Seller Financing of Temporary Buydowns

Part 2: Effects on Mortgage Default

U.S. Department of Housing and Urban Development
Office of Policy Development and Research

FINAL REPORT

Seller Financing of Temporary Buydowns
Part 2: Effects on Mortgage Default

by

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Submitted to:

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November 30, 1992

This research was funded by the U.S. Department of Housing and Urban Development under contract number HC-5835. Views and opinions expressed herein are those of the author and do not necessarily reflect those of the Department of Housing and Urban Development.

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ACKNOWLEDGEMENTS

Several individuals and organizations made significant contributions to this study. Funding for this research was provided by the U.S. Department of Housing and Urban Development under contract number HC-5835. Under separate contract, Westat undertook the substantial burden of coding data from FHA case binders and producing machine-readable files. Bill Jackson of Westat kept detailed notes on coding conventions and problems. Lisa Puhl of Westat provided codebooks and other helpful information on the nature of the resulting computer files.

Heather Leicester of Unicon served as the project programmer, designing programs that drew random case lists from which the ultimate samples were constructed, linking together files of data from a variety of sources, providing several levels of data screening and cleaning, and helping to write programs that performed some of the statistical analyses. Manuelita Ureta of Unicon supplied programs that were used in developing the software for hazard model estimation.

The author is grateful to Charles Capone of HUD for copious comments and suggestions on earlier drafts. Special gratitude is due Bill Reeder of HUD, who provided guidance and aid throughout the project. Bill participated in an integral way in deciding both the general direction and the details of the research. His oversight of the Westat data gathering and coding operation, as well as the Unicon analysis work, gave continuity to the project and ensured that the early data formation met ultimate analytical needs. He later provided special data corrections that could be obtained only by retrieving information from the hardcopy case files. Bill gave extensive guidance in understanding FHA rules and regulations and in understanding and interpreting FHA data. Finally, he provided useful comments on the draft reports that led up to this final report.

Neither these individuals nor the U.S. Department of Housing and Urban Development is responsible for the content of this report. The author retains sole responsibility for all errors.

EXECUTIVE SUMMARY

This report describes findings from the second portion of a two-part project on temporary buydowns. A temporary buydown is one of many creative financing techniques which enjoyed growing popularity in the late 1970s and early 1980s. Under a typical temporary buydown, a homebuyer's mortgage payments during the early years of the mortgage are subsidized by the seller, who pays a portion of the mortgage payments that would otherwise be paid solely by the borrower. This arrangement is effected by the seller's funding an escrow account that is depleted as funds are used to supplement the payments made by the mortgagor to the lender.

Because a temporary buydown offers monetary benefits to the homebuyer, a homebuyer would be willing to pay more for a home offering a temporary buydown as a part of the sales transaction. Indeed, findings in the first part of this project imply that on average 50 to 75 percent of the value of the buydown is capitalized into the sales price of the home. Unless a similar buydown were offered as part of subsequent sales transactions, however, any financing premium capitalized into the original sales price of the home would not be recaptured at resale. This reasoning suggests that temporary buydowns may leave the mortgage insurer more vulnerable to loss in the event of foreclosure. In addition, if the mortgagor cannot recapture the capitalized value of the buydown upon resale, the mortgagor has less equity than otherwise identical mortgagors who did not purchase under buydown arrangements. Reduced equity is likely to be translated into ultimately higher default rates for those utilizing buydowns.

Although prior studies suggest the importance of such equity considerations in default behavior, this literature has not examined in detail the effects of temporary buydowns. The purpose of this portion of the current project is to fill this gap in our empirical knowledge. By statistically comparing default behavior on buydown and nonbuydown transactions, holding other factors constant, the empirical estimates show the way in which buydown/nonbuydown default differentials evolve as mortgages age.

The study begins by developing an option-based model of default that is used to guide the empirical work. Temporary buydowns are shown to have two potential effects on default. First, as noted above, if buydowns are capitalized into sales prices, but the capitalized value cannot be recaptured upon resale, equity is reduced and the incentive to default correspondingly increased. A second effect works to offset the first effect, at least initially. Because the buydown escrow reverts to the lender in the event of default, and because buydown payments initially help to defray the mortgagor's monthly payments, the incentive to default is reduced during the buydown period. These two effects are expected to work, on net, to lower default rates at short durations but to raise default rates at longer mortgage durations.

Estimation of the impact of buydowns on default utilizes the same data as that used in the first part of the project: coded data from FHA casebinders for five samples of FHA-insured, 30-year, level-payment mortgages. Three samples consist of sales transactions for mortgages starting in 1982 in Denver, Phoenix, and San Antonio, respectively. The remaining two samples are for mortgages beginning in 1985 and the first seven months of 1986 in Phoenix and San Antonio, respectively. Data drawn for these samples include information on characteristics of the home, the sales transaction, and the buyer.

Estimation proceeds by specifying a proportional hazards model in which the probability of default at each point in time is specified as dependent upon the initial home sales price, the initial and current values of the buydown balance, the contemporaneous principal balance and value of the mortgage, and other factors. The estimates confirm the two basic predictions of buydown effects. First, in their default behavior, mortgagors act as if temporary buydowns are capitalized in house prices, generally at about full present value, but this capitalized value cannot be recaptured upon resale. This effect alone acts to increase default probabilities under buydowns. Second, the remaining buydown balance deters default behavior. Though usually estimated with less precision than the first effect, the point estimate for this second effect indicates a larger impact on default rates for a buydown of a given size. In general, the two effects together initially lower default rates but ultimately raise default rates of mortgages with buydowns relative to otherwise identical mortgages without buydowns. While there is a possibility that the estimates reflect, in part, differences among mortgagors that would exist even in the absence of buydowns, the estimates imply clear and substantial differences in default behavior associated with temporary buydowns.

Simulations of cumulative default rates for each of the estimated models utilize different buydown patterns and a variety of economic scenarios, the latter of which are represented by changes in house prices and unemployment rates. The simulation results show that poor economic conditions translate into much higher default rates than good economic conditions. Larger buydowns generally show lower cumulative default rates at low durations where the effect of future buydown payments has a large default-depressing effect. By three years after loan origination, however, effects on cumulative default rates are generally reversed, with larger buydowns leading to higher cumulative default rates. The latter differences appear to be especially dramatic when economic conditions are poor.

I. INTRODUCTION

A temporary buydown is one of many creative financing techniques which collectively grew in importance during the late 1970s and early 1980s. Under a typical temporary buydown, a homebuyer's mortgage payments during the early years of the mortgage are set at levels that correspond to lower mortgage interest rates than are actually being paid to the lender. The difference between the lower monthly payments made by the borrower and the higher levels actually received by the lender is provided by the seller of the home. For loan transactions insured by the FHA, the full difference in payments must be placed in an escrow account at the time of sale. The escrow account, which may or may not bear interest, is gradually drawn down through the life of the buydown as escrow funds are used to supplement the mortgage payments provided by the mortgagor to the lender.

A temporary buydown is clearly worth something to a prospective homebuyer since it reduces mortgage payments in the short-term and, as noted below, may have other benefits as well. Thus, a homebuyer would be willing to pay more for a home offering a temporary buydown as a part of the sales transaction than if no buydown were offered. Unless a similar buydown were offered as part of subsequent sales transactions, however, any premium captured in the original sales price of the home would not be recaptured if the home were resold.[1] This possibility apparently leaves the mortgage insurer more vulnerable to loss in the event of foreclosure.

Not only may temporary buydowns leave the insurer more susceptible to loss in the event of mortgage foreclosure, they may also increase the likelihood of default for at least two reasons. First, the same reason that an insurer would be exposed to a loss in the event of default—an initially higher sales price coupled with the infeasibility or inadvisability of offering a buydown as part of subsequent sales transactions—would reduce the incentive

[1]We leave open the question of whether subsequent sales transactions are in fact likely to include buydowns as well. The answer here turns on the fundamental issue of why buydowns exist at all and the way in which buyer and seller separately benefit from their presence. This question is discussed below and in greater length in a companion report (Cotterman [1992]).

for the original buyer to avoid default.[2] That is, when compared with other homebuyers purchasing homes with the same initial sales price, those who purchase under temporary buydown agreements have a home with a smaller resale value (in the absence of a buydown as part of a subsequent sale). After the temporary buydown escrow is exhausted, they face the same remaining debt burden to support this less highly valued asset, thus reducing the incentive not to abandon the home entirely.

Second, temporary buydowns facilitate loans that result in relatively heavier housing expense burdens. In particular, if underwriting criteria focus on initial housing expenses relative to income, then the reduction in initial monthly mortgage payments resulting from a temporary buydown permits a prospective homebuyer to qualify for a larger loan than would otherwise be made. That is, the full mortgage payments based on the actual coupon rate, which take effect after the buydown period has passed, would constrain the homebuyer to a smaller loan than that permitted by the subsidized mortgage payments that characterize the period of the buydown. Indeed, the fact that temporary buydowns would permit borrowers to qualify for larger loans is a potentially important motivation for their existence.

Both direct and indirect evidence on the likely effect of buydowns reinforces the logical arguments that suggest (a) at least partial capitalization of the buydown into the selling price of the home, and (b) increased default activity. The first piece of evidence is that other seller-provided financing benefits appear to be partially capitalized into sales prices of homes. For example, recent empirical studies show that a substantial fraction of seller-provided assumption and mortgage revenue bond financing is capitalized into house prices (see Durning and Quigley [1985]). Moreover, findings in the first part of this study (Cotterman [1992]) suggest that about 50 to 75 percent of the present value of buydown payments is capitalized into the sales prices of homes. Other evidence suggests that borrower's equity is an important deterrent to default (see, for example, Foster and Van Order [1984]). If borrowers are unable to recapture the value of the buydown upon resale, they would be

[2]As discussed below, the existence of the buydown funds to help finance early mortgage payments would initially counteract this effect.

expected to exhibit a higher probability of default. A third source of evidence is the default behavior of buyers who have graduated payment mortgages (GPMs). The rise in borrower's payments in the early years of a mortgage with a buydown mimics the behavior of payments under GPMs like those issued under the FHA 245(b) program; such GPMs have been found to have unusually high default rates.

In 1986, reasonable concerns over the effect of buydowns on the health of the FHA mortgage insurance fund led HUD to tighten appraisal and underwriting criteria so as to (a) limit the extent of seller financing contributions included in the prices of comparables and in the determination of maximum mortgage amounts, and (b) end the use of temporarily lower initial mortgage interest rates for loan-qualification purposes. These restrictive changes, instituted in August 1986, were later relaxed somewhat in 1987.

Despite introduction of policies to cope with perceived problems of temporary buydowns, and the apparent reduction in the use of seller financing concessions in general and temporary buydowns in particular, there remain questions regarding the actual default behavior engendered by temporary buydowns. In particular, did mortgages that were accompanied by temporary buydowns suffer from a higher incidence of default? This report attempts to answer this question directly by statistically contrasting the default experience of mortgages accompanied by buydowns with otherwise similar mortgages in which buydowns were absent.

II. THEORETICAL IDEAS AND EMPIRICAL IMPLEMENTATION

A. Theoretical Background and Structure of the Model

Although the housing finance literature contains numerous studies examining various kinds of creative finance techniques and seller financing concessions in general (see, for example, Jaffee [1984], Brueckner [1984], Agarwal and Phillips [1983 and 1984], Schwartz and Kapplin [1984], Sirmans, Smith, and Sirmans [1983], and Durning and Quigley [1985]), there has been little work specifically on temporary buydowns. Moreover, most of the existing literature on seller financing concessions has focused on the capitalization question directly—that is, asking the extent to which the sales price of a home embodies the capitalized value of financing concessions. The findings are, in general, that seller financing concessions are reflected in sales prices of homes but at less than full present value. The companion report on the first part of this study (Cotterman [1992]) extends these capitalization results to temporary buydowns.

The other strand of literature relevant to the current study is that on mortgage default. (See, for example, Campbell and Dietrich [1983], Foster and Van Order [1984], and Cunningham and Capone [1990]. Neal [1989] presents a useful summary.) This literature has tended to focus on the structure and causes of the default decision, and in particular on the role of ability-to-pay and/or equity considerations in generating default behavior. Little attention has been paid to the possible role of seller financing concessions and of temporary buydowns in particular.[3]

This report builds upon ideas in the option-based mortgage default literature with suitable modifications to allow for the features of temporary buydowns. We assume that the individual mortgagor faces three choices at any point in time: to continue to hold the existing mortgage, to prepay the mortgage, or to default on the mortgage. Absent such

[3]One exception is Cunningham and Capone (1990), whose study of default and prepayment includes a variable defined as the remaining duration of the buydown if the buydown rate is below the contemporaneous market rate, zero otherwise. They report that this variable has a negative and insignificant effect on the probability of default.

considerations as transactions costs, damage to credit rating, costs of moving, etc., three magnitudes are relevant for this decision: the principal balance on the current mortgage $P(t)$, current value of the mortgage $V(t)$, and the current value of the home $H(t)$.[4] Default occurs when the value of the home is the minimum of these three magnitudes; *i.e.*, $H(t) < \min(V(t), P(t))$; prepayment occurs when the principal balance is the minimum of the three, *i.e.*, $P(t) < \min(V(t), H(t))$; and the mortgage continues to exist when the value of the mortgage $V(t)$ is the minimum of the three.

TABLE 1

Definitions of Variables Used in the Theoretical Development

$P(t)$	Principal balance (at time t)
$V(t)$	Value of the mortgage (at time t)
$H(t)$	Home value (at time t)
$C(t)$	Costs of default (at time t) as a proportion of home value
S_0	Sales price in most recent transaction
$B(t)$	Buydown value: the present value of remaining seller financing concessions as of time t
$g(t)$	Growth rate of home values at time t

The reasoning here is straightforward. When the value of the mortgage exceeds the principal balance, prepaying dominates the status quo because the individual can obtain a new mortgage with a value equal to the principal balance. When the value of the home is less than both the current principal balance and the value of the current mortgage, however, default is optimal because one could, in principle, repurchase the same home for less than the cost of either holding or prepaying the existing mortgage.

Now consider the introduction of costs of default. Costs of default act as a buffer, making default less likely to occur, other things the same. Assuming for convenience that

[4]Table 1 lists the definitions of all variables utilized in the theoretical development.

default costs are proportional to the value of the home (which seems reasonable for costs like moving costs) but possibly vary with loan duration, the default condition becomes

$$H(t)(1 + C(t)) < \min(V(t), P(t)) \tag{1}$$

where $C(t)$ is costs of default as a proportion of home value.[5] (Analogously, costs of prepayment, which are ignored here, could be incorporated in the $P(t)$ term.)

Buydowns and other seller financing concessions potentially affect default in two ways. The first way is through their implicit relation to the value of the home at the time of the sale. Abstracting from the possibility of offering seller financing concessions as part of a resale transaction, the current market value of a home is expected to be directly related to the market value of the home on the date of purchase, which is in turn expressible as the sales price of the home less the capitalized value of seller financing concessions that were part of the original sales transaction. Specifically, we may relate the current value of the home $H(t)$ (in the absence of financing concessions upon resale) to the most recent sales price S_0 and the present value of seller financing concessions that were part of that transaction, $B(0)$, evaluated at the time of sale, by[6]

$$H(t) = (S_0 - \gamma B(0)) \exp[g(t) + \epsilon(t)] \tag{2}$$

where $g(t)$ is a deterministic function of time; $\epsilon(t)$ is a stochastic function; and $g(0) = \epsilon(0) = 0$.[7] The coefficient γ measures the extent to which the present value of seller financing concessions is reflected in the sales price of the home: $\gamma = 1$ when the buydown amount is reflected dollar for dollar in the selling price.

[5]Economic stress—loss of the mortgagor's job, unexpected increases in medical bills, etc.—could be treated as a negative cost of default, making default more likely to occur. An alternative model is to treat inequality (1) and the existence of economic stress (somehow measured) as jointly necessary and sufficient for default to occur.

[6]Notice that our notation draws a distinction between the most recent sales price of the home, S_0, and its value at that time in the absence of a buydown, $H(0)$.

[7]The influence of discount points could be treated in a fashion parallel to that accorded temporary buydowns and other seller financing concessions.

Note that the default condition, (1) above, may equivalently be expressed as $\ln H(t) + \ln(1 + C(t)) - \ln\min(V(t), P(t)) < 0$. Taking logs of Eq. (2) and substituting, the default condition may be expressed as

$$\ln S_0 + \ln(1 - \gamma B(0)/S_0) + g(t) + \epsilon(t) + \ln(1 + C(t)) - \ln\min(V(t), P(t)) < 0$$

or

$$\ln S_0 - \gamma B(0)/S_0 + g(t) + \epsilon(t) + \ln(1 + C(t)) - \ln\min(V(t), P(t)) < 0 \tag{3}$$

where we have used the approximation $\ln(1 + \delta) \approx \delta$ for small δ.[8] According to inequality (3), an increase in the initial value of the buydown $B(0)$, holding constant the sales price S_0, would reduce the left hand side of (3), making default more likely.

A second way in which a temporary buydown affects the default calculation is through its impact on the value of the mortgage, $V(t)$. During the early years of the mortgage, the buydown helps offset mortgage payments. Thus, until the buydown escrow is exhausted, there remain $B(t)$ dollars of buydown payments (evaluated in present value terms as of time t) remaining to be applied to future monthly mortgage payments. To emphasize the fact that, until the buydown is exhausted, the buydown reduces the value of future mortgage payments, we let $V(t)$ measure the value of the mortgage in the absence of the buydown subsidy (*i.e.*, if the full coupon rate were paid). Thus $V(t) - B(t)$ represents the value of the mortgage when the buydown is present.[9]

We assume that if the mortgage is prepaid, the present value of remaining (unused) buydown amounts are returned to the mortgagor,[10] and thus the presence of the buydown has no effect on the incentive to prepay the mortgage.[11] Hence $P(t)$ continues to represent

[8] This approximation follows from a Taylor's series expansion: $\ln(1 + \delta) = \delta - (1/2)\delta^2 + (1/3)\delta^3 - \cdots$, for $|\delta| \leq 1$ and $\delta \neq -1$. When δ is small, the total of all terms after the first is negligible.

[9] This treatment ignores the possible distinction between the present value of the remaining buydown and its market value.

[10] Although the buydown escrow can be, and sometimes is, returned to the mortgagor in the event of prepayment, our understanding is that more commonly the buydown escrow reverts to whoever funded it.

[11] An alternative scenario that would also render the buydown neutral in its effect on prepayment is to assume that the buydown escrow accumulates interest, payable to the mortgagor, at the contemporaneous market interest rate.

the principal balance on the mortgage that must be paid to the mortgage holder in the event of prepayment. The net mortgage balance to the mortgagor, however, is $P(t) - B(t)$ because $B(t)$ would be returned to the mortgagor in the event of prepayment. Viewed differently, although the mortgagor still owes $P(t)$ to the lender, and this represents the discounted value of remaining mortgage payments at the coupon rate, the value of these payments is reduced to the extent that future buydown payments offset monthly mortgage expenses.

Building these effects into (3), we may express the condition under which default occurs as

$$\ln S_0 - \gamma B(0)/S_0 + g(t) + \epsilon(t) + \ln(1 + C(t)) - \ln(\min(V(t), P(t)) - B(t)) < 0$$

or

$$\begin{aligned} &\ln S_0 - \gamma B(0)/S_0 + g(t) + \epsilon(t) + \ln(1 + C(t)) \\ &- \ln \min(V(t), P(t)) + (B(t)/\min(V(t), P(t))) < 0 \end{aligned} \tag{4}$$

where we have again used the approximation $\ln(1 + \delta) \approx \delta$ for small δ. Inequality (4) clarifies and summarizes the potential role of temporary buydowns in mortgage default behavior by isolating the two effects. The first effect is reflected in the term $(-\gamma B(0)/S_0)$. To the extent that the value of a temporary buydown increases the purchase price of home $(\gamma > 0)$ but cannot be recaptured upon resale of the same home, default is encouraged. That is, when comparing homes with equal purchase prices, those bought under temporary buydown arrangements would be expected to show greater propensity to default from this effect alone.

Buydowns do not operate on default propensities solely through the latter channel, however. A second effect is captured in the term $(B(t)/\min(V(t), P(t)))$, which reflects the effect of the buydown on the mortgagor's payment burden. The latter impact is important enough that the net effect of a buydown on default may well change over the early years of the mortgage. To see this, note that the initial sales price of the home S_0 will generally exceed both the initial principal balance $P(0)$ and the initial value of the mortgage $V(0)$.[12] If there is less than full capitalization of the value of the buydown into the sales price of the home, then $\gamma < 1$, and it must therefore be the case that

$$\gamma B(0)/S_0 < B(0)/\min(V(0), P(0)). \tag{5}$$

In view of the default condition given by (4), the initial net effect of the buydown is thus to make default less likely to occur. The reasoning is straightforward: when compared with equally priced homes not having temporary buydowns, the lower resale value of a home that is sold with a temporary buydown is initially more than offset by the buydown payments that will be applied to the early mortgage payments. Since these buydown payments would be sacrificed in the event of default, the likelihood of default is correspondingly reduced.[13] As the mortgage ages, the remaining buydown amount $B(t)$ falls eventually to zero. Thus there is some intermediate time at which the two buydown effects—one on the implied resale value, the other on the remaining house payment burden—exactly counterbalance, leaving the buydown with no net impact on default.[14] Beyond this break-even point, which must occur no later than the time at which buydown payments fall to zero, there is too little buydown remaining to offset the reduced home value, and the result is a higher default rate for homes with buydowns than for homes without buydowns.[15]

[12]We ignore the possibility that financed closing costs and financed mortgage insurance premia could push the initial principal balance over the initial sales price of the home.

