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In September 1994, the Federal Reserve Bank of Cleveland and the *Journal of Money, Credit, and Banking* sponsored a conference aimed at facilitating research on the structural context of U.S. monetary policy. The eight papers covered three broad topics: the macroeconomic effects of price rigidity and limited financial market participation by households, the interaction of inflation and financial intermediation, and the "deep structural" generation of empirically useful money measures. This article provides an overview of the conference.

That married men earn more than unmarried men is now a fairly well established fact. However, the source of this earnings premium remains debatable. In this paper, the authors use various econometric techniques to shed more light on the controversy, and find that much of the observed differential can be traced to the correlation between marital status and some unobservable individual effects. In other words, married men who earn more on average than single men would have earned more even if they were not married.

Violations of the absolute priority rule in both private workouts and Chapter 11 reorganizations have been enigmatic for financial economists. Why do such violations exist? Do they promote or curtail economic efficiency? This paper demonstrates that the answers depend on the specific contracting problem that a firm and its creditors face. As a result, an optimal bankruptcy institution should allow contract participants to decide ex ante whether such violations will occur.

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A Conference on Liquidity, Monetary Policy, and Financial Intermediation

by David Altig and
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Introduction

In September 1994, the Federal Reserve Bank of Cleveland and the *Journal of Money, Credit, and Banking* sponsored a conference on liquidity, monetary policy, and financial intermediation. This symposium was the fifth in a jointly sponsored series aimed at promoting research on basic issues in monetary policy, financial markets, and the payments system.

This particular conference dealt with monetary policy issues. The papers included examinations of the macroeconomic effects of price rigidity and "sluggish" savings decisions by households (that is, the assumption of limited participation in financial markets), the interaction of inflation and financial intermediation, and the "deep structural" estimation of parameters in models with money and financial intermediation. The common thread in all of these studies is the attempt to move us farther down the road to understanding the fundamental structures that ultimately determine the economic consequences of monetary policy. A complete list of the papers, their authors, and the discussants is provided in box 1.

This summary of the proceedings groups the papers (somewhat artificially) according to the type of model presented. The first group examines the general equilibrium effects of sticky prices, the second assumes that savings, rather than prices, are sluggish, and the third represents models of deep structural intermediation.

I. Sticky Prices

In traditional static IS-LM models with sticky prices, a monetary expansion leads to a fall in both nominal interest rates (the so-called liquidity effect) and real interest rates, which in turn stimulates investment spending and hence output. These types of models have come under attack recently on both empirical and methodological grounds. The lessons of the 1970s taught us that, contrary to the implications of the simplest versions of these models, high inflation concurrent with high unemployment is possible. However, it is generally recognized that such models were poorly specified and that the dynamic general equilibrium implications of price inflexibility may be much different—and empirically more plausible—than those of earlier static sticky-price models. For these reasons, there is a great deal of interest in

BOX 1

Papers Presented at the Conference on Liquidity, Monetary Policy, and Financial Intermediation, September 1994

"The Effects of Real and Monetary Shocks in a Business Cycle Model with Some Sticky Prices," by Lee E. Ohanian, Alan C. Stockman, and Lutz Kilian. Comment by Christian Gilles and John V. Leahy.

"The Quantitative Analytics of the Basic Neomonetarist Model," by Miles S. Kimball. Comment by Michael Woodford.

"Financial Intermediation and Monetary Policy in a General Equilibrium Banking Model," by Pamela Labadie. Comment by Deborah J. Lucas and Stephen D. Williamson.

"Monetary and Financial Interactions in the Business Cycle," by Timothy S. Fuerst. Comment by Charles L. Evans and Mark Gertler.

"Inside Money, Outside Money, and Short-Term Interest Rates," by V.V. Chari, Lawrence J. Christiano, and Martin Eichenbaum. Comment by Wilbur John Coleman II and Julio J. Rotemberg.

"Estimating Policy-Invariant Deep Parameters in the Financial Sector When Risk and Growth Matter," by William A. Barnett, Milka Kirova, and Meenakshi Pasupathy. Comment by Stephen G. Cecchetti and David A. Marshall.

"Liquidity Effects and Transactions Technologies," by Michael Dotsey and Peter Ireland. Comment by Finn E. Kydland, Donald E. Schlagenhauf, and Jeffrey Wrase.

"Computable General Equilibrium Models and Monetary Policy Advice," by David E. Altig, Charles T. Carlstrom, and Kevin J. Lansing. Comment by Eric M. Leeper and Edward C. Prescott.

SOURCE: *Journal of Money, Credit, and Banking*, vol. 27, no. 4, part 2 (November 1995).

examining price rigidities in dynamic general equilibrium frameworks.

In this vein, the papers by Miles Kimball and by Lee Ohanian, Alan Stockman, and Lutz Kilian take as their starting point this familiar position: Monetary policy has real effects because some or all goods in the economy have sticky prices. Both papers represent an attempt to construct explicitly dynamic macroeconomic models with less-than-perfect price flexibility, incorporating elements typically associated with simple Keynesian analysis into relatively standard real-business-cycle frameworks.

In "The Effects of Real and Monetary Shocks in a Business Cycle Model with Some Sticky Prices," Ohanian, Stockman, and Kilian (OSK) adopt a simple specification for price stickiness (dating back at least to Phelps and Taylor [1977]) in which firms that exhibit price rigidity preset their prices one period in advance. However, unlike the few other papers that are similarly constructed, OSK allow for both a sticky- and a flexible-price sector.[1] Their novel finding is that the cyclical behavior of aggregate variables is influenced only slightly by introducing price rigidities, even when the sticky-price sector is relatively large and despite the important distributional consequences associated with real and monetary shocks.

This surprising conclusion—which does not characterize one-sector sticky-price models—appears to arise from the assumption that the investment sector is not subject to price rigidities. The key insight into understanding this result is that modeling investment as a sticky-price good induces both an intratemporal and an intertemporal distortion, the latter of which is not present when investment is the flexible-price good. If investment were placed in the sticky-price sector, OSK's results would presumably change dramatically.

In "The Quantitative Analytics of the Basic Neomonetarist Model," Miles Kimball maintains the simpler one-sector setup of earlier papers. However, unlike OSK, who assume that all sticky prices are preset one period in advance, his introduction of less-than-perfect price flexibility is motivated by increasing returns to scale and imperfect competition. Like Calvo's (1983) model, each firm gets the opportunity to adjust its prices at intervals determined exogenously by a Poisson process.[2] One interesting implication of this setup is that the aggregate rate of price adjustment in general equilibrium differs from the rate at which prices adjust for individual firms. In fact, given Kimball's preferred parameter values, the rate of macroeconomic price adjustment is four times that of an individual firm. This surprising degree of

1 See, for example, Cooley and Hansen (1994), Cho and Cooley (1990), and King (1990).

2 The Poisson distribution specifies that the probability of an opportunity to adjust prices is the same for all time intervals of equal length, and that these probabilities are independent across any two periods.

persistence is potentially important, given that the persistence in most general equilibrium business-cycle models is very weak. (We will return to the issue of persistence below.)

The sticky prices at the heart of the OSK and Kimball papers are, of course, central to the typical textbook treatment of static Keynesian IS-LM analysis. Interestingly, it is unclear whether the liquidity effect survives the translation of price rigidity to dynamic general equilibrium contexts. In Kimball's model, a monetary injection stimulates investment spending, which, in the absence of adjustment costs, will increase the real interest rate. Because such policies also raise inflation expectations when money is positively serially correlated, the nominal interest rate rises unambiguously. This conclusion can be overturned by the introduction of adjustment costs *if* prices in the economy adjust quickly enough. However, unless the half-life for macroeconomic price adjustments is less than two quarters—which Kimball argues is unrealistically brief—real interest rates will increase with monetary injections, ruling out any hope for generating liquidity effects.

In OSK, monetary injections do temporarily lower the real rate of interest.[3] However, based on their chosen calibration, increases in anticipated inflation dominate these real effects, and the nominal rate rises following a positive monetary shock.

In light of these results, it does not appear straightforward to construct sticky-price models that generate liquidity effects. The key difficulty is that prices must adjust slowly to mitigate the expected inflation component. However, we know from Kimball's paper that slower price adjustment is precisely the condition that magnifies demand effects, thus increasing investment demand and the real rate of interest.[4]

II. Limited-Participation Models

The difficulty in generating a liquidity effect with sticky-price models leads Kimball to conclude that "it may be necessary to model any real-world tendency for the real (and hence the nominal) interest rate to fall in response to a monetary stimulus as a result of output being temporarily off the IS curve." In some sense, this is essentially the strategy of the so-called limited-participation framework pioneered by Lucas (1990) and Fuerst (1992). The key insight of these papers is embedded in the assumption that agents must adjust their portfolios slowly. Although investment equals saving ex post, limitations on financial market transactions break the household's ex post intertemporal linkage, at least temporarily. Such a break would appear to be a necessary condition for any model to simultaneously generate a liquidity effect and a hump-shaped response of consumption following a decline in the federal funds rate, both of which seem to characterize post–World War II data.[5]

In fact, a central motivation for the limited-participation framework is to provide a model in which monetary injections can generate both a liquidity effect and a temporary expansion of output. Four of the papers in this volume—Chari, Christiano, and Eichenbaum; Altig, Carlstrom, and Lansing; Dotsey and Ireland; and Fuerst—can be considered studies that flesh out the properties of models incorporating the limited-participation device.

"Inside Money, Outside Money, and Short-Term Interest Rates," by V.V. Chari, Lawrence Christiano, and Martin Eichenbaum (CCE), attempts to impose a theoretical structure on key monetary business-cycle regularities identified in Christiano and Eichenbaum (1992) and Christiano, Eichenbaum, and Evans (1995). Of particular concern is what the authors refer to as the "sign-switch" phenomenon: Nonborrowed reserves co-vary negatively with the federal funds rate, while broader measures of money co-vary positively.

An essential element of CCE's model is a careful disentangling of exogenous monetary shocks from endogenous responses of both the monetary authority and private intermediaries. The key identifying assumptions with respect to the monetary authority's behavior are that innovations in nonborrowed reserves are associated with exogenous policy shocks, and innovations in borrowed reserves are endogenous policy reactions to output, or technology, shocks. Because nonborrowed-reserve innovations represent unanticipated policy changes,

3 Because the OSK model presets prices for one period, this result is consistent with Kimball's conclusion that real rates decline when prices adjust relatively quickly.

4 Sticky wages do not resolve this quandary. Wages that take sufficiently long to adjust also lead to the prediction that the real interest rate is positively related to monetary surprises. Ohanian and Stockman (1995) provide an example of a two-sector model with price rigidity in which liquidity effects arise. However, this variant of their model does not include capital. Again, it appears that the treatment of investment is critical in sticky-price models.

