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REPRINT No. 16

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Reprinted from The Bell Journal of Economics
Vol. 12, No. 2, autumn 1981

An Investigation of Sex Discrimination in Commercial Banks' Direct Consumer Lending

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This article develops a model of prejudicial discrimination in the credit markets. Data on 30,000 commercial bank consumer loans were used to test the model. No systematic pattern of prejudicial sex discrimination was found-even before the Equal Credit Opportunity Act (ECOA) was passed. Instead, banks as a whole behaved as profit maximizers, and made loans on equivalent terms to equally risky customers, regardless of sex. These findings suggest that future regulatory initiatives, such as proposed extensions of ECOA, should be thoroughly scrutinized whenever they impose substantial costs under the assumption that firms are not profit maximizers.

1. Overview

M In 1974 the U.S. Congress enacted the Equal Credit Opportunity Act (ECOA) to eliminate discrimination in the granting of credit based upon a credit applicant's sex -or marital status. Subsequent amendments prohibited credit discrimination based on age, race, national origin, religion, political affiliation, or receipt of welfare benefits. Further amendments have been proposed that would ban credit discrimination based on occupation, employment, or residential location.

The driving force behind the enactment of ECOA was testimony presented before the National Commission on Consumer Finance (1972, pp. 153-156) and the 93rd U.S. Congress (1973a, 1973b, 1974). That testimony presented largely anecdotal evidence that women were not treated on an equal basis with men in the credit markets, particularly in the mortgage credit markets. However, the cumulative effect of that testimony was to convince Congress, and even creditors themselves, that discrimination against women existed more generally in the credit markets. For instance, the then President of the American Banking Association, Eugene Adams, was quoted in a reprinted speech (1973, p. 22, col. 3) as saying, "I think we have to acknowledge that banks, along with the rest of the credit industry, do in fact discriminate against women when it comes to granting credit. The question then becomes, is that discrimination justified?"[1]

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The author wishes to acknowledge significant help from his wife, Carol, who cooperated with him on an earlier paper and provided advice on this article. He also wishes to acknowledge David Bivin for providing valuable computational assistance and John M. Barron, Robert W. Johnson, William C. Dunkelberg, and an anonymous referee for providing helpful comments on earlier drafts of the article. Any errors and all opinions contained in the article are solely his own.

[1] This quotation reflects a semantic confusion that ran throughout the hearings and regulatory proceedings associated with the ECOA. in much testimony on the ECOA, the distinction between ',economic discrimination," based on relative credit risks and returns, and discrimination based on prejudice was not finely made. Thus, allegations were often made, as in Adams' speech (1973), that creditors "discriminated against" women in the granting of credit, even when the basis for such discriminated might have been "economic" rather than "prejudicial."
The ECOA tried to resolve this issue by stating that its purpose was to make credit available to all "creditworthy" borrowers without regard to sex or marital status. Such terminology could have justified the banning of only "prejudicial" discrimination without implying that the credit markets should provide a subsidy to protected classes of individuals to compensate for possible job or social discrimination that affected default probabilities. However, the initial drafters of Regulation B ignored this distinction. As a result, the ECOA as implemented by Regulation B, interferes with the making of economically efficient credit judgments because it limits the amount of information that can be used in making such judgments (Chandler and Ewert, 1976).

In credit markets, as in insurance markets, a certain amount of "economic discrimination" must take place-based on each credit applicant's potential risk. Economically, discrimination between good and bad risks is essential if a creditor wishes to avoid excessive credit losses. For that reason, this article uses the term "discrimination" to refer solely to "prejudicial discrimination." "Prejudicial discrimination" or "uneconomic discrimination" systematically causes the expected present value of loans made to members of one group to exceed the expected present value of equivalent loans made to other groups.

At the time the ECOA was enacted, only anecdotal evidence existed to suggest that sex discrimination was a serious problem in the credit markets. By random chance, however, since markets do not always work perfectly, one would always expect some members of one sex to be treated either more or less advantageously relative to equivalently situated individuals of the opposite sex. Thus, one should expect to find anecdotes that show discrimination against either sex. Consequently because of the costliness of the ECOA (Board of Governors of the Federal Reserve System, 1978; Smith, 1977), careful statistical work should have been conducted before the ECOA's enactment to determine the potential magnitude of the problems to be solved.