[13]With more than full capitalization ($\gamma > 1$), there need not necessarily be a period during which default probabilities are reduced.

[14]Strictly speaking, this statement assumes that $\min(V(t), P(t))$ is continuous in t.

[15]Allowing a buydown to occur at resale would not necessarily offset this effect, but it complicates the analysis. Although $H(t)$ has been defined to be the contemporaneous value of the home in the absence of a buydown, we may more generally view $H(t)$ as the net market value of the home under the optimal resale strategy, whether or not this strategy dictates that a buydown be offered. If the best strategy is to include a buydown as part of a resale, then $H(t)$ includes the net benefit to the mortgagor of offering the buydown as part of the sales transaction. In this more general case, there is an additional complication if the net cost to the mortgagor of supplying a buydown differs from the net benefit to prospective buyers, for then the net cost of defaulting could depend upon whether the individual repurchases his/her own home. That is, once one allows the possibility of a buydown upon resale, $H(t)$ may lose its interpretation as the unique market value of the asset.

B. Estimation

To turn this theoretical construct into an estimable statistical model, we formulate a hazard model that embodies the essential features of inequality (4). To begin, we note that inequality (4) embodies an implicit normalization in setting all coefficients (other than that on $B(0)/S_0$) to unity. Relaxing this normalization and rearranging so that observables appear on the right-hand side, we rewrite the default condition as

$$\begin{aligned}\epsilon(t) < &-\beta_0 \ln S_0 + \beta_0 \gamma B(0)/S_0 - \beta_0 g(t) - \beta_0 \ln(1 + C(t)) \\ &+\beta_0 \ln \min(V(t), P(t)) - \beta_0(B(t)/\min(V(t), P(t))) \qquad (6)\end{aligned}$$

where, for notational simplicity, we now redefine $\epsilon(t)$ to include all randomness and other unobservables.

To translate the default inequality (6) into a probability statement, we assume that the hazard rate for defaults $\lambda(t)$, which gives the probability of default at time t given that default has not occurred prior to t, is a function of the linear combination of observables that appears on the right-hand side of (6). Letting X denote this vector of observables, and letting β denote the corresponding vector of coefficients (all but one of which is β_0), the right-hand side of inequality (6) may be written as $X'\beta$. Taking the special case of a proportional hazards model in exponential form, the hazard function, or conditional probability of default, is expressed as

$$\lambda(t) = \lambda_0(t) \exp(X'\beta)$$

where $\lambda_0(t)$ is an arbitrary baseline hazard function that captures all direct dependence on duration,[16] and where for compactness of notation we have suppressed the dependence of λ on X and β.[17]

[16] The hazard also depends indirectly on time through time variation in the X.

[17] This framework may not be sufficient to capture the full range of theoretical possibilities. Specifically, the term $\epsilon(t)$ is important as a source of randomness in default behavior, but we have not been specific

For purposes of estimation and prediction, an important feature of this framework is that one can go from the hazard function to the survivor function, $A(t)$, which gives the probability that default has not occurred prior to t. The relevant relationship is given by $\lambda(t) = -d \ln A(t)/dt$; thus, the survivor function can be obtained by integrating the hazard function. From the survivor function, the density function of default times, $f(t)$, may be derived by differentiation: $f(t) = -dA(t)/dt$.

As with most mortgage data that are available for empirical implementation of models of default, the FHA data to be used here are characterized by censoring. That is, the data contain two fundamental types of observations: (a) those observed to end in default at some point over the observation period, and (b) those that do not end in default, either because the mortgage remains active at the end of the period of observation, or because it terminates by virtue of either prepayment or assumption by a new mortgagor over the observation interval.[18] The hazard function methodology enables one to incorporate easily both the former (uncensored) observations as well as the latter (right-censored) observations. Defining d_i to be unity when observation i is uncensored, and zero otherwise, the log likelihood function $\ln L(\beta)$ may be written as

$$\ln L(\beta) = \sum_i (d_i \ln f(t_i) + (1 - d_i) \ln A(t_i)) \tag{7}$$

where t_i denotes the time of default for uncensored observations and the length of the observation interval for censored observations. (Dependence of f and A on β and X has

about its form. One plausible assumption is that the randomness follows a random walk. If so, the hazard model specified may not fully capture the time dependence induced by such a process. In particular, since the random walk specification assumes that new noise generated at each instant simply gets added to the accumulation of noise generated earlier, then the fact that default has not occurred prior to time t is informative about the probability of default occurring at t. Hence the conditional probability of default occurring at t (and thus the hazard) should depend on past values of the explanatory variables. One way to construct the hazard function so that it does embody the random walk assumption is to use the theoretical construct to derive the distribution of first passage times, and then use the latter to derive the implied hazard function. Unfortunately, with a boundary that is time-varying (due to variation in the $V(t)$ and $P(t)$), the task of deriving the distribution of first passage times seems difficult indeed, though it remains a theoretical possibility.

[18]The fact that mortgages may terminate in prepayment, default, or assumption suggests a competing risks framework. Budgetary constraints do not permit this extension. We simply treat prepaid and assumed mortgages, together with those that remain active at the close of the observation period, as right-censored from the perspective of the default analysis. Results in Kalbfleisch and Prentice (1980) demonstrate that this treatment of the competing risk is appropriate.

been suppressed for expository convenience.)

Although maximization of Eq. (7) with respect to β would yield estimates with the usual (desirable) asymptotic properties, there are several practical difficulties in implementing this procedure in the instant case. First, the data to be utilized in this study do not indicate the exact time of default, but only the month of default. Thus, we modify the likelihood function, Eq. (7), by replacing the density of the time of default $f(t_i)$ with the probability that the time of default lies in the month that starts at time τ_{1i} and ends at time τ_{2i}:

$$\ln L(\beta) = \sum_i (d_i \ln(A(\tau_{1i}) - A(\tau_{2i})) + (1 - d_i) \ln A(t_i)). \tag{8}$$

Second, some of the explanatory variables that we propose above (*i.e.*, the components of X) are time-varying. While this feature raises no new theoretical problems in that the hazard may still be integrated to form the (log of the) survivor function, integration becomes much messier because it must be performed separately over each interval for which an X varies for a particular observation. When some explanatory variables vary nearly continuously, as is true here (see the discussion below), it is unclear how finely to divide the time interval to pick up movements in X. Finer division is more costly but presumably provides more precise estimates. For our purposes we use a month as the time interval over which explanatory variables are assumed to be unchanged.

Third, the data utilized in the current study are stratified according to outcomes.[19] In particular, defaults are oversampled relative to nondefaults. Following Manski and Lerman (1977), we account for this feature by employing a weighted exogenous sampling maximum likelihood (WESML) estimator. The WESML is formed by weighting each of the two branches of the likelihood function, Eq. (8), by the ratio of the population share with a particular outcome to the sample share with that same outcome.[20]

[19] As discussed below, the data are also stratified according to city, time period, and whether the subject home is newly constructed. These aspects are reasonably treated as exogenous to the default process, and thus stratification in these dimensions poses no problems in estimation.

[20] The computation of the asymptotic covariance matrix follows that presented in Manski and Lerman, p. 1984.

A fourth practical issue is how to specify the baseline hazard. In this study we employ the log logistic baseline hazard:[21]

$$\lambda_0(t) = \frac{\phi\theta t^{\theta-1}}{1+\phi t^{\theta}}, \theta > 0, \phi > 0.$$

This two-parameter family is well-suited for this purpose because it permits a "humped" shape that is generally thought to characterize default behavior. For $\theta > 1$, the baseline hazard first increases with duration, *i.e.*, the conditional probability of default at first rises with the age of the mortgage, holding constant the net effect of other influences on mortgage default. The baseline hazard eventually declines, however. For $0 < \theta \leq 1$, the baseline hazard decreases with duration throughout.

We emphasize that the slope and shape of the baseline hazard measure corresponding movements in the conditional default probability while *holding fixed* the net effect of other mortgage default factors for which the model explicitly controls. Some of these factors—*e.g.*, the remaining principal balance—will necessarily vary over the course of the mortgage. Unless offset by changes in other covariates, the net effect of these measured factors will change over the course of a loan. Thus, even on average we expect actual default behavior to deviate from that given by the baseline hazard. Although the baseline hazard should not therefore be viewed as representing typical default probabilities, it can legitimately be viewed as capturing the effects of loan duration *per se* on the conditional probability of default.

Building all of the above-mentioned features into the estimation procedure, we arrive at WESML estimates by maximizing the following quasi-likelihood:

$$\ln L(\beta) = \sum_i w_i(d_i \ln(A(\tau_{1i}) - A(\tau_{2i})) + (1-d_i)\ln A(t_i)). \tag{9}$$

where w_i is the weight attached to observation i (the ratio of the population share to the sample share with the same outcome), and where the survivor function for individual i,

[21] It would be of interest to entertain the more general forms of duration dependence (*e.g.*, the Box-Cox style transformations suggested by Flinn and Heckman [1980]), but these must be numerically integrated, adding substantially to the expense of estimation.

evaluated at time t, is

$$A(t) = \exp\left\{ - \sum_{m=1}^{m=M} \exp(X'_{mi}\beta)[\ln(1 + \phi(m/12)^{\theta}) - \ln(1 + \phi((m-1)/12)^{\theta})] \right\}$$

where time t contains M monthly intervals, and X_{mi} is the value of the vector of observables for individual i during month m.

III. THE NATURE OF THE SAMPLE AND THE SETTING

The empirical work to follow is based upon data on individual FHA-insured loans. These data reside in hardcopy form in casebinders maintained at HUD headquarters. To automate the hardcopy data we utilized the existing automated A43 data files maintained at HUD to select a sample of loans meeting the criteria discussed below. For this sample of loans, Westat coded and entered the data extracted from the individual casbinders. Analysis files were produced by merging the casebinder data with portions of the automated data that were already available.

The original sample design called for standard FHA-insured mortgages originating in two HUD field offices—Denver and San Antonio—during 1982 and 1985/86. More precisely, the sample was restricted to 30-year, level-payment, non-coinsured mortgages for single-family dwellings located in the largest SMSAs serviced by each of the two offices, and having a loan amortization start date in 1982 (for the first part of the observation interval), or from January 1, 1985, through July 31, 1986, inclusive (for the second part of the observation interval).[22] Loan-to-value ratios were restricted to lie between 0.6 and 1.2.[23] Although limitations of the automated data that were used to select the sample precluded the elimination of refinancing transactions at the time that the sample was drawn, refinancing transactions were eliminated when the data were coded from the FHA casebinders.

To construct the strata from which samples were drawn, each case was categorized according to the office of origination, the time period (1982 or 1985/86), whether the loan terminated in default by September 30, 1989,[24] and whether the home was "new" or "old."

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[22]The second part of the observation interval stops in mid-year in an attempt to avoid sales transactions taking place under the revised HUD rules that placed restrictions on underwriting and appraisal in the presence of buydowns.

[23]Use of the loan-to-value (LTV) ratio, as defined in the HUD automated (A43) data, is a bit problematic because the definition of the numerator depends on the nature of the loan processor. The financed portion of the up-front mortgage insurance premium is included in the numerator of LTV for HUD-processed cases, but is excluded from the numerator for direct endorsement cases.

[24]Default dates are those recorded in the automated (A43) data. As noted above, defaults were oversampled because they were so rare. Note that separation according to default status was based on whether the

Homes were categorized as "new" if they were new dwellings being sold by a builder, or were existing but not yet lived-in homes covered by blanket FHA insurance agreements with builders. Other homes were considered "old" homes. Within each stratum defined by the office, time period, default status, and new/old status, cases were randomly selected for inclusion in the sample.

Some of the cases selected for inclusion in the sample turned out to be unusable for a variety of reasons: incorrect automated data led to including some cases that did not meet the sample selection criteria; some loans turned out to be refinances; critical forms were missing from some cases; etc. Indeed, missing forms proved to be a large enough problem for the 1985/86 Denver cases that coding was suspended for the corresponding strata. Having been forced to abandon this portion of the sample, we added additional cells for Phoenix in both 1982 and 1985/86. Table 2 summarizes features of the original cell sizes, the final sample sizes, and the number of cases that were utilized to reach the ultimate sample for each stratum.

Utilizing the same criteria that were used to select the sample of FHA-insured loans, but broadening the time period covered, Figures 1 and 2 demonstrate features of the housing market in the three sample cities during the ten year period starting in October 1979 and ending in September 1989. Figure 1 shows the median mortgage interest rate for each city. Note that interest rates in the first portion of the sample observation interval (1982) were high by historic standards—reaching levels of about 17 percent—while interest rates were substantially lower during the 1985/86 period, dipping to about 10 percent.

Not surprisingly, the behavior of FHA loan transactions over this 10-year interval mirrors the behavior of mortgage interest rates. In particular, as shown in the three panels of Figure 2, the numbers of FHA loan transactions in total and for new homes alone were low in the early 1980s, when interest rates were high, but rebounded to high levels in the 1985/86

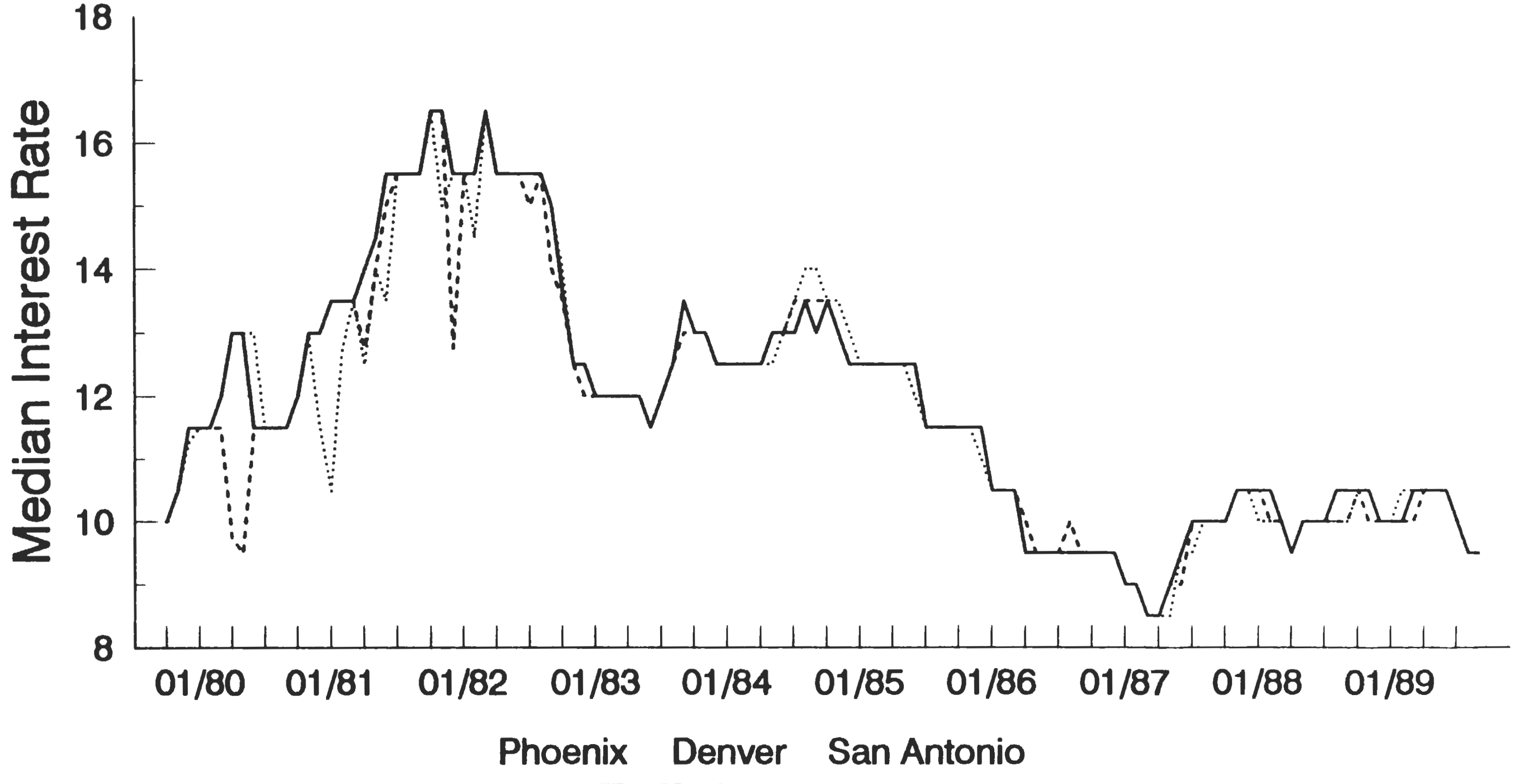
original borrower defaulted. Our major interest was on the characteristics of the home and the homebuyer. Because such information was not available for those who assumed an already existing loan, loans that were assumed before a default ultimately occurred were classified as a non-default by the original mortgagor.

TABLE 2

Cell Sizes by Stratum

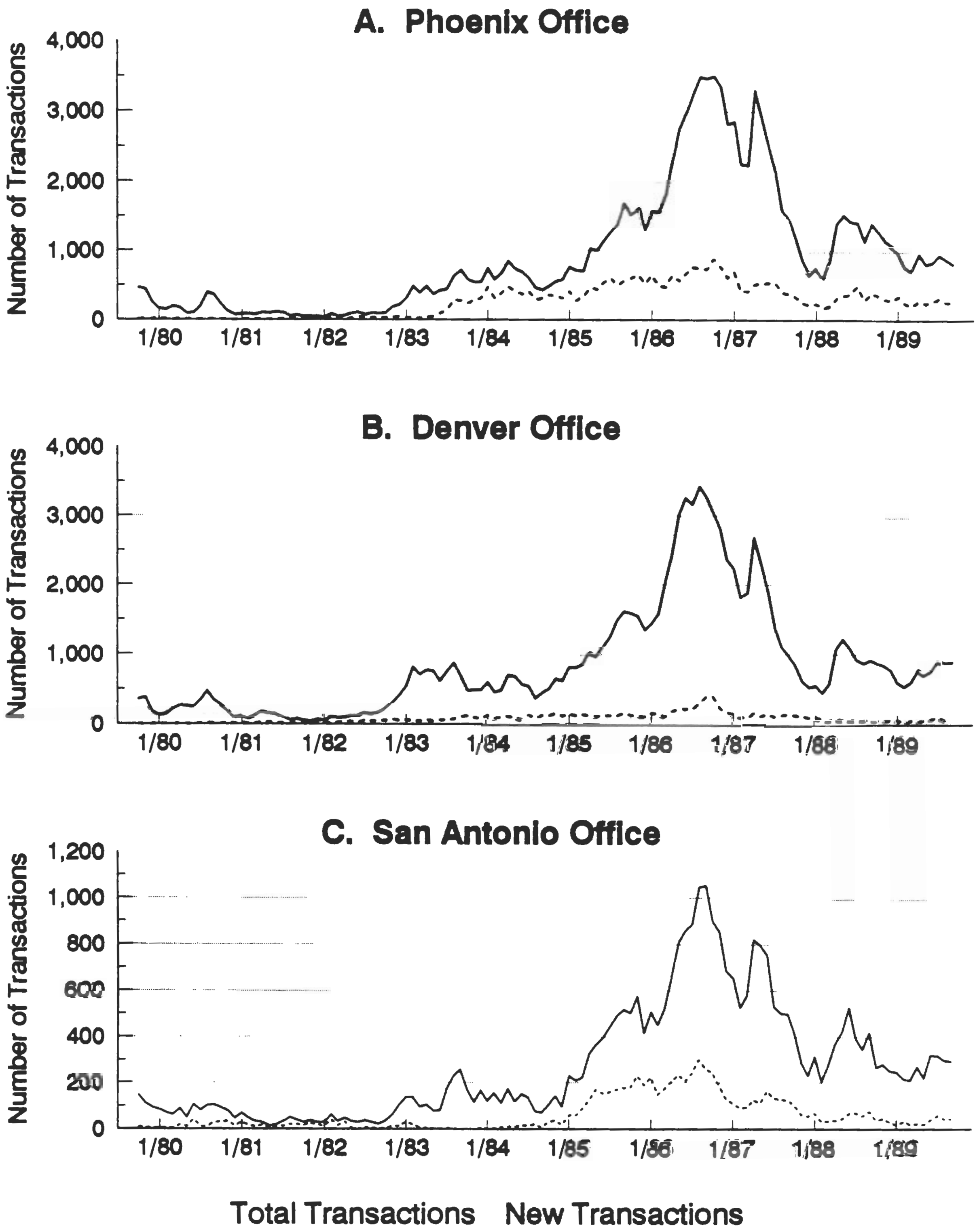
Office	Year	Default Occurred?	New/Old Home	Original Cell Size	Final Sample	Cases Used
Denver	82	no	old	1,302	174	394
Denver	82	no	new	418	187	394
Denver	82	yes	old	202	63	131
Denver	82	yes	new	96	44	96
San Antonio	82	no	old	339	256	339
San Antonio	82	no	new	134	102	134
San Antonio	85/86	no	old	5,471	296	606
San Antonio	85/86	no	new	2,839	264	968
San Antonio	82	yes	old	39	33	39
San Antonio	82	yes	new	30	23	30
San Antonio	85/86	yes	old	714	96	229
San Antonio	85/86	yes	new	386	83	249
Phoenix	82	no	old	742	226	461
Phoenix	82	no	new	284	65	284
Phoenix	85/86	no	old	19,012	252	669
Phoenix	85/86	no	new	9,507	247	412
Phoenix	82	yes	old	130	71	130
Phoenix	82	yes	new	51	8	51
Phoenix	85/86	yes	old	1,292	83	154
Phoenix	85/86	yes	new	658	86	117

Figure 1
Median Interest Rates on 30-Year FHA-Insured Loans, by Office and Month*



*See text for other sample restrictions.

