on the interlinked overall international, economic, and financial systems.<sup>6</sup> I characterize these systems by focusing on the types of actors, the number of major powers, and the presence or absence of international regimes or organizations.<sup>7</sup> Based on this structural-systemic approach, I find that epochs exhibit some distinctive characteristics that allow us to partially predict some characteristics of debt rescheduling outcomes for the particular epoch. The first epoch, for example, was characterized by newly independent states that were quite unstable politically. In the first three epochs, the debt was held primarily by bondholders rather than banks. The fourth epoch saw the active participation of international organizations.

<sup>5</sup> Because I do not investigate the origin and evolution of what some scholars have termed “debt cycles,” I use the term “epochs” to refer to my structural analysis of these four time periods.

<sup>6</sup> See Aggarwal (1983) and (1985) for a more detailed discussion of a nested systems approach.

<sup>7</sup> The idea of a “systemic analysis” is based on microeconomic theory. The basic argument is that the aggregate behavior of the system and the behavior of firms (or states) will be influenced by the structure of the system. In microeconomics, market structure is determined by the number of major firms, for example, monopoly, oligopoly, or atomistic markets. Similarly, in the international system, we can examine the effects of unipolarity, bipolarity, multipolarity, and so on in terms of the distribution of capabilities among states. Waltz has pioneered this approach in international relations, defining the structure of a system along the three dimensions of types of units, distribution of power, and hierarchy or anarchy. I modify this approach somewhat in looking at nested systems. For a detailed discussion of these points, see Waltz (1979) and Chapter 2 in this book.

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A focus on epochal factors, however, takes us only part of the way toward understanding the rescheduling process. Unlike the high degree of similarity in the pattern of borrowing and timing of default within a particular epoch, rescheduling patterns do not show the same consistency. Instead, I find significant variation of outcomes even within individual epochs, thus highlighting the need to move beyond a purely epochal analysis of debt rescheduling to a focus on specific bargaining episodes involving debtors, lenders, creditor governments, and international institutions.<sup>8</sup>

The second through fourth chapters present the core of my situational theory of bargaining to explain debt rescheduling cases. My goal is to explain all cases with the same variables, rather than to construct a unique explanation for each. To this end, Chapter 2 argues that it is most useful to focus on the bilateral relationship between individual debtors and groups of lenders. Intervention by creditor governments and international organizations can then be examined in terms of the extent to which they play a role in pressing either one or both of these actors to pursue particular policies. With this perspective in mind, I then show how bargaining efforts between a debtor and lenders can be modeled as a bargaining “game.” In particular, I present some basic ideas of game theory, and review alternative modeling approaches. To predict how actors in specific games will attempt to choose the best strategy to optimize their gains, I focus on the concept of a “Nash equilibrium” solution.

The third chapter tackles what I consider a fundamental but consistently neglected issue in game theoretic analysis. Whereas most analysts either establish the payoffs of the games they analyze by assumption, or simply by inferring the payoffs from the outcomes they observe, I attempt to derive these payoffs by using what I term a “situational theory of payoffs.” I first argue that actors have a number of goals. For debtors, in a crisis, these goals include a desire to (1) secure additional funds, (2) minimize the political and financial costs of adjustment, and (3) minimize penalties by maintaining good relations with lenders. By contrast, following a debt crisis, lenders generally (1) hesitate to commit additional resources, (2) wish to encourage debt-servicing, and (3) worry about possible penalties imposed by debtors. These goals will be prioritized by actors depending on their “individual situation,” defined here as a function of three key factors: direct debt rescheduling related resources, overall capabilities, and domestic coalitional stability.

Actors’ moves are guided by a consideration of both goals and constraints. The leaders of a politically unstable coalition, for example, will generally hesitate to undertake drastic adjustment programs that would create intense political conflict. Hence, we will see variation in actors’ valuation of different outcomes.<sup>9</sup>

<sup>8</sup> For analytical convenience, I refer to bondholders or banks as “lenders,” debtor countries as “debtors,” countries where the lenders are located as “creditor governments,” and international institutions such as the IMF or World Bank as “international organizations.”

