

# Mortgage Lending to Minorities: Where's The Bias?

Theodore E. Day

and

Stan J. Liebowitz\*

School of Management  
University of Texas at Dallas  
Richardson, Texas 75083

Economic Inquiry, January 1998, pp.1-27.

## ABSTRACT

This paper examines mortgage lending and concludes that studies based on data created by the Boston Fed should be reevaluated. A detailed examination of these data indicates that irregularities in these data, when combined with the most commonly used research methodology, appear to have biased previous research toward a finding of discrimination against minority applicants. When the most severe data irregularities are eliminated, evidence to support a hypothesis of discrimination disappears. The currently fashionable "flexible" underwriting standards of mortgage lenders may have the unintended consequences of increasing defaults for the 'beneficiaries' of these policies.

## **I. INTRODUCTION**

Anyone who has seen "It's a Wonderful Life" understands the emotional association of home ownership and the American Dream. In contrast to the flexible and good hearted George Bailey, whose bank is willing to look at a person's character when assessing credit worthiness, Mr. Potter, the movie's miserly and larcenous commercial banker, is unwilling to grant mortgages to worthy but poor applicants from the wrong side of town. This view that bankers are inflexible, insensitive, and inhospitable to certain groups of customers in their financing of home mortgages is not just a Hollywood creation, however. Similar stories have been told in many newspapers across the country, particularly since the government started to report data collected under the Home Mortgage Disclosure Act (HMDA) in 1990.<sup>1</sup>

The HMDA data allow a comparison of mortgage denial rates by race. These comparisons inevitably reveal that minorities (defined as Blacks and Hispanics) are denied mortgages far more frequently than are white applicants.<sup>2</sup> This has again led to the specter of mortgages being denied to worthy applicants, but this time the bankers are not fictional. Even when mortgage lenders are not accused of consciously practicing racial discrimination, they are often accused of "hidden" or "unconscious" discrimination.<sup>3</sup>

Unfortunately, the HMDA data contain little information that might help control for the economic characteristics of mortgage applicants, making it extremely difficult to conduct meaningful analyses.<sup>4</sup> This has not proven to be a deterrent, however, to numerous news and community organizations that

---

\*We would like to thank the editors of *Economic Inquiry* for their guidance, although all errors are our responsibility.

Liebowitz: Professor of Economics, School of Management, University of Texas at Dallas, Richardson Texas, phone: (972) 883-2807, fax: (972) 883-2818, liebowit@utdallas.edu. Day: Associate Professor of Finance, School of Management, University of Texas at Dallas, Richardson Texas, phone: (972) 883-2743, tday@utdallas.edu. JEL: J7, G28.

1 Congress, in 1989, amended the Home Mortgage Disclosure Act (HMDA), requiring banks to report certain details for every mortgage loan application that they received, including the loan decision, the income, the race, and sex of the applicant. Numerous analyses of these data have indicated that loan applications from members of certain minority groups are rejected far more frequently than are loan applications from whites, leading some to conclude that mortgage lenders are biased against these groups.

2 As an example see the *Wall Street Journal* for February 13, 1996 for a set of articles and analyses of HMDA data.

3 For example, a publication from the Federal Reserve Bank of Boston (1993) claims "Overt discrimination in mortgage lending is rarely seen today. Discrimination is more likely to be subtle, reflected in the failure to market loan products to potential minority customers and the failure of lenders to hire and promote staff from racial and ethnic minority groups. Unintentional discrimination may be observed when a lender's underwriting policies contain arbitrary or outdated criteria that effectively disqualify many urban or lower-income minority applicants."

4 This is not to say that controlled analyses using HMDA data are impossible. For example, Leong's dissertation examined mortgage dispositions for matched samples of white and minority owned banks before concluding that there was no evidence of discrimination by white-owned banks.

have used the data for their analyses.<sup>5</sup> The yearly comparisons of mortgage rejection rates using the HMDA data are generally very superficial, with little if any attempt to control for characteristics of loan applicants that should be relevant for mortgage dispositions. Examination of average rejection rates for demographic groups of loan applicants, for example, cannot provide a basis for reaching conclusions regarding discriminatory practices, since different groups can and do have very different economic characteristics such as income, wealth, credit histories, and so forth. In such cases, differential rejection rates might represent a perfectly rational and nondiscriminatory response by lenders to the differential risk and credit capacity evidenced by borrowers.

This unsatisfactory state of affairs was apparently altered when the Federal Reserve Bank of Boston conducted a survey of banks in the Boston vicinity in an attempt to augment the HMDA data with additional information relevant to mortgage lending decisions. The stated purpose of creating this new data set was specifically to allow serious researchers to control for various economic characteristics not available in the original HMDA data. We shall refer to this augmented data set as the "Fed-extended" HMDA data throughout the paper.

Based on their analysis of this data set, Munnell, Tootell, Browne and McEneaney (1996, referred to as MTBM hereafter) concluded that race was a significant factor in explaining the tendency for minority applications to be rejected more frequently than white applicants. A 1992 report by the same authors (MBMT) that was a precursor to the 1996 publication received a great deal of publicity, and has had a major impact on policy.

As a result, banks have become the focus of increasing regulatory oversight. Several mergers between banks have been jeopardized because of putative impropriety in their fair-lending activities.<sup>6</sup> Additionally, some banks have failed soundness evaluations based on their minority lending records.<sup>7</sup> The recent adoption of "flexible" underwriting standards, permitting bankers to grant loans to minority customers who would have failed to receive a mortgage under the old standards, can be viewed as a response to this negative publicity. This may be, at least in part, responsible for recent increases in defaults.<sup>8</sup> Government agencies are apparently encouraging a weakening of lending standards through

---

5 See for example Young [1997].

6 A merger proposed by Shawmut bank was disallowed by the Fed because of its mortgage lending record to minorities. See Bacon (1993).

7 According to Thomas (1992) 20% of banks in 1992 failed their soundness evaluations for this reason.

8 See Hirsch [1995] or Blumenthal [1996] who report increasing rates of defaults in the last few years, particularly on loans with small downpayments. Our conversations with underwriters indicates that defaults on loans with flexible underwriting standards are running at least 50% above the default rate of the weakest category of mortgages, those with 5% down. Since the flexible underwriting standards have smaller downpayments, and often do not have mortgage insurance, any default is more likely to result in a financial loss to the bank than would be the case for defaults on loans based on traditional underwriting guidelines.

the quid pro quo of more favorable decisions on bank mergers for banks with aggressive lending policies to minorities.<sup>9</sup>

In this paper we reexamine the issue of mortgage discrimination using the HMDA data and the Boston Fed extensions. We have discovered that the Boston Fed extensions to the data are plagued with inconsistencies, making highly suspect any conclusions based on analyses using this data set. These inconsistencies fall into two categories: (1) variables contained in the Fed-extended data that are internally inconsistent with one another; (2) inconsistencies between the public HMDA data and the HMDA data found in the Fed-extended sample. Additionally, we were granted access to a second data set that listed some inconsistencies between the information in the actual loan applications and the variables in the data set.

The paper proceeds as follows. First, we briefly describe the mortgage lending decision. Then we examine the likely impact of data errors on measured discrimination and demonstrate that measurement errors are not likely to bias the measure of discrimination toward zero. Next we discuss the data errors. Finally, we attempt to benchmark the impact of the data errors on attempts to measure discrimination in mortgage lending. We conclude that there is no evidence in these data to support a conclusion of discrimination against minority applicants although we caution that our best efforts can not remove all data problems and the attendant biases.

## ***II. THE MORTGAGE LENDING DECISION AND RACIAL DISCRIMINATION***

Mortgage lending decisions are primarily financial in nature, or at least are supposed to be. As a business decision, mortgage applications are more likely to be approved when a loan applicant seems likely to be able to repay the loan, or when, if default should occur, the collateral underlying the loan is sufficient to protect the lender from loss. Many loans are eventually sold in the secondary market, and many mortgage lenders have no intention of keeping the loans they make. In the Boston MSA in 1990, approximately half of the conventional loans (8322 of 17006) were sold in the secondary market within two years, according to the HMDA data. Purchasers of mortgages in secondary markets have concerns similar to those of the bankers originating the mortgage and have detailed guidelines under which these loans may be purchased.

---

<sup>9</sup> Wilke (1996) reports that some bankers offered below market rates on zero downpayment loans in minority areas. This behavior by banks was attributed in part to their hope to win regulatory approval for proposed mergers with other banks.

Mortgage lenders use several financial guidelines when assessing the quality of a loan, such as the ratio of monthly mortgage payments to income (expense/income ratio), the size of the loan relative to the value of the property (loan-to-value ratio), and the credit history of the applicant.<sup>10</sup> The expense to income ratio measures the likelihood of default based on the applicant's ability to meet the mortgage payments. The loan-to-value ratio is a proxy for the size of the loss that might occur in the event of default. Prior credit history should indicate whether the applicant is likely to overestimate his ability to meet future mortgage payments.

Rational mortgage lenders in competitive markets should approve any loan that has an expectation of earning a positive return. Although racial discrimination in commercial transactions might sometimes be a rational financial response to third party effects, the existence of financial gains from racial discrimination seems far less likely for mortgage lending. For example, in housing markets, real estate agents may discriminate against minorities because they are afraid of alienating potential white customers who might prefer not to have minorities in their neighborhoods. Similarly, the owners of retail establishments might discriminate against minority customers because their white customers prefer not to associate with minorities. Or white managers might discriminate against minority workers because their white workers prefer not to have minority coworkers. In each of these examples, the discriminator suffers a specific economic harm by engaging in discrimination: lost real estate commissions, lost sales, or lower productivity. This direct loss, however, might be outweighed by the indirect gain brought about by avoiding the alienation of a large customer base or work force. Thus economic self-interest and competition can not necessarily be counted on to keep discrimination at bay in a world where third parties are bigoted.<sup>11</sup>

For mortgage lenders, however, there is little concern with third party effects. Mortgage lenders making loans to minority applicants are not likely to suffer negative consequences from other customers for the simple reason that bigoted homeowners objecting to new minority neighbors have more direct objects of scorn -- the seller, or the real estate agent. Further, the source of the loan is generally unknown to the neighbors. Thus, economic self-interest punishes any act of bigotry in the

---

10 Mortgage lenders are usually willing to offer loans of up to 95% of the purchase price of the home. However, the loan applicant will generally have to purchase 'mortgage insurance' if the amount of the loan is greater than 80% of the price of the home, particularly if the loan is to be sold in the secondary market. Some special programs, provide exceptions to these general rules, allowing for example, a mortgage with no downpayment. In other instances, loans for more than the price of the home are sometimes made when extensive renovations on the home are going to be undertaken.

11 Nevertheless, as has been remarked in the literature, in each of these cases economic forces might argue for segregation, but not necessarily an inferior economic result. Minorities might not be allowed in certain areas, but that doesn't mean that the areas they inhabit need be inferior to majority areas. And economic forces, by themselves, imply that the lack of employment in some firms should be compensated for by the establishment of firms that have work forces that do not resent minority workers. Similarly, there would be an economic incentive to create retail establishments that cater to minorities, and there is no reason that these establishments need be of lower quality than the establishments that cater to the majority.

home mortgage market more fully than might be expected in many other circumstances.<sup>12</sup> Economic self-interest, therefore, should reduce racial discrimination in this market more completely than in many others. In addition, special programs and regulatory incentives inducing banks to increase their mortgage lending to minorities are countervailing forces that might be thought to provide minorities some advantages in securing mortgage financing.

Additionally, it seems logical to expect that competitive forces should work to eliminate discrimination. If one bank declines profitable loans in minority areas, it is natural to expect that other banks will step into the breach to provide those loans.<sup>13</sup> Still, if bigotry is common among mortgage lenders, it is conceivable that mortgage discrimination might be systematic.

When all the theorizing is finished, however, this important policy question can only be answered with careful empirical analysis.