Figure 2
Number of 30-Year FHA-Insured Loan Transactions in Total and on New Homes, by Month*



*See text for other sample restrictions.

period as interest rates declined.[25]

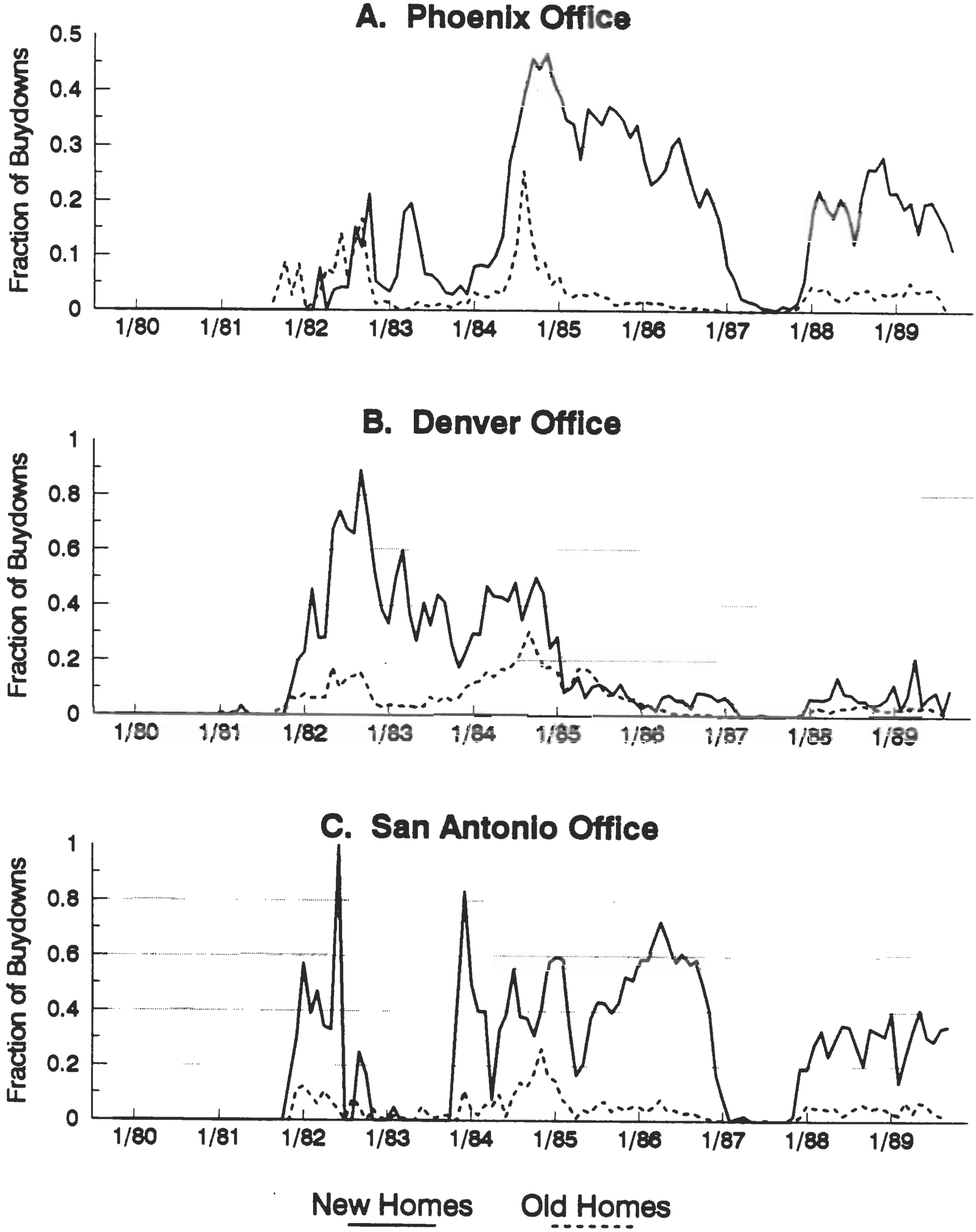
The relative frequency with which temporary buydowns were a part of the sales transaction changed over time and differed across cities as well. The panels of Figure 3 show, for new and old home sales separately, the fraction of transactions in which a temporary buydown occurred. Note that buydowns tended to be more common among new home sales than among old. For new home sales transactions, the periods of substantial buydown activity occurred later in Phoenix than in Denver, while San Antonio exhibited three distinct periods of heavy buydown activity. Among old home sales, buydown activity appears to have peaked in mid to late 1984 in all three cities.

Table 3 summarizes some of the data contained in the figures above but focuses solely on the sample periods, 1982 and 1985/86. The first row of the table shows that new home transactions were about one-quarter to one-third of all FHA loan transactions within each sample period and city, but they tended to be a larger share of the transactions in San Antonio than in Phoenix, and lower still in Denver.[26] The second row shows that buydowns were present in a larger share of all FHA transactions in the 1985/86 sample periods than in 1982. The breakdown by old and new homes, as shown in the third and fourth lines, illustrates that this increase in the popularity of buydown transactions occurred partly because of dramatic increases in the share of buydowns among new home transactions; the share of buydowns among old homes changes little from the 1982 to the 1985/86 sample periods.

While Table 3 illustrates the potential importance of buydowns in the market by measuring their relative frequency, Table 4 illustrates their importance in another dimension. Table 4 attempts to give an indication of the monetary significance of the average buydown by comparing it to average loan discount points. The top two rows of the table express the buydown amount as a percentage of the sales price of the home, on average, for new and

[25]Loan transactions are assigned to the month in which amortization began.
[26]Loan transactions were classified according to the year in which loan amortization began.

Figure 3
Fraction Having Temporary Buydowns among FHA-Insured, 30-Year Loans on Old and New Homes, by Month*



*See text for other sample restrictions.

TABLE 3

FHA Buydowns and New Home Loan Transactions

Characteristics of FHA Loan Originations	Phoenix		Denver	San Antonio	
	1982	1985/86	1982	1982	1985/86
Percent New Homes	27.8	33.4	25.5	30.3	34.3
Percent Buydowns	6.0	11.6	21.0	11.8	20.6
Among New Homes	7.5	31.5	58.0	29.3	50.9
Among Old Homes	5.5	1.6	8.4	4.2	4.9

Source: Computations based on A43 automated data.

TABLE 4

A Comparison of Buydown Amounts with Loan Discounts

Characteristics of FHA Loan Originations	Phoenix		Denver	San Antonio	
	1982	1985/1986	1982	1982	1985/1986
Buydown Amount as Percent of Sales Price of Home:					
Among New Homes with Buydowns	5.2	5.4	4.7	5.3	5.1
Among Old Homes with Buydowns	5.0	4.0	4.7	4.7	4.8
Loan Discount Points Paid By Seller as Percent of Sales Price of Home:					
Among New Homes with Loan Discount Points	6.9	5.4	5.6	10.4	7.7
Among Old Homes with Loan Discount Points	5.6	2.2	5.2	4.0	2.6

Source: Calculations based on sample data coded from FHA case files.

old home buydown transactions separately in each of the sample cities and time periods. The bottom two rows provide the analogous figures for loan discount points, *i.e.*, discount points paid by the seller relative to the sales price of the home, on average, among all home sales in which the seller paid discount points. Notice that the orders of magnitude are quite similar. Buydowns, when present, tend to be about five percent of the sales price of the home, which is the approximate cost of a 3-2-1 buydown.[27] There appears to be more cross-sample variation in loan discount points; differentials between new and old homes are especially dramatic for the San Antonio samples.

Table 5 carries the investigation one step further by presenting buyer characteristics and behavior associated with, or perhaps engendered by, the existence of buydowns. Each number in the body of the table presents, for a particular sample cell, an average value for those borrowers who used buydowns; the number immediately below (in parentheses) is the corresponding average in that same sample cell for borrowers who did not use buydowns. The first pair of rows shows that with only one exception borrowers who used buydowns were younger on average than those who did not. The second and third pair of rows show that, with few exceptions, mortgagors who used buydowns tended to have lower incomes than those who did not, a finding which suggests the use of buydowns to help buyers qualify for loans. This idea is reinforced in the fourth pair of rows, which illustrates that despite having lower average incomes, borrowers who used buydowns in their purchase of old homes tended (with one exception) to buy more expensive homes than buyers who did not use buydowns. The situation is reversed for purchasers of new homes: sales prices were, with one exception, lower on average for transactions with buydowns than for those without. Comparisons of average loan amounts for transactions with and without buydowns generally follow the same pattern as that of sales prices. There are two exceptions, however: for new home transactions in 1982 for Denver and in 1985/86 for San Antonio, average loan amounts for buydown transactions exceeded those for nonbuydowns even though average sales prices are higher for the nonbuydown transactions.

[27] As a matter of terminology, a 3-2-1 buydown effectively lowers the borrower's mortgage rate by three percentage points during the first year, by two percentage points during the second year, and by one percentage point during the third year. That is, the borrower's monthly mortgage payments during the first year are computed as if the interest rate on the loan were three percentage points below the coupon rate; payments during the second year are computed as if the interest rate were two percentage points below the coupon rate; and so forth. The lender always receives payments at the coupon rate, however, because the buydown escrow makes up the difference.

TABLE 5

Characteristics of Buydown and Nonbuydown (in Parentheses) FHA Loan Originations in Sample Cities and Time Periods

	OLD HOMES					NEW HOMES				
	Phoenix		Denver	San Antonio		Phoenix		Denver	San Antonio	
	1982	1985/86	1982	1982	1985/86	1982	1985/86	1982	1982	1985/86
Mean Age of Mortgagor	31.4 (32.2)	29.3 (33.8)	29.4 (32.1)	28.8 (29.9)	30.1 (33.8)	36.2 (35.4)	29.8 (34.5)	29.4 (30.1)	30.1 (33.3)	31.5 (32.3)
Mean Monthly Base Pay of Morgagor	1,976 (2,095)	1,870 (2,226)	1,903 (2,226)	1,356 (1,581)	1,944 (1,972)	1,910 (2,323)	1,954 (2,435)	2,316 (2,377)	2,026 (2,257)	1,869 (1,979)
Mean Monthly Net Effective Income of Mortgagor and Co-Mortgagor	2,264 (2,387)	2,092 (2,654)	2,678 (2,555)	1,966 (1,991)	2,274 (2,354)	2,224 (2,381)	2,479 (2,802)	2,509 (2,718)	2,515 (2,486)	2,272 (2,479)
Mean Sales Price of Home	65,020 (58,255)	61,436 (69,904)	72,285 (70,312)	46,365 (40,026)	67,289 (56,664)	71,490 (66,650)	77,569 (81,325)	76,509 (78,002)	58,676 (65,269)	70,321 (70,776)
Mean Loan Amount	61,855 (54,257)	60,778 (67,269)	65,526 (62,525)	42,962 (37,371)	66,289 (54,742)	65,769 (58,895)	76,599 (76,760)	70,097 (68,323)	54,402 (56,289)	69,429 (68,986)
Difference between Mean Sales Price of Home & Mean Loan Amount	3,165 (3,998)	658 (2,635)	6,759 (7,787)	3,403 (2,655)	1,000 (1,922)	5,721 (7,755)	970 (4,565)	6,412 (9,679)	4,274 (8,980)	892 (1,790)
Percent Defaulting as of 9/30/89	16.7 (14.8)	12.2 (6.3)	16.7 (13.1)	31.3 (9.4)	13.3 (11.5)	12.0 (15.5)	8.5 (5.5)	22.2 (13.9)	18.8 (18.1)	11.6 (12.4)

Source: All figures other than percent defaults are based on sample data coded from FHA case files. Percent defaulting is calculated from A43 automated data.

Skipping to the last pair of rows in Table 5, we see that in all but two cases a larger fraction of buydown transactions than of nonbuydown transactions terminated in default by September 30, 1989. This relationship is consistent with the idea that sales prices incorporate at least part of the value of any associated temporary buydown, but that the capitalized value of the buydown can not be recaptured on resale. However, the pattern of defaults may be reflecting other differences between average buydown and nonbuydown transactions. As shown in the second to the last pair of rows, the difference between the mean sales price of the home and the mean loan amount—measured initial equity—is generally higher for nonbuydown transactions. This difference in itself would tend to lead to higher default rates among buydown transactions. In addition, lower incomes are coupled with higher loan amounts for buydown transactions in many of the sample cells, suggesting that payment-to-income ratios are ultimately higher among the buydown transactions (*i.e.*, after buydown termination). To the extent that payment-to-income ratios matter in default behavior, the observed pattern of defaults may also be partly traceable to these differences in payment-to-income ratios.

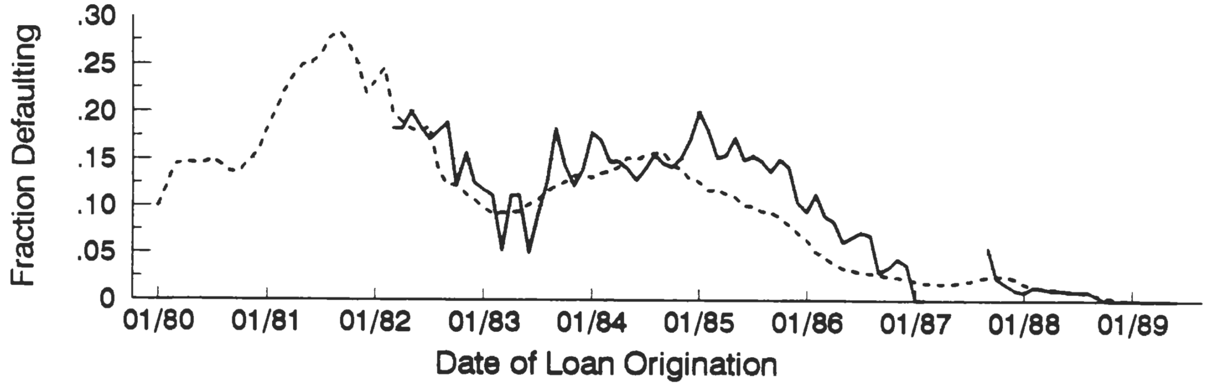
The six panels of Figure 4 contrast graphically the default behavior of buydown and nonbuydown transactions at various durations. These panels thus permit a glimpse into the way in which relative default behavior changes as mortgages age. Each panel shows, among all loans originating in a given month for a particular city and new/old home status, the (smoothed) fraction of loans ending in default as of September 30, 1989.[28] The solid line illustrates the behavior of loans with buydowns; the dashed line illustrates the default behavior of loans without buydowns.

Notice that for the pre-1987 period the plot for the buydowns tends generally to lie above the plot for the nonbuydowns, indicating heavier default activity among the buydown

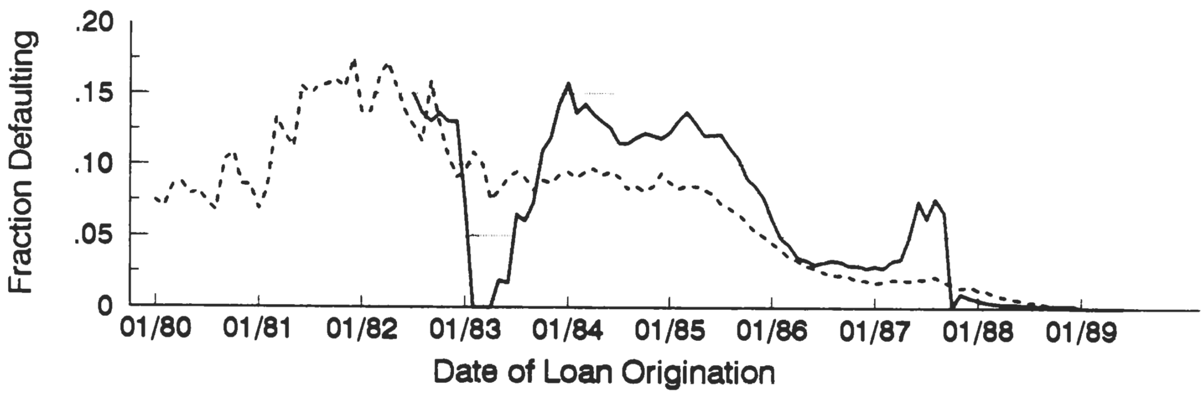
[28]More precisely, because these data were extremely volatile, they were smoothed by taking centered seven-month moving averages of the numerators and the denominators, and then forming the ratio. For example, to find the plotted point for buydowns in September 1984, we computed the total number of buydowns in loan transactions for June through December of 1984, and the number among these that ended in default by September 30, 1989. The ratio of the latter to the former was plotted as the value for September 1984. Fractions based on denominators of less than 16 were eliminated from the plots.

Figure 4:
Fraction of 30-Year, FHA-Insured Loans Ending in Default as of 9/30/89 by Buydown Status and Month of Loan Origination

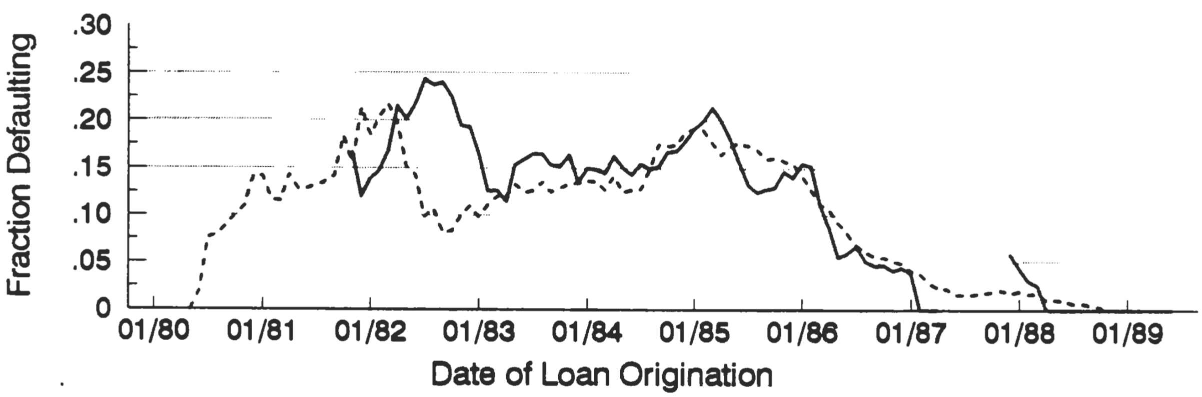
A. Phoenix Office, Old Homes



B. Phoenix Office, New Homes



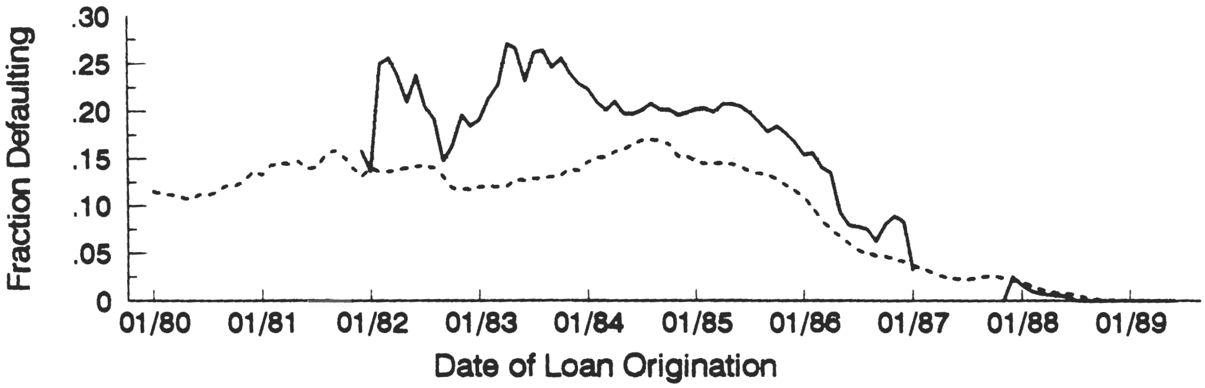
C. Denver Office, Old Homes



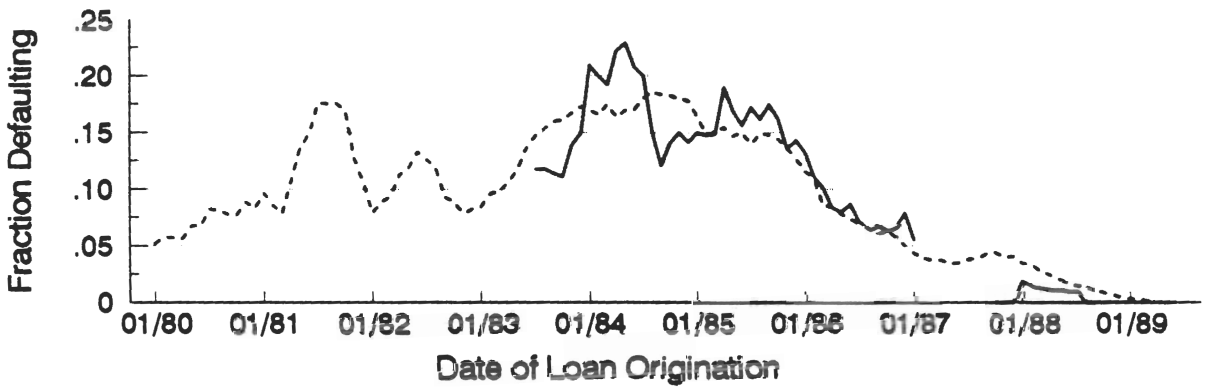
Buydown ———— Non-Buydown - - - - - - -

Figure 4, Cont.
Fraction of 30-Year, FHA-Insured Loans Ending in Default as of 9/30/89 by Buydown Status and Month of Loan Origination