5 See, for example, Christiano, Eichenbaum, and Evans (1995).

TABLE 1

Correlation Properties of Money and Output, 1954:IQ–1988:IIQ

	x_{t+2}	x_{t+1}	x_t	x_{t-1}	x_{t-2}
M1	0.33	0.34	0.29	0.18	0.10
Monetary base	0.37	0.39	0.34	0.26	0.20
Nonborrowed reserves	0.10	–0.06	–0.22	–0.32	–0.34

NOTE: Entries represent the correlation of x_t with output$_{t-k}$. All variables are logged and Hodrick–Prescott filtered.
SOURCE: V.V. Chari, Lawrence J. Christiano, and Martin Eichenbaum, "Inside Money, Outside Money, and Short-Term Interest Rates" (see box 1).

they interact with the limited-participation assumption to generate liquidity effects.[6] Broader aggregates, however, are dominated by both the positive response of the discount window and loan creation by financial intermediaries, thus accounting for the sign switch that the authors wish to capture.[7]

The model also broadly captures some of the simple dynamics of relationships between the federal funds rate and various monetary aggregates found in U.S. data (see table 1). Specifically, consistent with the data, the CCE model generates a positive correlation between the short-term interest rate and lagged values of the model's analogue to M1 and the monetary base, as well as a negative correlation with future values. In addition, the model exhibits the observed symmetric negative correlation between the interest rate and nonborrowed reserves. However, the leading relationship of the funds rate with the monetary variables is much stronger in the data than in the model. The authors attribute this to the offsetting influences of real and monetary shocks, and suggest that fully capturing these dynamics would require either strengthening the dynamic effect of monetary shocks or reducing that of real shocks.

Contrasted with CCE, the paper by David Altig, Charles Carlstrom, and Kevin Lansing (ACL) maintains the less-rich intermediary structure of earlier limited-participation models. In "Computable General Equilibrium Models and Monetary Policy Advice," ACL's innovation involves examining the model's short-run forecasting performance—an approach for "taking the model to the data" that has been largely unexplored in the context of quantitative general equilibrium analysis.[8] In addition, the setup in ACL incorporates a central-bank reaction function that involves operating on a nominal interest-rate target, as opposed to the standard strategy of expressing the reaction function in terms of a monetary aggregate.

ACL's goal is to investigate whether computable general equilibrium models have reached the stage where they can be directly useful to policymakers. The specific question posed is whether variations and extensions of fairly standard quantitative-theoretic models can provide accurate real-time forecasts of both inflation and real GDP growth. The results of this exercise are mixed. ACL argue that quantitative-theoretic models do appear to be capable of delivering a reasonable degree of forecasting accuracy: When mean-squared-error and mean-absolute-deviation metrics are used, the model's forecast errors with respect to inflation and output growth are comparable to those of the internal Federal Reserve Board forecasts constructed for Federal Open Market Committee briefings. However, to obtain inflation forecasts that are at least as good as Board staff projections, ACL make an ad hoc, "judgmental" adjustment to their model.[9]

As in the CCE paper, the failures in ACL provide some clues about the direction that limited-participation models must take to deliver a fully satisfactory empirical performance. For example, the problem with the nonjudgmental ACL model shows up clearly in its inflation forecasts for 1993. Over the course of that year, the federal funds rate and inflation both

6 Some additional persistence is built into the model by assuming that interperiod portfolio adjustment is costly, in contrast to the solely one-period sluggishness built into the Fuerst (1992) model.

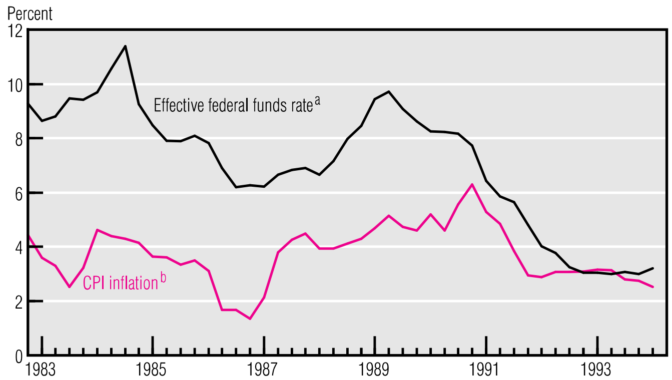
7 The identifying assumptions in CCE are somewhat stronger than those imposed in either Christiano and Eichenbaum (1992) or Christiano, Eichenbaum, and Evans (1995), in that these papers do not assume that nonborrowed-reserve innovations are entirely exogenous to current technology shocks. This difference is likely to be important: The CCE model counterfactually predicts a positive contemporaneous correlation between nonborrowed reserves and output. As the authors point out, the observed negative correlation could presumably be generated by incorporating a reaction function in which the monetary authority "leans against the wind." Whether such a model would continue to exhibit the sign-switch property is an open question.

8 See, however, Rotemberg (1994), which measures model performance in terms of the correlations among long-run forecastable movements of prices, output, and the monetary aggregates.

9 The ACL model treats deviations of the federal funds rate as exogenous inputs in simulating the path of inflation and output. Their judgmental adjustment amounts to replacing the deviation of the nominal federal funds rate from a constant mean (or the long-run value) with the deviation from a "moving" mean, measured as a moving average of past federal fund rates.

FIGURE 1

Federal Funds Rate and Inflation, 1982:IVQ–1994:IQ



a. Average daily rate on overnight fed funds as reported by the Federal Reserve Bank of New York.
b. Four-quarter percent change in the Consumer Price Index.
SOURCES: U.S. Department of Labor, Bureau of Labor Statistics; and Board of Governors of the Federal Reserve System.

stabilized near 3 percent, implying inflation-adjusted real rates near zero (see figure 1). Given the calibration of the model, the ACL framework delivers a long-run real interest rate of 3 percent. In the absence of persistence in real interest rates, or perhaps more extensive monetary non-neutralities than those delivered by a one-quarter limited-participation assumption, the only way to support a sustained 3 percent nominal funds rate is for monetary policy to engineer an expected inflation of approximately zero. But such an outcome is clearly inconsistent with the data, since it is unreasonable to believe that agents would have rationally expected zero inflation during that period.

ACL argue that the failure of their model is actually a failure present in most general equilibrium models; that is, most existing frameworks do not deliver the type of persistence in real variables that is found in the data.[10] In "Monetary and Financial Interactions in the Business Cycle," Timothy Fuerst examines this issue by investigating whether adding more extensive non-neutralities arising from financial markets can generate more serial correlation in real variables than does a standard model. In particular, he looks at whether persistence can be introduced by adding a financial structure that gives rise to countercyclical endogenous agency costs.

The basic idea of the Fuerst framework is to build in frictions similar to those discussed by Bernanke and Gertler (1989). Entrepreneurs live for one period and work to receive a wage income. They then use this income, along with additional funds borrowed from households, to produce capital. Individual entrepreneurs can costlessly observe how much capital they produce, but other agents must expend a resource cost to monitor the project's outcome. This agency problem leads to a standard debt contract and reduces the amount of investment (and thus capital accumulation) in equilibrium. The idea is that a positive technology shock today will increase an entrepreneur's net worth or wage income, which will mitigate agency costs and boost capital accumulation. The hope is that this extra capital will lead to greater persistence.

Fuerst finds that the amount of persistence generated by his experiments is nearly identical to that of standard quantitative business-cycle models. The reason is that his method of introducing agency costs leaves capital as the only method of propagating shocks across time. The failure is thus another illustration that capital adds little to the serial correlation properties of the standard quantitative business-cycle model.

Although Fuerst's model does exhibit a small propagation effect on net worth through the capital channel, his setup is fundamentally unable to generate persistent movements in net worth because entrepreneurs are assumed to live for only one period. However, allowing entrepreneurs to live for many periods opens up the possibility of a repeated game between households and entrepreneurs as well as an immense amount of potential heterogeneity, implying that straightforward extensions of Fuerst's model are nontrivial.[11]

A skeptical view of the limited-participation framework is provided by Michael Dotsey and Peter Ireland. In "Liquidity Effects and Transactions Technologies," they note the ad hoc one-period adjustment cost formulation in both ACL and Fuerst, and similarly criticize CCE's use of a more general adjustment cost formulation without proper calibration of the magnitude of these costs. Like CCE's model, Dotsey and Ireland's considers a financial intermediation structure that implies a spread between the

10 This idea is not new. For example, Cogley and Nason (1995) argue that persistence in most quantitative business cycle models is completely inherited from the exogenous shock process, which is assumed to be serially correlated. See Boldrin, Christiano, and Fisher (1995) for a recent attempt to build a framework that directly tackles this issue in the context of resolving asset-pricing puzzles that arise in standard business cycle models.

11 See Carlstrom and Fuerst (1996) for an extension of Fuerst's model that includes entrepreneurs who live for multiple periods.

deposit rate paid to households and the loan rate charged by intermediaries. Although less rich in detail than the CCE model, the Dotsey/Ireland framework expands the basic limited-participation setup by introducing explicit representations of the costs of adjusting both household and intermediary portfolios. The central question they address is whether the liquidity effects generated by existing limited-participation models survive the introduction of plausible specifications for such costs.[12]

The challenging aspect of this exercise is to calibrate the relevant adjustment cost functions. Dotsey and Ireland do so by capitalizing on the fact that their model delivers a wedge between loan and deposit rates that is dependent on the parameters of the representative intermediary's adjustment cost function.[13] The model is explicitly connected to the data by associating loan rates with commercial paper rates, and deposit rates with the return on small time deposits.

Unfortunately, it is not possible to calibrate directly to the spread itself, since generating the observed average differential would require introducing fixed marginal costs to loan production, which are themselves unobservable. The authors' provocative solution is to calibrate to the standard deviation of the difference between commercial-paper and time-deposit rates. This does not, however, pin down the key parameter in the household's cost function. Dotsey and Ireland proceed by assuming that household costs are a simple multiple of the costs to a financial intermediary.

The bottom line of the Dotsey/Ireland experiments is that the liquidity effects which motivate the limited-participation framework are not easily preserved when the assumption of infinite transaction costs is relaxed. Given their parameterization, they find that liquidity effects of the magnitude reported by, say, Christiano and Eichenbaum (1992), require household transaction costs that are roughly seven times as large as intermediaries' costs. This corresponds to about 123 minutes of forgone leisure per quarter.

III. Deep Structural Intermediation

Dotsey and Ireland conclude that their results militate for research efforts that return to "... the more careful methodology of building financial structure from microfoundations ...." Such efforts are represented in the papers by Pamela Labadie and by William Barnett, Milka Kirova, and Meenakshi Pasupathy.

The Labadie study, "Financial Intermediation and Monetary Policy in a General Equilibrium Banking Model," contains a detailed model with many salient features of the U.S. financial sector. Banks, for example, are subject to reserve requirements, hold assets that consist of loans (to both the private and public sectors) and equity capital, and have access to deposit insurance. Furthermore, the model contains a government sector that operates much like the Federal Reserve in that it sets reserve requirements, supplies deposit insurance, and conducts open-market operations that alter the aggregate ratio of bonds to money.

The Labadie framework incorporates several features typically associated with monetary non-neutrality. These include informational asymmetries that make intermediation costly, and household assets (deposits) with fixed nominal returns. Despite these elements, Labadie reports the surprising result that monetary policy actions which alter the size and composition of nominal assets are entirely neutral. Although she finds that non-neutralities appear in cases where monitoring costs are fixed in nominal terms, it is unclear how such a device should be interpreted.

It is apparent that some other special features of Labadie's model contribute to this result. Banks, for instance, write optimal state-contingent loan contracts that are expressed in real terms. Also, household saving consists solely of bank deposits and bank equity. Thus, it appears that any redistribution of wealth caused by the effects of unanticipated price changes on real deposit returns are rechanneled to the affected agents via changes in the market value of equity claims. Disentangling this and the many other elements of her very rich model could provide considerable insight into how the regulatory environment and financial market structure interact and what the consequences are for the macroeconomy.