### **III. HMDA DATA AND PROBLEMS WITH THE BOSTON FED EXTENSIONS**

The starting point for creation of the extended data by the Boston Fed was the 1990 HMDA data.<sup>14</sup> The follow-up survey conducted by the Boston Fed asked banks that had made at least 25 mortgage loans in the Boston MSA to provide additional information above and beyond the HMDA data they had already provided.<sup>15</sup>

Information was requested for each minority (Black and Hispanic) loan application in the Boston MSA, and a random sample of 3300 white applicants.<sup>16</sup> The additional data reported by the banks were then transcribed and merged with the original HMDA data. The final sample made available to outside researchers contained information on 2932 loan applications although the sample size in MTBM is

---

12 Loan officers usually receive a commission upon successful completion of a loan application.

13 One of the earliest criticisms is associated with Gary Becker (1993a, 1993b) who argued that examining the profitability of loans would allow a more appropriate test of the hypothesis.

14 The original HMDA variables include: type of loan, purpose of loan, type of occupancy, loan amount, loan decision, property location, applicant and co-applicant race and sex, applicant income, purchaser of loan, reason for denial.

15 Variables in the extension include: number of units in property purchased, marital status, number of dependents, dummy for two years employed in current line of work, dummy for two years in current job, whether self-employed, monthly housing expense, purchase price of property, amount of: other financing, liquid assets; number of credit reports in loan file; whether credit history meets guidelines; # of consumer credit lines on credit reports; mortgage credit history; consumer credit history; public credit history; Housing expense to income; Total obligations to income; Fixed or adjustable loan, term of loan, whether special program; appraised value of property; type of property; whether mortgage insurance sought; whether mortgage insurance approved; whether gifts as downpayment; whether co-signer of loan; whether unverifiable information; number of reviews; net worth. Also, the census information from the HMDA data was modified to make it difficult to determine the exact location of an applicant. For example, the relative income of a tract became a dummy variable indicating whether income was greater or lower than the MSA average. Similarly, information on the bank that the applicant dealt with was removed from the data.

16 Less than perfect returns from the survey reduced the size of the sample to 3062 in the 1992 report. The public data set Footnote continued on next page

2925.<sup>17</sup> If the data were carefully recorded, transcribed, and then double-checked for errors, the resulting data set should have been very useful. Unfortunately, something appears to have gone awry in this process.

Our examination of the data revealed many instances of what we would define as data errors. We define error in this case as an instance where the value contained in one variable is inconsistent with values contained in other variables for the same observation. For example: a particular observation (mortgage application) that has one variable indicating that the application was rejected by the bank, but another variable indicating that the bank sold that mortgage in the secondary market must be a data error since only approved mortgages can be sold in the secondary market. Similarly, if an observation has a ratio of monthly mortgage payments to monthly income that is reported as zero, we treat that observation as contaminated by errors since any mortgage requires repayment, and incomes can not be infinite. Additionally, we classify as errors those instances where variables take on values that are highly improbable compared to other variables in the same observation. For example, if a mortgage of \$125,000 is listed as having a monthly payment of \$50, implying an interest rate of -10.3%, we assume that one of the values is in error. *Note that each of these examples actually occurs in the data -- they are not hypothetical.*

Appendix 1 lists these errors in detail and should be read by anyone wishing to comprehend the nature and severity of these inconsistencies that are the central focus of this paper. Nevertheless, we present here a brief summary of these problems. There were seven applications where the ratio of monthly mortgage expense to income was reported as zero. Hundreds of mortgage applications had imputed interest rates either far below or far above market rates. There were several dozen seemingly absurd cases of reported net worth. For example, in one case the applicant has a net worth of -\$7,919,000 and a yearly income of only \$30,000, yet was approved for a mortgage. There were 44 loan applications sold in the secondary market even though the loans were classified as rejected. Given that forty-one of these forty-four cases were applications from minorities, this error appears to be anything but random.

Similarly, there were hundreds of loan applications that were approved, even though they did not meet the requirements for sale in the secondary market, such as the requirement that mortgage insurance be purchased when the downpayment is less than 20%. Although it is possible that banks may hold portfolios of mortgages that do not meet secondary market requirements, our discussions

---

had 2932 observations (Fed researchers report that they inadvertently included 130 VA and FHA loans in their 1992 work).

17 The 1996 article does not explain why the public sample had seven extra observations. Also missing from the public sample were data on the bank that held the loan, detailed data on the length of time that the applicant and co-applicant had been employed on the job and in the line of work (converted to a dummy indicating less than two years), years of education for applicant and co-applicant (converted to college dummy), and detailed census tract information. We leave it to the editors of the American Economic Review to determine if these differences contravene its policy that data must allow for Footnote continued on next page

with underwriters indicated that the very large number of loans that failed to meet these requirements seems highly improbable. Further, after making allowance for the possibility that the banks in this sample may hold large numbers of mortgages that do not meet secondary market requirements, there were 119 loan applications that failed to meet these secondary market guidelines and yet were reported to have been sold in the secondary market.

Yet for all the suspicious observations we were able to uncover, we were able to perform tests of internal consistency for only a small number of variables used in the study. It is important to note that most of the variables included in the study do not allow for consistency checks. Thus, it is likely that there are many more errors in the data than we have been able to document.

In addition to checks for internal consistency, we attempted to determine whether the HMDA component of the Fed-extended data is consistent with the public HMDA data. Since the Fed researchers started with the HMDA data and then added to it, the HMDA component of their extended data set should have been identical to the original HMDA data. Our examination, discussed below, indicates that there are over 400 observations in the Fed-extended data set that are inconsistent with the original HMDA data.

Since the authors of the Boston Fed report made no mention of any such inconsistencies in their 1992 report, we must assume that they were at that time unaware of them. Since then they have either claimed that what we are terming inconsistencies or data errors are not actually inconsistencies (Browne and Tootell 1995), or they have largely ignored these problems (MTBM 1996).<sup>18</sup>

After Liebowitz (1993) and Zandi (1993) first noted these data inconsistencies, virtually all follow-up research has accepted the view that there were serious errors in the data. Carr and Megbolugbe (1993) concluded that one third of the observations were questionable and Hunter and Walker take this as their starting point (1995).<sup>19</sup> Glennon and Stengel (1994) report many errors in the data. Horne (1994) finds that, for the narrow subset of the actual loan files that he was permitted to examine, more than half of the observations contain serious errors.

---

fully reproducible results.

18 MTBM barely mention these problems, focusing instead on a few observations mentioned as errors in Horne (1994, 1997). Their discussion, according to Horne's evidence (1998), appears to be both incorrect and unprofessional. Tootell and Brown (1995) provide a far more detailed defense of the data as reported in the Appendix.

19 Carr and Megbolugbe attempted to remove observations containing questionable data. In their table 3 they found 1045 suspicious observations out of 2816 total observations. They claim that after removing these observations the basic results of the Boston Fed hold up. Yet on their interest rate screen, they allow loans with interest rates as low as 4% and as high as 19% to remain in the sample, even though mortgage interest rates in 1990 were generally in a narrow range far removed from these values. Additionally, although consistency checks can only be performed for a small number of variables, Carr and Megbolugbe are comfortable in assuming that there are no other errors in the data. Glennon and Stengel (on page 27) are far less sanguine about cleansing the data of errors. They state "There is no obvious way these errors can be corrected short of reexamining the loan files, a solution we believe is impractical."

We now turn to an empirical examination of the mortgage discrimination hypothesis.

#### **IV. RESULTS WITH THE ORIGINAL DATA**

Table 1 reports summary statistics for several key explanatory variables that are included in the Fed enhanced data. These values are virtually identical to those reported in MTBM (remember that full replication is impossible since the data set they use is different than the one they provided to the public). The summary statistics indicate that minority loan applications have characteristics considerably different from those for the white population. For example, white applicants have considerably greater wealth, are far more likely to meet credit guidelines, and are far less likely to submit information that cannot be verified. Further, they are less likely to have loan-to-value ratios greater than 80%, and thus have less need for mortgage insurance. Note also that the minimum and maximum values for certain variables (e.g., obligation to income ratios of zero) immediately indicate problems with the data.

**Table 1**

	Whites (n=2247)			Minorities (n=685)		
	Mean	Minimum	Maximum	Mean	Minimum	Maximum
Mortgage Rejected	10.37%	0	1	28.32%	0	1
Meets Credit Guidelines	93.60%	0	1	77.40%	0	1
Unable To Verify	4.00%	0	1	10.90%	0	1
Total Obligation/income	32.76%	0%*	300%**	34.76%	6%	111%**
Denial Of Mortgage Insurance	27.6400	0%	100%	39.420	0%	100%
Housing Expense/income	25.18%	0*	300%**	26.24%	0*	73%
High Expense To Income ***	31.02%	0%	100%	36.50%	0	100%
Loan-to-value	73.60	2%**	830%**	84.40	18.8%	939%**
Amount Of Loan (000's)	143.9500	2	980	128.79	30	802
Income (000's)	77.6100	4	796	56.67	13	972
Liquid Assets (000's)	98.6700	0.0	8650	40.80	0	020
Net Worth (000's)	283.3300	-7919**	28023	91.64	-858	346

\* Indicative Of An Error.  
 \*\* Most Likely An Error.  
 \*\*\* Defined As Greater Than 28%.

The rejection rate for minorities is almost three times as great as that for whites, with an absolute difference of 18 percentage points. It is this simple statistic that is responsible for much of the negative publicity received by mortgage lenders.

In column 1 of Table 2 we estimate a regression using an OLS specification<sup>20</sup> that is similar to that of MTBM.<sup>21</sup> The coefficients and t-statistics are quite similar to those of MTBM although our measure of discrimination has a larger t-statistic. As is common in studies of discrimination, the coefficient on minority group membership is taken to measure the degree of racial discrimination. The .073 coefficient for the minority variable in regression 1 of table 2 indicates that 7 out of 100 minority applicants are rejected for reasons other than the economic characteristics controlled by the regression. This number appears quite large relative to the 10 out of 100 whites or 21 out of 100 minorities that are rejected for economic reasons.

**Table 2: Dependent Variable = 1 If Loan Is Rejected**

Variable	Fed-style Specification		Alternative Specification	
	B	T-Stat	B	T-Stat
Minority	0.073	5.12	0.028	2.38
Probability of Unemployment *	0.008	2.79	0.005	2.14
High Expense/income Ratio	0.052	3.58	0.027	2.17
Loan To Value Ratio	0.063	3.42	0.033	2.15
Denial Of Mortgage Insurance	0.667	18.94	0.455	14.98
Obligation/income Ratio	0.005	8.62	0.003	6.71
Self-employment	0.053	3.06	0.045	3.03
Neighborhood	0.014	1.27	0.018	1.89
Multifamily home	0.057	3.41	0.055	3.90
Consumer Credit History **	0.036	10.33		
Mortgage Credit History**	0.028	2.62		
Public Credit History**	0.205	9.54		
Unable To Verify Data			0.300	14.17
Loan Meets Credit Guidelines			-0.550	-32.67
(constant)	-0.290	-10.01	0.430	15.79
Adjusted R Squared	0.290		0.492	
# Of Observations	2931		2928	

\* MTBM measured a loan applicant's probability of unemployment by the unemployment rate of the major industrial group in which the applicant worked. The economic characteristics of two-digit industrial groups are generally poor proxies for the more detailed component industries (see Liebowitz, 1982) and the unemployment rate in the two digit industrial group is not even the aggregate of the unemployment rate in the component occupations. For these reasons, and also because this measure contradicts empirical evidence, (it implies that minorities have a lower probability of unemployment) we do not include this variable in later regressions. Its inclusion would have a minor positive impact on measured discrimination.

\*\* Higher values for these credit history variable indicate an inferior credit history.

Note that two variables are available in the Fed-extended data that dramatically increase the explanatory power of the regression. One variable is a measure of whether the bank was able to verify

20 Although we ran all regressions in both logit and OLS, the results from the two techniques were nearly identical in all important characteristics. Therefore, we report only the results from OLS regressions since they provide a natural and linear interpretation of the regression coefficients, greatly simplifying the analysis. The logit regression results are available upon request.