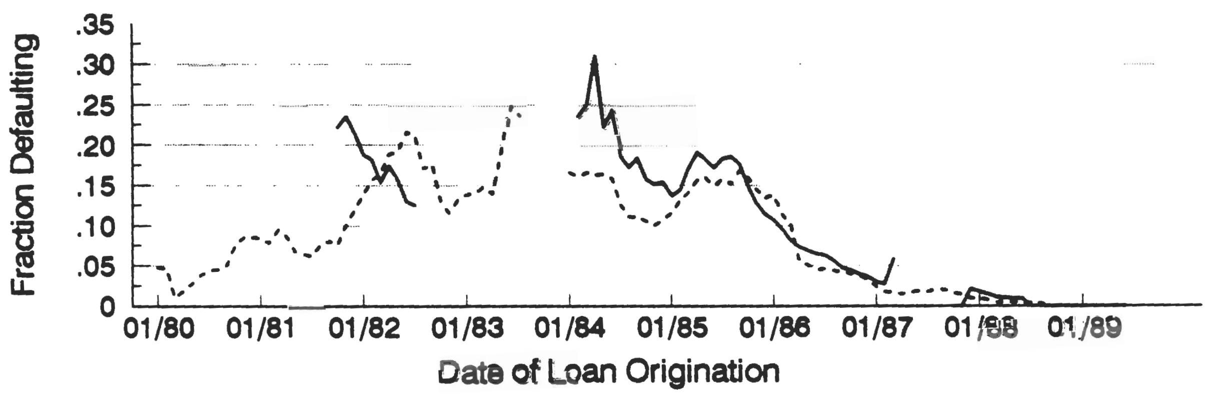
D. Denver Office, New Homes



E. San Antonio Office, Old Homes



F. San Antonio Office, New Homes



Buydown Non-Buydown

transactions. This difference is especially pronounced for the data on Denver new homes in panel (c). In contrast, the plots for the late-1987 period onward tend generally to exhibit somewhat lower default rates for the buydowns than for the nonbuydowns. While it would be hazardous to read too much into these simple contrasts, they do seem generally supportive of the ideas discussed earlier. As noted above, theory suggests that default rates for buydowns should be lower initially but higher eventually than for otherwise comparable nonbuydowns. While the plots in Figure 4 hold few features constant (month of loan origination, city, and old/new home status), the general tendency for cumulative default rates to be higher for buydowns than for nonbuydowns among older mortgages, but to exhibit the opposite pattern for younger mortgages, seems to support the theory.

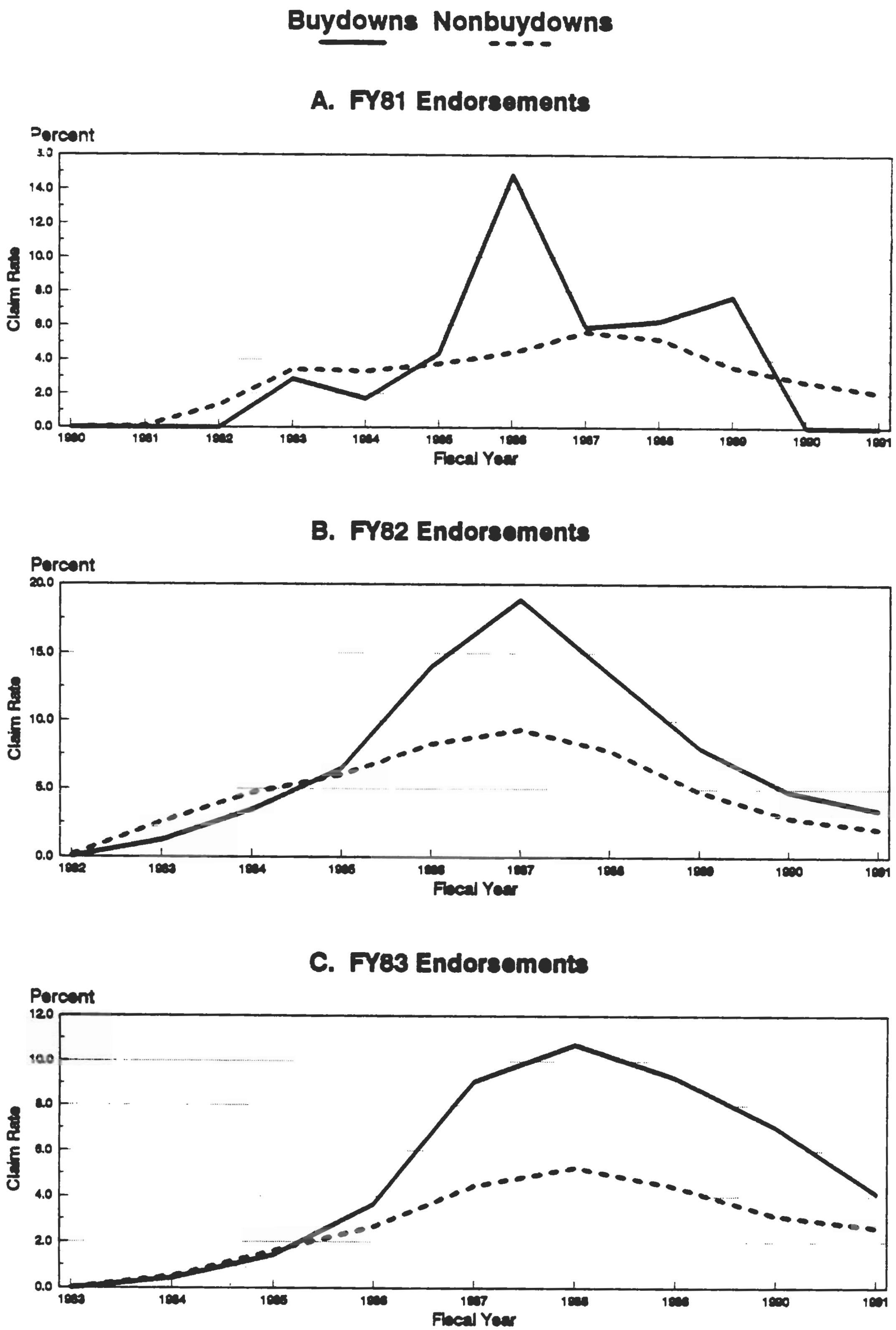
Data for the larger FHA market offer additional support to the hypothesis that buydowns temporarily lower, but ultimately raise, the conditional probability of default. Figure 5 plots claim rates[29] for FHA-insured, 30-year, level-payment mortgages by year of endorsement and buydown status. Notice that each panel shows the contemporaneous default experience of a set of loans that originate at (approximately) the same time. (In contrast, the previous set of plots shows cumulative default experience, as of a fixed date, for loans originating at various points in time.) The panels in Figure 5 show that in all but two endorsement years (1986 and 1987), default rates for buydown transactions are lower than for nonbuydown transactions at low durations; in all cases (other than the 1990 endorsements, for which there is too limited a period of observation) buydown default rates are above the nonbuydown default rates at higher durations.

Although many of these simple summaries seem consistent with the theoretical predictions of differential default experience under buydowns, they do not, of course, hold fixed many of the other default-related aspects of the mortgages at issue. To control for these other features, we turn to a more systematic way of estimating the impact of buydowns on default.

[29] Claim rates are computed as follows: (the number of claims during the year) ÷ (the number of loans active at the start of the year minus the number of nonclaim terminations during the year).

Figure 5

Claim Rates by Fiscal Year and Buydown Status



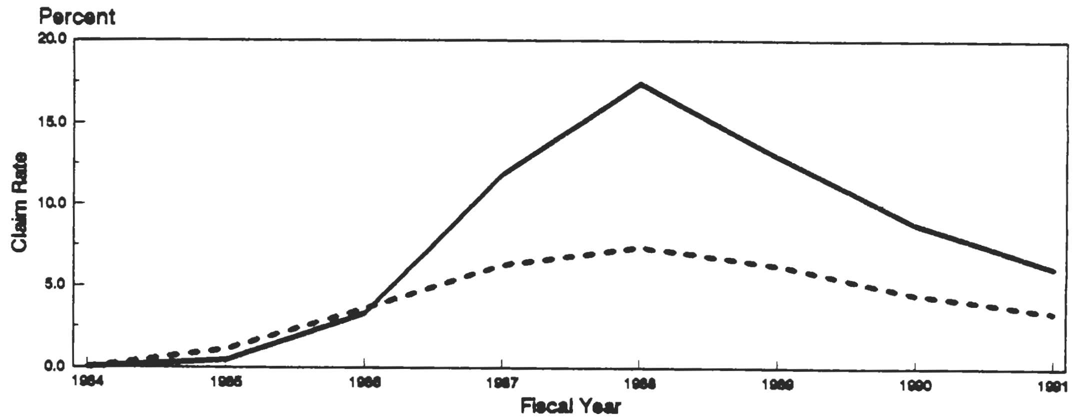
Source: Information supplied by HUD.

Figure 5, Cont.

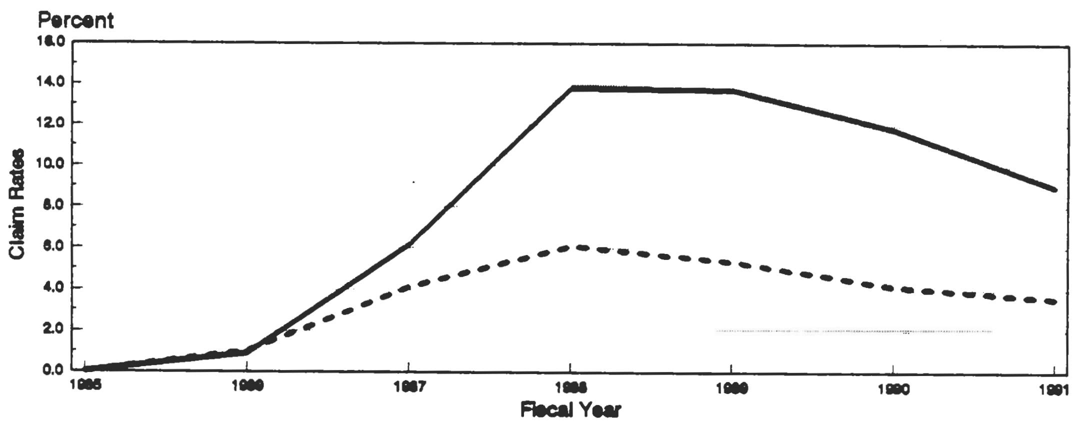
Claim Rates by Fiscal Year and Buydown Status

Buydowns **Nonbuydowns**

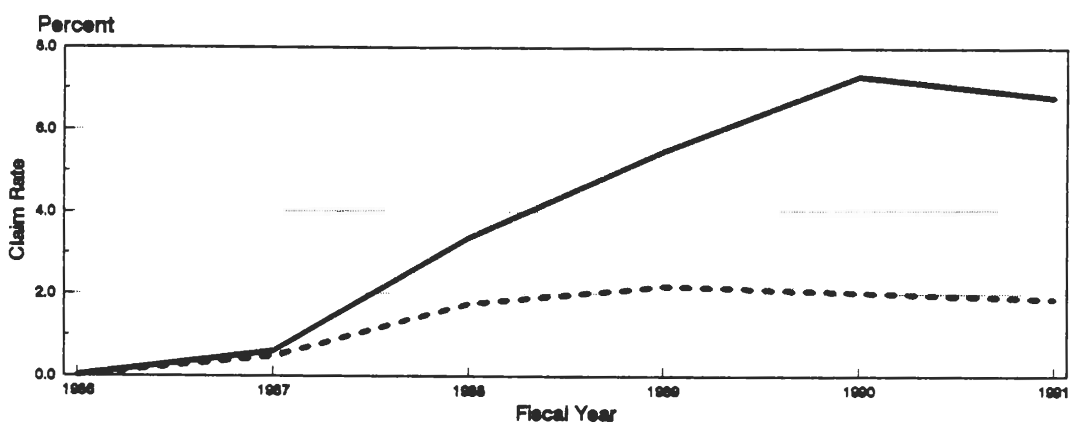
D. FY84 Endorsements



E. FY85 Endorsements



F. FY86 Endorsements



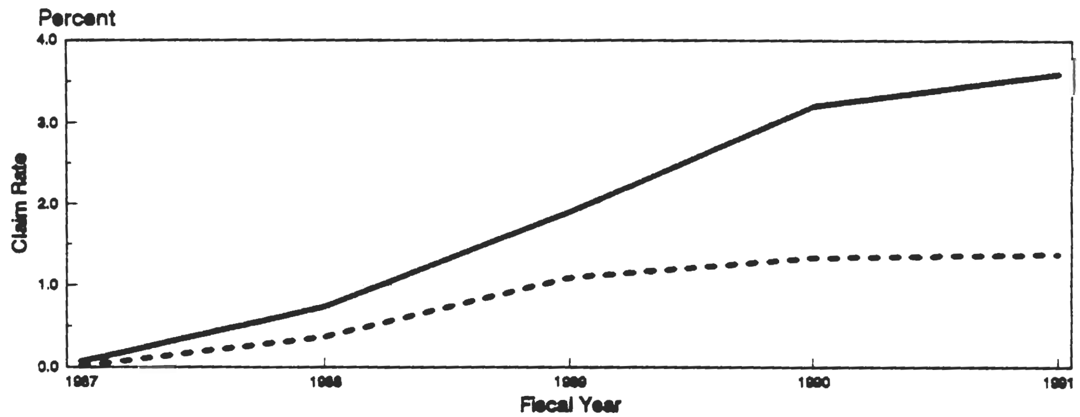
Source: Information supplied by HUD.

Figure 5, Cont.

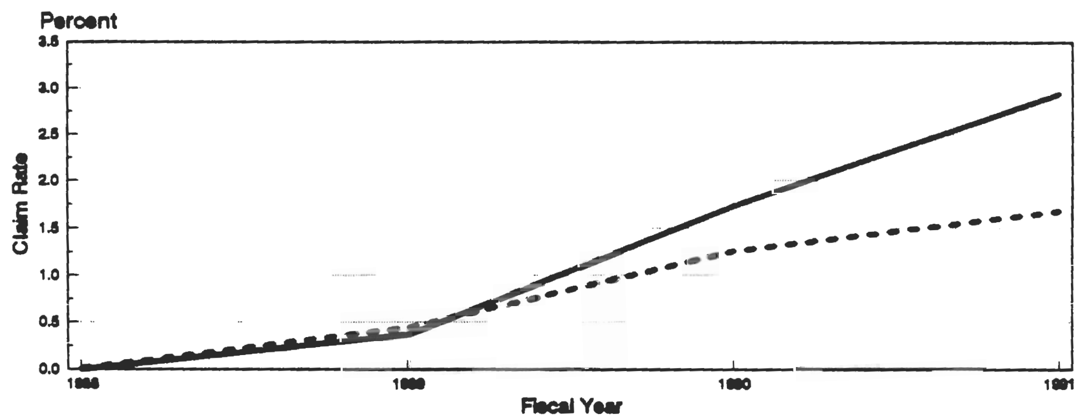
Claim Rates by Fiscal Year and Buydown Status

Buydowns Nonbuydowns

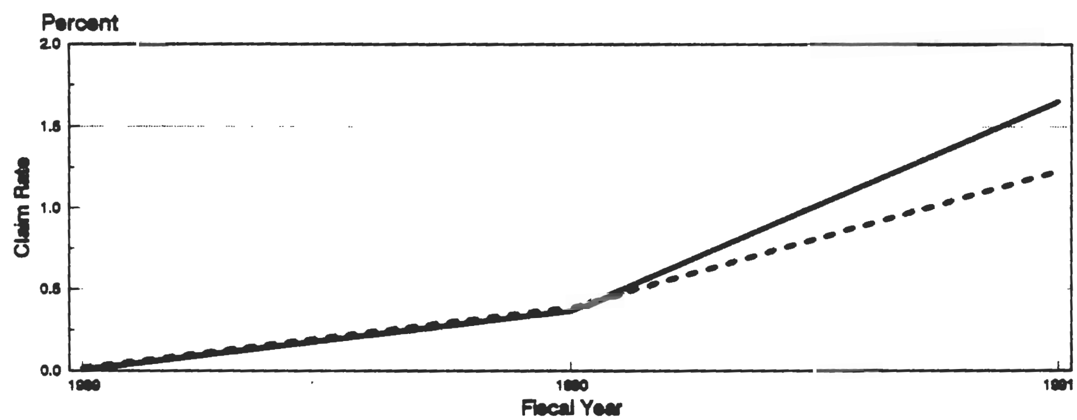
G. FY87 Endorsements



H. FY88 Endorsements



I. FY89 Endorsements



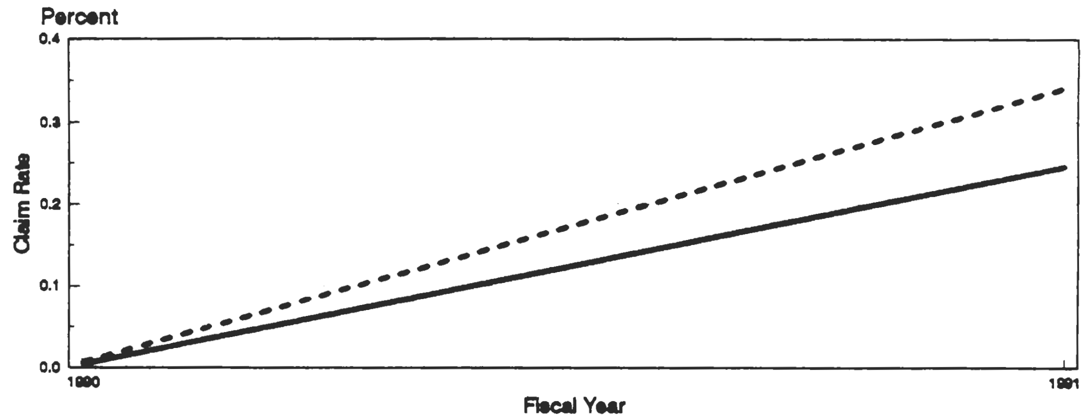
Source: Information supplied by HUD.

Figure 5, Cont.

Claim Rates by Fiscal Year and Buydown Status

Buydowns Nonbuydowns

J. FY90 Endorsements



Source: Information supplied by HUD.

IV. SPECIFICATION OF THE EXPLANATORY VARIABLES

The FHA casebinder data that was gathered for this project included details on the sales and mortgage transaction (selling price, mortgage interest rate, loan balance, etc.) and on the financial and demographic features of the borrower (income, assets, etc.). Other information, such as the nature and date of mortgage termination,[30] if any, was obtained from the automated A43 data. Utilizing such data to implement inequality (6) as the basis for a hazard model is straightforward in some respects. In particular, some variables, such as the sales price of the house S_0, are observed directly. Others, like the principal balance of the mortgage $P(t)$, can be computed precisely from knowledge of the terms of the mortgage and its age. Computation of the value of the mortgage $V(t)$, however, is a considerably more complex exercise in option value theory that should in principle account for a host of future contingencies.

Lacking the basis for making such calculations, we adopt a variant of the approach used by Foster and Van Order (1984) in which $V(t)$ is computed as the present value of remaining mortgage payments under the original contract, but at current mortgage rates and with exogenously given future prepayment behavior. The Foster and Van Order procedure is to assume that all loans prepay at 40 percent of the remaining term of the loan. In contrast, we assume probabilistic prepayment according to the empirical annual prepayment probabilities of FHA loans. More specifically, we estimate the current value V^* of the mortgage for an individual who, at time t, has already held a mortgage for duration d:

$$V^* = \sum_{j=d+1}^{j=360} \frac{f_j^* P_j + (1 - F_j^*) M_o}{(1 + i_t^*)^{j-d} (1 - F_{d+1}^*)}$$

where f_j^* and F_j^* are the empirical monthly prepayment probability and cumulative prepayment probability, respectively, at the j^{th} month for 30-year FHA loans;[31] P_j is the principal

[30] The default date recorded in the A43 file is used here as the date of termination for mortgages ending in default.