12 The standard model in this class assumes (at least implicitly) that intraperiod adjustment costs are infinite.

13 The model's cost functions — which depend on the ratio of deposits to money — are quadratic, so that the spread is proportional to the function's sole parameter. Although both the CCE and Dotsey/Ireland models generate deposit-to-loan-rate spreads, they arise from very different sources. The spread in CCE results from a combination of reserve requirements and the contribution of excess reserves in the representative intermediary's production function.

As in almost all of the papers presented at the conference, the Labadie model takes the measurement of money as a given. In studies where models are taken to the actual data, money is assumed to correspond to some standard monetary aggregate. An exception is the article by Barnett, Kirova, and Pasupathy (BKP), who explore a methodology to construct money from the fundamental problems solved by economic actors in a well-defined, explicit economic environment.

In "Estimating Policy-Invariant Deep Parameters in the Financial Sector When Risk and Growth Matter," BKP start from the perspective of the well-known Lucas critique; that is, sensible experiments involving policy simulations require knowledge of the functions describing private decision rules that are invariant to the class of policy interventions being considered. However, the authors, appealing to insights from Barnett's earlier work, take the argument one step further: Experiments involving policy simulations also require knowledge of the policy-invariant aggregator functions describing the theoretical monetary aggregates.[14]

The strategy in BKP is to jointly estimate the deep parameters of preferences and technologies—including the parameters of the relevant aggregator functions—from Euler-equation representations of the optimization problems of financial intermediaries, manufacturing firms, and households. Upon obtaining these estimates, the authors compare the implied theoretical aggregates from the separate sectors with the corresponding Divisia indexes and simple-sum aggregates. They argue that the Divisia indexes do a relatively good job of tracking their theoretical money measures, and that simple-sum aggregates—the class of which contains all the typical monetary aggregates used in the other papers—do substantially worse.

BKP's critique of the standard approach to measuring monetary assets is a serious challenge to anyone interested in the empirical relationship between money and the macroeconomy. In describing their methodology, BKP write:

> The purpose of all scientific research is to reveal the truth, not to alter the data in a manner that may tend to justify some preconceived policy view. The purpose of data is to measure something that exists, i.e., an aggregator function that is separable within the structure of the economy. (p. 1405)

14 See Barnett (1987), Barnett, Fisher, and Serletis (1992), and Barnett and Hahm (1994).

Contrast this view with the position taken by Friedman and Schwartz (1970):

> ... the definition of money is to be sought for not on the grounds of principle but on grounds of usefulness in organizing our knowledge of economic relationships. "Money" is that to which we choose to assign a number by specified operations; it is not something in existence to be discovered, like the American continent .... (p. 137)

Determining which of these views is correct has fundamental implications for the organization and development of monetary facts and, ultimately, for the conduct of monetary policy.

IV. Summary

Each paper presented at the conference investigates at least one piece of the puzzle that must be solved if policymakers are to use dynamic general equilibrium models for giving policy advice. OSK and Kimball both provide a cautionary note by showing that the implications of sticky prices may not be as apparent as many economists think. For instance, it is inherently very difficult for this assumption to deliver a liquidity effect—something that most policymakers take for granted. Although the limited-participation (or sluggish-portfolio) assumption was invented to specifically generate inverse movements in money shocks and nominal interest rates, Dotsey and Ireland question whether portfolio costs, when properly calibrated, are large enough to deliver the desired effect.

Similarly, when a fairly standard computable general equilibrium model is actually taken to the data and used for forecasting purposes, ACL conclude that existing models need either more extensive monetary non-neutralities or some other added friction in order to generate the persistence in real variables that characterizes the data. Yet the message of Labadie's paper is that adding frictions is not always sufficient to generate monetary non-neutralities, let alone ones that have lasting effects on the real economy.

These unanswered questions clearly leave researchers with much work to do before dynamic general equilibrium models supplant static IS-LM models for policymakers, as they have for most academic economists.

NOTE: To order a copy of these conference proceedings, see page 32.

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Marriage and Earnings

by Christopher Cornwell
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Introduction

Uncovering the determinants of earnings is an important and well-researched area in labor economics. In studies of race or sex discrimination, it is imperative to use statistical methods that control for various factors so that the researcher can obtain an unbiased measure of discrimination. Another area that has attracted interest is the interaction of wages and union membership. Again, controlling for certain factors enhances our understanding of how unions affect wages. Of course, knowing exactly *what* to control for is at the heart of the problem. In this paper, we examine one particular control variable that is often used in earnings regressions—an indivdual's marital status. In analyzing and interpreting the results obtained from earnings regressions, we hope to develop a better measure of how any policy affects (or does not affect) individual behavior.

The wage premium attributable to marriage has been well documented in the literature and is typically as large as that associated with union status. The source of this premium, however, remains debatable. Two common explanations of why married men earn more than unmarried men are 1) the division of labor in a married household allocates more of the man's time to the market,[1] and 2) married men have a lower cost of human capital acquisition, since a spouse may be working to help finance the additional human capital.[2] Both of these stories imply that marriage enhances productivity, and therefore wages, as a result of an increase in human capital. We suggest an alternative explanation for the marriage premium, derived from the job-matching literature, in which marriage signals certain unobservable individual characteristics that are valued by employers—including ability, honesty, loyalty, dependability, and

1 As evidence that married men are relatively more market intensive, it is often noted that they work more hours. For example, in the sample we draw from the 1971 wave of the National Longitudinal Survey of Young Men (NLSYM), mean hours per week for married men is 44.17, versus 41.28 for unmarried men. However, after age 22, the mean hours differential drops to less than one hour per week.

2 Some evidence that marriage facilitates human capital acquisition is provided by Kenny (1983), but the potential endogeneity of an individual's marital status is ignored. Furthermore, the argument that marriage makes it cheaper to accumulate human capital is difficult to reconcile with the fact that men who acquire more formal education tend to marry later than those who acquire less (Bergstrom and Schoeni [1992]).

determination.[3] Failure to control for the correlation of the fixed effects with marriage will lead to a bias in the marital status coefficient. That is, some of the returns attributable to marriage will actually be returns to some unobserved qualities correlated with marital status.

Under either of the above scenarios, marital status should not be treated as an exogenous determinant of the wage rate. However, it is assumed to be exogenous in most wage regressions that control for marital status, resulting in an estimated marriage premium that is biased upward.

In this paper, we reexamine the empirical relationship between marriage and wages. Our investigation proceeds along two lines. First, to the extent we can model the process that determines marriage, our cross-section procedures attempt to capture the kind of incentives that the human capital stories imply for the marriage premium. Second, we employ panel data estimation techniques that allow us to control for unobservable individual-specific effects that may be correlated with marital status. If we interpret these individual effects as ability—or as any of the qualities listed above that may lead to better job matches — then panel data estimation addresses a different source of endogeneity than that arising from the human capital arguments.

The clearest picture of the effects of marriage on wages emerges in our panel estimates. In every case where we condition on unobserved individual effects, the estimated marital status coefficient is essentially zero. We argue that these results support the view of marriage as a “signal'' of some underlying characteristics. Furthermore, specification tests confirm the importance of unobserved heterogeneity and reject the exogeneity of marital status.

I. Marriage, Wages, and Individual Effects

Virtually all cross-section wage regressions that control for marital status report a large, statistically significant wage premium for married men. Some of the more prominent examples are discussed by Reed and Harford (1989) and Korenman and Neumark (1991). Our view is that the marriage premium commonly reported in cross-section wage regressions is largely a statistical artifact, at least for young men. The wage premium can be explained in terms of unobservable individual characteristics that are positively correlated with marriage and wages. The characteristics that lead to “good'' (long and stable) marriages are the same characteristics that produce “good'' (long and stable) jobs and higher wages.

This view has some additional support from another strand of the literature relating to the returns to job tenure. Abraham and Farber (1987) propose that workers in long-tenure jobs earn more *in every year* on the job, and that most of the cross-section return to tenure is due to unobserved individual and job-match effects. They test their proposition by estimating wage equations conditional on predicted job duration. Interestingly, the results of their job duration model indicate that marriage has a large and positive statistically significant effect.

Further evidence exists in the quit behavior of married men. Consistent with the positive relationship between marriage and job duration is the depressing effect marriage has on quits. Shaw (1987) reports that the quit rate for married men aged 25–54 is less than half that of unmarried men. He also finds that marriage has its strongest deterrent effect on the quit behavior of younger men.

Only Nakosteen and Zimmer (1987) argue explicitly that marriage does not significantly affect wages. However, the empirical support for their argument is weak. Using a cross-section of 576 employed men between the ages of 18 and 24, taken from the 1977 wave of the Panel Study of Income Dynamics (PSID), they estimate an earnings equation in which marriage is modeled as a treatment effect, thereby making marital status endogenous.[4] When they restrict marital status to be exogenous and apply ordinary least squares (OLS) to their wage equation, Nakosteen and Zimmer obtain a statistically significant marital status coefficient estimate of 0.370. Relaxing the exogeneity restriction actually causes this coefficient estimate to *rise*, although it is no longer statistically significant. Furthermore, specification tests of exogeneity are inconclusive. Nevertheless, Nakosteen and Zimmer find that the true marriage premium is not significantly different from zero.

3 Reed and Harford (1989) provide another alternative—that the marriage premium represents a compensating differential required to induce married men to accept “undesirable” working conditions. In their view, marriage is related to the purchase of costly “family goods” such as children.

4 See Barnow, Cain, and Goldberger (1980) for an explanation of treatment effect models.

Korenman and Neumark, whose results are based on the 1976, 1978, and 1980 waves of the NLSYM, take the opposite stance. They claim that the gains to marriage are large for young (white) men, even after controlling for unobserved heterogeneity. In their preferred specification, years married and its square are included in the wage equation, along with the marital status dummy. Marriage premiums are derived from both cross-section and fixed effects ("within") estimates. The marriage premium at mean years married, calculated from the fixed effects estimates, is 15 percent, which is about 20 percent smaller than the premium yielded by their cross-section estimates.

However, the large gains to marriage reported by Korenman and Neumark may be misleading. First, only the estimated years-married coefficients are statistically significant. This is true in both their cross-section and fixed effects regressions. Thus, more than one-fifth of these premiums are due to shift-parameter estimates that are not significantly different from zero.

Second, when we include years married and its square along with tenure and its square in our sample, the former never enters statistically significantly, while the latter always does. Because married men hold longer jobs (experience less turnover), years married may be playing a role similar to tenure in the Korenman/Neumark regressions.

Third, Korenman and Neumark's results suggest that each additional year of marriage translates into a 1 to 2 percent wage gain. As indicated by Bergstrom and Schoeni (1992), this implies that men who married at age 17 should earn 10 to 20 percent more at age 40 than men who married at age 27, all else equal. However, their results show that, controlling for current age, men who married at 17 make 25 percent *less* on average than men who married at 27.[5] Finally, Cornwell and Rupert (1996) demonstrate that adding another year (1971) to the Korenman/Neumark sample changes the results substantially. This change can be attributed to the fact that most of the marital status changes in their sample represent individuals who are either leaving or entering marriage for the second time.[6]

II. Econometric Framework

Our econometric methodology addresses the possibility of marital status endogeneity in a variety of ways. Each of the techniques has shortcomings as well as merits. Therefore, we cover a wide range of procedures and attempt to derive a consensus regarding the contribution of marital status to earnings.