<sup>9</sup> It is important to keep in mind that this variation in constrained preferences does not imply that actors’ *basic* preferences or goals vary in the model. See Chapter 2 for a more detailed discussion of this point.

These valuations generate the range of payoffs of the debt games that I construct to examine bargaining outcomes. This chapter also examines the role that creditor governments or international organizations might have in the rescheduling process. In my model, these actors' decisions to intervene depend on their relative weighting of strategic, political, and economic factors.

Finally, Chapter 4 moves beyond static game representations to consider the evolution of bargaining outcomes. My "situational theory of change" argues that actors who fare poorly in negotiations are highly motivated to alter their bargaining situation – despite having reached an initially stable equilibrium. In such cases, actors might attempt to alter the course of future negotiations by manipulating either their own or their opponents' situation. The conditions under which actors are likely to do so, and the constraints that they face in such efforts, are issues I investigate in detail in this chapter.

Figure O.1 summarizes the theoretical approach and the chapters in which the different portions of my argument appear.

Parts II–V of the book examine 61 cases of debt rescheduling by focusing on specific rescheduling episodes from the four epochs. These cases were chosen for several purposes: to include sufficient variation in outcomes to provide a significant test of the ideas developed here, to see if decisionmakers learn from their predecessors' experience, and to examine some of the most crucial episodes of international debt rescheduling. I look at all episodes in Mexico and Peru across the epochs from the 1820s to the present and at Argentina and Brazil in the 1980s and 1990s. In terms of significance, Peru was one of the largest borrowers in the world in the 1850s and 1860s, while Brazil and Mexico have been the largest debtors in our present epoch. The cases I have chosen pose a rigorous challenge to my model by forcing it to cope with bargaining episodes that span the last 170 years and a diverse set of countries.

Finally, the conclusion reviews my findings and examines both the success rate of the model as well as the cases where it failed to accurately predict outcomes. In addition, I consider other lessons that emerge from my analysis of the case studies. I end with a discussion of how the model might be enriched and consider how it might be applied to other areas in international politics and economics.

### **An overview of the modeling approach**

I use the following questions to contrast my approach with that of others: (1) Why develop a model? (2) Why use game analysis? (3) What variables affect actors' valuations of outcomes? and (4) Why look at actors' efforts to promote changes in their situation?

First of all, why develop a model? I view a model as a tool to provide insight into the complexities of international negotiations. Without such formalization, it is difficult to systematically evaluate competing predictions and explanations. My model, like all others, omits many aspects of the debt negotiation process; but it should help to pinpoint essential elements in debt rescheduling, and to