[31] The empirical prepayment probabilities for Section 203, 30-year term mortgages were calculated from data contained in the HUD-provided "Survivorship and Decrement Table as of December 31, 1989, Based on Aggregate Insurance and Termination Experience for Home Mortgages Insured since 1970."

balance in month j of the loan; M_o is the monthly payment on the individual's mortgage; and i_t^* is the market interest rate for month t. This formulation uses the conditional density function of prepayment at duration d to evaluate prepayment probabilities in each future month j over the remaining months of the loan. At each such month, the individual prepays with probability $f_j^*/(1 - F_{d+1}^*)$ and continues to survive (without prepaying) with probability $(1 - F_j^*)/(1 - F_{d+1}^*)$. Utilizing this variant assumes, of course, that the mortgagor treats his or her anticipated prepayment probabilities as identical to those of the "representative" FHA borrower.

Notice that when using FHA experience, we discount expected future mortgage payments (including probabilistic prepayments) at contemporaneous mortgage interest rates, i_t^*. The latter are obtained by taking the mortgagor's original coupon rate and adding the difference between the current mortgage rate and that which prevailed at the time that the loan began.[32] Notice that we adjust the original coupon rate, rather than use the current rate directly, to preclude the presumably unrealistic possibility that those who borrow at above-market rates would immediately prepay to take advantage of lower interest rates. We assume instead that each individual borrows at the best possible rate at the time of loan origination, and that differences in contemporaneous mortgage rates across loans reflect other aspects of the mortgage transaction that are unobservable to us.

The current value of the remaining temporary buydown balance $B(t)$ is similarly discounted[33] utilizing i_t^*, the original coupon rate plus the difference in mortgage rates from the time the

[32]The latter difference in interest rates was derived on a monthly basis from Freddie Mac's survey of conventional loans. For each month, discount points and mortgage rates were combined into a single effective interest rate under the assumption that the "typical" loan would prepay in 10 years. For each month in our observation interval, we computed the effective interest rate as the (monthly) internal rate of return z that would satisfy the equation

$$1 - d = \sum_{t=1}^{120} m/(1+z)^t + B/(1+z)^{120}.$$

In this expression d is discount points as a fraction of loan value, and m and B are, respectively, the monthly payment amount and principal balance (after 10 years) on a \$1 conventional, 30-year loan at current rates.

[33]We ignore the influence of possible prepayment behavior in valuing the buydown, but it is very unlikely that a parallel treatment of prepayments would matter materially in the construction of this variable. Virtually all buydowns in the data are exhausted within three years, and the empirical FHA prepayment profile shows a cumulative prepayment probability of less than 10 percent at the three year point.

loan began.

It is worth emphasizing that we ignore possible endogeneity of buydowns within the default analysis. It seems plausible that individuals with a propensity to default—who may even plan to default from the outset—would choose mortgage schemes that have lower initial payments. If so, buydown amounts could pick up direct, causal effects on default, as well as indirect effects via the buyer's taste for defaulting.

The deterministic function $g(t)$ in inequality (6) is intended to capture proportionate growth in house prices. Our empirical measure is the proportionate change in the local housing price index presented in Haurin, Hendershott, and Kim (1991).[34] Because the latter index may not adequately measure price changes in the narrower market at issue here, however, we also use a separate proxy for local housing demand: the number of monthly FHA loan transactions in the local field office from which the original loan arose. Use of the latter series can be faulted on the grounds that these data measure activity in only one portion of the housing market—homes selling with FHA insurance. This narrow focus may be an advantage, however, if sales activity in the FHA-insured segment of the market is representative of the local markets in which the observations are located. That is, swings in FHA activity may better proxy the behavior of the local markets relevant to the sample observations than would a more broadly based measure of market activity.

We include another proxy—the monthly, national, civilian unemployment rate—that is subject to multiple interpretations. On the one hand, it may be viewed as a business cycle proxy that captures effects on local housing markets that are not adequately captured by the number of sales transactions or the local house price index. On the other hand, it may

[34]The latter Haurin, Hendershott, and Kim (HHK) price index is annual. We imputed quarterly values by combining this series with a quarterly National Association of Realtors (NAR) series that gave the median sales prices of existing single-family homes in the appropriate metropolitan area (National Association of Realtors [1991]). Using linear regressions of each NAR series on quarterly dummies, we first removed the seasonality in the NAR data. Quarterly values for the HHK series were imputed by adjusting (multiplying) the original HHK value for the year by the ratio of the adjusted NAR series for a particular quarter to the adjusted first quarter value for the same year. The implicit assumption is that the annual HHK series is a first quarter value, an assumption that seems reasonable given the nature of its construction as described in HHK. Quarterly values were then assumed to hold for each month of the corresponding quarter.

also be interpreted as measuring likely adverse changes in mortgagors' earnings, which could have an effect on default under an ability-to-pay theory.

The function $C(t)$ in inequality (6) is supposed to measure miscellaneous costs of default (as a fraction of house value): transactions costs, damage to one's credit rating, costs of moving, etc. These costs are not observed in our data, and we instead assume that such costs are an approximately constant fraction of house value. The constant of proportionality will thus be embedded in the intercept term of the estimated hazard model. In some specifications, we attempt to allow default costs to vary across observations by introducing proxies that may capture some sources of cost variation: marital status, age, and minority status.[35] Once again, multiple interpretations are possible, particularly for minority status. If incomes for whites grow at a different rate than for nonwhites over the observation period, then this variable may pick up resulting default differentials under an ability-to-pay theory of default. Alternatively, minority status may pick up differential growth in local housing demand if there is residential segregation combined with differential growth in housing demand across these segregated areas.

Finally, in some specifications we include the payment-to-income ratio (PTY) for each mortgagor, calculated by dividing total monthly housing expenses by net effective monthly income.[36] If this estimate of the prospective initial housing expense burden were a good predictor of the actual housing expense burden, it might also predict defaults under an ability-to-pay theory. The inaccuracy of this estimate is likely to increase substantially as mortgage duration rises, however. Part of the problem is that base period predictions typically have forecast error variances that increase with distance into the future. The other part of the problem is that nominal housing expenses change when the buydown subsidy terminates, if not before, and thus a single housing expense estimate cannot measure actual expenses at all mortgage durations. The purpose in including PTY in the analysis here is

[35] These variables were occasionally missing. When they were, the missing value was replaced with a mean value computed within the same city and observation interval.

[36] Monthly housing expenses and net effective income are recorded on the mortgagor's credit application.

to see whether that estimate, made at the time of credit application, has any predictive power after controlling for the remaining factors affecting default.

Table 6 provides additional details on the independent variables included in one or more of the empirical specifications and gives the abbreviations utilized in subsequent tables. Simple summary statistics for these variables are presented in Table 7. In general, variables describing financial aspects of the transaction were obtained from the settlement statement, while variables describing the mortgagor were obtained from the mortgage application. The major exceptions, aside from those already discussed, are the information on the amount and timing of buydown payments, which was obtained from the buydown or escrow agreement; the initial mortgage amount and the mortgage interest rate, which were obtained from the mortgage note or deed of trust;[37] and the national unemployment rate, which was obtained from published sources.

[37]The number of loan transactions in the city (see LNTRANS) was obtained from the HUD automated A43 data.

TABLE 6

Definitions and Abbreviations of Variables Used in Estimation

Variable Abbreviation	Variable Definition
LNPRICE	Log of the sales price of the home ($\ln S_0$)
BRATIO	The present discounted value of the monthly buydown payments, as of the date of sale, divided by the sales price of the home($B(0)/S_0$)
DRATIO	Discount points paid by the seller divided by the sales price of the home
LOGMIN	An estimate of $\ln \min(V(t), P(t))$, log of the minimum of the current principal balance ($P(t)$) and the current value of the mortgage ($V(t)$), where the latter is computed using empirical FHA prepayment probabilities
VBSHARE	An estimate of $B(t)/\min(V(t), P(t))$, the present value of remaining buydown payments $B(t)$ divided by the minimum of the current principal balance $P(t)$ and the current value of the mortgage $V(t)$. The value of the mortgage is computed using empirical FHA prepayment probabilities.
LNHPIND	The log of the ratio of the current housing price index to that at the time that the loan began.
LNTRANS	The log of the ratio of current monthly loan transactions (in that city) to monthly transactions when the loan began.
CYCDIF	The current national civilian unemployment rate minus the national civilian unemployment rate in the month in which the loan began.
MARSTAT	Indicator variable for marital status = 1 if married, 0 otherwise
MINORITY	Indicator variable =1 if race/ethnicity is white, non-Hispanic; 0 otherwise
AGE	Age of mortgagor (in years) at the time of mortgage application
AGESQ	The square of AGE.
PTY	The ratio of total monthly housing expenses to net effective monthly income.

TABLE 7

Summary Statistics

	PHOENIX				DENVER		SAN ANTONIO			
	1982		1985/86		1982		1982		1985/86	
Variable	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
LNPRICE	10.99	.07104	11.154	.08682	11.181	.04543	10.732	.17712	10.960	.14769
BRATIO	.02183	.02240	.02142	.02169	.01984	.02015	.02204	.02235	.02103	.02209
LOGMIN	10.921	.08755	11.112	.10011	11.067	.04162	10.651	.18923	10.926	.14664
VBSHARE	.00405	.01029	.00601	.01177	.00368	.00953	.00396	.01022	.0061	.01172
LNHPIND	.14423	.06985	.04888	.01874	.036	.04710	.11678	.04911	−.05482	.04246
CYCDIF	−3.0273	1.569	−1.2133	.72772	−3.0459	1.5356	−2.4862	1.6531	−1.2284	.70175
DRATIO	.05915	.01043	.03406	.01816	.04411	.01370	.06920	.0169	.04188	.01737
LNTRANS	2.085	.69901	−.05297	.38229	1.545	.59368	1.6966	.78662	−.22711	.37926
PTY	.40453	.02064	.35471	.01814	.36882	.03173	.3488	.01285	.33979	.01991
AGE	35.474	3.3844	35.030	3.9138	35.494	3.2799	33.715	1.9904	34.47	2.6227
AGESQ	1344.6	282.0	1315.3	352.39	1309.4	228.20	1197.8	139.00	266.9	202.61
MINORITY	.92931	.06071	.87476	.11714	.93375	.05264	.84975	.12430	58541	.10898
MARSTAT	.74030	.07973	.72042	.06706	.59922	.12504	.79879	.11957	57520	.13853

Source: Summary statistics are computed at each month of mortgage duration using inverse sampling weights, then averaged (unweighted) over all durations.

V. EMPIRICAL FINDINGS

The development of the empirical model above implies constraints on various components of the coefficient vector β. In particular, according to inequality (6), the coefficients on the variables $\ln S_0$ (LNPRICE), $g(t)$ (LNHPIND), and $B(t)/\min(V(t), P(t))$ (VBSHARE) should be equal in magnitude and opposite in sign to the coefficient on $\ln \min(V(t), P(t))$ (LOGMIN).[38] However, the relationship between these coefficients and that on $B(0)/S_0$ (BRATIO) depends on the magnitude of the unknown capitalization rate γ.[39]

Imposing these constraints in the course of estimation would increase efficiency if, in fact, the constraints are valid. The validity of the constraints involved here, however, presumes the correctness of the underlying theoretical model, the completeness of the variable list, and the absence of measurement error in the variables. In the first instance, the theoretical constraints may not hold because the theory may not be a sufficiently accurate description of reality. Moreover, while a correct theoretical structure would imply the validity of constraints between a complete set of ideally measured variables, the set of variables used here is incomplete, and measurement may in some cases be far from ideal. If so, the theoretically appropriate constraints may not hold because they are invalid when applied to the set of variables as measured in practice. Indeed, given the nature of variable construction in the instant case, there seem likely to be large differences in the extent of measurement error across the different variables.

To see how these considerations apply, first note that the sales price used in constructing LNPRICE is observed directly and is likely to be very precisely measured. On the other hand, the $V(t)$ component of LOGMIN is only estimated—perhaps crudely—thus suggesting the possibility of a large measurement error component. In addition, the relationship between LNPRICE and LOGMIN depends on the validity of the underlying option

[38]Constraints on the coefficient on $\ln(1 + C(t))$ in inequality (6) are irrelevant because this variable is only proxied in the empirical work.

[39]Similarly, the coefficient on DRATIO depends on the extent to which discount points are capitalized into sales prices.

value theory, which may or may not capture the essential elements of default behavior. VBSHARE also relies on these same calculations, as well as on the correct choice of the interest rate used in computing the present value of buydown payments. Moreover, its link to LNPRICE again depends on the adequacy of the option-value framework as embodied here. The variable LNHPIND relies on the relevance of the HHK house price index; LNHPIND could be in error because the underlying series is only estimated and because the series may or may not be applicable to the section of the housing market at issue here. Measurement of BRATIO relies on the choice of the interest rate. Moreover, the theoretical link between BRATIO and the other variables depends on the unknown capitalization rate. A major purpose of this exercise is to estimate this capitalization rate; imposing a value on prior grounds would be entirely inappropriate.

Measurement error is thus likely to arise in different ways and to occur in varying degrees in the different variables. In addition, the theoretical linkages among variables may or may not be correct, and in any case these linkages are testable implications[40] of the theory that need not be imposed on prior grounds, given that the model is overidentified. Because imposing constraints when they are invalid may do more harm than good, and because imposing valid constraints will only improve efficiency, we begin by estimating the model in unconstrained form.[41] Appendix B presents additional results in which selected constraints are imposed in estimation.

Tables 8–12 present the empirical hazard models for the five samples separated according to city and time period.[42] The inequality restrictions on the values of the parameters in the baseline hazard are more easily imposed by transforming the basic parameters into a new set of coefficients that will automatically embody the necessary constraints. For this

[40] Testability assumes away the measurement error problem.

[41] Although we hope, for example, to estimate the price effect (coefficient on LNPRICE) more precisely by not linking it to potentially more error-ridden variables like LNHPIND, the reasoning is admittedly informal. Error in any variable presumably affects the quasi-likelihood estimates of all coefficients, and there is no guarantee that we do better (in any sense) by not contaminating better measured variables with constraints to poorly measured variables.

[42] Corresponding estimates for various pooled samples—within Phoenix, within San Antonio, and overall—are presented in Appendix A.

TABLE 8

Estimates of Log Logistic Hazard Model of Default
Phoenix 1982
(N = 370)

Variable	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic
INTERCEPT	14.938	47.338	.315	7.048	7.800	.903	8.944	8.623	1.037
LNPRICE	– 10.717	3.520	– 3.044	– 10.760	3.302	– 3.258	– 10.692	3.480	– 3.072
BRATIO	– 5.031	14.493	– .347	– 1.927	9.564	– .201	– 4.019	10.581	– .379
LOGMIN	10.448	3.632	2.876	10.508	3.429	3.064	10.431	3.571	2.920
VBSHARE	– 11.083	28.049	– .395	– 16.114	12.987	– 1.240	– 13.332	13.751	– .969
LNHPIND	13.986	5.375	2.602	14.056	5.532	2.540	13.949	5.388	2.588
CYCDIF	.907	.236	3.828	.938	.241	3.877	.906	.236	3.825
DRATIO	– 4.764	2.434	– 1.956	– 4.819	2.576	– 1.870	– 4.771	2.551	– 1.869
LNTRANS	– .340	.226	– 1.507	– .353	.239	– 1.473	– .338	.229	– 1.475
AGE	—	—	—	.087	.117	.745	—	—	—
AGESQ	—	—	—	.000	.001	– .629	—	—	—
MINORITY	—	—	—	– .368	.474	– .778	—	—	—
MARSTAT	—	—	—	– .162	.268	– .607	—	—	—
PTY	—	—	—	—	—	—	– .155	1.381	– .112
LOGTHETA	.295	.141	2.091	.300	.141	2.121	.294	.144	2.029
LOGPHI	– 13.810	47.685	– .289	– 7.472	5.989	– 1.247	– 7.848	7.052	– 1.112
Weighted Log Likelihood		**– 378.894**			**– 377.569**			**– 378.894**	

TABLE 9

Estimates of Log Logistic Hazard Model of Default
Phoenix 1985/86
(N = 668)

Variable	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic
INTERCEPT	7.295	4.978	1.465	7.506	4.914	1.527	7.197	4.809	1.496
LNPRICE	−18.773	3.935	−4.769	−20.596	4.491	−4.585	−18.759	4.315	−4.347
BRATIO	21.764	5.255	4.141	21.816	5.332	4.091	21.716	5.186	4.186
LOGMIN	17.793	4.098	4.341	19.862	4.660	4.261	17.775	4.480	3.966
VBSHARE	−39.240	14.827	−2.646	−40.384	14.161	−2.851	−39.204	14.397	−2.722
LNHPIND	8.874	2.777	3.195	9.137	2.803	3.259	8.824	2.704	3.263
CYCDIF	−.283	.322	−.880	−.347	.321	−1.080	−.286	.322	−.888
DRATIO	−4.821	3.343	−1.442	−4.502	3.461	−1.300	−4.866	1.419	−3.428
LNTRANS	.272	.204	1.334	.307	.200	1.533	.271	.207	1.308
AGE	—	—	—	−.147	.075	−1.956	—	—	—
AGESQ	—	—	—	.001	.000	2.092	—	—	—
MINORITY	—	—	—	−.152	.291	−.522	—	—	—
MARSTAT	—	—	—	−.359	.194	−1.849	—	—	—
PTY	—	—	—	—	—	—	.415	1.269	.327
LOGTHETA	.747	.169	4.403	.740	.157	4.709	.749	.164	4.542
LOGPHI	−.471	.652	−.722	−.484	.781	−.620	−.462	.708	−.653
Weighted Log Likelihood		−339.661			−337.467			−339.636	

TABLE 10

Estimates of Log Logistic Hazard Model of Default
Denver 1982
(N = 468)

Variable	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic
INTERCEPT	1.176	8.156	.144	1.738	9.143	.190	1.288	7.852	.164
LNPRICE	−16.232	4.198	−3.866	−16.385	5.057	−3.239	−16.191	4.040	−4.007
BRATIO	15.235	7.475	2.038	17.241	7.854	2.195	15.282	7.431	2.056
LOGMIN	16.195	4.414	3.668	16.308	5.316	3.067	16.152	4.278	3.775
VBSHARE	−43.437	18.529	−2.344	−45.443	18.494	−2.457	−43.379	18.458	−2.350
LNHPIND	− 6.945	2.512	−2.764	− 7.034	2.650	−2.654	− 6.820	2.367	−2.880
CYCDIF	.094	.138	.681	.096	.132	.733	.097	.131	.744
DRATIO	− .571	3.362	− .169	− .559	3.482	− .160	− .601	3.355	− .179
LNTRANS	.289	.207	1.396	.286	.214	1.334	.284	.203	1.398
AGE	—	—	—	− .027	.130	− .212	—	—	—
AGESQ	—	—	—	.000	.001	.309	—	—	—
MINORITY	—	—	—	− .111	.362	− .308	—	—	—
MARSTAT	—	—	—	.247	.265	.929	—	—	—
PTY	—	—	—	—	—	—	.217	1.273	.170
LOGTHETA	.365	.244	1.494	.369	.248	1.485	.358	.239	1.494
LOGPHI	− 3.150	1.231	−2.557	− 2.992	.769	−3.886	− 3.317	.502	−6.599
Weighted Log Likelihood		**−439.767**			**−438.977**			**−439.756**	

TABLE 11

Estimates of Log Logistic Hazard Model of Default
San Antonio 1982

(N = 414)

Variable	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic
INTERCEPT	13.837	39.836	.347	4.779	4.872	.980	7.680	5.038	1.524
LNPRICE	– 7.435	1.993	– 3.729	– 6.989	2.098	– 3.330	– 7.527	2.116	– 3.555
BRATIO	17.608	7.737	2.275	18.825	7.925	2.375	18.015	7.539	2.389
LOGMIN	7.175	2.057	3.487	6.801	2.167	3.138	7.204	2.189	3.289
VBSHARE	– 33.886	26.551	– 1.276	– 35.605	26.049	– 1.366	– 35.694	24.403	– 1.462
LNHPIND	– 4.464	2.721	– 1.640	– 3.968	2.737	– 1.449	– 4.517	2.796	– 1.615
CYCDIF	.337	.138	2.436	.424	.151	2.807	.340	.143	2.380
DRATIO	7.663	3.348	2.288	7.769	3.429	2.265	7.739	3.322	2.329
LNTRANS	.126	.211	.598	.113	.211	.537	.121	.212	.570
AGE	—	—	—	.154	.119	1.289	—	—	—
AGESQ	—	—	—	– .001	.001	– 1.343	—	—	—
MINORITY	—	—	—	– .703	.360	– 1.952	—	—	—
MARSTAT	—	—	—	.582	.392	1.484	—	—	—
PTY	—	—	—	—	—	—	.677	1.197	.565
LOGTHETA	.757	.187	4.043	.800	.190	4.208	.761	.196	3.876
LOGPHI	– 15.278	40.768	– .374	– 9.770	2.952	– 3.309	– 8.679	2.801	– 3.097
Weighted Log Likelihood		**– 368.145**			**– 364.084**			**– 368.058**	

TABLE 12

Estimates of Log Logistic Hazard Model of Default
San Antonio 1985/86
(N = 739)

Variable	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic
INTERCEPT	−2.243	3.227	−.695	−4.897	3.635	−1.347	−2.168	3.226	−.672
LNPRICE	−7.871	1.785	−4.409	−7.280	1.739	−4.185	−7.928	1.783	−4.446
BRATIO	9.077	5.803	1.563	9.999	5.716	1.749	8.961	5.716	1.567
LOGMIN	7.921	1.862	4.252	7.598	1.802	4.216	7.959	1.856	4.287
VBSHARE	−9.890	14.118	−.700	−10.107	13.918	−.726	−9.799	14.291	−.685
LNHPIND	1.578	2.700	.584	1.676	2.707	.619	1.586	2.706	.586
CYCDIF	.433	.385	1.125	.447	.390	1.144	.429	.388	1.105
DRATIO	.352	1.965	.179	−.064	2.034	−.031	.423	1.987	.213
LNTRANS	−.102	.224	−.455	−.098	.222	−.444	−.108	.224	−.481
AGE	—	—	—	−.015	.051	−.295	—	—	—
AGESQ	—	—	—	.000	.000	.411	—	—	—
MINORITY	—	—	—	−.421	.204	−2.058	—	—	—
MARSTAT	—	—	—	.302	.197	1.527	—	—	—
PTY	—	—	—	—	—	—	.392	1.054	.372
LOGTHETA	.961	.182	5.279	.963	.185	5.187	.961	.190	5.045
LOGPHI	−2.070	.458	−4.516	−2.098	.467	−4.487	−2.058	.473	−4.350
Weighted Log Likelihood		**−610.176**			**−607.480**			**−610.128**	

reason, the likelihood function is respecified and maximized in terms of the transformed parameters. The reported results reflect this transformation: we report $\ln \phi$ (LOGPHI) and $\ln \theta$ (LOGTHETA) rather than ϕ and θ.