We begin with a general model of wage determination for married (M) and single (S) men, where the wage-generating process is initially assumed to be different for each type:[7]

$$(1)\quad y_{it}^{M} = \delta^{M} + X_{it}^{M\prime}\beta^{M} + u_{it}^{M}$$

$$(2)\quad y_{it}^{S} = \delta^{S} + X_{it}^{S\prime}\beta^{S} + u_{it}^{S},$$

where i indexes individuals ($i = 1, \ldots, N$) and t indexes time periods ($t = 1, \ldots T$), y_{it} is the natural logarithm of the real wage, X_{it} is a vector of explanatory variables, and the u_{it}'s are disturbances with time-invariant and time-varying components, expressed as

$$(3)\quad u_{it}^{M} = \alpha_i^{M} + \epsilon_{it}^{M}$$

$$(4)\quad u_{it}^{S} = \alpha_i^{S} + \epsilon_{it}^{S}.$$

The α_i's, which vary over individuals but not over time, capture unobserved, individual-specific attributes that may be valued in both the labor and marriage markets. The ϵ_{it}'s, which vary over individuals and time, reflect aspects of the wage-determining process that can be represented as statistical noise.

In our sample, we cannot reject the null hypothesis that $\beta^M = \beta^S$.[8] Thus, we express the model given in equations (1)–(4) in terms of a single wage equation,

$$(5)\quad y_{it} = \delta^{S} + X_{it}'\beta + (\delta^{M} - \delta^{S})\, M_{it} + \eta_{it},$$

where

$$(6)\quad \eta_{it} = u_{it}^{S} + (u_{it}^{M} - u_{it}^{S})\, M_{it}$$

and M_{it} is a dummy variable indicating marital status.

5 Bergstrom and Schoeni point out that to reconcile these results, one would have to argue that if men who married at 27 had married at 17, they would have earned 35 to 40 percent more per year than men who actually did marry at 17.

6 By appending the additional (earlier) year to the Korenman/Neumark data set, we can look at the earnings of younger men or, more specifically, at the earnings of men who have never been married. The evidence suggests that to the extent there is a gain to marital status, it is purely an intercept shift, with no additional effect attributed to the number of years married.

7 Our approach is similar to that employed by Robinson (1989) in his analysis of union wage effects.

8 The value of the *F*-statistic from a comparison of regressions of married and single men over the two periods covered in our data set is only 0.75, and the 95 percent critical value is about 2.8.

If the correlation between marriage and the error term is zero (that is, $E[M_{it}\eta_{it}] = 0$), then treating (5) as a standard cross-section wage regression and estimating by OLS produces a consistent estimate of the marriage premium, $(\delta^M - \delta^S)$. However, for reasons outlined in the previous section, this is *not* likely to be the case. Thus, in general, the marriage premium cannot be identified by OLS.

Since M_{it} is unlikely to be orthogonal to η_{it}, identification of the marriage premium is problematic. The difficulty is that the wage differential comprises two terms: $(\delta^M - \delta^S)$ and $(u_{it}^M - u_{it}^S)$. The first term is fixed and the same for all men. If men are randomly distributed across married and single states, then this term represents the true wage premium. Even if assignment across married and single states is random, however, standard cross-section estimation may still be inappropriate. If the source of endogeneity of M_{it} is unobserved individual attributes that are valued by employers as well as potential marriage partners, then consistent estimation would imply conditioning on the α_i's. Panel data are important in this regard, a point that we elaborate on below.

If assignment to the married and single states is not random, then identification of the true wage premium is complicated by the second term, $(u_{it}^M - u_{it}^S)$. In this case, the expected increase in the wage rate as a result of marriage is $(\delta^M - \delta^S) + E[(u_{it}^M - u_{it}^S) \mid M_{it} = 1]$. Separate identification of $(\delta^M - \delta^S)$ requires additional restrictions on our model so that the process generating marital status can be parameterized. The restrictions may involve orthogonality conditions that define a set of exogenous variables for marital status, or distributional assumptions like bivariate normality of y_{it} and M_{it}. The validity of such restrictions is an empirical question. Imposing them when they are empirically invalid is a misspecification, the statistical consequences of which may be less acceptable than the failure to control for nonrandom assignment.

Alternative Estimators and Specification Tests

We consider three different approaches to estimating the effect of marriage on wages: instrumental variables (IV), the inverse Mills ratio (IMR) method, and methods that exploit the availability of panel data. Each approach imposes a different set of restrictions on the model. The choice of which approach to use is determined largely by whether the restrictions can be justified.

The appeal of both the IV and IMR methods hinges on the ability to specify the process governing marital status.[9] Let the reduced form for this process be expressed as

$$(7) \qquad M_{it}^* = Z_{it}'\gamma + v_{it},$$

where M_{it}^* is a latent index representing the net gain to marriage, Z_{it} is a vector of explanatory variables that includes X_{it}, and v_{it} is a zero mean disturbance with variance σ_v^2. We observe only a discrete realization of M_{it}^*, which we define as the dummy variable M_{it}. Note that $M_{it} = 1$ if $M_{it}^* > 0$, and $M_{it} = 0$ if $M_{it}^* \leq 0$.

Instrumental Variables

The IV procedure exploits the orthogonality condition

$$(8) \qquad E[g(Z_{it})\eta_{it}] = 0,$$

where Z_{it} contains at least one regressor not in X_{it}, and g is some known transformation of Z_{it}. The set of restrictions in (8) are used in (7) to construct an instrumental variable for M_{it} that has been purged of correlation with η_{it}. No distributional assumption about the v_{it}'s is necessary, although one may be imposed. For example, if the v_{it}'s are independently and identically distributed (i.i.d.) standard normal and are independent of Z_{it}, then (7) can be estimated by probit maximum likelihood. The resulting instrument for M_{it} would be $\hat{M}_{it} = \Phi(-Z_{it}'\hat{\gamma})$, where Φ is the standard normal cumulative distribution function. In any case, the instrumental variable is inserted in a least squares regression of y_{it} on $(X_{it}, \hat{M}_{it})$, and the estimated coefficient of $\hat{M}_{it}$ is taken to be the measure of the effect of marriage on wages. However, if the condition in (8) is violated, the IV estimate of the marriage premium will not be consistent. Assuming (8) holds, the IV estimator provides a natural contrast to OLS, thereby providing a Wu–Hausman-type test of the exogeneity of marital status (see Wu [1973] and Hausman [1978]).

9 A detailed discussion of these methods is provided by Heckman and Robb (1985).

Inverse Mills Ratio

The IMR method addresses the endogeneity of marital status in the context of the nonlinear regression function

$$(9)\quad E(y_{it} \mid Z_{it}, M_{it}) = \delta^S + X_{it}'\beta + (\delta^M - \delta^S)M_{it} + \xi h(Z_{it}, M_{it}; \gamma),$$

where

$$(10)\quad h(Z_{it}, M_{it}; \gamma) = M_{it}E(\eta_{it} | Z_{it}, M_{it} = 1) + (1 - M_{it})E(\eta_{it} | Z_{it}, M_{it} = 0),$$

$\xi = \sigma_{\eta v} / \sigma_v^2$, and $\sigma_{\eta v} = \text{cov}(\eta, v)$. Note that h comprises the relevant inverse Mills ratios. If the v_{it}'s are assumed to be i.i.d. standard normal, then the inverse Mills ratios are, respectively, φ/Φ and $-\varphi/(1 - \Phi)$, where φ is the standard normal probability density function, and both φ and Φ are evaluated at $-Z_{it}'\gamma$.

This method is designed to correct for the self-selection of individuals into marriage; that is, it accounts for whether there is something different about individuals who marry versus those who do not. Estimation of (9) can be accomplished through a simple two-step procedure. First, probit maximum likelihood is applied to (7) to obtain an estimate of γ, which is used to construct $\hat{h}(Z_{it}, M_{it}; \hat{\gamma})$. Then, $\hat{h}$ is substituted for h in (9), and the resulting regression is estimated by least squares.[10] The statistical significance of $\hat{h}$ in this regression provides another test of the exogeneity of marital status.

Consistency of the IMR method also depends on whether (8) is satisfied, as well as on knowledge of the functional form of h. In a given empirical application, the IMR method may not be robust to departure from the functional-form assumption (say, normality). Another problem is that the inverse Mills ratios may simply be proxying for omitted nonlinearities, in which case interpretation as a correction for self-selection becomes difficult.

Panel Data

With panel data, alternative estimators to the cross-section approaches of IV and IMR exist. For convenience, assume that $u_{it}^M = u_{it}^S \;\forall\; i$. This will have no adverse statistical consequences if 1) men have only the same knowledge as the econometrician regarding their ε_{it}'s, but have exclusive knowledge of their α_i's, and 2) men's unobservable characteristics have the same impact on wages regardless of whether an individual is married or single (Robinson [1989]). Then, (6) becomes

$$(11)\quad y_{it} = \delta^S + X_{it}'\beta + (\delta^M - \delta^S)M_{it} + \alpha_i + \varepsilon_{it}.$$

Finally, assume that the α_i's and ε_{it}'s are i.i.d. random variables, uncorrelated with each other, with zero means and constant variances σ_α^2 and σ_ε^2.

In a standard human capital wage equation, it is likely that $E(X_{it}'\alpha_i) \neq 0$, since X_{it} typically contains measures of education, work experience, and job tenure that are correlated with unobserved ability reflected in the α_i's. Furthermore, because men with large amounts of human capital are more attractive as potential spouses, it is likely that X_{it} and M_{it} are correlated. Hence, attempts to estimate the effect of marriage on wages should go beyond correction for self-selection per se.

The "Within" Estimator

The simplest procedure for consistently estimating (11) is the so-called within estimator, which is calculated by applying least squares to data that have been transformed into deviations from individual means. Since the α_i's are treated as fixed parameters, the within estimator is consistent regardless of the relationship between (X_{it}', M_{it}) and α_i. Alternatively, the within estimator can be viewed as an instrumental variables estimator, with the instruments being the deviations from the means, which are orthogonal to the α_i's by construction.

Of course, if the α_i's are uncorrelated with (X_{it}', M_{it}), more efficient estimation of (11) is possible via generalized least squares (GLS). Like the within estimator, GLS can also be computed from a least squares regression on transformed data. In this case, the transformation is to "whiten" the errors.[11] Another advantage of panel data is that this proposition can be tested. The efficiency of GLS under the null hypothesis

10 Other estimation strategies include direct nonlinear least squares estimation of (9) and maximum likelihood. To estimate by maximum likelihood, one must be willing to assume that (η, v) is distributed bivariate normal. See Barnow, Cain, and Goldberger (1980) and Heckman and Robb (1985) for details.

11 This transformation follows from Fuller and Battese (1973) and amounts to a "$(1 - \theta)$ differencing" of the data, where $\theta = [\sigma_\epsilon^2 / (\sigma_\epsilon^2 + T\sigma_\alpha^2)]^{1/2}$.