21 We follow the specification on the 1992 MBMT report for a single loan-to-value ratio whereas in the 1996 article this variable is separated into three variables. This has little effect on the results and is done to save space.

the information provided by the loan applicant. The motivation for including this variable is straightforward -- if mortgage lenders are unable to verify the information on the loan application, then the information on the application is not informative. Including this variable has not proven particularly contentious since it has only a relatively small effect on the minority coefficient.

The second variable with great explanatory power indicates whether the applicant meets the internal credit guidelines of the bank. The use of this variable has been the source of some controversy since it dramatically reduces measured discrimination.<sup>22</sup> The most serious criticism is that this variable might reflect bias on the part of bankers. We do not believe that this conclusion is warranted. First, credit histories are often rated mechanically (a process known as credit scoring) by a computer program, or at least in a separate department, where the race of the applicant may not even be known. Second, our attempts to check this hypothesis did not provide any support.<sup>23</sup> The alternative is to use a set of three credit history variables constructed by the Boston Fed.<sup>24</sup>

So as to sidestep controversy at this time, we shall present results using both measures of credit history.<sup>25</sup> There are many other specification problems that can be raised, but we largely wish to sidestep this particular quagmire.<sup>26</sup>

---

22 Browne and Tootell claim that this variable is merely a proxy for loan denial., i.e., that bankers merely indicated that each rejected loan did not meet the credit guidelines so as to enhance consistency with their lending decision. We see no reason that bankers would have answered the meets-guideline question any less seriously than they answered the other questions in the survey. Also, the persons answering the survey questionnaire obviously did not just blindly state that rejected applicants didn't meet the guidelines since 45% of all rejected loans met the banks' credit guidelines.

23 If banks wanting to deny minority loan applications falsely state that the loan did not meet guidelines, then it would have the effect of increasing the share of minority loans not meeting the guidelines that are rejected, *ceteris paribus*. Such discrimination would increase the power of the meetsguidelines variable in a regression for the minority subset. In actuality, a regression using the subset of minority applicants indicates that the meetsguidelines variable has a smaller coefficient than a regression for the subset of white applicants (.535 vs. .570), a result inconsistent with this tainted variable hypothesis. The simple correlation between rejection and meetsguidelines is virtually identical between the two groups. The share of minority applicants not meeting the credit guidelines that turn out to be rejected is greater than is the share of loans to white applicants not meeting the guidelines that turn out to be rejected (81.9% vs. 74.8%) which might seem to support the tainted variable hypothesis, but of course these groups have different economic characteristics. If a regression is run on applications that do not meet the credit guidelines the (insignificant) coefficient measuring discrimination is smaller than for the sample of loans that meet guidelines and for the sample as a whole, which is also inconsistent with the view that this variable is tainted by discrimination. Nevertheless, we need to proceed with caution.

24 The Fed's three credit history variables do not account for the age of the credit problem, the size of a delinquency, or the possibility that different banks will have different guidelines. Thus the MTBM credit history variables are not likely to reflect the full impact of an applicant's credit history on a bank's lending decision. Nor do they provide information on the financing limits of credit cards or the usage patterns of checking accounts.

25 It has also been mentioned (Browne and Tootell, Carr and Megbolugbe) that if the meets-guidelines variable is made the dependent variable in a regression with race and other explanatory factors, that these other factors often prove to be significant. We do not believe that this is either decisive or surprising. In fact, if the three Boston Fed Credit History guidelines are made independent variables in such regressions, exactly the same type of results are found.

26 There are undoubtedly many imperfections in the specification used by MTBM and ourselves that we will largely ignore. For example, the relationship between measures of financial ability to carry the loan and mortgage decisions are not likely to be related in a linear fashion. There are also questions regarding the inclusion of applications requiring mortgage insurance, since rejection of mortgage insurance is not made by the mortgage lender. These questions, while of importance, are not the focus of this study. Also, there are potential simultaneity problems (see Yezer et. al., 1993).

## **V. THE IMPACT OF MEASUREMENT ERRORS ON THE DISCRIMINATION COEFFICIENT**

There is often a tendency to ignore, or at least minimize, the impact of randomly occurring errors in data. Although such errors reduce the precision of estimated regression coefficients, the presence of errors in the data is often a convenient explanation for the fact that a model fails to fit the data perfectly.

The impact of data errors on the analysis of discrimination in mortgage lending can be much more serious, however. The motivation for collecting additional information on the financial characteristics of loan applications is to determine whether the higher rejection rate for minority loan applications is attributable to the generally weaker financial condition of minority applicants or to the impact of racial discrimination on lending decisions.

If data errors were truly random, they would affect the mortgage outcome variable and the measurement of race, as well as all other variables. In this instance, the measured differential in rejection rates between ethnic groups would diminish as measurement errors increased. The measured rejection rate differential in the Fed-extended data, however, does not show any indication of being biased toward zero. Note from Table 1 that the average rejection rate for minorities relative to whites is 28:10 for the Fed-extended data. This is in general agreement with previous examinations based on HMDA data, and also in line with the HMDA data for the Boston MSA which has a ratio of 29:10. Therefore, data errors in the Fed-extended data do not seem to impact the group rejection rates.

This should not be too surprising. Not all variables are equally likely to be the victims of data errors. Given that the racial classification of the loan applicant was used as the basis for creating the survey sample, there should be few errors in the dummy variable for the race of the applicant. Further, the variable that measures loan outcome (accept/reject) should not be subject to the same degree of error as the financial variables included in the data set. This is true because (1) the different rejection rates for each race are so well documented that had the data not conformed to this empirical regularity, the researchers would surely have reexamined the data to discover the source of this discrepancy; (2) the loan outcome and race variables are both represented by a variable of a single digit that can take on only three values,<sup>27</sup> thus making it less likely for errors to be introduced than would be the case for variables requiring multiple digits; and (3) these variables both come from the original HMDA data

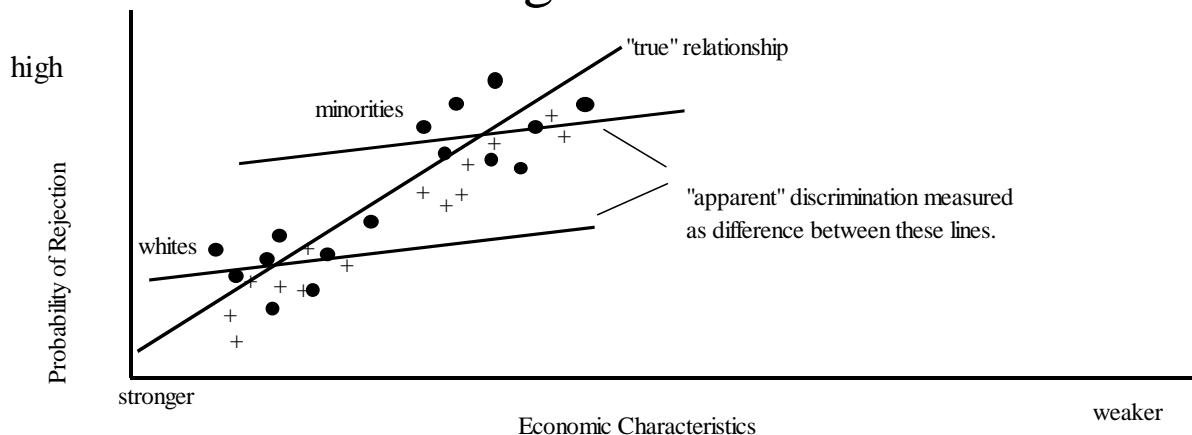
---

27 In the HMDA data, loan outcome and race each can take on more than three values, but the Boston Fed limited their sample to three values.

and did not have to be collected in the survey.<sup>28</sup> Data collected from scratch should be more prone to (transcription) error than data moved from one data set to another.<sup>29</sup>

With loan disposition and minority variables less likely to be impacted by error, any noise introduced through data errors in variables related to the economic strength or weakness of an application (and thus the probability of rejection or approval) is likely to increase measured discrimination. This is illustrated in Figure 1. In Figure 1, the "+"s represent the true relationship between loan disposition and the economic variables which is represented by the regression line in the figure. Note that noise in the measurement of variables on the x-axis moves the measured observations randomly to the left or right of the true values, as indicated by the dots. This noise obscures the true relationship, so that the differences between groups now pick up most of the variation in the dependent variable.<sup>30</sup> This masking of the true relationship has the effect of loading the differential acceptance rates for minorities and whites into the race variable instead of the economic characteristics variables.

**Figure 1**



The question now becomes whether it is possible to sufficiently "cleanse" the data of errors so that a better understanding of the true relationship can be ascertained. If some of the observations contain errors, and others do not, the purpose of cleansing will be to eliminate the former observations while

28 Note, however, that errors were apparently introduced in the transcription from HMDA to Boston Fed data for applications classified as both rejected and sold, as discussed above. Also, as discussed below, there are some errors in the variable that measured acceptance or rejection.

29 Although, as we report below, there appear to have been serious problems in moving HMDA data to the Boston Fed data.

30 Technically, this argument is valid when there is but a single independent variable measuring economic characteristics. When there are multiple variables measuring economic strength it is no longer possible to say precisely what the effect of noise on any one variable is on the measured discrimination. But it is clear that with enough noise in variables measuring economic strength, the measurement of discrimination will equal the differential rejection rates between groups. Even more importantly, there is no reason to believe that measured discrimination approaches zero as the noise in the economic Footnote continued on next page

preserving the latter. Given the very large number of errors found examining only a small number of variables, however, it is not necessarily the case that removing the observations that contain known errors necessarily reduces the density of errors in the remaining data.

We presume, in the following sections, that errors are not randomly distributed, but are likely to cluster in certain observations. This would be true if, say, individuals transcribing the data at the behest of either the mortgage lender or the Fed researchers, tended to get tired at certain times of day, or if certain individuals were unusually unreliable. In these instances an error in one variable would increase the likelihood that the observation would contain errors in other variables and thus provides a rationale for removing that observation even if that variable plays no role in the regression analysis. If this assumption is correct, the removal of observations with known errors in any variable should reduce the proportion of errors in the data. We are certain, however, that errors will remain even after our best efforts to remove them but hope that the errors are less preponderant.

## ***VI. THE IMPACT OF ERRORS FOUND BY EXAMINING LOAN FILES***

Not all data errors are created equal -- some are far more likely to distort results than are others. And no error is as likely to influence results than an error in the dependent variable, assuming that these errors are not random. Horne (1994) documented a number of misclassified mortgage decisions by examining the actual loan files for the subset of the loan applications from the banks insured by the FDIC.<sup>31</sup> He identified 26 loans for which the information in the loan files indicated that the bank's actions should not have been classified as rejections, yet were classified as "rejections" by the Boston Fed.<sup>32</sup>

We used the Freedom of Information Act to obtain a list of these loans. The list indicated that five applications were misclassified as rejections when the applications were actually accepted. Of the remaining classification problems, four were applications to special lending programs (i.e., designed to help low income applicants) that were rejected by the program administrator (rather than the bank) on the grounds that these applicants were overqualified. Note that since these (primarily minority) applicants are relatively well qualified, a statistical analysis would indicate no economic justification for the bank itself to reject these applications, and these applications would inappropriately support a hypothesis of discrimination. In eight cases the applicant rejected the bank's offer of a loan with

---

variables increases.

31 Horne, working for the FDIC, was able to gain access to the actual loan files for the subset of banks regulated by the FDIC. He focused his examination on 95 rejected loans that the MTBM regression model indicated should have been approved. His is the only study that had access to the loan files.

32 MTBM dispute Horne's assessment of these applications. Regardless of who is correct, as we show below, almost all of these 26 observations exhibit other problems, in that the HMDA variables for these observations are inconsistent with the public HMDA data.

slightly different terms than those requested in the loan application.<sup>33</sup> Finally, although nine applications were withdrawn before the bank reached a decision, they were classified as rejections.