Each table contains estimates from three specifications. The second specification includes all variables that are in the first specification, but also includes the demographic variables age (and its square), marital status, and minority status. The third specification drops the demographic variables but adds the estimated initial payment-to-income ratio PTY. As can be seen from these tables, the estimates that are most critical to this study—those on the variables LNPRICE, BRATIO, VBSHARE, and LOGMIN in particular—are fairly insensitive to the choice of specification. Indeed, even when using a broader array of possible specifications than those presented here, these most important estimates are generally very robust.

Consider first the variable LNPRICE. As expected, higher initial sales prices lead to lower default hazard rates, holding constant the amount borrowed (as reflected in LOGMIN) and the remaining explanatory variables. This downward impact on the conditional probability of default is generally measured with substantial precision.

The effect of a buydown on default rates[43] is, as predicted, twofold. The first impact occurs if the buydown is capitalized into the sales price of the home, but the capitalized value cannot be recaptured if the home is resold. Under these circumstances the buydown should positively affect default rates. The finding of a positive coefficient on BRATIO confirms this effect in four of the five samples. The size of this capitalization effect cannot be judged against an external standard, however, but only by comparison with the effect of selling price itself, as measured by the coefficient on LNPRICE. That is, if the full capitalized value of the buydown is reflected in the selling price of the home but cannot effectively be recaptured upon resale, this will be revealed in default behavior by a response

[43]In this section we use the terms default rate, default probability, and default hazard interchangeably to refer to the conditional probability of default at a particular time, given survival to that point in time.

to BRATIO that is equal in magnitude and opposite in sign to the effect of LNPRICE.[44] Correspondingly, a coefficient on BRATIO that is larger in magnitude but opposite in sign to the coefficient on LNPRICE implies greater than full capitalization; in parallel fashion, a BRATIO effect that is smaller in magnitude but opposite in sign implies less than full capitalization.

Measuring the implied capitalization effect as the ratio of the coefficient on BRATIO to that on LNPRICE, we find that the estimated capitalization effects are close to one for three of the samples: a value of 1.16 for Phoenix in 1985/86, 0.94 for Denver in 1982, and 1.15 for San Antonio in 1985/86. The implied capitalization rate in the San Antonio 1982 sample is 2.38, and it is of the wrong sign in the Phoenix 1982 sample. Although the point estimates suggest at least some variation in capitalization rates across samples, the implied estimates are, of course, subject to sampling error. While the correct (asymptotic) standard errors for coefficient ratios are difficult to compute, making it difficult to assess directly the statistical significance of differences in implied capitalization effects, the case of full capitalization is an especially useful benchmark against which to test. Utilizing the first specification in each sample, a test of whether the coefficient on BRATIO is identical to that on LNPRICE is never rejected at standard significance levels.[45] Hence, in none of these samples can we reject a capitalization rate of 1.[46] In their default responses, mortgagors act as if temporary buydowns are fully capitalized into the sales prices of homes.

[44] That is, the coefficient on LNPRICE is an estimate of β_0 in inequality (6) above; call this estimate $\hat{\beta}_0$. Similarly, the coefficient on BRATIO is an estimate of $\gamma\beta_0$, say $\widehat{\gamma\beta_0}$. The ratio of these two coefficient estimates is a consistent estimate of the capitalization rate γ, $\hat{\gamma} = \widehat{\gamma\beta_0}/\hat{\beta}_0$.

[45] A standard procedure to test constraints in a maximum likelihood setting is the likelihood ratio test. To our knowledge, however, the validity of such tests has not been established for the quasi-likelihood procedure utilized here. For this reason, tests were instead conducted by replacing the LNPRICE variable with LNPRICE—BRATIO, and leaving all other variables unchanged. Under the null hypothesis that LNPRICE and BRATIO have numerically equal but opposite effects on default, the coefficient on the composite variable LNPRICE—BRATIO should pick up both default effects, and the coefficient on BRATIO alone should be zero. In all cases the asymptotic normal statistic on BRATIO are insignificantly different from zero at standard significance levels. The asymptotic normal statistics are as follows: -1.04 for Phoenix 1982, 0.56 for Phoenix 1985/86, -0.10 for Denver 1982, 1.25 for San Antonio 1982, and 0.20 for San Antonio 1985/86. We note that standard significance levels may not provide an appropriate reference point for these and similar tests described later in this section. The problem is that these tests essentially demand strong evidence against the null hypothesis (that a constraint holds) in order to reject it.

[46] Appendix B presents some estimates in which we impose the constraint of a unitary capitalization rate.

The second impact of a temporary buydown occurs via the effective reduction in monthly mortgage payments during time over which the buydown escrow is disbursed to the mortgage lender. Since the remaining escrow balance reduces the payment burden but is sacrificed in the event of default, a larger remaining buydown balance should reduce default probabilities. As shown by the negative estimated effect of VBSHARE, this theoretical prediction is borne out in each of the samples and specifications.

The latter effect of a buydown on default is conceptually distinct from the first (capitalization) effect in that, as a theoretical matter, each effect can exist independently of the other. The theoretical development above implies, however, that the coefficient on VBSHARE should be the same as on LNPRICE, but the point estimates of the VBSHARE coefficient in Tables 8–12 often appear to be substantially larger than the corresponding LNPRICE coefficient. Testing for equality of effects in the first specification for each sample, we find that equality is rejected at conventional significance levels in only one case (Phoenix 1985/86).[47]

Although the estimated VBSHARE effects are generally not statistically significantly different from the estimated effect of LNPRICE, the point estimates sometimes appear to be much larger (in absolute value), and this feature merits discussion. One possibility is that VBSHARE picks up other time-covarying effects not successfully captured by the remaining variables. For example, as discussed below, the variables designed to pick up changes in the housing market often seem to perform poorly, leaving open the possibility that VBSHARE happens to be spuriously correlated with the true but unobserved changes in local housing demand. Two factors seem to mitigate against such an explanation. First, the estimated VBSHARE effect is large even in samples in which one or more of the housing market demand variables do seem to work well, *e.g.*, in the Denver 1982 sample. Second, the large

[47]These tests were performed in manner analogous those employed in testing for equality between the coefficients on LNPRICE and BRATIO: we replaced LNPRICE with the variable LNPRICE + VBSHARE, and retained all of the remaining variables. The asymptotic normal statistics on VBSHARE alone are as follows: 0.27 for Phoenix 1982, -3.05 for Phoenix 1985/86, -1.42 for Denver 1982, -0.99 for San Antonio 1982, and -0.14 for San Antonio 1985/86. Appendix B presents some estimates in which the VBSHARE response, among others, is constrained.

estimated VBSHARE effects occur in samples that are in different cities and different time periods, a feature that renders less plausible an explanation based on spurious correlation with an omitted market demand variable.

An alternative explanation is that the VBSHARE variable is picking up differences in default tendencies between buydown and nonbuydown buyers that would exist even in the absence of buydowns. Under this theory, individuals self-sort by choosing to use or not to use a buydown. Perhaps those who choose buydowns would have a more rapidly rising default profile even in the absence of their buydown activity. While the latter effect could operate even if borrowers do not choose to use buydowns with an eye towards eventual default, the additional possibility that some borrowers may use a buydown in anticipation of default cannot be completely dismissed. Such behavior may be reflected in initially lower default rates that rise as the buydown subsidy vanishes; such effects will in turn be captured in the coefficients on VBSHARE and BRATIO. It is important to recognize, however, that the effects of self-sorting and possible "gaming" of the system are legitimately part of the buydown effect, as long as the purpose of the analysis is to contrast actual behavior under buydown and nonbuydown transactions. If instead, the purpose of the analysis is to ask how a randomly selected buyer would behave if forced to accept or not to accept a buydown, then these added effects of self-sorting and "gaming" the system should be omitted.

These caveats aside, taking the point estimates of the coefficients on BRATIO and VBSHARE together permits us to examine the full effect of buydowns on default. Assuming that the amount borrowed is less than the sales price of the home, the implied default hazard for buydowns necessarily starts out below that of nonbuydowns but, in the typical case, later overtakes and exceeds the nonbuydown hazard, other things the same. To see this, note that the denominator of the VBSHARE variable is the minimum of the principal balance and the value of the mortgage, and this minimum must initially be less than the sales price of the home, which is the denominator of BRATIO. In addition the coefficient estimate on VBSHARE is larger in absolute value than the coefficient estimate on BRATIO. Thus, at least initially, the downward push on the default rate from the VBSHARE effect must dominate the upward push on the default rate from the BRATIO effect, resulting in a lower initial default rate for buydowns, other things the same. When the buydown is used up, however, VBSHARE goes to zero, and from this point forward only the positive effect of BRATIO on defaults remains.[48] Thus, at least by the time that the buydown payments are exhausted, contemporaneous default rates must be higher for buydown transactions than for otherwise identical nonbuydown transactions.

The variable LOGMIN measures the effect on default of the minimum of the principal balance and value of the remaining mortgage payments. Holding constant the price of the home and the remaining explanatory variables, we expect to find the LOGMIN variable positively related to default. Moreover, the theoretical development above, which is based on an option value approach to default behavior, predicts that the coefficients on LOGMIN and LNPRICE should be identical in magnitude but opposite in sign. The results in Tables 8–12 show a remarkable degree of similarity between the absolute values of the LNPRICE and LOGMIN coefficients. With only one exception (Phoenix 1985/86), statistical tests also fail to reject the hypothesis that these effects are equal in absolute value but opposite in sign.[49]

The coefficients on the remaining variables tend to vary more substantially across specifications. The coefficient on LNHPIND should be negative if it accurately measures the proportionate change over time in house prices in the markets at issue. The fact that the coefficient is often of the wrong (positive) sign may indicate that more narrowly defined markets in which the sample homes are embedded behave differently than the wider markets over which the price indices are measured. Alternative measures of price movements—the monthly national consumer price index or the median sales price of homes sold in the

[48]This discussion ignores the anomalous findings for the BRATIO effect in the Phoenix 1982 sample.

[49]Performing tests like those described earlier, but now replacing LNPRICE with LNPRICE—LOGMIN, we find the following asymptotic normal statistics on LOGMIN: -0.68 for Phoenix 1982, -2.26 for Phoenix 1985/86, -0.05 for Denver 1982, -0.78 for San Antonio 1982, and 0.17 for San Antonio 1985/86. Appendix B presents estimates in which the coefficient on LOGMIN is constrained to be equal in magnitude but opposite in sign to that on LNPRICE; these estimates differ little from those in Tables 8–12.

metropolitan area (as measured by the National Association of Realtors)—failed to perform any better.

The positive coefficient on the variable CYCDIF in most specifications shows that default probabilities rise as the national unemployment rate increases. One interpretation is that increases in the national unemployment rate are negatively related to mortgagors' incomes, perhaps especially so for the FHA-insured mortgages at issue here. Under an ability-to-pay theory of mortgage default, downward movements (or less rapid upward movements) in income are translated into increased default activity. An alternative interpretation—one more in keeping with the option-based default theory underlying the development here—is that increases in the national unemployment rate are associated with lower levels of housing demand, and these effects may be especially important in the local markets in which the sample homes are located. Under this interpretation, increases in the national unemployment rate are reflected in effectively reduced home sales prices, and the extent of the reduction is not fully captured in the LNHPIND variable; the result is an increased incentive to default picked up in the CYCDIF coefficient.

To the extent that discount points act like temporary buydowns, in the sense that they may be capitalized into house prices but cannot be recaptured upon resale, one expects to find DRATIO to have a positive impact on default probabilities. Only in the San Antonio 1982 sample do we see any real evidence of this effect. There the implied capitalization rate is close to unity.

As a measure of local housing demand, LNTRANS generally does not perform well, often having the unexpected (positive) sign.[50] One possibility is that the level of FHA-insured lending activity has a different meaning in different markets because of differences in the characteristics of the borrowers generally served by FHA in these markets. In markets in which FHA-insured loans are utilized more heavily at the upper end of the market, increased

[50]Replacing LNTRANS with an alternative housing demand proxy—the change in the number of housing permits issued per month—failed to offer any improvement. Quarterly time series on housing starts and on authorizations were also available over the full time period in Phoenix. These series performed no better than LNTRANS in the Phoenix samples.

FHA activity may indicate a generally stronger housing market. In markets in which FHA generally services only the low end of the market, increased FHA activity may in part reflect buyers' substitution of lower-priced for higher-priced homes.

The second of the three specifications in Tables 8–12 introduces demographic characteristics of mortgagors. The sign patterns on most of these coefficients tend to vary across the samples, and coefficients are typically measured with little precision. The only parameter that has the same sign in all specifications is minority status. The coefficient estimate on MINORITY indicates that white nonhispanics have lower default probabilities, other things the same, but in only one case (San Antonio 1985/86) is this effect significantly different from zero at conventional levels. As discussed earlier, this default response could indicate differential income growth between white nonhispanics and others, or it may indicate differences in the growth of housing demand between areas populated by white nonhispanics and areas populated by other ethnic groups.

The variables AGE and AGESQ were introduced to permit default probabilities to vary with age in a nonmonotonic fashion.[51] Sign patterns vary across the samples, and only for the Phoenix 1985/86 sample are effects measured with reasonable precision. In that specification, default probabilities are estimated to decline with age until about age 39, after which default probabilities rise with age.

The use of PTY in the third specification is an attempt to control directly for initial ability to pay. Although the effect is positive in all but one sample (Phoenix 1982), the estimated impact is quite small and is never statistically significantly different from zero. One can question whether the PTY variable, which is constructed from estimates made at the time of credit application, is an accurate measure of even the initial payment-to-income ratio. As discussed above, moreover, the initial payment-to-income ratio is unlikely to be a good predictor of subsequent payment-to-income ratios, particularly for buydown

[51] Specifications using AGE alone showed, in four of the five samples, a very weak and imprecisely measured increase in default probabilities with age. In the remaining sample the effect of AGE alone was weak and negative.

transactions.[52]

The transformed coefficients, LOGPHI and LOGTHETA, are of interest because they indicate the shape of the baseline hazard. A value of LOGTHETA greater than zero ($\theta > 1$) indicates that the baseline hazard rises with duration until it peaks out at duration $d^* = (\frac{\theta-1}{\phi})^{1/\theta}$; subsequently the baseline hazard declines with duration. Since all values of LOGTHETA exceed zero, baseline hazards are estimated to increase with duration for each sample. In many cases, however, the estimates imply that the peak in the baseline hazard would occur well beyond the 30-year mortgage duration. For the first specification, the peak in the baseline occurs at about 2.7 years for the Phoenix 1985/86 sample and at about 2.6 years for the San Antonio 1985/86 sample; in the remaining samples the peak is estimated to occur well beyond the 30-year mark.

Two points are noteworthy in interpreting the latter findings regarding the baseline hazard. First, the use of the log logistic baseline hazard requires the data to estimate two parameters that determine the shape of the hazard. As revealed by the fact that we generally find only one of these two parameters estimated with reasonable precision in a given sample, the data have difficulty in successfully distinguishing the shape of the hazard. This observation is perhaps not surprising, given that we employ a relatively small amount of data and a very limited observation period over which to estimate a two-parameter family. For the only case in which the data support fairly precise estimates of both parameters (in the first specification)—the San Antonio 1985/86 sample—we do obtain the traditional hump in the baseline hazard at a seemingly reasonable time.

The second point is that the baseline hazard shows how default probabilities (conditional on survival) vary with duration if all other control variables are held constant. In contrast, standard plots of claim rates with duration, as conventionally computed, do not attempt to hold fixed other factors that may influence default. Indeed, even if nothing else varies with

[52]For this reason, findings here should not be taken as rejection of an ability-to-pay theory of default behavior.

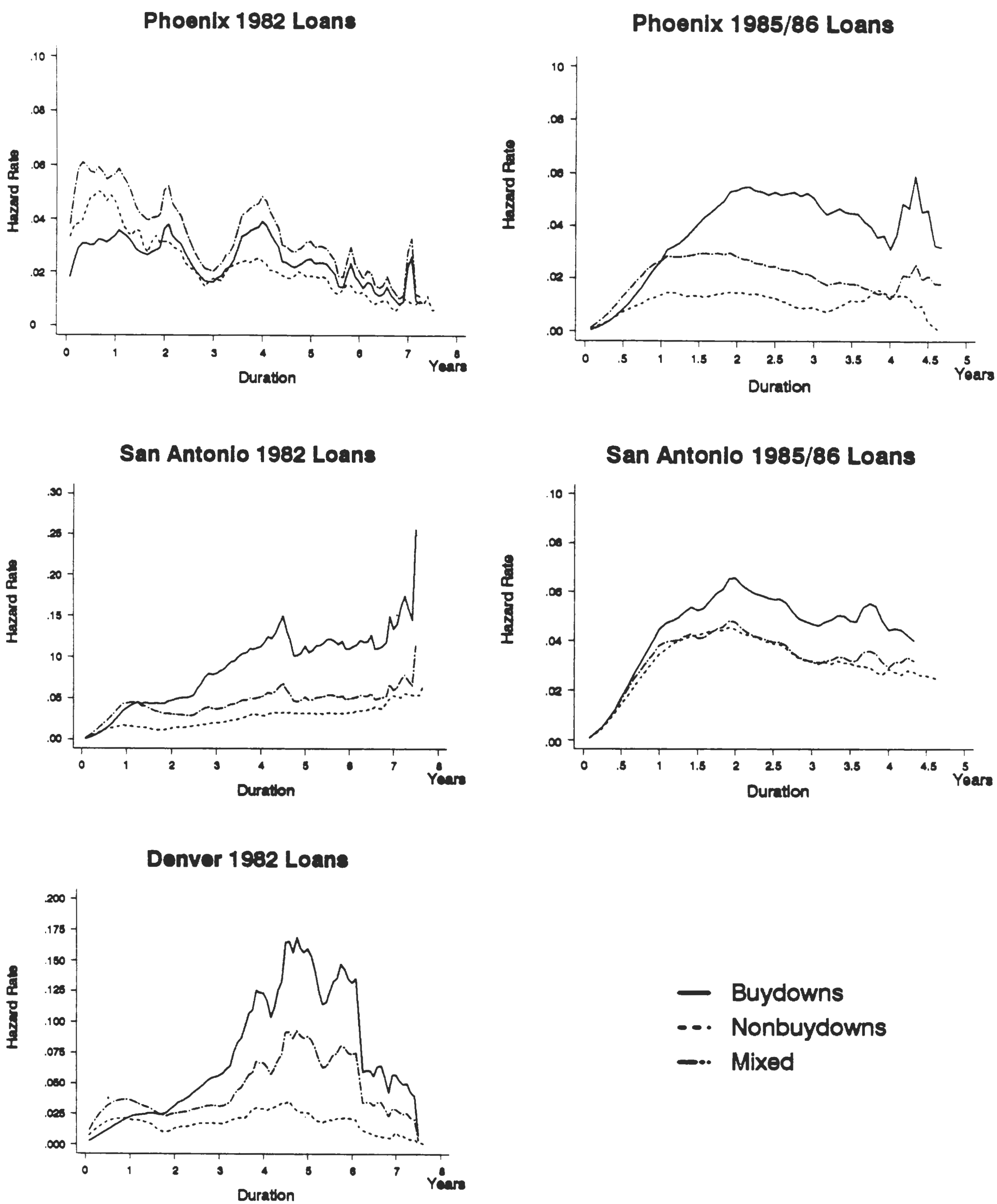
mortgage duration, principal balances of individual fixed-rate mortgages must decline with age, and this effect by itself would lead to declining default rates with duration. Naturally, other causal factors will also typically vary over time, and there can be no claim that an empirical default profile that tracks actual aggregate experience should look like a baseline hazard that attempts to hold constant various factors affecting default.