TABLE 1

Means and Standard Deviations by Year, and Percent of Sample Changing Marital Status

Variable	1971 Cross Section	1971 Panel	1976 Panel
LOG REAL WAGE	1.122 (0.438)	1.179 (0.437)	1.399 (0.414)
MARITAL STATUS	0.718 (0.450)	0.716 (0.451)	0.895 (0.306)
AGE	23.96 (3.18)	24.00 (3.18)	29.00 (3.18)
TENURE	3.209 (2.532)	3.321 (2.551)	6.197 (4.032)
UNION	0.314 (0.464)	0.326 (0.469)	0.342 (0.475)
SOUTH	0.360 (0.480)	0.367 (0.482)	0.379 (0.485)
SMSA	0.679 (0.467)	0.687 (0.464)	0.688 (0.463)
EDUCATION	12.738 (2.519)	12.802 (2.467)	13.321 (2.620)
RACE	0.141 (0.348)	0.149 (0.356)	0.149 (0.356)
No. of observations	1,073	860	860

Marital Status Changers	Never Married to Married	Married to Not Married
Frequency	197	43
Percent of observations	22.9	5.0

NOTE: Standard errors are in parentheses.
SOURCE: National Longitudinal Survey of Young Men.

that $E[(X_{it}', M_{it})\alpha_i] = 0$, along with the robustness of the within estimator to departures from the null, form the basis of a Wu–Hausman test of the difference between the GLS and within estimates of (11).

The Hausman–Taylor Estimator

A consistent and potentially more efficient alternative to the within estimator is Hausman and Taylor's (1981) efficient instrumental variables procedure (HT-IV).[12] HT-IV exploits knowledge of the uncorrelatedness of certain columns of X_{it}' with α_i to increase the instrument set beyond that of the within estimator.[13] Computation involves an IV regression on the same data transformation required for GLS. Thus, the efficiency gains to HT-IV come from an expanded instrument set and whitened errors.

More efficient estimation of the parameters of (11) is one motivation for considering HT-IV. Another is that HT-IV permits a direct test of the uncorrelatedness of M_{it} with α_i. The GLS/within contrast does not indicate which explanatory variables are correlated with the effects if the null is rejected. However, using a Wu–Hausman test of the difference between HT-IV estimates that maintain a legitimate set of over-identifying restrictions with HT-IV estimates that take M_{it} to be exogenous, we can determine whether marital status is in fact exogenous.

Note that both the within and HT-IV estimators are distinguished from the cross-section procedures, which depend on over-identifying restrictions like those defined in (8) and/or on special distributional assumptions. In addition, within and HT-IV allow consideration of another source of nonidentifiability of the marriage premium that is not addressed by the cross-section approaches, namely, correlation between X_{it} and the effects. Consistent estimation of $(\delta^M - \delta^S)$ is not possible if $E(X_{it}'\alpha_i) \neq 0$, unless X_{it} is orthogonal to M_{it}, *even if* $E(M_{it}\alpha_i) = 0$.

III. Estimation of the Marriage Premium

Data

Our data are drawn from the 1971 and 1976 waves of the NLSYM. The primary advantage of the NLSYM is that it allows us to follow individuals moving from the single (and not previously married) to the married state. Between 1971 and 1976, roughly 23 percent of the young men in our sample went from never married to married, while only about 5 percent were divorced or separated. The 1971 cross-section data set consists of 1,073 young men who were between the ages of 19 and 29 in 1971 and who worked more than 40 weeks during the year. Our panel is constructed from the individuals in the cross-section data set who are also observed in 1976. Attrition reduces the number of observations in both periods to 860, of which about 15 percent are black.

In addition to marital status, we observe (in both years) each man's wage, age (AGE), years

12 Extensions of this procedure to broader classes of instrument sets can be found in Amemiya and MaCurdy (1986) and Breusch, Mizon, and Schmidt (1989).

13 HT-IV uses the individual means and deviations from means of each time-varying exogenous variable in X_{it} as separate instruments (see Breusch, Mizon, and Schmidt [1989]).

TABLE 2

Cross-Section Marital Status Premiums from the 1971 NLSYM

	OLS	IV1	IV2	IMR
CONSTANT	–2.156	–5.381	–4.858	–3.835
	(0.697)	(2.721)	(2.278)	(1.330)
MARITAL STATUS	0.054	–0.691	–0.570	–0.334
	(0.025)	(0.554)	(0.488)	(0.253)
AGE	0.188	0.471	0.425	0.335
	(0.059)	(0.238)	(0.199)	(0.115)
AGE^2	–0.003	–0.008	–0.007	–0.006
	(0.001)	(0.004)	(0.004)	(0.002)
UNION	0.214	0.223	0.221	0.219
	(0.023)	(0.032)	(0.030)	(0.026)
SOUTH	–0.122	–0.095	–0.100	–0.108
	(0.024)	(0.038)	(0.035)	(0.028)
SMSA	0.157	0.156	0.156	0.156
	(0.023)	(0.030)	(0.028)	(0.025)
EDUCATION	0.032	0.027	0.028	0.029
	(0.004)	(0.007)	(0.007)	(0.005)
RACE	–0.169	–0.287	–0.268	–0.231
	(0.033)	(0.103)	(0.087)	(0.053)
h				0.196
				(0.149)

NOTE: Standard errors are in parentheses.
SOURCE: National Longitudinal Survey of Young Men.

of tenure with his employer (TENURE), years of education (EDUCATION), union status (UNION = 1 if a union member), race (RACE = 1 if black), and residence (SOUTH = 1 if resident of the South; SMSA = 1 if resident of a Standard Metropolitan Statistical Area).[14] The variable MARITAL STATUS has a value of 1 if the individual is married and living with his spouse, and zero otherwise. Table 1 provides the means and standard deviations for the variables in our samples, and documents the percentage of men who change their marital status.

Results

First, we estimate our model using purely cross-sectional methods. In each case, X_{it} contains the explanatory variables AGE, AGE^2, UNION, SOUTH, SMSA, EDUCATION, and RACE. The dependent variable in every regression is the natural logarithm of the real wage. The results of cross-section estimation are presented in table 2. The OLS estimates, given in the first column, serve as a baseline for comparing estimates obtained from procedures that allow for marital status endogeneity.

In general, the results of our OLS regressions are typical of those found elsewhere in the literature. Of interest here is the coefficient of marital status, which is estimated to be 0.054 with a standard error of 0.025 (statistically significant at the 5 percent level). We have argued that the marriage premium estimated by OLS is biased upward due to the endogeneity of marital status. A Wu–Hausman test of the exogeneity of marital status can be performed by comparing OLS and IV estimates of our model.

To construct an instrument for M_{it}, we need an empirical specification of the decision rule that determines marriage (equation [7]); that is, a regressor (or set of regressors) that is not included in X_{it}. From the NLSYM, we obtain two family-background variables for this purpose: number of siblings (NSIB) and years of education of the father (FED). The Wu–Hausman test amounts to a test of significance of $\hat{M}_{it}$ in a regression of y_{it} on $(X'_{it}, M_{it}, \hat{M}_{it})$. We conduct the test using a $\hat{M}_{it}$ calculated from both OLS and probit maximum likelihood estimates of γ. The p-values of the test statistics for the null hypothesis that the coefficient of $\hat{M}_{it}$ equals zero are 0.089 and 0.108, respectively. Taken together, these tests provide weak evidence against the exogeneity of marital status.

Instrumental Variables

Assuming that marital status is endogenous, we turn to the IV estimates of our model, which are presented in the second and third columns of table 2. The results in the IV1 column are based on a $\hat{M}_{it}$ obtained by OLS, while those in the IV2 column are based on a $\hat{M}_{it}$ derived from probit maximum likelihood. In both cases, we fail to reject, even at a 10 percent level of significance, the null hypothesis that the estimated marital status coefficient is zero. Moreover, both IV marital status coefficient estimates are less than zero and are large in absolute value.

Consistency of these IV estimates depends on the validity of the over-identifying restrictions exploited in the construction of $\hat{M}_{it}$. A test of the over-identifying restrictions examines whether (NSIB, FED) jointly adds to the

14 The wage-rate variable provided by the NLSYM is the hourly wage for hourly workers and salary/usual hours for salaried workers. Our wage-rate variable is deflated by the Consumer Price Index and is expressed in 1970 dollars.

predictive power of our model. The value of the test statistic, which is distributed as $F_{3,992}$, is 2.337. Hence, the restrictions cannot be rejected at the 5 percent level of significance. On the other hand, the p-value of the F-statistic is only 0.949, and only NSIB (aside from certain variables in X_{it}) enters (7) statistically significantly. This may be part of the reason that, using our IV procedures, we are unable to arrive at any stronger conclusions about the impact of marital status endogeneity on the estimated marriage premium.[15]

Inverse Mills Ratio

As an alternative to IV, we estimate our model using the two-step IMR procedure. First, we estimate (7) by probit maximum likelihood to obtain an estimate of γ. This estimate is then used to form the estimates of the relevant IMR terms that comprise h in (10). Second, we apply OLS to (9), where $\hat{h}$ is substituted for h. The results of this two-step procedure are given in the fourth column of table 2. The marital status coefficient estimate is –0.334 and, like the IV estimates, is not statistically significant. Furthermore, the "selection term," $\hat{h}$, does not enter the regression statistically significantly, thereby providing no (additional) evidence of the endogeneity of marital status.[16]

In sum, cross-sectional approaches to the identification of the marriage premium yield no definitive conclusions. The evidence against the exogeneity of marital status is relatively weak. When marital status is treated as endogenous, the resulting coefficient estimates are large and negative. Moreover, when the estimated marital status coefficient is large in absolute value, the fact that it is not statistically different from zero may have little meaning.

Next, we consider estimation techniques that exploit the availability of panel data.

Panel Data

We proceed under the assumptions of (11) and focus on the role of unobservable individual-specific characteristics in estimating the return to marriage. The advantage of panel data is the ability to control for such characteristics, which may be correlated with (X_{it}', M_{it}). Consequently, we can address another likely source of non-identifiability of the marriage premium, namely, the potential correlation of X_{it} with the effects. Furthermore, our panel data procedures do not require the data to satisfy orthogonality conditions such as those in (8), or to meet any special distributional assumptions. This is important, since the cross-section estimates appear to be sensitive to these kinds of restrictions.

Under the null hypothesis that the explanatory variables are uncorrelated with the effects, OLS applied to the individual means of the variables (the so-called between estimator) yields consistent estimates of all the coefficients in our model. Conditional on X_{it} as defined above, we estimate our model with our panel data set using the between estimator as a basis for comparison with procedures that are consistent under the alternative hypothesis.

The between estimates, which exploit only the cross-section dimension of the panel, are presented in the first two columns of table 3. Like the OLS estimates in table 2, they are typical of cross-section estimates derived from human capital wage equations.

Focusing on the returns to marriage, the first column reports a statistically significant marriage premium of 7 percent. However, the regression associated with the first column does not condition on tenure. When TENURE and TENURE^2 are included, the estimated MARITAL STATUS coefficient declines by one-third and is no longer statistically significant. At the same time, the estimated returns to tenure are sizable. This pattern is repeated in Korenman and Neumark when they introduce years married to their regressions. But if Abraham and Farber are correct, tenure and years married simply capture individual characteristics that lead to longer-lasting and higher-wage jobs.

This might be expected given the evidence cited in Abraham and Farber (1987) on the relationship between marital status and job duration (completed tenure). Table 4 presents further evidence on this relationship from our cross-section data set. Here, we report the results obtained from estimating an exponential hazard of job duration (correcting for right-censoring) conditional on MARITAL STATUS, AGE, AGE^2, UNION, BLUE COLLAR (= 1 if the individual is employed in a blue-collar occupation), EDUCATION, and RACE. Other than the effect of union membership, marriage has the largest positive impact on job duration.

15 The instrument set (NSIB, FED) represents our best attempt, using our 1971 NLSYM cross-section sample, at a specification of (7).