The major purpose of this article is to test for the existence of systematic prejudicial discrimination in the credit markets. This study is organized as follows. First, it develops a model of credit market discrimination and derives its implications. Second, it argues that the discrimination model does not apply to banks. Third, it uses data on bank loans to test whether the nondiscrimination hypothesis, as the null hypothesis, should be rejected in favor of the alternative, credit discrimination, hypothesis. Finally, it suggests that potentially costly regulations, such as the ECOA or its proposed extensions that assume firms do not act as profit maximizers should be subjected to statistical scrutiny before they are enacted.

2. A model of bank credit discrimination

In the absence of discrimination, we would expect a profit-maximizing creditor to make any loan for which the expected present value, PV, was nonnegative. However, if individuals who have a "taste to discriminate" adjust perceived prices or wages as posited by Becker (1971), the expected present value that such a lender requires to make a loan would be adjusted by a discrimination coefficient. Thus, for a potential discriminator, the decision criterion for granting a loan would be that the expected "adjusted present value," APV, be non-negative, where, for any type of loan, the APV is defined as follows:[2]

[2] Note that the "taste for discrimination" factor in this model reflects purely "prejudicial" discrimination. One sex could still be "economically" discriminated against, in the sense that a lower proportion of credit applicants from that group would obtain credit (given equal loan rates) if a higher proportion of its members had high default probabilities.

$$APV_j^i = PV_j^i + DC_j^i = (1 - p_j^i)G_j^i + p_j^i L_j^i + DC_j^i \quad (1)$$

for all homogeneous types of consumer loans, j, and all borrower (sex) types, i, where

APV_j^i = the adjusted present value for loan type j made to borrower type i;
PV_j^i = the present value of loan type j to borrower type i;
DC_j^i = the discrimination coefficient for borrower type i on loan type j (DC will be negative if a borrower type is discriminated against);
p_j^i = the probability of default on loan type j by borrower type i;
G_j^i = the expected present value of the gain on loan type j made to borrower type i if no default occurs; and
L_j^i = the expected present value of a loss on loan type j made to borrower type i if a loss does occur.

The case for discrimination in the context of this model[2] rests on the assumption that DC_j^i is nonzero. In particular, if bank loan officers systematically discriminated against females, one would expect to find that $DC_j^f < 0 \leq DC_j^m$, where f refers to female and m to male borrowers. Since a loan will not be made unless its expected adjusted present value to the lender is nonnegative, on equivalent loans of type j, female borrowers will have to compensate for their negative discrimination coefficient (DC_j^f) by showing promise that (i) $L_j^f < L_j^m$, (ii) $p_j^f < p_j^m$, or (iii) $G_j^f > G_j^m$.

Banks may influence L_j^f and L_j^m, the magnitudes of credit loss once a borrower defaults, by adjusting downpayment, loan maturity, collateral, or cosigner requirements. More restrictive downpayment, maturity, collateral, and cosigner requirements also decrease a borrower's probability of default. Banks can adjust the probability of default in their loan portfolios with even greater precision by rejecting credit applicants who appear more likely to default than others. Thus, a bank loan officer who discriminated against women probably would adjust his behavior so that $p_j^f < p_j^m$ and, possibly, $L_j^f < L_j^m$ as well. Alternatively, a loan officer who was inclined to discriminate might raise rates charged on loans to females so that $G_j^f > G_j^m$. If credit discrimination were widespread, many women would likely pay the premium rates to obtain credit rather than spend resources searching for a rate closer to G_j^m from another lender.

Thus, if lenders discriminated on the basis of sex, one would expect to find systematic evidence that $G_j^f \neq G_j^m$, $L_j^f \neq L_j^m$, or $p_j^f \neq p_j^m$ in a manner consistent with one of the discriminatory patterns shown in Table 1. In Table 1, loan risk, as measured by both p_j^m vs. p_j^f and L_j^m vs. L_j^f is shown in one row because factors that affect the potential loss on a loan (L) generally will also affect the

probability of loss (p) - as most factors that reduce the potential loss to the bank concomitantly increase the potential loss to the consumer, and thereby reduce the probability that a consumer will default.