To display graphically how buydowns affect default, we plot estimated default profiles separately for buydown and nonbuydown transactions. For each month of mortgage duration in each sample, we first compute the average values of all explanatory variables by buydown status. Next, we calculate the estimated hazard rates implied by the first specification in Tables 8–12. Figure 6 graphs the resulting hazards by month for each of the samples. Note that the buydown and nonbuydown profiles may differ for two reasons. First, the variables BRATIO and VBSHARE are necessarily zero for nonbuydown transactions, and thus differences in these critical variables cause differences in the resulting hazard rates. Secondly, however, buydowns and nonbuydowns differ on average in their values of other variables, and these differences are also reflected in the estimated profiles. Since this second source of difference between buydowns and nonbuydowns is of subsidiary interest, we have produced a third default profile—denoted "Mixed"—that utilizes average values of explanatory variables among those with buydowns, except that BRATIO and VBSHARE are set to zero. Comparing this profile to the default profile for the buydown sample reveals differences due solely to the presence of BRATIO and VBSHARE, *i.e.*, due solely to the presence of buydowns and not to variation arising from other differences between average buydown and nonbuydown transactions.

With the exception of the Phoenix 1982 sample, in which BRATIO has a perverse estimated impact on default, the plots show the expected pattern of initially lower but ultimately higher default hazards under buydowns. While the lower hazard rates at short durations are difficult to discern in some samples, they show up very clearly in others, *e.g.*, in Denver 1982, especially when contrasting the buydown profile and the mixed profile.

Figure 6

Estimated Hazard Rates by Loan Duration



VI. INTERPRETATION AND RECONCILIATION OF FINDINGS

The empirical estimates of the effects of buydowns on default probabilities generally conform to theoretical predictions and seem reasonable. As expected, buydowns tend to lower default probabilities temporarily, but this effect is reversed by the time that the buydown subsidy ends. The implied rates of capitalization of buydowns into selling prices of homes are also generally reasonable when considered in isolation.

Differences do appear, however, when comparing implied capitalization rates derived here, which often exceed 100 percent, with the estimates obtained for these same samples in the first part of this study. (See Cotterman [1992].) Those estimates, obtained from hedonic regressions of sales prices on house characteristics and the present value of buydown payments, generally indicate capitalization rates of about 50 to 75 percent. Although variation in results across studies may not be at all surprising, given the very different statistical methodologies that are employed, the differences merit discussion.

The first point to note is that despite the differences in the point estimates of the capitalization rate, there is common ground. All of these estimates are subject to sampling error, and the estimates from the hedonic regressions in the first part of this study are often measured fairly imprecisely. To illustrate the importance of sampling variation, we note that capitalization estimates from the arithmetic hedonic price regressions differ significantly from 100 percent in only one case (Phoenix 1982), and in that case the estimated impact exceeds 100 percent. As noted above, the implied capitalization rates from the estimated default hazards in this part of the study are never significantly different from 100 percent. Thus, while point estimates of the capitalization rate tend to be lower in the hedonic regressions than in the default hazards, the sampling distributions have large enough variances to overlap substantially.

The differences in estimated capitalization rates that we do obtain may be partly traceable to statistical biases. As noted in Cotterman (1992), capitalization effects estimated via hedonic regressions may well be downward biased because of omitted variables. We lack sufficient controls for house quality and strength of the housing market, and it seems especially likely that buydowns are more prevalent in a slow market.

The same kind of bias seems much less likely to affect the implied capitalization rate in the estimated default hazards, for here we control directly for sales price in asking how buydowns affect default probabilities. There may well be an upward bias in the estimated impact of buydowns on default arising from another source, however. As noted above, buyers utilizing buydowns may be more likely to default even in the absence of buydowns. Such differences are properly reflected in estimated buydown impacts when the purpose is to predict differential default behavior of those who do or do not choose to use buydowns, and for such purposes there is no bias. Implied capitalization rates, however, are biased upwards: only part of the increased default activity under buydowns reflects capitalization of the buydown. The rest reflects a differential tendency to default among those using buydowns, and this behavior cannot be distinguished empirically from implicit capitalization.

In view of the possible biases in the two approaches to estimating capitalization of buydowns, choosing a middle ground is reasonable. A capitalization rate of 100 percent, for example, lies somewhat above the typical estimates achieved via hedonic price regressions and somewhat below the typical point estimates derived from the default models, and this rate is generally not rejected by either set of models. For purposes of evaluating the likelihood of default, however, one is justified in relying solely on the estimated default models presented above.

VII. SIMULATIONS

In this section we use the empirical hazard models to trace out the expected cumulative default rate for "representative" homebuyers in each sample utilizing various buydown patterns in each of four specific economic scenarios. These scenarios are chosen to highlight the special importance of house price changes for default behavior in the presence of buydowns. Each economic scenario consists of an assumption regarding changes in house prices and in unemployment rates subsequent to loan origination. As a shorthand, we label the four scenarios "Recession," "Stagnation," "Mild Expansion," and "Vigorous Expansion." Although we have in mind local house price and business cycle scenarios, we cannot distinguish between local and national cycles within the structure of this model.

Table 13 summarizes the assumptions underlying each economic scenario. The critical differences across scenarios occur in the first five years; the subsequent two years (years six and seven) show adjustment to a long run trend that is realized in the eighth year and beyond. The assumed long run trend for changes in house prices, realized by the eighth year in all simulations, is annual growth at the rate of three percent.[53] By the start of the eighth year, unemployment rates are assumed to have adjusted to a long run level of 5.2 percent. Because the 1982 unemployment rate of 9.7 percent was far higher than the 7.2 percent level experienced in 1985, and because initial conditions are reflected in the estimated hazard models for the different samples, we assume different annual changes in unemployment rates for simulations based on 1982 loan samples than for the 1985/86-based simulations.[54] Nonetheless, all assumed unemployment rate changes generate an unemployment rate of 5.2 percent for the eighth simulation year; unemployment rates are assumed to remain constant thereafter.

The four economic scenarios may be briefly described as follows. The recession scenario assumes sharp declines of six percent in house prices for each of the first two years, followed

[53] All changes in house prices are assumed to occur with monthly compounding at the stated annual rate.
[54] Constant changes in unemployment rates are assumed to occur in each month of the indicated year.

TABLE 13

Assumptions on Annual Rates of Change in Home Prices and in Percent Unemployed

	Years from Start of Mortgage							
	1	2	3	4	5	6	7	8
Recession Scenario:								
Percentage Change in House Prices	– 6.00	– 6.00	0	0	0	2.00	2.00	3.00
Change in Percent Unemployed –								
1982 Starts	0	0	– 1.25	– 1.25	– 1.00	– .50	– .50	0
1985/86 Starts	1.00	1.00	– 1.00	– 1.00	– 1.00	– .50	– .50	0
Stagnation Scenario:								
Percentage Change in House Prices	0	0	0	0	0	1.00	2.00	3.00
Change in Percent Unemployed –								
1982 Starts	– .50	– .50	– .50	– .50	– .50	– 1.00	– 1.00	0
1985/86 Starts	0	0	0	0	0	– 1.00	– 1.00	0
Mild Expansion Scenario:								
Percentage Change in House Prices	3.50	3.50	3.50	3.50	3.50	3.00	3.00	3.00
Change in Percent Unemployed –								
1982 Starts	– 1.00	– 1.00	– 1.00	– .75	– .75	0	0	0
1985/86 Starts	– .40	– .40	– .40	– .40	– .40	0	0	0
Vigorous Expansion Scenario:								
Percentage Change in House Prices	5.00	5.00	5.00	5.00	5.00	4.00	4.00	3.00
Change in Percent Unemployed –								
1982 Starts	– 1.50	– 1.50	– 1.00	– .75	– .75	.50	.50	0
1985/1986 Starts	– 1.00	– 1.00	– .50	– .50	0	.50	.50	0

by a three-year period of leveling and a subsequent upswing to attain a long-term growth rate of three percent per annum. Unemployment rates are assumed to remain high (for the 1982 loan starts) or to grow initially (for the 1985/86 starts). Starting in the third simulation year, unemployment rates are assumed to fall rapidly for three years and then more gradually for the following two years.

The stagnation scenario assumes no house price growth for five years, followed by a gradual upward trend to achieve the long run rate of three percent annual growth. Unemployment rates are assumed to fall slightly in each year when the recessionary year of 1982 is used as the starting point; unemployment rates are assumed to be initially constant when the 1985/86 loan starts are used. For both starting points, unemployment rates are assumed to decline more steeply in the sixth and seventh years.

The mild expansion scenario assumes that house price growth initially occurs at a rate of 3.5 percent per year—somewhat above the assumed long-run growth rate—and then falls to the long-run rate of three percent by the sixth year. Unemployment rates are assumed to decline substantially over the initial five years for the 1982 loan starts, with somewhat smaller unemployment rate declines characterizing the 1985/86 starts.

The vigorous expansion scenario assumes five percent growth in house prices for each of the first five years, falling to normal by the eighth year. Unemployment rates are assumed to fall substantially for both 1982 and 1985/86 starts, though more dramatically in the former. The initial decline in unemployment rates is assumed to be so large that a small upward adjustment of one-half percentage point is obtained in both the sixth and seventh simulation years.

For each of the four different economic scenarios, we investigate expected default behavior under four different buydown patterns: no buydown, a 2-1 buydown, a 3-2-1 buydown, and a 5-3-1 buydown. Because one potential reason for the existence of buydowns is to permit borrowers to obtain larger loans than they would be able to obtain in the absence of buydowns, we assume that larger buydowns are in fact used to support larger loans and the purchase of higher priced homes. Specifically, we begin with the mean loan amount for buydown purchasers in each of our samples. Since a 3-2-1 buydown appears to have been the predominant buydown pattern in our data, we assume that the mean loan amount observed in each sample corresponds to a 3-2-1 buydown, and that our "representative" buyers would meet minimum income qualification standards for a loan at that buydown rate.

When using other buydown assumptions—no buydown or 2-1 or 5-3-1 buydowns—we modify the loan amount so that our representative borrowers again meet minimum payment-to-income standards (based on mortgage payments in the first year of the loan). These computations involve calculation of loan amounts under each buydown scenario and thus require an assumption regarding mortgage rates. We use the mean coupon rate for each sample of buydowns.

To establish a house price for representative borrowers, we calculate the maximum house price that could be purchased with a minimal downpayment and the maximal loan amount, where the latter is calculated as described above. The minimum downpayment is computed as three percent of the first $25,000 of the sum of selling price and closing costs, plus five percent of the excess over $25,000. Closing costs are assumed to be 2.5 percent of the selling price. Table 14 summarizes the initial mortgage amounts, home sales prices, and other features of the representative borrowers whose default behavior is examined in the simulations.

For all scenarios we assume that interest rates start at the average coupon rate among buydown borrowers in each sample, and then remains fixed for the next 30 years. The assumption of unchanging interest rates helps minimize the importance of prepayment behavior, which lies outside scope of the estimated model. The ratio of seller-paid discount points to the sales price of the home (DRATIO) is set to the sample mean for buydown borrowers in each sample. (See Table 14.) The variable LNTRANS, whose estimated coefficient

TABLE 14

Assumed Starting Values for Simulations

	City and Time of Loan Origination				
	Phoenix 1982	Phoenix 1982/86	Denver 1982	San Antonio 1982	San Antonio 1985/86
Coupon Rate	14.2	11.4	14.1	14.9	9.9
DRATIO	0.056	0.030	0.024	0.088	0.059
Buydown Case			**Loan Amount**		
No Buydown	50566	56651	55127	41692	51624
2-1 Buydown	58155	66784	63451	47692	61809
3-2-1 Buydown	62757	73072	68503	51311	68210
5-3-1 Buydown	74128	88998	80996	60199	84645
Buydown Case			**Sales Price**		
No Buydown	51415	57665	56100	42303	52503
2-1 Buydown	59209	68071	64648	48464	62962
3-2-1 Buydown	63935	74528	69836	52181	69535
5-3-1 Buydown	75613	90884	82666	61308	86413

is often of the wrong sign, is set to zero.

To estimate the cumulative default rate at different durations, we utilize the first specification reported in Tables 8–12, with one exception. To account for the impact of changes in house prices we utilize the estimated coefficient on LNPRICE rather than the coefficient on LNHPIND. In principle, these two coefficients should be equal. Inequality in the estimates may reflect substantial error in the underlying price index series used to generate LNHPIND; in contrast, LNPRICE is likely to be measured with great accuracy, and we thus rely on its coefficient estimate.

Simulation results are summarized in Table 15, which presents cumulative default rates at the 10-year mark for each economic scenario and each buydown pattern. Because the hazard rate tends to become small after several years of loan duration, the 10-year cumu-

TABLE 15

Simulation Results: Cumulative Default Rates 10 Years After Loan Origination

	Phoenix 1982				Phoenix 1985/86				Denver 1982			
	Buydown				Buydown				Buydown			
Scenario	0	2-1	3-2-1	5-3-1	0	2-1	3-2-1	5-3-1	0	2-1	3-2-1	5-3-1
Recession	0.569	0.494	0.418	0.356	0.593	0.707	0.806	0.882	0.996	0.999	1.000	1.000
Stagnation	0.193	0.156	0.126	0.102	0.135	0.169	0.202	0.248	0.604	0.706	0.777	0.871
Mild Expansion	0.059	0.044	0.032	0.024	0.043	0.048	0.048	0.056	0.174	0.192	0.188	0.231
Vigorous Expansion	0.035	0.025	0.018	0.013	0.035	0.038	0.036	0.041	0.114	0.116	0.104	0.122

	San Antonio 1982				San Antonio 1985/86			
	Buydown				Buydown			
Scenario	0	2-1	3-2-1	5-3-1	0	2-1	3-2-1	5-3-1
Recession	0.828	0.922	0.969	0.993	0.728	0.796	0.843	0.892
Stagnation	0.545	0.680	0.791	0.893	0.327	0.382	0.432	0.493
Mild Expansion	0.179	0.241	0.302	0.397	0.106	0.126	0.143	0.167
Vigorous Expansion	0.101	0.135	0.168	0.225	0.054	0.064	0.072	0.084

lative rate is generally close to the cumulative rate over much longer durations. More complete results, covering several duration points and demonstrating more clearly the initial upswing in defaults, are presented in Table 16.

Several aspects of the simulation results are noteworthy. First, the summary in Table 15 illustrates in dramatic fashion the influence of economic conditions on default. For a given type of buydown, the cumulative default rate tends to be much higher when house prices and unemployment rates move adversely.

Second, as demonstrated in Table 16, cumulative default rates at the one year mark are typically lower for larger buydowns; the only exceptions occur for the San Antonio 1985/86 simulations. That is, with the latter exception, at very low durations the initial default-reducing effect of future buydown payments is generally large enough to offset the default-enhancing capitalization of the buydown into the sales price.

Third, Table 16 demonstrates that larger buydowns are generally associated with higher cumulative default rates by the end of the third year. While the cumulative default rates exhibited for larger buydowns are often dramatically higher, there are two kinds of exceptions to this general pattern. The first kind of exception occurs for the Phoenix 1982 simulations. Because the estimated capitalization effect of buydowns on the default hazard for the Phoenix 1982 sample is an imprecisely measured negative number (see BRATIO in Table 8), simulated cumulative default rates are consistently lower for larger buydowns.

The second kind of exception is that there are a few instances in which 3-2-1 buydowns are associated with slightly lower cumulative default rates than are 2-1 buydowns (or, in one case, even no buydown at all). This kind of exception occurs in the Denver 1982 simulations under the two expansionary scenarios, and for San Antonio 1985/86 simulations in the vigorous expansion scenario. In these exceptional cases, the default-depressing effect of future buydown payments is large enough at very low durations that it more than compensates for the continuing default-enhancing effects of buydown capitalization. That is,

TABLE 16

Simulation Results: Cumulative Default Rates

		Phoenix 1982				Phoenix 1985/86				Denver 1982			
	Yrs. After Loan	Buydown				Buydown				Buydown			
Scenario	Origination	0	2-1	3-2-1	5-3-1	0	2-1	3-2-1	5-3-1	0	2-1	3-2-1	5-3-1
Recession	1	0.105	0.076	0.054	0.039	0.040	0.034	0.025	0.022	0.171	0.138	0.090	0.074
	3	0.493	0.420	0.346	0.290	0.290	0.356	0.383	0.454	0.872	0.931	0.936	0.975
	5	0.554	0.478	0.403	0.342	0.483	0.589	0.681	0.772	0.981	0.995	0.999	1.000
	7	0.565	0.489	0.414	0.352	0.570	0.683	0.782	0.863	0.993	0.999	1.000	1.000
	10	0.569	0.494	0.418	0.356	0.593	0.707	0.806	0.882	0.996	0.999	1.000	1.000
	20	0.571	0.496	0.420	0.358	0.595	0.709	0.808	0.883	0.996	1.000	1.000	1.000
	30	0.571	0.496	0.420	0.358	0.595	0.709	0.808	0.883	0.996	1.000	1.000	1.000
Stagnation	1	0.055	0.039	0.027	0.019	0.022	0.019	0.014	0.011	0.091	0.070	0.044	0.035
	3	0.141	0.111	0.085	0.067	0.081	0.095	0.096	0.114	0.324	0.370	0.357	0.431
	5	0.180	0.145	0.116	0.093	0.112	0.137	0.157	0.192	0.491	0.580	0.633	0.741
	7	0.191	0.154	0.124	0.100	0.129	0.160	0.190	0.233	0.575	0.674	0.743	0.842
	10	0.193	0.156	0.126	0.102	0.135	0.169	0.202	0.248	0.604	0.706	0.777	0.871
	20	0.195	0.157	0.127	0.103	0.135	0.169	0.203	0.249	0.609	0.711	0.782	0.875
	30	0.195	0.157	0.127	0.103	0.135	0.169	0.203	0.249	0.609	0.711	0.782	0.875
Mild Expansion	1	0.035	0.024	0.017	0.012	0.015	0.013	0.009	0.008	0.064	0.047	0.029	0.023
	3	0.055	0.041	0.030	0.022	0.038	0.041	0.038	0.042	0.144	0.149	0.128	0.149
	5	0.058	0.043	0.032	0.024	0.042	0.046	0.046	0.053	0.166	0.180	0.172	0.209
	7	0.058	0.043	0.032	0.024	0.042	0.047	0.048	0.055	0.172	0.189	0.184	0.225
	10	0.059	0.044	0.032	0.024	0.043	0.048	0.048	0.056	0.174	0.192	0.188	0.231
	20	0.059	0.044	0.033	0.025	0.043	0.048	0.048	0.056	0.175	0.192	0.189	0.232
	30	0.059	0.044	0.033	0.025	0.043	0.048	0.048	0.056	0.175	0.192	0.189	0.232
Vigorous Expansion	1	0.026	0.018	0.013	0.009	0.014	0.012	0.009	0.007	0.055	0.040	0.025	0.019
	3	0.034	0.024	0.017	0.013	0.032	0.034	0.031	0.034	0.103	0.101	0.082	0.092
	5	0.034	0.025	0.018	0.013	0.034	0.037	0.036	0.040	0.112	0.113	0.099	0.116
	7	0.035	0.025	0.018	0.013	0.035	0.038	0.036	0.041	0.114	0.115	0.103	0.120
	10	0.035	0.025	0.018	0.013	0.035	0.038	0.036	0.041	0.114	0.116	0.104	0.122
	20	0.035	0.025	0.018	0.013	0.035	0.038	0.036	0.041	0.114	0.116	0.104	0.122
	30	0.035	0.025	0.018	0.013	0.035	0.038	0.036	0.041	0.114	0.116	0.104	0.122

TABLE 16

Simulation Results: Cumulative Default Rates (Continued)

		San Antonio 1982				San Antonio 1985/86			
	Yrs. After Loan	Buydown				Buydown			
Scenario	Origination	0	2-1	3-2-1	5-3-1	0	2-1	3-2-1	5-3-1
Recession	1	0.033	0.032	0.025	0.024	0.037	0.041	0.042	0.046
	3	0.381	0.485	0.531	0.650	0.533	0.603	0.647	0.712
	5	0.622	0.752	0.837	0.923	0.682	0.752	0.801	0.856
	7	0.742	0.859	0.927	0.976	0.715	0.784	0.831	0.882
	10	0.828	0.922	0.969	0.993	0.728	0.796	0.843	0.892
	20	0.888	0.959	0.988	0.998	0.733	0.800	0.847	0.895
	30	0.889	0.959	0.988	0.998	0.733	0.800	0.847	0.895
Stagnation	1	0.021	0.020	0.016	0.015	0.019	0.020	0.021	0.023
	3	0.162	0.212	0.235	0.307	0.160	0.189	0.209	0.243
	5	0.338	0.444	0.537	0.664	0.272	0.320	0.361	0.415
	7	0.456	0.584	0.695	0.815	0.313	0.367	0.415	0.474
	10	0.545	0.680	0.791	0.893	0.327	0.382	0.432	0.493
	20	0.623	0.757	0.860	0.940	0.331	0.387	0.437	0.499
	30	0.624	0.759	0.862	0.941	0.331	0.387	0.437	0.499
Mild Expansion	1	0.016	0.015	0.012	0.011	0.013	0.015	0.015	0.016
	3	0.076	0.097	0.103	0.134	0.073	0.086	0.094	0.110
	5	0.118	0.156	0.187	0.248	0.095	0.113	0.127	0.149
	7	0.147	0.197	0.242	0.321	0.103	0.122	0.137	0.161
	10	0.179	0.241	0.302	0.397	0.106	0.126	0.143	0.167
	20	0.212	0.286	0.361	0.468	0.107	0.128	0.144	0.169
	30	0.213	0.287	0.362	0.470	0.107	0.128	0.144	0.169
Vigorous Expansion	1	0.013	0.012	0.009	0.009	0.010	0.011	0.011	0.012
	3	0.050	0.061	0.064	0.082	0.041	0.048	0.052	0.060
	5	0.069	0.090	0.105	0.140	0.049	0.058	0.064	0.075
	7	0.083	0.110	0.133	0.178	0.052	0.062	0.069	0.081
	10	0.101	0.135	0.168	0.225	0.054	0.064	0.072	0.084
	20	0.119	0.161	0.203	0.272	0.055	0.065	0.072	0.085
	30	0.119	0.161	0.204	0.273	0.055	0.065	0.072	0.085

larger buydowns initially reduce hazard rates so that cumulative default rates at the end of the first year are lower when there are larger buydowns. Even though the hazard rate rises more steeply with duration for larger buydowns, thereby generating more default activity in later years, the increased default activity at longer durations is, in these exceptional cases, not large enough to offset the initially lower default rates for larger buydowns.