Table 3: Dependent Variable = 1 If Loan Is Rejected									
	"Meets Guidelines" Credit History				Boston Fed Credit History				
	Full Data Set		Removing 26 Misclassified Applications		Full Data Set		Removing 26 Misclassified Applications		
Variable	B	T-Stat	B	T-Stat	B	T-Stat	B	T-Stat	
Minority	0.0271	2.27	0.0068	0.58	0.0531	3.96	0.0325	2.45	
High Expense/income Ratio	0.0264	2.14	0.0235	1.95	0.0450	3.26	0.0432	3.18	
Loan To Value Ratio	0.0326	2.11	0.0359	2.38	0.0419	2.41	0.0454	2.67	
Denial Of Mortgage Insurance	0.4549	14.98	0.4677	15.81	0.5802	17.27	0.5943	18.06	
Obligation/income Ratio	0.0030	6.74	0.0031	6.96	0.0040	7.93	0.0040	8.20	
Self-employment	0.0496	3.42	0.0500	3.53	0.0542	3.33	0.0541	3.39	
Neighborhood	0.0182	1.89	0.0170	1.814	0.0143	1.33	0.0133	1.26	
Multifamily home	0.0569	4.02	0.0624	4.51	0.0554	3.50	0.0596	3.83	
Unable To Verify Data	0.3027	14.23	0.2808	13.17	0.4377	18.87	0.4218	18.05	
Loan Meets Credit Guidelines	-0.5490	-32.68	-0.5542	-33.37					
Consumer Credit Problems					0.0312	9.51	0.0310	9.59	
Mortgage Credit Problems					0.0233	2.33	0.0215	2.20	
Public Credit Problems					0.2008	9.89	0.2007	10.00	
(constant)	0.4497	17.31	0.4518	17.67	-0.2243	-8.60	-0.2256	-8.81	
Adjusted R Squared	0.4920		0.4970		0.3650		0.3650		
# Of Observations	2928		2902		2931		2905		

To ascertain the impact of these misclassifications, we reestimated the model after removing these 26 applications from the sample.<sup>34</sup> The results, which are reported in Table 3, show that excluding the incorrectly coded observations reduces the estimated coefficient for the minority variable from .0271 to .0068, a level that is not statistically significant. A similar size change in the minority coefficient holds for the specification that includes the original MTBM credit history variables. When the 26 misclassified loans are removed, the coefficient drops from .053 to .033, although it remains statistically significant.

33 Horne finds that for the sample of the loan applications examined by the Boston Fed, minorities were much more likely to decline these counteroffers than were whites.

34 We classified two of these applications as rejected special programs, although Horne did not so classify them that way. In Footnote continued on next page

It is important to note that the 26 misclassified loans found by Horne come from the subset of loan applications obtained from banks insured by the FDIC, which represents only 45% of the entire sample of loans. Since there is no reason to believe that the frequency of misclassification is substantially different for the non-FDIC component of the Boston Fed sample, it is reasonable to expect that the elimination of the classification errors in the remaining 55% of the sample would have reduced the estimated coefficient of the minority variable even further.

Unfortunately, these misclassifications are "unobservable" and therefore cannot be corrected or eliminated from the sample, as was done with the classification errors documented by Horne. However, if there is a linear relation between the number of misclassified applications and the estimated minority coefficient, we can use the results presented in Table 3 to assess the impact of any remaining classification errors on the estimated coefficient for the minority variable.

To test the linearity assumption, we approximate the relation between the estimated minority coefficient and the number of misclassified applications by successively eliminating a larger and larger number of misclassified loans and reestimating the coefficient for the minority variable. To hold constant the characteristics of the misclassified loans as the number of misclassified applications eliminated from the sample changes, we performed several replications for each possible number of misclassifications to be eliminated, randomly choosing the misclassifications to be eliminated and then averaging the estimated coefficients of the minority variable from each replication.<sup>35</sup> The results are presented in Figure 2.

Figure 2 shows that the relation between the number of misclassified loans and the estimated coefficient for the minority variable appears to be linear (this diagram is based on our alternative regression model). Therefore, it seems reasonable to extrapolate the trend line in Figure 2 to predict the impact of the misclassification errors for the non-FDIC component of the sample.<sup>36</sup> Note that if the trend line is extrapolated to a total of thirty-seven misclassified applications, which is consistent with only eleven additional misclassifications among the non-FDIC applications, the estimated coefficient for the minority variable goes to zero becoming negative as the number of misclassifications in the non-FDIC data increases. Even using the Fed credit history variables, all statistical significance is lost.

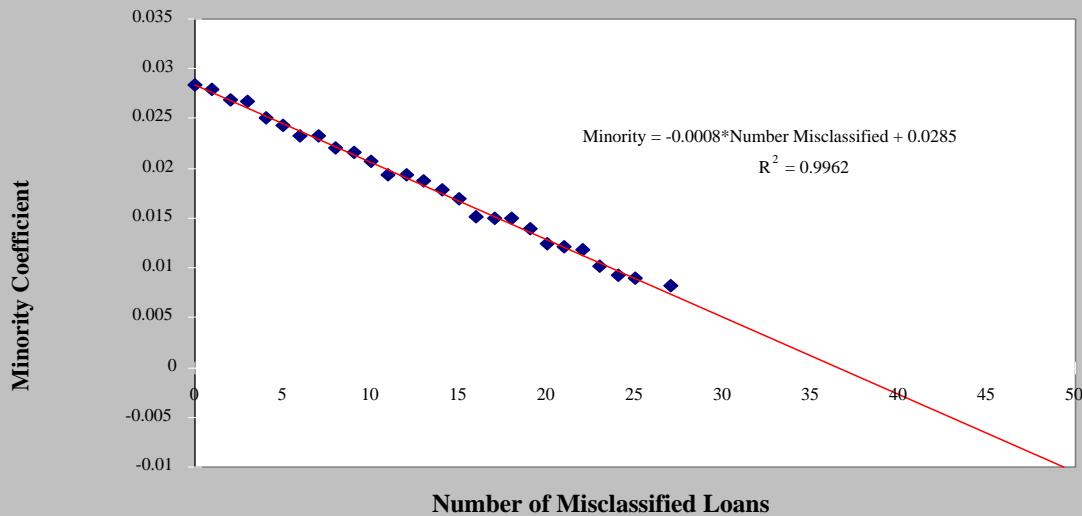
---

these cases the special program appeared to be devised by the Bank.

35 For example, to determine the impact of eliminating ten misclassified applications from the sample, we randomly remove ten of the 26 misclassified applications from the sample. The minority coefficient was then estimated using the remaining data. This procedure was repeated a dozen times, removing ten randomly selected misclassifications at each iteration. The twelve estimates of the minority coefficient were then averaged to determine an average for minority coefficient given that ten misclassified applications have been eliminated from the sample. Similar calculations were performed to reflect the impact of removing each possible number of misclassified applications, from one to 25.

36 This line actually understates somewhat the impact of the misclassified observations on the minority coefficient because it is based on removing the faulty observations, whereas several of the misclassified observations could actually be fixed, and when they are changed the coefficient falls by a larger amount than if they are removed.

**Figure 2: Relationship Between Number of Misclassified Loans and Minority Coefficient**



Thus we conclude that when a small number of the most egregious errors in the data are removed, the impact of race on lending decisions becomes statistically insignificant, and even might reverse sign. Note that this still leaves intact the great majority of data errors and any associated bias in the coefficient measuring discrimination.

## VII. REMOVING INTEREST RATE EXTREMES

The size of the loan and the monthly loan payment are linked by the rate of interest charged on the mortgage. Since this rate was relatively constant during the period in which these loan applications were processed, consistency of the reported loan amount with the monthly payment requires that the implied interest rate on the loan lie within a band of interest rates whose width is determined by the variation in rates during the year 1990 (ranging approximately from 9.75%-10.75%).<sup>37</sup> By successively estimating the results for subsets of the data lying within progressively narrower bands on the imputed interest rates, we are able to examine whether the measured impact of discrimination increases or decreases as we narrow the bounds on the acceptable degree of error in the data.<sup>38</sup> This particular check on the internal consistency of the data is of importance since it revealed a very large number of suspicious observations for variables that are central to many of the financial ratios that are used to estimate the probability of a loan's approval.

Table 4 presents the results of this filtering. After removing the 26 observations identified by the FDIC as having errors in the dependent variable (with no extrapolation for the FDIC type errors still in

37 FHA monthly mortgage rates, St. Louis Fed.

38 The interest rates are calculated after removing estimates of property taxes and insurance.

the sample), we removed observations having abnormal interest rates. We created two levels of filters. First, we took cutoffs at 14% and 5%, which removed about 10% of the observations. Then we applied a stronger filter, removing observations with interest rates higher than 12% and lower than 7%. This had the impact of removing about 20% of the observations.

Table 4: Removal Of Questionable Interest Rate Observations									
	Using "Meets Credit Guidelines" for Credit History				Using Boston Fed Credit History				
Restricting Loans To Imputed Rates Between:	14% And 5%		12 % And 7%		14% And 5%		12 % And 7%		
Variable	B	T	B	T	B	T	B	T	
Minority	0.0014	0.11	0.0020	0.15	0.0293	2.12	0.0249	1.69	
High Expense/income	0.0206	1.66	0.0237	1.78	0.0413	2.94	0.0430	2.87	
Loan-to-value	0.0611	3.15	0.0970	3.34	0.0676	3.06	0.1379	4.18	
Denial Of Mortgage Insurance	0.4816	15.74	0.4464	13.22	0.6052	17.68	0.5865	15.62	
Obligation/income	0.0031	6.82	0.0034	6.93	0.0041	8.01	0.0045	8.26	
Self-employed	0.0580	3.91	0.0390	2.42	0.0592	3.52	0.0377	2.08	
Neighborhood	0.0123	1.26	0.0128	1.22	0.0116	1.05	0.0112	0.95	
Multifamily Home	0.0569	3.80	0.0557	3.59	0.0532	3.14	0.0510	2.92	
Unable To Verify Data	0.2830	12.80	0.2783	11.61	0.4212	17.26	0.4203	15.98	
Loan Meets Guidelines	-0.5553	-32.35	-0.5520	-28.84					
Consumer Credit History					0.0302	8.96	0.0285	7.90	
Mortgage Credit History					0.0189	1.85	0.0150	1.36	
Public Credit History					0.2008	9.69	0.1792	7.87	
(constant)	0.4339	15.67	0.3955	11.63	-0.2386	-8.57	-0.2931	-8.95	
Number Of Observations	2640		2289		2643		2292		
Adjusted R Square	0.50		0.49		0.36		0.35		
B = Coefficient; T = T-Statistic; Property Tax and Insurance Adjustment included in imputed rates									

We include two specifications for the regression. The first contains our specification with the meets-guidelines variable. The second uses the Boston Fed credit history variables. Clearly, as the filter restricts the sample of loans toward more and more reasonable interest rates, the minority coefficient diminishes. With either specification, evidence for discrimination is too weak for us to accept a hypothesis suggesting that banks discriminate against minorities. Further, we are only filtering out those observations for which this limited check on variables could be conducted. Noise is still a likely problem.

## VIII. USING THE PUBLIC HMDA DATA TO MEASURE BANK "TOUGHNESS"

Examination of the public HMDA data allows us to perform some tests not possible using the Fed-extended data, and also provides a check on the Fed-extended data. For example, it is possible to

examine the propensity of a bank to reject applications using the public HMDA data since one of the variables is the identity of the bank to which a mortgage application was submitted, whereas the publicly available Fed-extended data removes this information.<sup>39</sup>

Although we use the term "toughness" for banks with high rejection rates, this variable may well be a proxy for other factors having little to do with toughness *per se*. For example, it is likely that the variability in average rejection rates is related to the characteristics of the neighborhood and clientele of the bank that are not fully picked up by the other neighborhood variables. Banks with well-informed customers, for example, are likely to have fewer customers applying for loans for which they are extremely unqualified.

Variation in average rejection rates are also likely to be related to variations in prescreening, the efforts of banks to match loan applicants with the loan product that best fits their needs. Prescreening will, among other things, tend to reduce the number of weak loan applicants filing a formal loan application.<sup>40</sup> Those banks with weak prescreening will tend to have higher rejection rates than banks with strong prescreening. Note that if banks were sensitive to charges of discrimination on a prescreen, causing them to be less vigilant in preventing weak minority borrowers from filing formal applications, the rejection rate of formal applications for minority customers would increase.