TABLE 1

POSSIBLE PATTERNS OF DISCRIMINATION

CONDITIONS	$L_j^f < L_j^m$ OR $p_j^f < p_j^m$	$L_j^f = L_j^m$ AND $p_j^f = p_j^m$	$L_j^f > L_j^m$ OR $p_j^f > p_j^m$
$G_j^f > G_j^m$	I: STRONG DISCRIMINATION AGAINST FEMALES	II: DISCRIMINATION AGAINST FEMALES ON THE BASIS OF RATES BUT NOT ON THE BASIS OF CREDIT AVAILABILITY, COLLATERAL, OR COSIGNER REQUIREMENTS.	III: AMBIGUOUS CASE: HIGHER RATES PAID BY FEMALES MAY MERELY COMPENSATE FOR HIGHER RISKS OF LOSS ON THEIR LOANS.
$G_j^f = G_j^m$	IV: DISCRIMINATION AGAINST FEMALES ON THE BASIS OF CREDIT AVAILABILITY, COLLATERAL, OR COSIGNER REQUIREMENTS, BUT NOT ON RATES.	V: NO EVIDENCE OF DISCRIMINATION	VI: DISCRIMINATION AGAINST MALES ON THE BASIS OF CREDIT AVAILABILITY, COLLATERAL, OR COSIGNER REQUIREMENTS, BUT NOT ON RATES.
$G_j^f < G_j^m$	VII: AMBIGUOUS CASE: LOWER RATES PAID BY FEMALES MAY COUNTERBALANCE LOWER RISKS OF LOSS ON LOANS MADE TO THEM.	VIII: DISCRIMINATION AGAINST MALES ON THE BASIS OF RATES BUT NOT ON THE BASIS OF CREDIT AVAILABILITY, COLLATERAL, OR COSIGNER REQUIREMENTS.	IX: STRONG DISCRIMINATION AGAINST MALES

However, credit markets differ from the work environment that Becker (1971) envisioned, where contact between prejudicially discriminating parties and recipients of discrimination is frequent. In the credit market the usual contact between the lender (the potential discriminator) and the borrower is limited to the brief period when the loan application is made. Thus, discrimination (DC^f or DC^m) resulting from an employee's aversion to contact with members of another group is likely to be much lower in a credit transaction than in a work environment.

It is potentially costly for bank loan officers to justify rejections of loan applications from good credit risk females. Regardless of the sex of the borrower, good loans would raise the performance record of the portfolio for which each loan officer was held responsible. Even if a loan officer were inclined to discriminate, profit-maximizing behavior would call for management to induce loan officers to negotiate and grant all loans possible for which PV ≥ 0, and to behave as if $DC^f = DC^m = 0$ when doing so.[3]

Thus, this study took as its null hypothesis the "profit-maximization" hypothesis that $DC_j^f = DC_j^m = 0$ for all loan types j. To support the alternative "discrimination" hypothesis, one must find empirical evidence to support one of the discriminatory lending patterns summarized in Table 1.

3. Empirical tests

This study conducted empirical tests to determine whether there were any significant sex-related differences in (i) the ratio of losses to the amount of loans (Lj), (ii) the probability of loss (pj), and (iii) rates of charge (Gj) on (otherwise similar) direct bank consumer loans that were consistent with any of the patterns of discrimination described in Table 1. It considered seven major types of direct loans-auto loans secured by new cars, auto loans secured by used cars, other auto loans, loans to purchase household goods, home improvement loans, debt consolidation loans, and all other consumer loans (except those used for business purposes or to purchase a plane, boat, or mobile home)-as well as the total for all the aforementioned categories.

[3] Before the ECOA, only if a loan officer were willing to take a reduction in pay would a profit-oriented management be willing to tolerate subpar portfolio performance caused by the loan officer's discriminatory actions.

The data used in the study were obtained from 30 banks in five regions of the country. Each sample region consisted of a major metropolitan area plus surrounding environs. The data were collected by the Federal Reserve System over the period 1966-1971. In the Federal Reserve survey, sample banks reported thorough information on the loan characteristics and the socioeconomic characteristics of the borrower on 100% of the loans they charged-off (i.e., wrote off their books because of nonpayment). They reported similar information on randomly selected samples of 10% of their paid-off and new loans. Because the purpose of the Federal Reserve study was to investigate cyclical changes in credit availability, and sex discrimination in lending was not an issue at the time the data were collected, it is unlikely that the data were biased in any manner related to sex discrimination.