16 We also estimated (9) by maximum likelihood, which assumes (η_{it}, ν_{it}) to be bivariate normal. The maximum likelihood estimate of $(\delta^M - \delta^S)$ is 0.032 with a standard error of 0.033, so it is not statistically significant. In addition, the correlation coefficient $\rho_{\eta\nu}$ is only –0.117 and is also not statistically significant.

TABLE 3

Estimated Marital Status Premiums from the 1971 and 1976 NLSYM

	Between	Between	Within	HT–IV
CONSTANT	–1.908	–1.851		–0.968
	(0.809)	(0.794)		(0.161)
MARITAL STATUS	0.070	0.0461	–0.012	–0.017
	(0.037)	(0.036)	(0.025)	(0.024)
AGE	0.150	0.141	0.118	0.124
	(0.062)	(0.061)	(0.022)	(0.021)
AGE^2	0.002	–0.002	–0.002	–0.002
	(0.001)	(0.001)	(0.000)	(0.000)
TENURE		0.055	0.015	0.013
		(0.012)	(0.007)	(0.007)
TENURE^2		–0.003	–0.001	–0.001
		(0.001)	(0.000)	(0.000)
UNION	0.162	0.142	0.190	0.181
	(0.025)	(0.025)	(0.027)	(0.020)
SOUTH	–0.093	–0.084	–0.058	–0.082
	(0.023)	(0.023)	(0.056)	(0.022)
SMSA	0.198	0.197	0.070	0.158
	(0.023)	(0.023)	(0.041)	(0.021)
EDUCATION	0.040	0.041	0.045	0.054
	(0.004)	(0.004)	(0.014)	(0.012)
RACE	–0.203	–0.202		–0.193
	(0.031)	(0.030)		(0.035)
χ^2			34.770	8.030

NOTE: Standard errors are in parentheses.
SOURCE: National Longitudinal Survey of Young Men.

TABLE 4

Exponential Hazard of Job Duration

	Coefficient Estimate	Standard Error
CONSTANT	1.526	2.412
MARITAL STATUS	0.284	0.086
AGE	–0.076	0.205
AGE^2	0.004	0.004
UNION	0.512	0.083
BLUE COLLAR	–0.012	0.087
EDUCATION	–0.008	0.018
RACE	0.125	0.107
Log-likelihood	–1,597	
No. of observations	1,073	

SOURCE: National Longitudinal Survey of Young Men.

Within Estimates

The between estimates are consistent only if (X'_{it}, M_{it}) is orthogonal to the effects. The within estimates, given in the third column of table 3, are consistent regardless of whether the effects are correlated with the explanatory variables. Our within regression yields an estimated marriage premium that is essentially zero. The MARITAL STATUS coefficient estimate is –0.012 with a standard error of 0.025. Conditioning on unobserved heterogeneity has a similar effect on the return to tenure, as the estimated TENURE coefficient declines from 0.055 to 0.015. One interpretation of these results, consistent with Abraham and Farber, is that the cross-section gains to marriage and tenure largely reflect unobserved individual characteristics that are valued by the firm.

The outcomes of two specification tests support this interpretation. First, the assumption of the between regressions that (X'_{it}, M_{it}) is orthogonal to the effects is soundly rejected. A Hausman test of the difference in the between and within estimates yields a test statistic that is asymptotically distributed as χ^2_9 and that has a value of 34.770. The interpretation of the test statistic is as follows: The within estimates are consistent under the null or alternative hypothesis, while the between estimates are consistent only if there is no correlation between (X'_{it}, M_{it}) and α_i, implying that the estimates should be somewhat close if no correlation exists. However, the statistic reported above shows that the estimates are quite far apart, leading us to reject the assumption of no correlation.

Hausman–Taylor Estimates

While the Hausman test demonstrates the general importance of unobserved heterogeneity in our model, it does not specifically address the uncorrelatedness of M_{it} with α_i. This is accomplished by appealing to the HT-IV estimator of Hausman and Taylor (1981). Efficiency gains over the within estimator are obtained by exploiting information about the uncorrelatedness of certain columns of X'_{it} with α_i and accounting for the variance components. The null hypothesis that M_{it} is orthogonal to α_i can be tested by comparing HT-IV estimates derived from a legitimate set of over-identifying restrictions with HT-IV estimates computed assuming M_{it} is exogenous.

Hausman tests contrasting HT-IV and within estimates reveal that a legitimate instrument can be constructed by assuming that AGE, AGE^2, UNION, SOUTH, SMSA, and RACE are uncorrelated with the α_i's. This result comes from the fact that the within estimator is consistent regardless of the correlation of the α_i's with the above variables, while the HT-IV's are not. Therefore, if the results are close in some sense (as defined by a Hausman test, for example), that would lead to the acceptance of the variables being uncorrelated with the α_i's. The test statistic, which is asymptotically distributed as χ^2_5, has a value of 8.03, which is well within the 95 percent statistic of 12.8. The HT-IV estimates based on this instrument set are presented in the last column of table 3. In general, they corroborate the results from within estimation. The estimated marital status coefficient is again very small and statistically insignificant. The HT-IV tenure coefficient estimate is 0.013, quite close to the within estimate.

A Hausman test of the difference between the HT-IV estimates in the last column of table 3 and those obtained from HT-IV estimation with marital status added to the instrument set rejects the exogeneity of marital status. The test statistic equals 3.89 and is asymptotically distributed as χ^2_1 with a p-value of 0.048.

IV. Conclusion

We use cross-section and panel data estimation procedures to determine the effect of marriage on the wages of a sample of young men drawn from the NLSYM. Whenever we control for unobservable individual effects, the estimated returns to marriage are virtually zero. In addition, specification tests reject the hypothesis that marital status is uncorrelated with the effects. We conclude that the usual cross-section marriage premium is essentially a statistical artifact, at least for young men. Within the job-matching literature, this conclusion has a reasonable interpretation: As an explanatory variable in human capital wage equations, marital status appears to fulfill a role similar to that of tenure, namely, proxying for unobservable individual-specific characteristics that are valued by the firm.

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Absolute Priority Rule Violations in Bankruptcy

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Introduction

Any transaction involving a continuing relationship over time depends on a mechanism by which parties can commit themselves to some future behavior. This often involves writing contracts. In most cases, we depend on government to enforce these contracts through a court system. Indeed, one of government's most important roles in any economy is defining and enforcing private property rights. Since contracts are simply a means of transferring private property, the use of courts to enforce them has a certain logical appeal.

Loan agreements are one of the most common types of contracts in our economy. Lenders agree to invest in a business and the owners of that business agree to repay the loan, with interest, at some future date. If the borrower fails to repay the loan, his creditors may force him into bankruptcy and seize his assets. By definition, debt contracts require that creditors be paid before the firm's owners receive any value. In other words, creditors are assumed to have "priority" over a firm's equity holders.

This principle is known as the absolute priority rule (APR). Simply stated, this rule requires that the debtor receive no value from his assets until all of his creditors have been repaid in full.[1] While this rule would seem quite simple to implement, it is routinely circumvented in practice. In fact, bankruptcy courts themselves play a major role in abrogating this feature of debt contracts. If private loan contracts are entered into voluntarily, why do courts allow (and even encourage) their terms to be violated on a regular basis? More important, what impact do these violations have on the cost of financial contracting and, hence, economic efficiency?

This article addresses these questions by analyzing the impact of APR violations on financial contracts. We begin in the next section by reviewing the magnitude of these violations and the frequency with which they occur. In section II, we develop a simple model to analyze the efficiency of APR violations. We complicate this model with several market frictions to show how the impact of these violations depends on which friction is present. Section III discusses the model's implications for the proper role of bankruptcy law in enforcing these contracts. Section IV concludes.

1 The APR also states that senior creditors should be paid before junior creditors. In this paper, we consider only APR violations between the borrower and a (single) lender.

TABLE 1

Empirical Research on APR Violations

Article	Data	Dates	Frequency	Magnitude
Franks and Torous (1989)	30 firms with publicly traded debt filing for bankruptcy	1970–84	66.67%	
LoPucki and Whitford (1990)	43 firms with more than \$100 million in assets and at least one publicly traded security under Chapter 11	1979–88	48.84%	
Eberhart, Moore, and Roenfeldt (1990)	30 firms with publicly traded stock under Chapter 11	1979–86	76.67%	7.57%
Weiss (1990)	37 NYSE and AMEX firms under Chapter 11	1980–86	72.97%	
Franks and Torous (1994)	82 firms with publicly traded debt under Chapter 11 or an informal workout	1983–90		9.51% workouts 2.28% Chapter 11
Tashjian, Lease, and McConnell (1996)	48 firms with a publicly traded security or more than \$95 million in assets, reorganizing with a prepackaged bankruptcy	1980–93	72.92%	1.59%
Betker (1995)	75 firms with publicly traded securities under Chapter 11	1982–90	72.00%	2.86%

SOURCE: Authors' review of the literature.

I. The Prevalence of APR Violations

A growing body of empirical evidence supports the conclusion that APR violations are commonplace both in Chapter 11 reorganizations and in informal workouts. Using different samples of large corporations with publicly traded securities, numerous researchers have found that equity holders receive value from financially distressed firms in violation of the APR in nearly 75 percent of all reorganizations.[2] This appears to be true whether one looks at private, informal workouts, conventional reorganizations, or "prepackaged bankruptcies" in which the details of the reorganization are negotiated before the bankruptcy petition has been filed.

The frequency with which APR violations occur might be misleading if the magnitude of these deviations as a percentage of the firm's value were relatively small. Indeed, some commentators have suggested that value paid to equity is simply a token to speed up the process and has little economic significance: "Shareholders were tossed a bone, crumbs off the table, to get the deal done..."[3] Existing evidence, however, suggests that this is not generally the case. Estimates of the magnitude of APR violations in favor of equity vary, but in reorganizations in which such violations occur, equity holders appear to receive between 4 and 10 percent of the firm's value.[4] And although the evidence is limited, some have suggested that these deviations are larger for small firms whose owners

2 See Franks and Torous (1989), LoPucki and Whitford (1990), Weiss (1990), Eberhart, Moore, and Roenfeldt (1990), and Betker (1995).

3 Quoted in Weiss (1990), p. 294.

4 See Eberhart, Moore, and Roenfeldt (1990), Franks and Torous (1994), Tashjian, Lease, and McConnell (1996), and Betker (1995). Franks and Torous note that the larger deviations found by Eberhart, Moore, and Roenfeldt may be a consequence of the latter's older sample of distressed firms: "With the growth in the market for distressed debt securities and the greater involvement of institutional investors such as 'vulture funds,' debtholders may have increased their bargaining power at the expense of equity holders" (Franks and Torous [1994], p. 364).

also manage the company.[5] Table 1 summarizes recent empirical research on APR violations.

One major caveat should be kept in mind when considering these findings: All the studies of bankruptcy resolution cited here have focused on firms with publicly traded stock and/or debt.[6] However, such firms comprise only a small subset of those filing for Chapter 11 bankruptcy or initiating out-of-court debt workouts. As a result, the number of firms included in these studies averages less than 50. In contrast, there were over 176,000 Chapter 11 cases filed nationwide in the first 10 years after the new Bankruptcy Code was implemented in 1979 (Flynn [1989]). Even after eliminating single-asset real estate partnerships and "house" filings to focus on what might reasonably be considered true "business" reorganizations, these studies have depressingly small and biased samples of "average" reorganizations.[7] Indeed, bankruptcy judge Lisa Fenning notes that only five out of more than 600 Chapter 11 cases on her docket involve publicly traded companies.[8] Clearly, we must be cautious and avoid overinterpreting these empirical studies.