To illustrate this point, consider the Denver 1982 simulations under the vigorous expansion scenario in Table 16. Focusing on the columns for the 3-2-1 buydown and for no buydown, deduct the cumulative default rate at one year duration from the cumulative default rate at 30 years to obtain the fraction defaulting over the last 29 years of duration. These fractions are 0.059 (= 0.114 - 0.055) for the case of no buydown, and 0.079 (=0.104 - 0.025) for a 3-2-1 buydown. The fraction defaulting after the first year is thus 0.020 higher for a 3-2-1 buydown than for no buydown at all. However, the difference in the fraction defaulting during the first year is reversed in sign and is larger in magnitude: 0.055 of those with no buydowns default, versus 0.025 of those with 3-2-1 buydowns, for a difference of 0.030. Thus, putting the two pieces together, the increased default activity for 3-2-1 buydowns after the first year of duration is not enough to offset the lower default rate achieved during the first year. The result is a very slightly higher cumulative rate after 30 years for no buydown than for a 3-2-1 buydown.

It should be emphasized that the latter cases are the rare exceptions rather than the rule. They occur only in selected instances and only under the assumptions of the expansionary scenarios, where default rates tend to be much lower in any case. In all other cases (except Phoenix 1982), larger buydowns soon lead to higher cumulative default rates, and the differences tend to be especially pronounced for the recessionary and stagnation scenarios. That is, in those economic circumstances in which defaults are likely to be of greater importance overall, larger buydowns tend ultimately to result in substantially higher cumulative default rates.

VIII. CONCLUSIONS

According to the option-based theory of default discussed in this paper, a temporary buydown is expected to have a two-fold effect on default probabilities. First, to the extent that the capitalized value of a buydown is built into the sales price of a home but cannot (optimally) be recaptured upon resale, one expects to find that default rates of mortgages with buydowns are higher than for otherwise identical mortgages in which buydowns are absent. The second effect works in the opposite direction. During the period over which buydown payments help to lower the mortgagor's monthly housing expense, the value of the mortgagor's remaining payment stream is correspondingly reduced. Because the buydown escrow may retain its value to the mortgagor only if the mortgage remains active or is prepaid—*i.e.*, it can not be cashed in if default occurs—the effectively reduced value of the mortgage during the period of the buydown subsidy acts to decrease the incentive to default. These two opposing buydown forces work, on balance, to reduce the likelihood of default at low durations, but ultimately to increase the probability of default.[55]

The estimation of the proportional hazard model of default generally offers strong support for the predictions of the option-based theoretical model. As revealed by the parameter estimates themselves, as well as by estimated default profiles for buydown and nonbuydown mortgages, default probabilities are at first lower for buydowns but eventually rise above the corresponding default probabilities for nonbuydowns.

A comparison of the estimated effects of buydowns and of initial sales prices on default yields an implied capitalization rate. Point estimates of the implied capitalization rate vary, but they are often around 100 percent and are never significantly different from 100 percent. Estimated capitalization rates presented here tend to be higher than those obtained via hedonic regressions on the same samples, however. These differences may reflect systematic differences in intrinsic default propensities between borrowers who use buydowns and those

[55]Strictly speaking, this statement assumes that there is no more than full capitalization of the buydown, although these results can, but do not necessarily, hold if there is more than full capitalization.

who do not. Although such systematic differences may result in an overstatement of the buydown capitalization rate, they do not affect the usefulness of the estimated default models as representations of the behavior of mortgagors who use or do not use buydowns.

Simulations of the estimated models for different buydown patterns and under varying economic conditions illustrate the importance of both factors. Poor economic conditions, as reflected in declines in house prices coupled with high unemployment rates, produce much higher default rates than good economic conditions. Although larger buydowns tend to lower simulated default rates initially, cumulative default rates are generally higher by the third year. The effects of larger buydowns in generating ultimately higher cumulative default rates tend to be especially dramatic under poor economic conditions.

By showing conceptually how temporary buydowns affect default behavior and by estimating and simulating the magnitude of effects, the default models presented in this paper provide a useful guide to policy makers concerned with default behavior under temporary buydowns.

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APPENDIX A
ESTIMATES FROM POOLED SAMPLES

Tables 17–19 present estimated hazard models of default based on samples that are pooled across time periods (Tables 17 and 18) or across cities and time periods (Table 19). The same specifications used for the separate samples are utilized here as well, and the results are generally similar to those discussed above.

In a standard maximum likelihood setting it is possible to test for the appropriateness of pooling through the use of a likelihood ratio test. That is, the hypothesis to be tested is that the corresponding parameters in the two (or more) samples are the same; if so, the samples may be aggregated. To our knowledge, however, the distribution of the likelihood ratio test in this quasi-likelihood context has not yet been established, and thus we have performed no tests. Indeed, the algebra implies that, under the weighting scheme used here, the weighted log likelihood function for a pooled sample (*i.e.*, with the constraints imposed) may be larger than the sum of the weighted log likelihoods for the unpooled samples. To see this, note that the quasi-likelihood procedure maximizes the weighted log likelihood

$$\lambda = \sum_{k=1}^{K} w_k l_k$$

where w_k is the weight attached to observation k, and l_k is the (unweighted) log likelihood for observation k. The weight w_k is here defined according to cell membership: the ratio of the proportion of the population in a cell to the proportion of the sample in the same cell. Cells are, in turn, defined according to default status, new/old status, city, and observation period. This approach seems conservative because it permits each of the characteristics that defines a cell to be systematically related to the outcome (default or nondefault). The problem that arises, however, is that when one pools across samples, the relevant population and sample proportions change, and thus the weight attached to each observation changes as well. This reweighting can (and does) result in weighted log likelihoods that are larger

TABLE 17

Estimates of Log Logistic Hazard Model of Default
Phoenix
(N = 1,038)

Variable	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic
INTERCEPT	10.288	4.239	2.426	10.711	4.416	2.425	10.171	3.578	2.842
LNPRICE	−15.770	2.870	−5.494	−17.032	3.609	−4.719	−15.766	3.052	−5.165
BRATIO	23.224	5.070	4.580	23.332	5.200	4.486	23.187	5.033	4.606
LOGMIN	14.630	2.961	4.939	16.080	3.719	4.323	14.621	3.144	4.649
VBSHARE	−44.851	13.464	−3.331	−45.840	14.427	−3.177	−44.747	13.727	−3.259
LNHPIND	7.246	2.480	2.921	7.463	2.661	2.803	7.199	2.454	2.933
CYCDIF	.251	.138	1.815	.234	.125	1.866	.250	.125	1.999
DRATIO	−3.952	2.816	−1.403	−3.688	3.017	−1.222	−4.003	2.978	−1.344
LNTRANS	.138	.115	1.199	.152	.116	1.307	.134	.115	1.170
AGE	—	—	—	−.121	.081	−1.488	—	—	—
AGESQ	—	—	—	.001	.000	1.617	—	—	—
MINORITY	—	—	—	−.137	.280	−.488	—	—	—
MARSTAT	—	—	—	−.324	.201	−1.613	—	—	—
PTY	—	—	—	—	—	—	.492	1.116	.440
LOGTHETA	.671	.122	5.485	.670	.123	5.445	.673	.115	5.810
LOGPHI	−1.514	.497	−3.046	−1.581	.385	−4.098	−1.508	.484	−3.112
Weighted Log Likelihood		**−593.755**			**−590.789**			**−593.694**	

TABLE 18

Estimates of Log Logistic Hazard Model of Default
San Antonio
(N = 1,153)

Variable	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic
INTERCEPT	– 1.989	2.604	– .763	– 4.047	2.948	– 1.372	– 1.908	2.600	– .733
LNPRICE	– 7.694	1.502	– 5.121	– 7.042	1.441	– 4.884	– 7.747	1.435	– 5.399
BRATIO	10.039	4.206	2.386	11.391	4.988	2.283	10.131	5.096	1.988
LOGMIN	7.730	1.547	4.995	7.282	1.484	4.905	7.762	1.477	5.253
VBSHARE	– 11.294	9.058	– 1.246	– 12.136	12.654	– .959	– 11.777	12.622	– .933
LNHPIND	1.191	1.581	.753	1.821	1.779	1.023	1.162	1.585	.733
CYCDIF	.220	.111	1.977	.250	.128	1.948	.215	.115	1.855
DRATIO	1.267	1.810	.700	.942	1.717	.548	1.336	1.783	.749
LNTRANS	.075	.129	.580	.077	.142	.541	.072	.133	.542
AGE	—	—	—	– .004	.051	– .092	—	—	—
AGESQ	—	—	—	.000	.000	.199	—	—	—
MINORITY	—	—	—	– .406	.186	– 2.182	—	—	—
MARSTAT	—	—	—	.335	.181	1.845	—	—	—
PTY	—	—	—	—	—	—	.340	.946	.359
LOGTHETA	.814	.164	4.955	.809	.181	4.448	.816	.177	4.600
LOGPHI	– 2.302	.499	– 4.611	– 2.461	.940	2.616	– 2.260	.748	– 3.020
Weighted Log Likelihood		**– 829.696**			**– 825.995**			**– 829.644**	

TABLE 19

Estimates of Log Logistic Hazard Model of Default
Full Sample
(N = 2,659)

Variable	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic
INTERCEPT	4.660	2.060	2.261	4.299	2.245	1.914	4.732	2.037	2.323
LNPRICE	−11.844	1.211	−9.773	−11.899	1.161	−10.248	−11.894	1.250	−9.514
BRATIO	21.679	3.816	5.680	21.972	3.890	5.647	21.626	3.859	5.603
LOGMIN	11.233	1.241	9.048	11.446	1.187	9.639	11.255	1.272	8.845
VBSHARE	−36.685	9.604	−3.819	−37.114	9.525	−3.896	−36.616	9.259	−3.954
LNHPIND	.029	1.016	.029	.449	1.075	.418	−.013	1.003	−.013
CYCDIF	.067	.073	.914	.063	.075	.843	.066	.070	.942
DRATIO	−.494	1.522	−.324	−.324	1.476	−.219	−.515	1.496	−.344
LNTRANS	.321	.069	4.646	.326	.070	4.641	.315	.068	4.611
AGE	—	—	7.449	−.063	.044	−1.445	—	—	—
AGESQ	—	—	−5.109	.000	.000	1.650	—	—	—
MINORITY	—	—	—	−.265	.176	−1.501	—	—	—
MARSTAT	—	—	—	−.103	.142	−.727	—	—	—
PTY	—	—	—	—	—	—	.666	.763	.873
LOGTHETA	.688	.092	—	.682	.092	7.408	.689	.092	7.420
LOGPHI	−1.754	.343	—	−1.784	.330	−5.399	−1.744	.340	−5.128
Weighted Log Likelihood		**−1668.826**			**−1665.190**			**−1668.469**	

for pooled samples than for the sum of the individual component samples.[56]

[56]It would be possible to renormalize the weights and compare the reweighted log likelihood for the pooled sample to an appropriate linear combination of the reweighted log likelihoods for the individual samples. Alternatively, one could hold weights fixed upon pooling, thus assuming that there are as many relevant "populations" as there are sample pools.

APPENDIX B

CONSTRAINED ESTIMATES

Tables 20–24 present the estimated hazard models that result from imposing some of the constraints discussed in the text. The first set of estimates in each table imposes the constraint that LNPRICE and LOGMIN have effects that are equal in magnitude but opposite in sign, as implied by the underlying option value framework. The variable LPR-LMN, representing the difference between LNPRICE and LOGMIN, now replaces the two individual variables LNPRICE and LOGMIN, thus imposing the desired constraint. Comparing these results with the corresponding estimates in Tables 8–12, we note that the estimated effect of the newly combined variable, LPR-LMN, is generally close to the original coefficient estimate on LNPRICE, and the remaining coefficient estimates typically change little.

The second set of estimates in each table again imposes the constraint that LNPRICE and LOGMIN have effects that are equal in magnitude but opposite in sign, but in addition the coefficient on BRATIO is now assumed to be the same as that on LOGMIN. Thus in addition to the assumptions underlying the first set of estimates in each table, we now impose the additional assumption that there is full capitalization of the buydown. The variable LP-LM-B, representing LNPRICE minus LOGMIN minus BRATIO, now replaces LNPRICE, LOGMIN, and BRATIO, thus imposing the desired constraints. Note that the coefficient estimate on the combined effect is generally quite close to the estimate of LPR-LMN obtained in the first set of estimates of Tables 20–24. Aside from the rather substantial changes in the estimated effect of VBSHARE for the Phoenix 1982 and San Antonio 1982 samples, most changes in estimated effects are modest when the new constraint is imposed.

The third set of estimates in each table starts with the constraints present in the second set (LNPRICE and LOGMIN have effects that are equal in magnitude but opposite in sign, and the coefficient on BRATIO is the same as that on LOGMIN). Now, however, we impose the additional constraint that the coefficient on VBSHARE is the same as that

TABLE 20

Constrained Estimates of Log Logistic Hazard Model of Default
Phoenix 1982
(N = 370)

Variable	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic
INTERCEPT	12.396	58.672	.211	12.360	52.474	.236	11.871	36.178	.329
BRATIO	− 6.110	14.182	− .431						
VBSHARE	− 11.156	28.223	− .395	− 36.227	22.018	− 1.645			
LNHPIND	14.292	5.492	2.602	13.987	5.456	2.563	13.488	4.898	2.754
CYCDIF	.928	.240	3.862	.932	.237	3.925	.912	.221	4.124
DRATIO	− 4.969	2.410	− 2.062	− 5.158	2.458	− 2.099	− 5.422	2.326	− 2.331
LNTRANS	− .339	.217	− 1.564	− .354	.226	− 1.569	− .354	.207	− 1.706
LPR-LMN	− 11.378	3.530	− 3.223						
LP-LM-B			− 10.287	2.831	− 3.634				
LP-L-B+V					− 9.885	2.806	− 3.523		
LOGTHETA	.305	.144	2.123	.302	.139	2.174	.327	.133	2.454
LPGPHI	− 14.141	58.514	− .242	− 14.119	52.552	− .269	− 13.706	36.025	− .380
Weighted Log Likelihood		**− 347.782**			**− 348.349**			**− 349.178**	

TABLE 21

Constrained Estimates of Log Logistic Hazard Model of Default
Phoeniz 1985/86
(N = 668)

Variable	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic
INTERCEPT	−3.540	.687	−5.159	−3.557	.704	−5.052	−3.707	.677	−5.478
BRATIO	19.799	5.089	3.890						
VBSHARE	−37.828	14.341	−2.638	−41.981	12.057	−3.482			
LNHPIND	8.570	2.768	3.096	8.551	2.706	3.160	8.743	2.681	3.261
CYCDIF	−.377	.342	−1.102	−.341	.322	−1.059	−.428	.319	−1.341
DRATIO	−5.284	3.129	−1.689	−5.157	3.279	−1.573	−4.861	3.053	−1.592
LNTRANS	.317	.203	1.562	.304	.197	1.544	.288	.197	1.460
LPR–LMN	−23.167	4.590	−5.047						
LP–LM–B				−21.571	2.928	−7.368			
LP–L–B+V							−19.541	2.899	−6.741
LOGTHETA	.737	.149	4.947	.744	.170	4.363	.727	.164	4.437
LPGPHI	−.507	.755	−.672	−.461	.741	−.623	−.496	.756	−.656
Weighted Log Likelihood		−344.319			−344.371			−345.040	

TABLE 22

Constrained Estimates of Log Logistic Hazard Model of Default
Denver 1982
(N = 468)

Variable	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic
INTERCEPT	−.777	1.336	.582	.736	1.406	.523	.519	1.398	.371
BRATIO	15.196	7.302	2.081						
VBSHARE	−43.445	17.899	−2.428	−44.766	16.311	−2.744			
LNHPIND	−6.954	2.595	−2.680	−6.999	2.651	−2.640	−6.623	2.411	−2.747
CYCDIF	.094	.131	.721	.095	.134	.711	.046	.128	.360
DRATIO	−.552	3.303	−.167	−.470	3.306	−.142	.512	3.196	.160
LNTRANS	.290	.199	1.457	.286	.205	1.394	.317	.191	1.662
LPR–LMN	−16.274	3.874	−4.201						
LP–LM–B				−16.018	3.552	−4.509			
LP–L–B+V							−15.429	3.413	−4.520
LOGTHETA	.366	.236	1.551	.364	.238	1.529	.350	.245	1.430
LPGPHI	−3.155	1.203	−2.622	−3.125	1.260	−2.479	−3.228	1.309	−2.466
Weighted Log Likelihood		**−431.145**			**−431.153**			**−432.426**	

TABLE 23

Constrained Estimates of Log Logistic Hazard Model of Default
San Antonio 1982
(N = 414)

Variable	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic
INTERCEPT	11.351	28.433	.399	11.900	53.807	.221	11.740	52.007	.226
BRATIO	16.429	7.443	2.207						
VBSHARE	− 33.794	26.582	− 1.271	− 21.371	23.256	− .919			
LNHPIND	− 4.580	2.828	− 1.620	− 4.205	2.731	− 1.540	− 3.873	2.287	− 1.694
CYCDIF	.358	.138	2.591	.374	.136	2.762	.357	.131	2.733
DRATIO	6.640	2.969	2.236	7.459	2.672	2.792	7.310	2.753	2.655
LNTRANS	.136	.198	.687	.134	.197	.682	.139	.193	.722
LPR–LMN	− 7.617	1.938	− 3.931						
LP–LM–B				− 8.542	1.891	− 4.516			
LP–L–B+V							− 8.378	1.809	− 4.631
LOGTHETA	.780	.192	4.065	.810	.180	4.505	.808	.166	4.869
LPGPHI	− 15.466	28.370	− .545	− 16.066	54.034	− .297	− 16.008	52.132	− .307
Weighted Log Likelihood		**− 368.854**			**− 368.112**			**− 368.399**	

TABLE 24

Constrained Estimates of Log Logistic Hazard Model of Default
San Antonio 1985/86
(N = 739)

Variable	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic	Coefficient Estimate	Asymptotic Standard Error	Asymptotic Normal Statistic
INTERCEPT	−1.702	.525	−3.241	−1.692	.529	−3.197	−1.692	.530	−3.196
BRATIO	9.319	5.648	1.650						
VBSHARE	−9.941	14.046	−.708	−7.213	10.624	−.679			
LNHPIND	1.601	2.688	.596	1.538	2.692	.571	1.548	2.704	.572
CYCDIF	.436	.390	1.118	.439	.389	1.128	.439	.388	1.130
DRATIO	.395	1.952	.203	.515	1.926	.268	.554	1.860	.298
LNTRANS	−.104	.227	−.457	−.109	.222	−.492	−.110	.223	−.492
LPR–LMN	−7.787	1.803	−4.319						
LP–LM–B				−7.941	1.691	−4.695			
LP–L–B+V							−7.955	1.718	−4.631
LOGTHETA	.962	.185	5.186	.968	.183	5.275	.966	.183	5.283
LPGPHI	−2.068	.457	−4.525	−2.086	.450	−4.634	−2.080	.453	−4.592
Weighted Log Likelihood		**−521.527**			**−521.544**			**−521.545**	

on LNPRICE. Thus in addition to the assumptions underlying the second set of estimates in each table, we now impose the additional assumption that the option value framework applies to the buydown payment stream as well. The variable LP-L-B+V, representing LNPRICE minus LOGMIN minus BRATIO plus VBSHARE, now replaces the individual variables LNPRICE, LOGMIN, BRATIO, and VBSHARE, thus imposing the desired constraints. Note that the coefficient estimate on the combined effect is generally quite close to the estimate of LP-LM-B obtained in the second set of estimates of Tables 20–24. The additional constraint appears to cause few changes in other estimated effects.