□ **Tests for differences in loss ratios by sex (H_0: $L_j^f = L_m^m$ vs. H_A: $L_j^f \neq L_j^m$).** Data on approximately 3,000 charged-off loans were available for use in this study. From information on the amount charged-off and the amount of loan, ratios of charged-off loan balances to loan amounts were tabulated for each of the loan types studied. The results of those tallies are shown in Table 2. They are presented in two forms. On the left-hand side they are presented as averages of charge-off/loan amount ratios for each of the various types of loan. In particular, for all N_j loans of type *j*, if CO_{ij} equals the amount charged-off and L_{ij} equals the amount of loan for each loan *i*, the left-hand-side statistics are calculated as $[\sum_i C0_{ij}/L_{ij}]/N_j$. On the right-hand side the loan-loss ratios are calculated as $\sum_i Co_{ij}/\sum iL_{ij}$ for each loan type *j*. Thus, those statistics measure average values of total charge-offs for each sex and loan type relative to the total value of loans in each sex and loan type category. We assume these statistics are distributed normally according to the Central Limit theorem. Standard deviations were constructed for each statistic and used in t-tests to determine whether there were significant differences in mean charge-off rates for loans made to males versus loans made to females. In the left-hand columns, variances and standard deviations were calculated on a per loan basis. In the right-hand columns, variances and standard deviations were calculated on a per dollar of loan extensions basis (i.e., in calculating standard deviations, the loss ratio for the *k*th loan was weighted by $L_{kj}/\sum_i L_{ij}$, or the size of the loan relative to the total value of all loans in that loan category).

The statistics presented in Table 2 show that charge-off-to-loan-value ratios for males and females are practically identical. Further, even where they are substantially different, as is the case with home improvement loans, the variance in charge-off ratios is sufficiently great that no difference (as measured by t statistics) is even close to being significant. Thus, these tests provide no evidence that the null hypothesis Ho: $L_{f/j} = L_{m/j}$ should be rejected either for any major type of direct bank consumer loan or for direct bank consumer loans in general.

□ **Tests for differences in credit availability by sex (H_0: $p_j^f = p_j^m$ vs. H_A: $p_j^f \neq p_j^m$).** These tests are based on the assumption that the *best* credit applicants of either sex will get credit because of management review of loan officers and their loan portfolios. Thus, all credit applicants with a nonnegative APV will be provided with credit, even if $DC_j < 0$, because such loans enhance the performance of a loan officers portfolio sufficiently to offset his propensity to discriminate.

If top credit risk customers of both sexes received credit, a loan officer with a taste for prejudicial discrimination would grant loans less freely to marginal risk credit applicants against whom he was inclined to discriminate. That is, if a loan's PV were small, the APV could more easily (and less conspicuously) be negative. Further, as the PV became smaller owing to higher chances of default, for a given discrimination coefficient, the probability of rejection would rise with the chance of default. As a result, if bank loan officers

TABLE 2

CHARGEOFF RATIOS AND STANDARD DEVIATIONS BY SEX AND TYPE OF LOAN, FOR BANK DIRECT CONSUMER LOANS

Type of Loan	Males	Females	Differenced and (t-test value)	Males	Females	Differenced and (t-test value)
New Auto Secured Loan	136	24		136	24	
New Auto Secured Loan	.400	.350	.050	.388	.372	.016
New Auto Secured Loan	(.276)	(.287)	(.126)	(.274)	(.302)	(.039)
Used Auto Secured Loan	387	45		387	45	
Used Auto Secured Loan	.484	.462	.022	.461	.421	.040
Used Auto Secured Loan	(.295)	(.287)	(.053)	(.289)	(.274)	(.100)
Other Auto Loans	67	9		67	9	
Other Auto Loans	.656	.632	.024	.610	.575	.035
Other Auto Loans	(.344)	(.333)	(.050)	(.367)	(.299)	(.074)
Household Goods Loans	119	13		119	13	
Household Goods Loans	.630	.646	-.016	.623	.579	.44
Household Goods Loans	(.306)	(.326)	(-.036)	(.315)	(.318)	(.098)
Home Improvement Loans	153	7		153	7	
Home Improvement Loans	.437	.628	-.191	.411	.525	-11.4
Home Improvement Loans	(.348)	(.367)	(-.378)	(.345)	(.391)	(-.218)
Debt Consolidation Loans	512	75		512	75	
Debt Consolidation Loans	.640	.665	-.025	.640	.631	.009
Debt Consolidation Loans	(.322)	(.328)	(-.054)	(.317)	(.359)	(.019)
All Other Consumer Loans (except plane, boat, mob. Home and business loans)	1018	141		1018	141	
All Other Consumer Loans (except plane, boat, mob. Home and business loans)	.666	.627	.039	.681	.641	.040
All Other Consumer Loans (except plane, boat, mob. Home and business loans)	(.328)	(.325)	(.084)	(.337)	(.348)	(.083)
All Bank Direct Loans	2475	328		2475	328	
All Bank Direct Loans	.599	.601	-.002	.565	.539	.026
All Bank Direct Loans	(.332)	(.330)	(-.004)	(.336)	(.349)	(0.054)

were to limit credit to only the best credit applicants of one sex, while giving preferential treatment to borderline credit applicants of the other sex, one would expect to find that the probability of default was significantly lower for borrowers of the sex discriminated against than for borrowers of the favored sex.