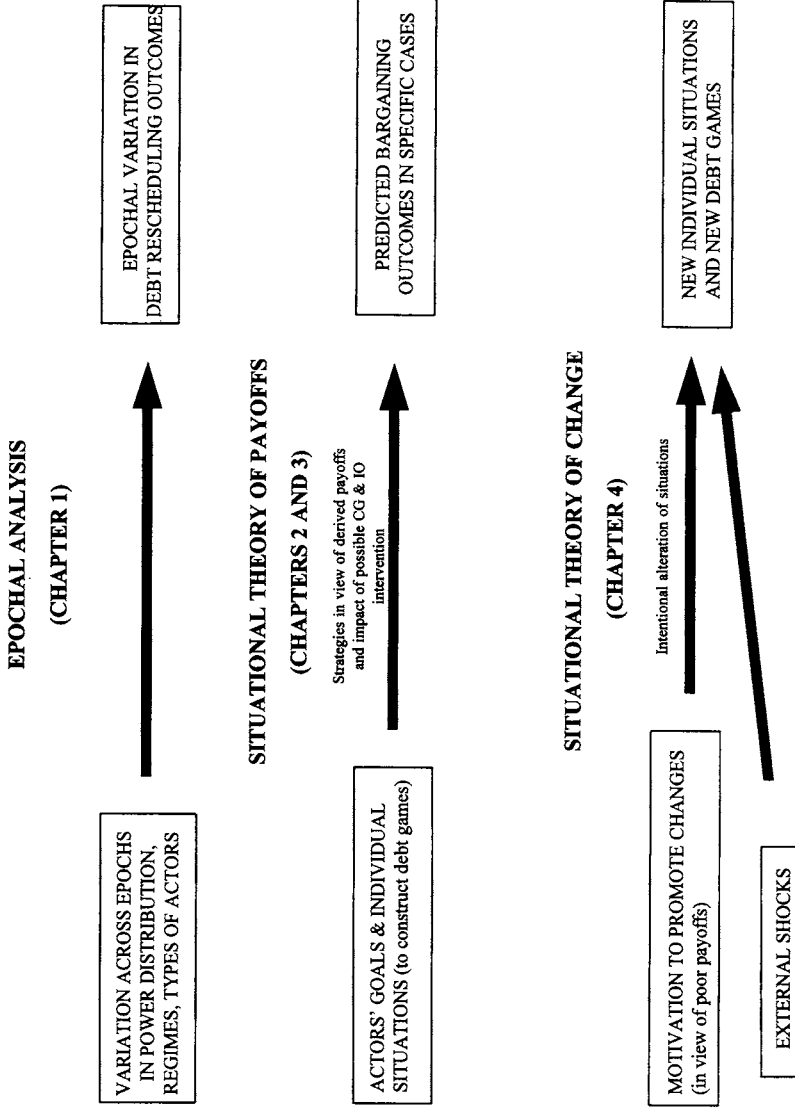


Figure O.1. A conceptual map to analyze debt rescheduling

simplify the examination of “What if?” questions, thereby introducing greater analytical rigor and clarifying issues of policy. I have chosen to build a relatively simple model in order to facilitate empirical analysis and to focus on elements that can be operationalized in view of available data. Once this model has proven to be effective for empirical analysis, we can later introduce greater sophistication into the effort.<sup>10</sup>

Second, why use game theoretic analysis? In debt negotiations, as in most other types of international bargaining, protagonists interact with one or more opponents who are presumed to be rational in the specific sense that each player chooses a policy favoring its own interests, while anticipating that its opponent will respond in kind. Game theory allows us to model this strategic and rational decisionmaking problem by considering what actors expect to gain as a result of different combinations of their own and their counterpart’s policy choices. Depending on how these “payoffs” are ordered in different situations, rational actors should behave in predictable ways.

Although this seems obvious, much work in international relations fails to explicitly recognize this strategic dependence. For example, although Bruce Bueno de Mesquita’s outstanding study *The War Trap* provides an approach to analyzing actors’ preference orderings, he “combines” them through an expected utility approach to find outcomes resulting from actors’ interaction. This mode of analysis is appropriate for examining individual decisionmaking under conditions of risk or uncertainty – but not for actors involved in a situation of strategic interaction.<sup>11</sup> Actors contemplating a decision to go to war will likely consider their opponents’ reactions, which in turn will depend on their opponents’ reciprocal expectations. Recognizing this omission, in his recent co-authored work with David Lalman, Bueno de Mesquita develops a game theoretic model that incorporates strategic interaction.<sup>12</sup>

Analysts of international conflict are not the only ones who often fail to examine strategic interaction. Scholars studying the development and stability of international regimes often assume that a single power will simply impose its will on others,<sup>13</sup> without focusing on the bargaining involved in such cases.<sup>14</sup> A new strand of research has questioned the analytical foundations of “hegemonic stability” theory, and has argued that strategic interaction among a few major powers can also lead to the formation of international regimes.<sup>15</sup> This analytical

<sup>10</sup> As an example, the conflict modeling pursued by Bruce Bueno de Mesquita (1981) and Bueno de Mesquita and Lalman (1986 and 1992) can be seen to be a progressive research program despite criticisms of its earlier formulations.

<sup>11</sup> See Harsanyi (1977) for a good discussion of decisionmaking models. The formulation he uses is similar to the one described at length by Luce and Raiffa (1957). I elaborate on these distinctions in Chapter 2.

<sup>12</sup> Bueno de Mesquita and Lalman (1992). <sup>13</sup> See, e.g., Krasner (1976).

<sup>14</sup> See Aggarwal (1985) for an analytical discussion and tracing of the bargaining process in such cases.