A bank's "toughness" appeared to be surprisingly consistent across various types of customers. For example, we found that a bank's average rejection rate for whites was a very good predictor of the bank's average rejection rate for minorities. The simple correlations ranged from .38 for all banks (123 cases), to .66 for banks with more than 10 minority applications (30 cases), to .78 for banks with more than 15 minority applications (23 cases). Similarly, a bank's average rejection rate for refinancing-loans was a good predictor of the bank's average rejection rate for conventional loans, with the two measures having a correlation coefficient of .50. Clearly, a bank's propensity to reject one type of mortgage application is related to its propensity to reject other types of mortgage applications. Banks also varied greatly in their propensity to reject, with average rejection rates for the four quartiles taking on values of 0%, 2%, 11%, 27%.

This variation in rejection rates across banks suggests that the distribution of white and minority loan applicants across lenders may well be important in explaining differences in group rejection rates. In other words, if minorities frequented mortgage lenders that had a high propensity to reject applications, they would, as a group, be expected to have higher rejection rates than if they frequented

---

39 In their initial 1992 report, MTBM created their own variable for "bank toughness" and concluded that bank toughness was not important. In the 1996 paper they create dummy variables for each bank but do not report the results. This is one of the variables to which outside researchers do not have access.

40 For a discussion of prescreening, see Rosenblatt (1996). He reports that for each formal application that was rejected (for a particular bank that allowed him access to its data), the bank lost approximately \$750 (in addition to the money lost by the applicant). If this is typical, banks obviously have an incentive to reduce rejections, which they can do with prescreens.

more typical banks. To ascertain the potential importance of bank toughness on the group rejection rates, we computed a measure of the propensity to use tough banks for our two groups of customers.<sup>41</sup>

If we use a bank's overall rejection rate as a measure of toughness, a bank that discriminated against minorities might appear to be tough merely because of its discriminatory behavior. We avoid this problem by measuring toughness using only the rejection rate for each bank's white loan applicants. The average toughness of banks frequented by minority loan applicants is then estimated by computing an average of each bank's white rejection rate, weighted by the number of minority customers. A similar calculation is used in calculating the average toughness of banks frequented by white customers.

The results, reported in Table 5a, indicate that on average, minority customers patronize banks that are approximately twice as tough as the banks patronized by white customers.<sup>42</sup> Thus if minorities were identical to the white customers at the banks that they patronized, and if there were no racial discrimination by any banks, minority applicants would be rejected about twice as often as whites.

Table 5a	Average Toughness Of Banks (based On Bank's Rejection Rate For Whites)	Number Of Observations
Banks Frequented by White Applicants	0.10	19823
Banks Frequented by Minority Applicants	0.19	1338

Note that in constructing a measure of toughness to be used in regressions explaining the disposition of mortgage applications, it is important to avoid any circularity arising from the use of the originating bank's rejection rate to explain the disposition of a loan that was itself used to determine the rejection rate. For the pure HMDA data, we can avoid this problem by constructing a measure of lender toughness based on the lender's rejection rate for refinancings, whereas our regressions will be based only on home purchases.

The HMDA data also provide more detailed information on neighborhood characteristics than the Fed-extended data. The Fed-extended data converted the detailed neighborhood data (such as income in the census tract) into dichotomous dummies that measured whether the neighborhood income was above or below average.

---

<sup>41</sup> Since we can create a toughness variable that avoids all possibility of discrimination, we believe it is superior to use this variable as an independent variable in a regression as opposed to a dummy for each bank, which would be unable to distinguish between discrimination and toughness.

<sup>42</sup> Leong (1995) has also found the minorities tend to use banks that are tougher, although his results are not this dramatic. We are not at all sure how general these very strong results are. A single bank with a very large number of minority applicants is largely responsible for this result.

Table 5B provides regression results based on loan applications for home purchases from the public HMDA data. To examine the sensitivity of the results to the precise definition of lender toughness, the results are presented using a measure of bank-toughness based on conventional loans (which is subject to some circularity), and a second measure of toughness based on refinancings.

**Table 5B: Explaining Loan Disposition (home Purchase) With Public HMDA Data**

	B	T	B	T	B	T	B	T
Minority	0.197	21.99	0.170	18.60	0.093	10.41	0.122	13.38
Relative Income In Purchase Neighborhood*			-9.20e-04	-2.38	-0.000632	-8.85	-0.00079	10.85
Bank Toughness (home Purchase)					0.913	41.94		
Bank Toughness (refinancing)							0.581	32.19
(constant)	0.0980	41.68	0.198	23.57	0.0698	8.13	0.105	11.99
Adjusted R Square	0.0250		0.0330		0.116		0.085	
Number Of Observations	19158		18754		18754		18326	
B = Coefficient; T = T-Statistic								
* Relative to MSA average.								

The results show that bank toughness is very influential in explaining rejection rates, regardless of how toughness is defined. Further, bank toughness and neighborhood income reduce the differential rejection rates for whites and minorities by about half, in spite of the fact that detailed financial characteristics for individual loan applicants are unavailable in these data. This reduction in rejection differentials is almost as large as that found using the complete set of variables in the Fed-extended data. Thus, it is apparent that bank toughness plays an important role in mortgage approvals with neighborhood income playing a smaller but important role. We will include these variables in the work reported below.

## **IX. COMPARING PUBLIC HMDA WITH FED-EXTENDED HMDA**

Given that the public HMDA data was the starting point for the Fed-enhanced data, any inconsistency between the observations common to both data sets constitutes evidence that the Fed-enhanced HMDA has been contaminated at some point. Since errors in any of the HMDA variables crucial to the regression model, such as loan disposition, race, size of loan and so forth, will distort regression results, we will examine our results after removing any loan applications for which the Fed-enhanced HMDA data did not correspond with the public HMDA data.

## Matching Fed-enhanced and HMDA Data

If the Fed's migration of data somehow alters the base HMDA variables, we are immediately alerted to the possibility of data errors. In this section, we search for observations where the HMDA data did not remain intact after the migration to the Fed-extended data. Given that these observations are likely carriers of contaminated data, they are removed from the sample so as to cleanse the data of this particular type of error.

The public HMDA data include variables on the loan decision, the amount of the loan and the applicant's income, the race and sex of the applicant and co-applicant, and the purchaser of the loan. The values that can be taken by these variables allow for a very large number of possible combinations. In fact, the values of these variables will generally allow each observation to be uniquely identified. Therefore, we can use these variables to construct a key, or unique identifier, for each observation that can be used to match the Fed-extended data with the public HMDA data. Clearly, the key will not be perfect, since there are some instances where the key can not distinguish between several observations in either the public HMDA data or the extended Fed data. Nevertheless, this approach allows most observations in the two data sets to be matched. Appendix 2 provides details of the matching procedure and the determination of unmatched cases.

The key created from these variables was able to uniquely identify 2833 of the 2932 possible extended-Fed observations. Our matching procedure allowed us to match 2174 of these 2833 observations to unique HMDA observations, leaving 659 cases that could not be matched. The imperfect ability of the key to distinguish among several non-uniquely identified cases in the HMDA data set was responsible for 228 cases not being matched. This implies that in 431 cases the Fed-extended data did not match up with the public HMDA data.

These 431 unmatched cases are a serious cause for concern. There was every reason to expect that the two data sets would contain identical values for the HMDA variables since the Fed researchers did not endeavor to alter the HMDA data.<sup>43</sup> The large number of unmatched cases appears consistent with the general pattern of data errors found in this data set, presumably caused by the data handling process engineered by the Fed researchers. Can we trust the 431 observations that apparently did not migrate intact from one data set to another? Since the failure of these observations to match the HMDA data is likely to be attributable to errors in the data, the inclusion of these observations could bias any regression results. Therefore, we focus attention on the observations that were consistent with the original HMDA data.

---

43 Munnell et. al. (1992) report that they discovered some errors in the HMDA data, such as when one suburban bank discovered that 51 applicants were incorrectly coded as Hispanic. However, the Fed apparently removed questionable HMDA observations from the sample and did not attempt to fix them, since they state that these errors had the effect of reducing the sample size. Therefore, deviations between the Fed data and the HMDA data are not due to intentional Footnote continued on next page

Table 6: Matched Data		
Variable (mean)	Whites (1678)	Minority (496)
Application Rejected	0.070	0.175
Self-employed	0.135	0.081
High Expense To Income Ratio	0.188	0.224
High Loan To Value Ratio	0.100	0.230
Income	80.780	60.610

The economic characteristics of the white and minority borrowers included in the 2174 matched cases are generally similar to those of the applicants in the more complete data set, as can be seen from Table 6. Note that the difference in rejection rates for the two groups is 10.5 percentage points, which is quite a bit smaller than for the entire Fed-extended sample, although the ratio of minority to white rejection rates is approximately 2.5:1.

The matched HMDA/Fed-extended data allow us to include information from the original HMDA, such as bank toughness and neighborhood income, as well as all the variables collected by the Fed. Examination of the matched data indicates that observations with unusually high and low interest rates, high loan-to-value ratios, and so forth are still present in the sample.

Before we turn to regression results with these matched data, we note that the measure of toughness used in these regressions was constructed to avoid circularity. The matched Fed observations were removed from the HMDA data, and bank rejection rates were constructed for the remaining observations. These bank rejection rates were then included in the matched data.

### Results From the Matching Experiment

Table 7 provides the results from the regressions with this matched data set. In the first column we use a specification that closely resembles the Boston Fed's original specification. The minority coefficient in column 1 is only about half as large as for the complete Fed-enhanced sample, indicating that the bias related to inconsistencies in the HMDA data may well play an important role in studies based upon the Fed-extended data set.<sup>44</sup>

Successive enhancements to the regression specification and data cleansing are then applied in the following columns of Table 7. First, in column 2, we add bank toughness and the unable-to-verify-data variable. This reduces to minority coefficient to 2.7% and borderline significance.

---

changes made to the HMDA data by the Fed.

44 Note that this smaller differential between minority and white rejection rates should only alter the results if the regression techniques and data are unable to provide an unbiased measure of the relationship between economic characteristics and mortgage decisions.

**Table 7: Dependent Variable = 1 If Loan Is Rejected**

Fed-Type Specification	(1)	Adding Bank Toughness And Unable To Verify	(2)	Restricting Observations To Interest Rates Between 14% And 5% And Removing 4 FDIC Problems	(3)	Replacing Fed Credit History Variables With Meets-guidelines Variable And Interest Rates Between 12% And 7%	(4)	
Variable	B	T	B	T	B	T	B	T
Minority	0.0377	2.57	0.0277	2.00	0.0156	1.07	-0.0065	-0.48
High Expense To Income Ratio	0.0333	2.16	0.0330	2.28	0.0266	1.76	0.0154	1.08
Loan To Value Ratio	0.0832	3.46	0.0684	3.01	0.1611	4.47	0.1489	4.25
Denial Of Mortgage Insurance	0.7038	12.48	0.6744	12.69	0.6830	12.27	0.6251	12.00
Obligation To Income Ratio	0.0023	3.36	0.0018	2.76	0.0015	2.01	0.0020	2.71
Self-employment	0.0646	3.73	0.0631	3.87	0.0649	3.77	0.0476	2.88
Neighborhood	-0.0004	2.00	-0.0003	1.60	-0.0003	1.37	-0.0003	1.83
Multifamily Home	0.0364	2.10	0.0308	1.89	0.0222	1.24	0.0311	1.90
Consumer Credit Problem	0.0257	7.21	0.0225	6.68	0.0224	6.38		
Mortgage Credit Problem	0.0133	1.22	0.0088	0.85	0.0037	0.34		
Public Credit Problem (bankruptcy)	0.2196	9.66	0.2040	9.51	0.2039	9.18		
Toughness Of Bank			0.6261	8.88	0.6332	8.67	0.4390	6.21
Unable To Verify Data			0.4055	13.71	0.3812	12.01	0.2628	8.47
Loan Meets Credit Guidelines							-0.5344	-23.45
(constant)	-0.1330	3.26	-0.1851	4.74	-0.2345	5.26	0.3640	7.58
Adjusted R Squared	0.1940		0.2850		0.2730		0.4020	
# Of Observations	2174		2174		1979		1722	
B = Coefficient; T = T-Statistic								

Next, we remove the FDIC misclassifications that remain in the data. The nature of the FDIC reported misclassifications for the original Fed-extended and our matched Fed-HMDA sample are reported in the Table 8. Note that almost all of the serious errors found by Horne appear to come from the sample of applications that do not match the HMDA data. Therefore, there are only four remaining FDIC errors to remove. Removing the four FDIC indicated misclassified rejections lowers the coefficient to 2.1% and the t-statistic to 1.55, though to save space we do not report this complete regression in the table.