II. APR Violations and Efficiency

Many have argued that APR violations occur because they are privately optimal for bankruptcy participants. If strict adherence to the APR creates perverse investment incentives once the firm is in bankruptcy, it may be privately optimal (ex post) for everyone involved to abrogate such rules and renegotiate their contracts.[9] Under this view, APR violations—both inside Chapter 11 and in out-of-court workouts—are a desirable consequence of renegotiation between the firm and its creditors; APR violations are essentially payoffs by lenders to encourage the firm's shareholders to make good investment decisions once the firm is in financial distress. Unfortunately, this view fails to take into account how such behavior affects ex ante efficiency through the terms of the original financial contract, which is ultimately the only way to evaluate the efficiency of APR violations fully.

To focus on this problem, we develop a simple model of financial contracting. Consider an entrepreneur who wants to open a firm and invest in a project, but needs to borrow I dollars from an outside investor to do so. In return for this loan, the entrepreneur agrees to repay his lender R dollars from his firm's future profit. For ease of exposition, we will often refer to R as "the interest rate."[10] Of course, the firm's profit is not guaranteed. Let x denote the firm's realized profit, which can take values on the interval $[\underline{x}, \overline{x}]$. Let $f(x)$ be the probability that any given x is realized (that is, its probability density function) and, as is standard, let $F(x)$ be the associated distribution function. To model APR violations, let δ represent the fraction of the firm's profit retained by the entrepreneur in bankruptcy.

The entrepreneur will default whenever doing so gives him a higher return (that is, whenever $x - R < \delta x$). Define $\hat{x} = R/(1-\delta)$ as the critical level of profit below which default occurs. The entrepreneur's expected return from his business, E, is then:

$$(1) \qquad E = \int_{\underline{x}}^{\hat{x}} \delta x f(x)dx + \int_{\hat{x}}^{\overline{x}} (x - R) f(x)\, dx.$$

When bankruptcy occurs, the entrepreneur receives only fraction δ of the firm's profit x; by weighting this by $f(x)$ and integrating over all levels of profit for which default occurs, we obtain the first term in E. On the other hand, when the firm's profit exceeds $\hat{x}$, the entrepreneur uses it to repay his loan and keeps the rest. Weighting this by $f(x)$ and integrating over all $x > \hat{x}$ gives us the second term in E.

In a competitive lending market, the equilibrium interest rate, R^*, is set to ensure that the lender is just willing to make the loan:[11]

$$(2) \qquad L = \int_{\underline{x}}^{\hat{x}^*} (1-\delta)xf(x)\,dx + \int_{\hat{x}^*}^{\overline{x}} R^* f(x)dx - I = 0.$$

As above, the first term in this expression represents the lender's expected return when

5 See LoPucki (1983) and LoPucki and Whitford (1990).

6 LoPucki (1983) is an exception.

7 House filings are Chapter 11 filings by individuals whose home mortgages exceed the Chapter 13 debt limit. The 1994 changes to the Bankruptcy Code should make such filings less common.

8 Fenning (1993).

9 See Bulow and Shoven (1978), White (1980, 1983), Gertner and Scharfstein (1991), and Berkovitch and Israel (1991) for models that promote this idea.

10 Technically, R is the "face value" of the debt and is equal to $(1 + r)I$, where r is the nominal interest rate on the loan.

11 Implicit in this specification is the assumption that the competitive return on riskless assets is 1, so that the lender's cost of funds is only I.

default occurs, and is the firm's profit in these states minus the APR violation. The second term in L follows from the fact that the lender is simply paid R^* in all nondefault states.

In this simple model, APR violations have no impact on the firm's cost of financing. While it is true that once the firm is in bankruptcy the entrepreneur is "better off" with large APR violations, these gains are entirely offset by increases in the interest rate the firm is forced to pay. To see this, we substitute the equilibrium solution for R into (1) to get

$$(3) \qquad E = \int_{\underline{x}}^{\bar{x}} x f(x)\,dx - I.$$

The fact that δ does not appear in this expression shows us that the firm's profit is unaffected by the size of the APR violation.[12]

In this simple model, the magnitude of APR violations has no impact on the cost of the initial financial contract. Of course, this analysis ignores many of the problems that plague real-world financial contracting. Throughout the rest of this section, we extend this model with several standard complications and show how the effect of APR violations depends on which problem is present.

Costly Bankruptcy

One of the most basic problems in financial contracting is the fact that bankruptcy is costly. Let c denote the cost paid by the lender whenever he forces the entrepreneur into bankruptcy (for simplicity, assume $c < \underline{x}$).[13] As before, the equilibrium interest rate, R^*, must be set to ensure that the lender earns a competitive return:

$$(4) \qquad L = \int_{\underline{x}}^{\hat{x}^*} [(1-\delta)x - c] f(x)\,dx + \int_{\hat{x}^*}^{\bar{x}} R^* f(x)\,dx - I = 0.$$

In the appendix, we verify that, as before, increases in the magnitude of the APR violation make default more likely (that is, $d\hat{x}^*/d\delta > 0$).

Substituting (4) into the entrepreneur's expected profit (1), we get

$$(5) \qquad E = \int_{\underline{x}}^{\bar{x}} x f(x)\,dx - I - cF(\hat{x}^*).$$

This expression demonstrates how APR violations affect the terms of the loan agreement. Since $\hat{x}^*$ increases with δ, larger APR violations make bankruptcy occur more frequently. As a result, the added expected bankruptcy costs, $cF(\hat{x}^*)$, lower the entrepreneur's ex ante expected return.

In this environment, APR violations may create an additional problem. Although the lender's expected return is generally increasing in the interest rate, eventually the added expected bankruptcy costs associated with higher interest rates outweigh their benefits; that is, L will eventually be decreasing in R. Williamson (1986) shows that this effect can lead to credit rationing, since changes in the interest rate may be insufficient to clear the loan market.

Increases in the magnitude of APR violations have the same impact: By reducing the lender's payoff in default states and increasing the probability that bankruptcy will occur, a point comes at which the lender can no longer be compensated for additional violations of the APR through increases in the interest rate. In other words, APR violations exacerbate credit-rationing problems.

Thus, when bankruptcy is costly, there are strong reasons to avoid APR violations. First, these violations raise the interest rate the entrepreneur must pay, increasing the chance that default—and its corresponding costs—will occur. Furthermore, violations make credit rationing more likely, thereby limiting the entrepreneur's investment opportunities. Why, then, do they occur with such frequency? We next turn to one possible reason.

Asymmetric Liquidation Value

The model presented above assumes that the firm had no capital assets once the project was completed or, alternatively, that the firm had no "going-concern" value. But much of the justification for a reorganization procedure derives from the belief that many firms in financial distress are in fact economically viable and should be reorganized rather than liquidated.[14]

To focus on this idea, we return to our original model (in which bankruptcy is costless) and simplify it by assuming that only two levels of

12 On the other hand, APR violations can lead to credit-rationing problems, even in this simple model, since they make default occur more frequently. We discuss this problem in the subsection that follows.

13 This, then, is the costly state verification environment developed by Townsend (1979) and Gale and Hellwig (1985).

14 Harris and Raviv (1993) develop a model based on this issue and come to similar conclusions.

profit are possible. In good states of the world, which occur with probability π, the entrepreneur's business earns x_H. In contrast, when business is bad, the firm earns only x_L; this occurs with probability $(1 - \pi)$. Furthermore, assume that when business is good the entrepreneur can repay his debt, but in bad states he cannot; that is, $x_H > R > x_L$.

In addition to its profit, x, the firm has capital assets worth A once its project is completed; these can be thought of as the value of the firm's expected future profit. If this value is the same regardless of who owns the firm, our results remain unchanged: APR violations have no impact on the terms of the financial contract. On the other hand, if the firm's assets are worth more in the hands of the entrepreneur, there will be an incentive to modify the financial contract to allow him to retain control of the firm even after filing for bankruptcy.

Let α represent the fraction of the firm's assets (and hence future profit) retained by the entrepreneur during bankruptcy. In this case, the entrepreneur's expected profit[15] is

$$(6) \qquad E = (1 - \pi)(\delta x_L + \alpha A) + \pi(x_H - R + A).$$

Let γ be the fraction of the firm's ongoing value that is lost by transferring these assets to the lender. Once again, the equilibrium interest rate must be set to guarantee the lender a competitive return:

$$(7) \qquad L = (1 - \pi)[(1 - \delta)x_L + (1 - \alpha)\gamma A] + \pi R^* - I = 0.$$

Substituting this into the entrepreneur's expected profit gives us

$$(8) \qquad E = (1 - \pi)(x_L + A) + \pi(x_H + A) + (1 - \pi)(1 - \gamma)(\alpha - 1)A - I.$$

As before, it is irrelevant whether the entrepreneur is allowed to keep some of the profit (the size of δ) when the firm defaults; the interest rate adjusts so as to keep the entrepreneur's expected return unchanged. Likewise, when $\gamma = 1$ and the firm's capital assets have the same value regardless of who controls them, the size of α does not matter; that is, APR violations involving the firm's capital assets are irrelevant. In this case, we are back to our original model.

Notice, however, that the same is not true when γ is less than one. Differentiating (8) with respect to α gives us

$$(9) \qquad \frac{dE}{d\alpha} = (1 - \pi)(1 - \gamma)A > 0;$$

since these assets are worth less to the lender than they are to the entrepreneur, APR violations of this sort are beneficial.

Why are both α and γ necessary to analyze the impact of APR violations in this environment? The intuition is clear: APR violations are beneficial only when they are applied to A, since this is the only part of the firm's value that is worth more in the hands of the entrepreneur. If allowing the lender to keep some of x_L has any detrimental impact (such as costly bankruptcy), the desirability of distinguishing between these two types of APR violations is obvious.

One might wonder whether there is a practical distinction between x_L and A. For large, publicly traded firms, this distinction may be irrelevant. After all, the going-concern value of Johnson & Johnson is likely to be unaffected by the identity of its stockholders (that is, their γ is equal to one). On the other hand, firms that are owned and managed by an entrepreneur who brings specialized skills to his company are likely to have small γ's. In this case, it might be reasonable to allow the entrepreneur to keep control of his firm after bankruptcy, but all of the firm's liquid assets should be transferred to its creditors.

Risk Shifting

Perhaps the most common problem in financial contracting is the borrower's incentive to undertake actions that affect the riskiness of his business.[16] Suppose that, by exerting effort, the entrepreneur can affect the likelihood that the firm will be successful. If the entrepreneur works hard, the firm will earn x_H with probability π_1; without effort, it will earn x_H with probability $\pi_2 < \pi_1$. In addition, assume that the amount of effort required (or alternatively, the cost of this effort) is not discovered until after the loan is made; let e represent the effort ultimately required. Finally, suppose that the lender cannot observe whether effort is exerted.

After learning the effort required, the entrepreneur's expected return from the "good" project is $(1 - \pi_1)\delta x_L + \pi_1(x_H - R) - e$, while his expected return from the "bad" project is $(1 - \pi_2)\delta x_L + \pi_2(x_H - R)$. Ultimately, whether

15 This expression is analogous to equation (1); note that we have assumed only two possible states of the world.

16 Bebchuk (1991) develops a different model of risk shifting and comes to similar conclusions. See also Innes (1990).

the entrepreneur chooses to undertake the good project (that is, exert effort) will depend on how much effort is required. He will select the good project as long as his realized e is less than e^*, where

$$(10) \quad e^* = (\pi_1 - \pi_2)(x_H - R - \delta x_L).$$

In what follows, it will be useful to know how often the entrepreneur will select the good project, which requires us to know the distribution of e. Assume for simplicity that e is distributed uniformly on the interval [0,1]. In this case, the probability that the entrepreneur will choose the good project (that is, that $e < e^*$) is simply e^*.