<sup>15</sup> See Keohane (1984) and Snidal (1985b). Keohane provides an institutional maintenance argument based on information theory to suggest the possibility of stability with multiple powers. Snidal provides a more formal game theoretic approach to this question.

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examination of strategic interaction has markedly contributed to our understanding of the development of such arrangements.

The third question, on choosing variables to derive actors' payoffs, is central. Studies that examine bargaining outcomes and then simply reinterpret them in "preference" and "game" terms are helpful only for illustrative purposes. Much more useful is considering how actors order their preferences in light of the constraints they face. Game theory, as such, is of no help in deriving actors' payoffs for different policy choices: it is simply one approach to rigorously analyzing players' interaction in view of specific payoffs.<sup>16</sup>

Unfortunately, most studies operate under the assumption that empirical situations can be modeled with a few specific types of games – without investigating the basis for actors' payoffs. The most popular game analyzed in the literature is the Prisoner's Dilemma (PD). Yet many cases of international bargaining do *not* have payoffs of the type found in this game. Unless one derives payoffs carefully, game analyses are either harmful – because they might misrepresent the true payoffs in a bargaining situation – or next to useless because they simply restate in game theoretic terms what we already know about the negotiations in a particular case. Hence, I expend considerable effort in deriving and justifying my choice of payoffs for particular bargaining situations.

Many excellent studies in international relations have shed some light on how one might specify actors' preferences, although for the most part, these works have emphasized the role of one variable. For example, in his important study of the patterns of openness in world trade over the last 150 years, Stephen Krasner postulates the basic goals that all states are likely to pursue, and then generates an informal rank ordering of countries' payoffs by focusing on the influence of the country's position in the international system.<sup>17</sup> He argues that large and small states will prefer open trading systems whereas medium powers will generally be more protectionist. Yet Krasner does not examine the impact of domestic coalitions or actors' beliefs on their ordering of preferences except in an ad hoc manner to explain significant anomalies in the findings.

Others, such as Peter Katzenstein, have looked at internal state structures as determinants of actors' preferences. His analysis of European foreign economic policies examines the relationship between state and society as the key explanatory factor.<sup>18</sup> But recognizing the inadequacy of a purely domestic focus, in a more recent study on small European states, Katzenstein incorporates the effect of the international system on state policy choices.<sup>19</sup> In this vein, work by Robert

<sup>16</sup> Pierre Allan (1983) in his study of decisionmaking was one of the first scholars in international relations to raise the issue of examining the origin of payoffs. In this work, he shows how bargaining games might vary solely as a result of changes in overall actors' power capabilities. Duncan Snidal also points to the need to develop payoffs systematically (1985a), but does not formally attempt to do so in this work. See also Oye (1985a) for a discussion of the basis for defining payoffs.

<sup>17</sup> Krasner (1976), pp. 319–322.

<sup>18</sup> See Katzenstein (1976) and his edited volume (1977) on foreign economic policies of the major Western European countries.

<sup>19</sup> See Katzenstein (1985).



Putnam also provides a highly promising approach to combining international and domestic political explanations. His analysis of international bargaining,<sup>20</sup> using what he terms a “two level” games approach (one game is international negotiations and the other domestic negotiations), provides some heuristic value. Still, Putnam does not present a systematic methodology to reconcile the competing constraints that actors face, thus limiting the utility of this approach.

Ernst Haas, Judith Goldstein, and others have concentrated on cognitive consensus or ideological predilections to examine actors’ preferences,<sup>21</sup> yielding useful insights into bargaining behavior. Nonetheless, as Haas notes, combining this approach with international power explanations provides a more satisfactory explanation of international regime change than simply examining actors’ cognition.

While every approach gives some insight into the analysis of actors’ behavior, we need to move beyond simply picking a favorite variable by finding a procedure capable of integrating several important factors.<sup>22</sup> My effort focuses on variables that reflect international and domestic concerns to describe the “individual situation” of actors. By formally hypothesizing how actors might differently weight their goals in response to changes in these variables, I provide a more systematic approach to determine actors’ valuation of outcomes.