Second, we attempt to restrict the sample to more reasonable interest rates. First we limit our observations to those with interest rates between 5% and 14%, which lowers the minority coefficient to

1.6% and the T-Statistic to 1.07.<sup>45</sup> Further limiting observations to those with interest rates between 7% and 12% lowers the coefficient to 1%, and the T-Statistic to .66 (This regression is also not shown).

Type Of Misclassification	Fed Sample	Matched Fed-
Withdrawn By Applicant	9	2
Counter Offer	8	0
Rejected By Special Program Because	4	2
Really Approved	5	0
Number Of Misclassifications	26	4
Number Of Observations	2932	2174

Finally, replacing the Boston Fed credit history variable with the meets-guidelines variable lowers the coefficient to -.6% with a T-Statistic of -.47.

What can we conclude from the sample of observations for which the HMDA variables are correctly reported in the Fed-enhanced data set? First, this cleansing of the data provides results of the same general variety as the earlier attempts at cleansing the data. Clearly, given the likelihood of errors in the remaining data, there is little or no support for the hypothesis that mortgage lenders systematically discriminate against minorities. Taking our results in combination with our understanding that noise might very well bias upward the coefficient measuring discrimination, there is even a hint that mortgage lenders might favor minority applicants. We caution, however, that these results can not warrant such a conclusion at this time.

## X. CONCLUSION

There are good economic reasons to be skeptical of claims that lenders discriminate against minorities in their approval of mortgage applications. Discriminators who would turn down a good loan harm themselves by turning down a profit opportunity. Further, the current regulatory climate has put great pressure on mortgage lenders to ensure that discrimination is not practiced by its employees.

The support for the belief that banks discriminate is based largely on the data constructed by the Boston Fed. Yet we have shown this data set to be deeply flawed in a way that is likely to bias the results. Although some other researchers believe that the errors in the data can be repaired and that

---

45 This is the imputed interest rate adjusted for property tax and insurance.

such repaired data support the conclusion of the Boston Fed, our analysis of this data indicates otherwise. Our reworking of the data provides no evidence for the conclusion that banks systematically discriminate against minority groups. But we find it unreasonable to think that we or anyone else can fully discover all the errors in this data with the techniques at hand. It seems imprudent, to us, to base any policy decisions on analyses of these data.

Unfortunately, the Boston study has had a tremendous influence on public policy, perhaps because it comes from a major government agency with a major publicity apparatus. Its recent publication in a leading economics journal can only increase its standing. Listen to Lawrence Lindsay's assessment made after being informed of the numerous problems with the study: "The study may be imperfect, but it remains a landmark study that sheds an important light onto the issue of potential discrimination in lending."<sup>46</sup>

Spokesmen for the banking industry have been relatively silent during this debate. Their public actions have been largely limited to statements of repentance, payments of money to minority organizations, and promises to develop new techniques for marketing loans to the minority community, such as the euphemistically named 'flexible underwriting standards'.<sup>47</sup> Although their silence might be taken as an admission of guilt, other forces in the regulatory climate have operated to constrain bankers from acting any differently. Thus, serious academic studies are the only method for determining the truth of the matter.

If we are correct, the media frenzy associated with the release of the HMDA data every year has been largely counterproductive for achieving an even playing field in the mortgage market. The "progress" that has been made in "helping" minorities may not be progress at all. After the warm and fuzzy glow of "flexible underwriting standards" has worn off, we may discover that they are nothing more than standards that led to bad loans. Certainly, a careful investigation of these underwriting standards is in order. If the "traditional" bank lending processes were rational, we are likely to find, with the adoption of flexible underwriting standards, that we are merely encouraging banks to make unsound loans. If this is the case, current policy will not have helped its intended beneficiaries if in future years they are dispossessed from their homes due to an inability to make their mortgage payments. It will be ironic and unfortunate if minority applicants wind up paying a very heavy price for a misguided policy based on badly mangled data.

Finally, we must ask whether this type of problem is endemic to other studies of discrimination. If imperfect data tend to cause findings of discrimination where none may occur, then extreme vigilance is required by those conducting such studies to ensure that the data used are pristine. Have researchers taken sufficient care in their creation and use of data? Are the data used sufficiently good proxies for

---

<sup>46</sup> Correspondence dated March 1, 1994.

<sup>47</sup> See Hansell [1993].

the purposes to which they are put? At this time we can only ask the question -- others will have to provide the answers.

## References

- Bacon, Kenneth and Putka, Gary, "Shawmut's Plan For Acquisition Rejected By Fed," Wall Street Journal, November 16, 1993, Pg. 1.
- Becker, Gary., Nobel Lecture: The Economic Way of Looking At Behavior, Journal Of Political Economy, June 1993a, 1-.
- Becker, Gary., "The Evidence Against Banks Doesn't Prove Bias," Business Week, April 19, 1993b, Page 18.
- Blumenthal, Karen, "Home-mortgage Delinquencies Are Steadily Inching Upward," Wall Street Journal, 11 June 1996, Interactive Edition.
- Browne, Lynn. E., And Tootell, Geoffrey M. B. "Mortgage Lending In Boston -- A Response To The Critics," New England Economic Review, Sept. 1995, 53-78.
- Carr, James H., And Megbolugbe, Isaac, F., "The Federal Reserve Bank Of Boston Study On Mortgage Lending Revisited," Office Of Housing Research, Fannie Mae, November 1993.
- Federal Reserve Bank Of Boston, "Closing The Gap: A Guide To Equal Opportunity Lending", 1993.
- Glennon, Dennis And Stengel, Mitchell "An Evaluation Of The Federal Reserve Bank Of Boston's Study Of Racial Discrimination In Mortgage Lending, Office Of The Comptroller Of The Currency, Working Paper 94-2, April 1994.
- Hansell, Saul, "Shamed By Publicity, Banks Stress Minority Mortgages," New York Times, August 30, 1993, Page C1.
- Hirsch, James, "Critics Say A Well-intentioned Loan Plan Helped Minorities Buy Overpriced Homes;" Wall Street Journal, July 20, 1995, Page B1.
- Horne, David K., "The Role Of Race In Mortgage Lending," Fdic Banking Review, Spring/summer 1994.
- Hunter, William C. and Walker, Mary B., "The Cultural Affinity Hypothesis And Mortgage Lending Decisions, Working Paper, Federal Reserve Bank Of Chicago, July 1995.
- Leong, Thomas "
- Liebowitz, Stan "A Study That Deserves No Credit", Wall Street Journal, September 1, 1993.
- Liebowitz, S. J. "What Do Census Price-cost Margins Measure?," Journal Of Law And Economics, October, 1982, 231-246.
- Munnell, A., Browne, L., Mcneaney, J., Tootell, G., Mortgage Lending In Boston: Interpreting Hmda Data, Federal Reserve Bank Of Boston, Working Paper # 92-1, October 1992.
- Munnell, A., Tootell, G., Browne, L., Mcneaney, J., "mortgage Lending In Boston: Interpreting Hmda Data", American Economic Review, March 1996, Pp. 25-54.
- Rosenblatt, Eric "A Reconsideration Of Discrimination In Mortgage Underwriting With Data From A National Mortgage Bank." Working Paper Presented at Conference On Discrimination In Financial Services, Chicago Fed, March 1996.
- Wilke, John "Giving Credit: Mortgage Lending To Minorities Shows A Sharp Increase In 1994," Wall Street Journal, February 13, 1996, Page 1.
- Young, Ernie, "Acorn Study Reports Falling Rate Of Mortgage Loans For Minorities ", Philadelphia Daily News, September 12, 1997.
- Yezer, Anthony M., Robert F. Phillips And Robert P. Trost "Bias In Estimates Of Discrimination And Default In Mortgage Lending: The Effects Of Simultaneity And Self-Selection", Journal Of Real Estate Finance And Economics, Vol. 9 (3), 1994, 197-216.

Zandi, Mark "Boston Fed's Study Was Deeply Flawed," American Banker, August 29, 1993.

## ***Appendix 1: Internal Inconsistencies In The Data - Critique, Response, Rejoinder***

The following is a listing of the internal inconsistencies that we encountered with the data. After listing each data "error" we describe the response put forward by the Boston Fed researchers when they have done so, and then provide our reply. Although we get the last word here, we believe that we have accurately reflected the arguments of the Fed researchers.

### **Interest Rates**

Both the term of the loan and the monthly payments are included in the Boston Fed data, allowing us to calculate an interest rate on the each of the loans in the data set. Since the monthly payment generally includes taxes and insurance, our calculated interest rate will overstate the actual interest rate on the loan, although we attempt to ameliorate this bias as described below.

We discovered dozens, if not hundreds, of observations for which our imputed interest rates were either too high or too low to be believed. The majority of errors appeared to be in either the amount of the loan application or in the reported monthly housing expenditure. Since both of these variables are used to compute other variables that are central to the statistical analysis (the expense ratio and loan-to-value ratio), these cases must be classified as serious transcription errors.

For example, we found many loans with interest rates well above market. There were 155 loans with interest rates above 16 per cent and 60 loans with interest rates above 20%.

Table A1 lists a few loans with very high imputed interest rates (found in the first column) ranging from 42% to 85%. Most readers will recognize these rates as being outlandish.

**Table A1: Some Applications With Unreasonably High Interest Rates**

Imputed Interest Rate	Net Worth	Loan Amount	Loan To Value Ratio	Monthly Mortgage Expense	Yearly Income	Appraised Value of Home	Corrected Interest Rate
42%	-1,000	77,000	0.20	2,691	120,000	386,000	34%
44%	-357,000	103,000	0.67	3,792	277,000	155,000	42%
49%	103,800	40,000	0.28	1,638	32,000	142,000	44%
55%	56,000	29,000	0.21	1,336	61,000	140,000	48%
58%	-34,000	26,000	0.20	1,260	66,000	130,000	51%
61%	174,900	48,000	0.15	2,421	87,000	325,000	50%
65%	230,000	43,000	0.15	2,319	91,000	288,000	55%
70%	106,600	40,000	0.15	2,324	60,000	270,000	60%
85%	43,000	9,000	0.15	636	28,000	62,000	74%

Although property taxes and insurance are included in the monthly payments, thus overstating the true interest rates, it seemed unlikely that property taxes and insurance could even begin to account for

the very high interest rates that appear in these observations.<sup>48</sup> In a previous correspondence to the Federal Reserve's Board of Governors, however, Tootell had claimed that these high interest rates were due entirely to the inclusion of property taxes and insurance:

Low loan-to-value ratios make the housing expense for these applications seem high given the loan amount, simply because the taxes and insurance premiums on a house with a small loan relative to its value are a large percentage of the housing expense. Thus using the housing expense to impute the interest rate is completely invalid for observations with low loan-to-value ratios.<sup>49</sup>

Browne and Tootell largely repeat this:

This [imputing interest rates] is a rough technique and will not work for multi-unit properties or properties for which the mortgage loan is small in relation to other elements of housing expenses... Almost all the imputed rates that they [Day and Liebowitz] find are too high involve properties for which the loan-to-value ratios are very low (less than 35 percent) In a few cases the term of loan may be incorrect, throwing off imputations of interest rates.<sup>50</sup>

It is true that imputed interest rates for loans with loan-to-value ratios might appear unreasonably high due to the relatively greater fraction of the monthly payment that is attributable to taxes and insurance. But first note that it is definitionally true that when interest rates are too high then either the payment is too high, or the loan amount too low. Thus, it would not be surprising that loan-to-value ratios are frequently small for this group of applications if the loan amount is understated.