The lender, knowing that the entrepreneur will choose the good project with probability e^* and the bad project with probability $1- e^*$, will demand an interest rate that guarantees him zero expected profit:

$$(11) \quad L = [e^*(1-\pi_1) + (1-e^*)(1-\pi_2)](1-\delta)x_L + [e^*\pi_1 + (1-e^*)\pi_2]R^* - I = 0.$$

Before he takes the loan, the entrepreneur's expected return is simply his expected profit from each of the projects, weighted by the probability that he will choose each, minus his expected effort conditional on the good project being chosen:

$$(12) \quad E = (1-e^*)[\pi_2(x_H - R) + (1-\pi_2)\delta x_L] + e^*[\pi_1(x_H - R) + (1-\pi_1)\delta x_L] - \frac{e^{*2}}{2}.$$

Substituting R* into this expression gives us:

$$(13) \quad E = e^*(\pi_1 - \pi_2)(x_H - x_L) + \pi_2 x_H + (1-\pi_2)x_L - \frac{e^{*2}}{2} - I.$$

As in our original problem, δ has no direct effect on the entrepreneur's ex ante expected return; the interest rate simply adjusts to ensure that the lender makes a competitive return. On the other hand, such APR violations do have an indirect effect through their impact on the probability that the entrepreneur will exert effort and choose the good project. Differentiating (13) with respect to δ yields

$$(14) \quad \frac{dE}{d\delta} = \frac{de^*}{d\delta}[(\pi_1 - \pi_2)(x_H - x_L) - e^*]$$

$$= \frac{de^*}{d\delta}(\pi_1 - \pi_2)[R - (1-\delta)x_L].$$

Now, $R > (1-\delta)x_L$ by assumption. In the appendix, we demonstrate that $de^*/d\delta \leq 0$, that is, that the presence of large APR violations makes the entrepreneur less likely to choose the good project.[17] Combining these results shows that the entrepreneur's expected profit is decreasing in δ. Hence, when risk shifting is a problem, APR violations are ex ante inefficient.

The intuition behind this is straightforward. As before, the direct benefit to the entrepreneur of receiving compensation when the firm fails is exactly offset by the higher interest rate he must pay.[18] On the other hand, APR violations reduce the entrepreneur's incentive to undertake the good project. Why is this the case? Since effort is costly for the entrepreneur, he would like to avoid it whenever possible. Nevertheless, he is willing to exert some effort, since doing so makes it more likely that the firm will be successful, reaping him a higher return. The presence of these violations, however, reduces the pain of bankruptcy and hence the relative benefits of this effort. After all, why should the entrepreneur work hard if he can be assured of a sizable payoff even when his business bombs? As a result, the entrepreneur exerts less effort than he would if there were no APR violations.

III. Policy Implications

The results of the last section suggest that an optimal bankruptcy institution would allow debtors and creditors to decide ex ante whether APR violations will occur. In other words, the parties to the loan agreement should be allowed to write a contract that specifies under what conditions APR violations will and will not occur.

Although the desirability of such a system might seem obvious, current bankruptcy law does not enforce agreements like these. Once a firm enters bankruptcy, it must follow the rules and procedures set out in the Bankruptcy Code, and no one is allowed to forfeit his future right to file for bankruptcy when he signs a loan agreement. This might not be a problem if it weren't for the fact that current bankruptcy law strongly encourages APR violations, regardless of whether they are efficient.

17 For small δ, $de^*/d\delta$ may be zero; in this range, the payments that the entrepreneur receives in bankruptcy are not large enough to discourage him from choosing the good project, regardless of the level of effort required.

18 Once again, however, a credit-rationing problem is possible.

Several features of the code make this true. First, the debtor retains control of the firm throughout the process, except in extraordinary circumstances. Second, the debtor is allowed to obtain "debtor-in-possession financing" to continue operation of the business; this financing is automatically given priority over all of the firm's unsecured claims. Third, the debtor is granted 120 days to propose a plan of reorganization; during this time, no other parties may propose alternative plans.[19] Finally, if the debtor's reorganization plan is not approved by its creditors, it may attempt to enforce a "cram-down," getting the judge to impose the plan against the creditors' wishes.[20] Each of these factors gives the debtor leverage in the reorganization, increasing the likelihood (and magnitude) of APR violations.

Although one might appeal to asymmetric liquidation values as a justification for APR violations, a formal bankruptcy procedure that *mandates* them seems unwarranted, especially in light of other problems that make APR violations inefficient. After all, nothing prevents the firm and its creditors from writing a loan agreement that would keep the firm's capital assets in the entrepreneur's hands, even in default.

This points out an additional complication that must be present to justify a special bankruptcy law: incomplete contracting. If the future value of the firm's capital assets is uncertain, and the entrepreneur and the lender cannot agree on a way to measure its value, some outside arbiter may be useful. While bankruptcy courts can certainly fill this role, the implicit assumption that the contract participants cannot designate such an arbiter in their agreement seems extreme. On the other hand, bankruptcy law may be able to provide a useful baseline to reduce the costs of contracting on improbable events.

Potential conflicts among different creditors might provide another justification for bankruptcy laws.[21] In their rush to retrieve some value from a financially distressed firm, the theory goes, lenders may inadvertently reduce the total value of the firm's assets that are available for distribution. This might happen if the firm's assets are worth more undivided, but individual creditors have liens on specific assets. Worse yet, this rush might cause financially viable firms to be liquidated. Setting aside the question of why the firm and its creditors cannot foresee these problems and write their contracts so as to prevent them, this rationale for bankruptcy law does not necessarily mandate that it violate contractual priorities that are determined ex ante.

Nonetheless, many firms may feel that the fact-finding and mediation services provided by a formal bankruptcy institution provide a cost-effective way of writing financial contracts. Similarly, conflicts among creditors may be sufficiently severe to justify the use of such an institution. As a result, one would be overzealous in recommending total repeal of the Bankruptcy Code.

It is clear, however, that any bankruptcy procedure should merely provide an optional starting point for private contracts. If everyone involved finds it convenient to use this institution, they may. But if they find the procedure unnecessarily restrictive, they should have the opportunity, when they write their financial contract, to opt out of it entirely. That is, the parties to the loan agreement should be allowed to decide up front, when they write their agreement, whether a formal bankruptcy procedure will be used in the event of financial distress.

On the one hand, small entrepreneurial firms with highly uncertain markets and products may find Chapter 11 protection beneficial. As discussed above, Chapter 11 gives equity substantial bargaining power in the renegotiation process. Since these firms are more likely to benefit from the ability to recontract when new information is available, and their managers are more likely to possess special skills that affect the firm's going-concern value, this added bargaining power and the resulting violations in the APR are more likely to be beneficial. Firms in this situation would typically include the right to seek Chapter 11 protection in their debt contracts.

In contrast, firms that have greater opportunities to adjust their activities to the detriment of their creditors would generally choose to opt out of this protection. Formally forfeiting their right to Chapter 11 protection would clearly signal their creditors of their intention to avoid high-risk projects. Likewise, large, publicly

19 This exclusivity period is often extended indefinitely (Franks and Torous [1989] and LoPucki and Whitford [1990]).

20 Cram-downs are rather uncommon, and are allowed only in cases in which all dissenting creditors receive at least what they are due under the APR when the firm is liquidated. A cram-down may nonetheless impose an APR violation if the firm would be worth more if it continued than if it were liquidated, or if the face value of the securities offered to dissenting creditors is substantially above their true market value. Furthermore, the threat of a cram-down, which is costly to fight, may cause some creditors to accept lower payouts than they might otherwise.

21 See Jackson (1986) for a complete discussion of this argument.

traded firms whose going-concern value is unaffected by their ownership would benefit from such an option.

IV. Conclusion

This paper has demonstrated how the efficiency of APR violations depends on the nature of the contracting problem present. When the firm's future profit will be higher if it is controlled by the entrepreneur, it makes sense for him to retain the firm's capital assets—if not its past profits—after bankruptcy. On the other hand, APR violations of any sort have the detrimental effect of raising interest rates, thereby increasing expected bankruptcy costs and worsening credit-rationing problems. Furthermore, APR violations can reduce the entrepreneur's incentive to work hard in order to ensure his firm's profitability.

The diversity of these implications suggests that an optimal bankruptcy law would allow firms and their creditors to decide ex ante whether (and what type of) APR violations will occur in the event of financial distress. While such decisions could reasonably be left to private contracts, a formal bankruptcy law may be desirable for other reasons. If this law de facto encourages APR violations, it is clear that it should also include an "opt-out" provision that allows private agents to determine whether its structure will be beneficial to them. This is not allowed under current U.S. bankruptcy law.

In such a world, we might expect owner-operators of small firms to include APR violations in their contracts, since these firms are the most likely to lose value from transferring their capital assets. In contrast, the value of large, publicly traded companies is less likely to be affected by their ownership, and we would therefore expect such companies to avoid APR violations of any type, as would firms of any size whose profit streams are easily affected by managerial effort.

Appendix

In this appendix, we prove some of the more technical results required in the text. The first is the fact that, in the model with costly bankruptcy, $\hat{x}^*$ is increasing in δ. Totally differentiating (4) shows that

$$(15)\quad \frac{d\hat{x}^*}{d\delta} = \frac{\hat{x}^*[1 - F(\hat{x}^*)] + \int_{\underline{x}}^{\hat{x}^*} xf(x)dx}{(1-\delta)[1 - F(\hat{x}^*)] - cf(\hat{x}^*)}.$$

The numerator of this expression is clearly positive, as is the denominator whenever

$$(16)\quad \frac{c}{1-\delta} < \frac{1 - F(\hat{x}^*)}{f(\hat{x}^*)}.$$

Longhofer (1995) shows that whenever this condition does not hold, no lending occurs in equilibrium. That is, when c or δ is too large, credit rationing results.

The second fact we must prove is that $de^*/d\delta \leq 0$ in the model with risk shifting. Solving (11) for R^*, substituting into (10), and simplifying shows that e^* is defined by

$$(17)\quad e^{*2}\mu_2 + e^*\mu_1 + \mu_0 = 0,$$

where $\mu_0 = I - \pi_2 x_H - (1 - \pi_2)x_L + \delta x_L$,
$\mu_1 = \pi_2 - (\pi_1 - \pi_2)^2(x_H - x_L)$, and
$\mu_2 = (\pi_1 - \pi_2)$.

Although two roots will solve this equation, differentiation of (13) with respect to e^* shows that the larger root will always be the one chosen in equilibrium. Using the quadratic formula to solve for e^*, it is straightforward to verify that

$$(18)\quad \frac{de^*}{d\delta} = -x_L(\mu_1^2 - 4\mu_2\mu_0)^{-1/2},$$

which must be nonpositive whenever a real solution for e^* exists.

It is worth asking what happens when the optimal e^*, as given by the quadratic formula, is greater than one. This would imply that the entrepreneur will always choose the good project, regardless of the level of effort ultimately required. In this case, small APR violations will have no impact on the firm's ex ante profit. Larger violations, however, will still reduce the chance that the entrepreneur will choose the good project.

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