Fourth and finally, scholars using game theory in international relations have generally failed to examine possible shifts in actors’ positions. Actors will obviously bargain to obtain the most favorable outcome when faced with the urgent need to make a strategic choice. But if they are dissatisfied with the outcome,<sup>23</sup> they may try to manipulate the constraints they face – despite having arrived at what scholars find to be the mathematically determined equilibrium for their bargaining situation. Specifically, I argue that actors receiving poor payoffs are highly motivated to change their situation and will draw upon three types of power resources to do so:<sup>24</sup> (1) international arrangements (through appeals to principles and norms or rules and procedures), (2) capabilities (issue-specific and those in other areas), and (3) alliances (with non-state and state actors). A more detailed discussion of these power resources and the constraints that actors face in using them is found in Chapter 4.

In short, my basic objective in this book is to develop both a static and a dynamic model to explain debt rescheduling outcomes. While I draw upon game theoretic and other rational choice tools in this effort, my primary concern is

<sup>20</sup> See Putnam (1988) and Evans, Jacobson, and Putnam (1993).

<sup>21</sup> Haas (1980), Goldstein (1993). Also see work by Rothstein (1984) and Young (1980).

<sup>22</sup> Cohen (1990), p. 269, in a review of a large number of studies on trade, notes that “What is needed is a methodology that considers domestic- and system-level variables simultaneously, rather than sequentially, and specifies whatever interactions there may be among all relevant variables in a rigorous manner.” Some studies, such as Odell (1982), have attempted to examine the interplay of different variables in explaining foreign policy. This work does not, however, explicitly address the question of deriving game payoffs.

<sup>23</sup> David Laitin has referred to such equilibria as “unhappy equilibria.” Personal communication.

<sup>24</sup> This idea of three power resources in negotiations was first discussed in Aggarwal and Allan (1983). See also Allan (1984) and Aggarwal (1987).

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with setting up the problem correctly: that is, I wish to carefully specify the payoffs that underlie game analyses and also show how they might change as a result of either intentional actions or exogenous shocks.

### Caveats

Some caveats about my own analytical efforts are worth mentioning. First, I do not theorize about why the factors underlying different historical epochs change over time. This is doubtless an interesting and essential issue, but for the purposes of this book, I regard these changes as given.

Second, the book provides a detailed analysis of the bilateral bargaining processes and outcomes; it does not seek historical completeness or to discover unknown facts for each bargaining episode. My analysis is based in part on an exhaustive analysis of available secondary sources for all countries, both in English and Spanish, entailing a total of over 800 articles and books. However, I found that secondary sources were often inadequate for my needs. Thus, I used extensive primary source material in the Bancroft Library at Berkeley for the first two epochs, and obtained several hundred pages of original documents from the National Archives in Washington, D.C., for the Peruvian and Mexican cases in the 1930s–1950s. The current cases also draw upon about 70 interviews which I conducted personally with bankers, debt negotiators from the four countries, and officials from creditor governments and international organizations. Thus, in addition to the findings that result from the application of my models, I have compiled and synthesized a very substantial body of empirical material on the four countries.

Third, I formally derive actors' valuations of different outcomes in view of the constraints they face, but from an empirical standpoint, our inability to observe such payoffs directly complicates the testing of the model. Consequently, our assessment of the model's validity rests on the degree of agreement between predicted game outcomes (based on the derived payoff structure and bargaining choices) with the outcomes we actually observe. But by examining the cases in detail through a tracing of the bargaining process, I am able to develop some insight into why the model does not work in particular cases.<sup>25</sup>

Finally, although many comparative and historical analytical studies implicitly code cases, they often do not specify the rules they use to do so. While some may question my coding of the dependent and independent variables, I have attempted to lay out the coding rules as clearly as possible to enable others to replicate – and challenge – my analysis and interpretation of events.

To summarize, my work makes three specific contributions to the study of international debt crises and to the broader scope of international relations analysis. First, unlike most social scientists (and some economists), I try to specify how actors will value different bargaining game outcomes. Moreover, in contrast to

<sup>25</sup> See George (1979) and Aggarwal (1985).