Most importantly, however, we can examine directly the impact of property taxes and insurance. Browne and Tootell apparently think that the possibility that high interest rates might be explained in this manner is sufficient to conclude that they are. But they did not examine whether insurance and property taxes actually did account for the unreasonably high interest rates.

As it turns out, the property tax rate in the Boston Area is publicly available information, and typical insurance payments can be easily approximated. The last column in Table A1 provides interest rates adjusted exactly in this manner. Note that although this adjustment does lower the imputed interest rates somewhat, the adjusted rates are still far in excess of the rates prevailing in the mortgage market during the sample period, and in fact, in any period that we are familiar with. The most plausible explanation for the extreme levels of these imputed rates is a serious error in either the reported amount of the loan or the estimated monthly payment.<sup>51</sup>

Naturally, there are similar problems with very low interest rates, as shown in Table A2. Here we have a set of loans with negative interest rates (there are 47 such loans in the data, and 68 if we adjust for taxes and insurance) which are part of a larger set of loans with interest rates that, from the point of view of the consumer, are simply too good to be true. For example, there are 100 fixed rate loans with imputed rates below 7% (unadjusted for taxes and insurance) and 202 fixed rate loans with rates below 7% (adjusted for taxes and insurance), at a time when interest rates were in the vicinity of 10%.

---

48 We had a footnote about the impact of property taxes and insurance in our earliest working paper.

49 Page 2 in a memo from Geoff Tootell dated January 20, 1994, sent to Laurence Lindsay.

50 Page 74 in Browne and Tootell

51 The property tax rate for Boston in 1993 was 1.4%, which we were told was higher than the 1990 property tax rate since housing prices had increased, so we set it at 1.3%. We also examined the "Places Rated" almanac to determine the highest property rates in the country, which, if used, would not have altered our conclusions. We set the insurance rate at .5%.

Table A2: Some Applications With Unreasonably Low Interest Rates

Loan Amount	Loan To Value	Imputed Rate	Loan Term	Monthly Mortgage	Expense/ Income	Yearly Income	Value Of Home
			(months)	Expense			
145,000	0.73	-109%	36	441	6	85,000	199,000
400,000	0.55	-533%	12	154	8	34,000	730,000
125,00	0.58	-10%	360	50	32	50,000	217,000
55,000	0.50	-8%	180	156	4	97,000	110,000
184,000	0.94	-6%	360	188	5	52,000	195,000
802,000	6.68	-5%	360	1024	31	58,000	120,000
182,000	0.93	-3%	360	301	32	32,000	196,000
183,000	1.11	-3%	360	306	10	42,000	165,000
75,000	0.43	-2%	360	160	25	76,000	175,000
60,000	0.35	-2%	300	162	4	80,000	170,000

Upon examining the data, it is clear that for some of the most outrageous interest rates, the reported loan term most likely has a digit missing, as the loan term is indicated to be 18 or 36 months (20 such loans), although it is conceivable that we could have some very short balloon payments. In other instances, the loan amount, or monthly payment must be incorrectly recorded.

Once again, Browne and Tootell put forward an explanation:

Almost all of the imputed rates that they [Day and Liebowitz] conclude are too low involve two-to-four-unit properties, for which the housing expense is reduced by rental income.<sup>52</sup>

There are two types of errors indicated in this explanation. As a factual matter, of 47 loans with negative interest rates, defined without adjusting for insurance and taxes so as to be consistent with Browne and Tootell, only 10 are multi-unit properties. For 80 loans with interest rates below 4%, only 30 are multi-unit homes. Thus not even a majority, to say nothing of Browne and Tootell's "almost all", are two-to-four unit properties. Three of the loans in the table above are multi-unit homes.

Browne and Tootell state that for multi-unit homes, banks reduce the monthly payment on the mortgage by the expected rental income. This is possible, but if it were true as a general rule we should expect to see that multi-unit homes would have below normal interest rates and, as well, four unit homes should have lower interest rates than three unit homes, and so forth. In fact, what we find is that four-unit homes have higher imputed interest rates than single family homes, (12.2% to 10.6%) whereas two and three unit homes have average imputed rates of 9.2% and 9.3%. What actually appears to happen in this market is that banks sometimes reduce the monthly payment by the rental amount when that is necessary to help get the loan approved.

This leads to a serious conceptual error in the econometrics. Since rent from tenants is sometimes subtracted from monthly payments and sometimes added to income, two economically indistinguishable applications for multi-unit homes might have very different measured income and monthly housing payments. One application might add rental income to the denominator of the

---

52 Browne and Tootell, page 74.

obligation ratio, the other might subtract it from the numerator. When the ratio of mortgage payments to income is constructed in these two different ways how can any analysis using the ratio as an independent variable be expected to provide useful results? One answer might be to remove multi-unit homes from the analysis as we had suggested in earlier work, but Browne and Tootell warn us "splitting the sample is not justified."<sup>53</sup> They apparently never realized that their own analysis requires that they do exactly that.

### **Loans Rejected And Sold**

There were 44 mortgages that were classified as rejections although they were also classified as having been sold in the secondary market. This is clearly impossible, but it does not appear to be a random transcription error since 41 out of the 44 mortgages were applications from minorities, an event very unlikely to happen by chance.

MTBM have not attempted to explain how a loan that is rejected can then be sold to the secondary market. Instead they note that both the mortgage disposition (accept/reject) variable and the variable indicating if and to whom a mortgage is sold come from the original HMDA data, and not from the additional data collected in their survey.

Day and Liebowitz also note that some rejected mortgage applications were apparently sold. Both [variables] came from the original HMDA survey... We did not use data on loan sales and did not try to validate these figures.<sup>54</sup>

Thus, they claim that this error is not their doing. Upon further examination, however, it appears that these errors were not in the original HMDA data. The original HMDA data for the Boston MSA, which consists of over nineteen thousand (conventional) loans, contain only ten observations which have the attributes (error) of being both sold and rejected. Only one of these is a minority observation. So, at a minimum, 40 of these 44 inconsistencies are not in the original HMDA data. Further examination revealed that none of the 44 errors in MTBM's data matched the HMDA errors, so we can actually attribute all of these errors to MTBM's data set. The variables may have the same names as those in the HMDA data, but the inconsistent values appear to have developed during the creation of the Boston Fed data set.

### **Net Worth**

Our review of the reported net worth of applicants in the Boston Fed sample revealed that there are at least 5 mortgage applications where the applicant has a net worth of less than negative one million dollars. But this is only the tip of the iceberg. Assuming that 50% of income was used to repay indebtedness, there are 27 mortgage applicants who would need more than ten years to pay their (prior to this mortgage) debt off, even if the interest rate were zero.

Table A3 lists five extreme cases of negative net worth where the mortgage application was approved.

The application on the first row indicates an applicant with almost eight million dollars in net debt, and an income of thirty thousand dollars, being approved for a mortgage of fifty five thousand dollars. To us this seems unreasonable. For this loan, as well as many others, the applicants do not have sufficient income to even pay off the interest on their debt, even at a modest 10% rate of interest. Is this evidence of data errors? Not according to Browne and Tootell. Here is their explanation:

---

53 Brown and Tootell, Page 73.

54 Brown and Tootell, Page 74.

Thus [Liebowitz] cites as obvious examples of errors applicants who were approved for loans despite having negative net worth. This is an effective rhetorical technique since, at first glance it does seem odd that someone with negative net worth would be approved for a loan. On reflection, however, one can posit many reasons for why a negative net worth would not preclude receiving a loan, particularly as the net worth figures do not include the value of human capital.<sup>55</sup>

Table A3: Approved Loans That Seemingly Can Never Be Paid Off						
Net Worth	Loan Amount	Imputed Interest Rate	Loan Term	Monthly Mortgage Expense	Yearly Income	Home Appraisal
-7,919,000	55,000	18%	180	895	30,000	174,000
-4,333,000	103,000	12%	360	1,030	51,000	114,000
-4,288,000	145,000	-109%	36	441	85,000	199,000
-1,969,000	187,000	16%	360	2,553	165,000	390,000
-1,483,000	160,000	13%	360	1,718	78,000	234,000

Browne and Tootell seem to have come up with an effective rhetorical technique of their own: tell only part of the story. They neglect to tell the reader that these negative net worth figures are in the millions of dollars. They go on:

We chose not to exclude unusual observations from the data base because we had no standard, other than intuition, for what were reasonable values... For example, there were physicians with very large assets and even larger liabilities... Other researchers can choose to drop these observations.<sup>56</sup>

We find it implausible that these observations represent loan applications from medical doctors since the level of debt is far beyond the cost even of medical school and the incomes seem to low for doctors. There is a deeper issue involved here as well: Does common sense and intuition have no role in economic analysis?

### Loan-to-value Ratios And Mortgage Insurance

The authors of the Boston Fed study state on page 17 of their 1992 report that:

More importantly, the secondary market will not accept a mortgage loan that has a loan-to-value ratio in excess of 80 percent without private mortgage insurance. Thus, any applicant with a high loan-to-value ratio who is refused private mortgage insurance is likely to be denied the loan.

and go on to state on page 31 of their report:

A high loan to value ratio raises the probability of denial, but the effect is relatively small. This result occurs because virtually all applicants with loan-to-value ratios over 80 percent must secure private mortgage insurance."

---

55 Browne and Tootell Page 66.

56 Page 73 of B and T.

Our discussions with bankers indicate that MTBM are correct in these statements. Consequently, we were surprised to find that out of 1129 loans with loan-to-value ratios greater than 80%, 517 loan applicants, almost half, failed to apply for mortgage insurance, yet most of them were approved. Additionally, 119 applications were reported as sold in the secondary market even though the loan-to-value ratio exceeded 80% and the applicant did not apply for mortgage insurance.

There also were 55 applications with loan-to-value ratios greater than 100% (meaning that the mortgage was larger than the purchase price of the home), which we have been told by several bankers is very unusual, yet 20 of these loans were approved. Additionally, there were 123 applications having loan-to-value ratios in excess of .95, most of which were also approved. This is particularly surprising since .95 is usually the maximum allowable loan-to-value ratio. Many of these loans are recorded as having been sold in the secondary market even though they were in clear violation of secondary market requirements.

After we reported these facts,<sup>57</sup> the Boston Fed researchers decided that the need for mortgage insurance was not as great as they stated in 1992. Browne and Tootell state:

Because Fannie Mae generally requires mortgage insurance on high loan-to-value loans, the existence of such applications, [Liebowitz] argues, is proof of error. But while the secondary market usually requires mortgage insurance on such loans, exceptions can be made. More importantly, many of these applications were denied and others were kept in the lenders' portfolio and, thus, not subject to secondary market guidelines.<sup>58</sup>

Is it really reasonable to conclude that hundreds of loans in this sample were "exceptions"? Also, it is our understanding that even when loans are not sold, they generally conform to secondary market guidelines.

### **Income And Expense/Income Ratios**

The Boston Fed collected data on both the monthly employment income and monthly other income for both the loan applicant and the co-applicant. In addition, the original HMDA data set includes a separate variable for the aggregate yearly income of the applicant and the co-applicant. One might expect that these variables should be consistent with each other. Yet, there are 157 applications where the two measures of income differ by more than 20% and 66 cases where the difference is more than 50%. Some of these instances can be found in the two rightmost columns of Table 4A.

As a defense to the difference between the HMDA income and the Fed extended income variable, Browne and Tootell state:

Another misstatement [by Liebowitz] is categorizing as errors observations where yearly and monthly income figures do not agree. The yearly figures are from the lenders' original HMDA submissions. They were not part of the Boston Fed survey nor were they used by the Boston Fed, although they were made available to researchers as part of the public data set. The Boston Fed did not use the HMDA income figures and instead requested the monthly income figures from the loan applications form because the latter were more precisely defined.<sup>59</sup>

---

57 We pointed out many of these problems in Liebowitz (1993), letters to the Fed, a seminar at the Dallas Fed, and various working papers.

58 Browne and Tootell, page 66.

59 Browne and Tootell, Page 73.

**Table A4: Inconsistent Measures of Income**

Expense to Income Ratio in Boston Survey	Calculated Ratio using monthly total income	Calculated Ratio using monthly employment Income	Calculated Ratio using yearly HMDA data	Yearly Income (HMDA; 000's)	Calculated Income From Fed Survey
35	41.12	44.06	35.25	35	30
26	41.03	45.90	26.13	44	28
47	42.74	67.46	47.17	46	51
28	11.28	43.52	27.81	58	143
13	49.82	63.62	13.78	120	33
35	49.89	74.58	35.68	60	43
23	230.31	230.31	23.02	61	6
29	39.67	41.22	29.41	43	32
28	43.75	46.12	27.93	59	38
28	33.75	34.44	27.98	64	53
17	39.13	57.02	17.03	101	44
29	287.29	287.29	28.66	140	14
34	25.29	28.35	34.07	23	31
33	61.38	71.41	32.74	53	28

There are several problems with their claim. First, the Table A4 presents numerous instances (out of a much larger group) where the expense/income ratio collected in the Fed survey (left column) fits better with the HMDA yearly data (fourth column) and not the "more precisely defined" Boston Fed monthly data (second and third column). Of course, an alternative explanation is merely that the monthly income in the Fed survey is a data error, or perhaps the reported expense/income ratio is at fault. Given the plethora of data errors, either of these possibilities would come as no surprise.

Note that the Boston Fed collected data on the monthly housing expense as well as the ratio of monthly housing expense to monthly income, which is then used as an explanatory variable in their study. We used the Fed collected monthly income and monthly housing expense figures in our checks for consistency with the reported expense to income ratio. When we constructed the ratio of housing expense to income, we were often unable to replicate the ratio that is included as a separate explanatory variable.<sup>60</sup> The differences that we find are quite dramatic. In 55 instances the deviations in the ratios are greater than 50%, in 140 cases the deviations are greater than 25% and in 337 cases the deviations are greater than 10%.

What this says to us is that for the hundreds of cases where the calculated ratio disagrees with the reported ratio, either the monthly expense is misstated, or the monthly income, or the income/expense ratio. The Fed researchers wish to assume that the expense to income ratio is correct, since it is an important variable in the regressions, and that all the errors are in the separate numerator and denominator. But this appears to be just wishful thinking. For example, Table A5 presents cases where it seems fairly easy to conclude that the expense to income ratio is improperly recorded.

---

60 In other words, the Boston Fed data include as separate variables the numerator, the denominator and the ratio, yet the three variables are inconsistent.

**Table A5: Inconsistent Expense to Income Ratios**

Expense to Income Ratio in Fed Survey	Calculated Ratio using monthly total income	Calculated Ratio using monthly employment	Calculated Ratio using yearly HMDA data Income
0	31.44	34.39	32.45
3	24.76	53.94	33.41
38	78.08	78.08	80.24
67.96	20.86	30.12	24.16
0	71.14	104.04	68.84
43	18.83	22.22	18.78
0.29	29.00	34.52	34.80
0.14	13.55	14.75	14.75
37	26.91	26.91	47.38
58	43.38	43.38	29.52
0.37	36.73	36.73	36.73
0	26.74	26.74	26.74
0.35	35.33	42.79	43.81
0	37.17	42.70	37.12
0.085	32.85	32.85	15.19
38.24	20.83	25.52	17.62
4	27.32	27.32	38.46
0	68.10	211.12	67.38
110	11.50	13.86	11.50
110	11.50	13.86	11.50
0.29	28.56	36.79	321.05
0	45.90	45.90	45.89
4	18.25	20.06	18.18
66.1	77.88	92.27	44.82
47.2	18.65	29.89	20.10
0	49.75	74.61	48.49
0.52	52.45	90.38	52.17

Among these cases are seven observations where the expense to income ratio was reported as zero, an impossibility if the loan was to be paid off. In other instances, it is quite clear that the ratio had the decimal point in the wrong place. In yet other cases the reported ratio is unreasonably high compared to the calculated ratio. These are, however, a minority of the cases where the reported ratios differ from the calculated ratios. In most of those (as in Table A4) we really can not be sure where the error leading to the inconsistency lies.

### **Miscellaneous Problems**

The same observation appears twice, but since both were rejected it could be the same applicants applying at two different banks.

There are 3 loans that were approved with expense to income ratios greater than .70, and an additional 8 with ratios over .50 (.28 is the usual cutoff). There were 3 applications with expense/income ratios over 1.

There were 9 applications with obligation to income ratios over 100%, one of which, remarkably, was listed as approved. Five out of 23 applications with obligation/income ratios above 70% were approved (36% is the normal cutoff).

There were 5 applications with obligation/income ratios of less than 1%. Why even bother getting a loan under these circumstances?

There were 6 applications where the obligation ratio is reported to be less than the expense ratio, although that is impossible.

## ***Appendix 2: Matching The Fed-Extended And HMDA Data***

Our merging procedure consisted of taking the 1990 HMDA data for the Boston MSA and removing all loan applications that were not considered in the Boston Fed sample. These included loans not for the purchase of a home, or that were insured by the government (FHA, VA, FmHA), or that were made to individuals classified as belonging to a race other than white, Hispanic or black. This reduces the number of loans from 50,484 to 19,163. Removing loans from banks with less than 25 loans (to further mimic the Boston Fed) reduced the sample to 17,885, with 1299 black and Hispanic applications. It is from this group that the Boston Fed derived their sample of 2932 applicants, with 685 minority applicants.<sup>61</sup>

Using nine HMDA variables as a key,<sup>62</sup> we matched the two samples. First we removed any observations that contained duplicate information for these nine variables. This reduced the HMDA sample by 14%, to 15419 cases, and the Boston Fed sample by 3%, to 2833 cases. Then we matched the HMDA data to the Boston Fed data using these same nine variables. We were able to successfully match 2174 cases, or approximately 77% of the remaining Fed-extended sample. Table A6 illustrates the process of matching the data.

<b>Table A6: Matching HMDA and Boston Fed data</b>	
1. Observations In Fed-extended Data	2932
2. Unique Observations Based On Key Variables	2833
3. Observations In HMDA Data (MSA1120)	50484
4. Removing Refinancings, Government Insured And Races Other Than White, Black And Hispanic	19163
5. Eliminating Banks With Less Than 25 Loans	17885
6. Unique Observations Based On Key Variables	15473
7. # Of Unique Permutations Of Key Variables For 2412 Duplicate HMDA Observations	1061
8. Matches Of 2833 Fed-extended And 15743 HMDA Observations	2174
9. Unmatched Fed-extended Observations [2-8]	659
10. Matches Of 2833 Fed-extended And 1061 HMDA Observations	228
11. Unmatched Fed-extended Observations [9-10]	431

Because there were 2833 cases in the Fed-extended data that were unique with respect to the key variables, but only 2174 observations were matched, this left 659 instances where the Fed-extended data did not match up to the HMDA data. Our inability to match some of these 659 cases was due to non-unique cases in the HMDA data. We calculated the number of unmatched cases that were due to nonunique HMDA data in the following way. For the nonunique HMDA observations, we formed a

---

61 Again, this is based on the Fed's publicly available data, not the sample on which their 1992 report or 1995 paper is based, each of which differs slightly.

62 The variables were: loan action, race and sex of applicant and co-applicant, income, loan amount, if and by whom the loan was purchased, and whether the applicant intended to occupy the home.

list of all unique combinations of the 9 key variables. This list was then matched with the Fed-extended data to see how many "non-matches" were due to "duplicate" HMDA observations. Every observation in the Fed-extended data should have been a match with some observations in this complete list of all permutations of the nine variables found in the HMDA data. In fact, only an additional 228 of the 659 unmatched Fed-extended observations were matched in this experiment. Thus, we conclude that at least 431 observations in the Fed-extended data do not match up with the HMDA for those variables that were supposed to be common between the two data sets. This estimate of 431 is biased downward slightly since it does not include the 99 observations in the Fed-extended data set that were not unique with respect to the nine key variables. It is impossible to know how many of these latter observations have a match with the HMDA data.

For the 431 non-matched observations, our working hypothesis is that the Fed researchers most likely made some errors in their manipulation of the data, although the cause of the mismatch is largely irrelevant to our purposes.<sup>63</sup>

---

<sup>63</sup> It is possible that the Fed researchers worked with an early and error prone sample. But even if the differences in the data were not the fault of the Fed researchers, the later data should allow a more accurate answer to the questions at issue.

**Table A4: Inconsistent Measures of Income**

Expense to Income Ratio in Fed Survey	Calculated Ratio using monthly total income	Calculated Ratio using monthly employment	Calculated Ratio using yearly HMDA	Monthly Employment Income	Monthly Employment Income Co- Applicant	Monthly Total Income Applicant	Monthly Total Income Co- Applicant	Monthly Mortgage Expense	Yearly HMDA Income (000's)	Calculated total Income
17.5	25.32	25.32	17.35	2500	1212	2500	1212	940	65	45
21.6	36.98	36.98	21.64	2500	2083	2500	2083	1695	94	55
29	79.08	79.08	27.67	1458	0	1458	0	1153	50	17
35	41.12	44.06	35.25	2333	0	2500	0	1028	35	30
26	41.03	45.90	26.13	0	2087	0	2335	958	44	28
31.5	40.86	40.86	31.92	1953	0	1953	0	798	30	23
47	42.74	67.46	47.17	1340	1340	2890	1340	1808	46	51
28	11.28	43.52	27.81	1833	1255	7150	4765	1344	58	143
36	29.45	29.45	36.00	2750	0	2750	0	810	27	33
21	16.01	16.01	21.10	4002	6872	4002	6872	1741	99	130
26	41.18	41.18	26.42	1246	1481	1246	1481	1123	51	33
28	55.91	55.91	27.84	884	1647	884	1647	1415	61	30
37	49.85	49.85	37.14	1306	1302	1306	1302	1300	42	31
10.2	27.57	27.57	10.92	4583	833	4583	833	1493	164	65
13	49.82	63.62	13.78	2166	0	2766	0	1378	120	33
35	49.89	74.58	35.68	2392	0	3576	0	1784	60	43
23	230.31	230.31	23.02	508	0	508	0	1170	61	6
23.2	16.30	16.30	23.00	6000	0	6000	0	977.7	51	72
30	54.98	54.98	30.20	2687	1387	2687	1387	2240	89	49
27	37.87	37.87	27.45	2304	2167	2304	2167	1693	74	54
29	66.45	66.45	28.18	1696	0	1696	0	1127	48	20
35	77.66	77.66	35.33	1213	0	1213	0	942	32	15
29	39.67	41.22	29.41	1430	1127	1530	1127	1054	43	32
28	43.75	46.12	27.93	1621	1356	1782	1356	1373	59	38
34	28.47	28.47	33.89	4166	0	4166	0	1186	42	50
28	33.75	34.44	27.98	2816	1516	2905	1516	1492	64	53
27	49.14	49.14	26.74	3583	1950	3583	1950	2719	122	66
27.8	36.93	36.93	28.00	3033	0	3033	0	1120	48	36
22	20.50	20.50	22.02	4384	1793	4384	1793	1266	69	74

22	37.08	37.08	22.00	2818	0	2818	0	1045	57	34
17	11.90	11.90	17.13	3987	2611	3987	2611	785	55	79
17	39.13	57.02	17.03	953	1560	1517	2145	1433	101	44
29	287.29	287.29	28.66	1164	0	1164	0	3344	140	14
34	25.29	28.35	34.07	2303	0	2582	0	653	23	31
27.6	33.22	33.22	27.76	3760	0	3760	0	1249	54	45
33	61.38	71.41	32.74	2025	0	2356	0	1446	53	28