Systematic discrimination of this type implies that members of the sex discriminated against will be disproportionately represented among a sample of all "good" loans relative to a sample of "bad" loans made by discriminatory commercial banks. Provided that risk distributions of accepted applicants are similar (see Section

4), if significant sex-related differences in proportions of “good" and "bad" borrowers were found, it would be tantamount to showing that the probability of default was lower for accepted members of the less favored group than for those of the more favored group (Peterson and Peterson, 1978).

In the subsequent analysis, we used data from the Federal Reserve samples of charged-off and paid-off bank consumer loans. Exact default probabilities could not be calculated from these samples because they were collected with different sampling techniques and frequencies. However, it is possible to test whether loans to either sex were disproportionately represented in either sample. If either sex were discriminated against, the percentage representation of that sex in the sample of charged-off loans would be lower than its percentage representation in the sample of good paid-off loans.

Table 3 presents data on the number of good and bad loans of various types made to males and females. In addition, the proportions of good and bad loans made to males are recorded. The right-hand column presents t-statistics that can be used to test for the significance of differences in the proportions of loans made to males (or females, with a change in sign) in the two samples. The t-statistics were calculated on the assumption that for relatively large numbers of observations (Ostle, 1963, pp. 76-78) a binomial distribution can be approximated by a normal distribution.

Analysis of the t-statistics and of the proportions of males and females represented among all "good" and charged-off direct bank consumer loans recorded in Table 3 shows that no significant differences (at the 95%, two-tailed level) existed in the proportionate representation of each sex either for any particular type of consumer loan or for all bank direct consumer loans in total.[4] These results are consistent with the null hypothesis that for direct consumer loans, $p_{f/t} = p_{m/j}$ for all j.

Tests of differences in returns (loan rates) by sex (H_O: $G_{m/j} = G_{f/j}$ vs. H_A: $G_{m/j} \neq G_{f/j}$). Sample banks reported detailed information on loan characteristics, and the socioeconomic characteristics of borrowers for a random sample of 10% of their new consumer loans.[5] Reported data on the amount of the loan, the number and amount of monthly payments, and the amount of the final payment were used to calculate the (implied) interest rate applicable to each loan.

After eliminating (1) loans with inadequate information to compute loan rates, (2) loans with calculated rates below 3% or above 51% (as such figures were assumed to be either unrealistically low or high, possibly because of incorrect data), and (3) loans for any maturity that was not an even multiple of six months,[6] approximately 13,500 new bank direct loans remained in the sample. These were divided alternately. The first half of the sample was used to experiment with alternative specifications to explain how loan terms and differences in socioeconomic characteristics affected bank loan rates. The second half of the sample was held out and used only to generate the final regression results reported in Table 4.

[4] To achieve a 95% confidence level that no one of n simultaneous tests is statistically significant, the confidence level for each individual test should be set at x so that $x^n = .95$. In this case no adjustment of this sort was necessary, because not even one of the individual tests was, *prima facie*, significant at the 95% (two-tailed confidence level.

[5] Data on new rather than preexisting or closed-out loans were used because requested information on socioeconomic characteristics of borrowers and on loan terms were much more complete on such loans.

[6] Loans with maturities not evenly divisible into six-month intervals were deleted because some institutions record 36-month loans as 37-month loans (the last payment is due 37-months hence). If the loan were treated as a 37-month loan, an erroneous rate would be calculated. Further, if a loan were erroneously recorded as 26-months rather than 36-months, an erroneous rate would be calculated. Since the vast majority of loans in the sample had maturities evenly divisible by six months, this edit standard reduced the available data base only slightly, while substantially reducing the chance that incorrect rate calculations would result from recording errors.

Four basic models of the determinants of bank loan rates were tested. First, for each major type of consumer loan, the loan rate was regressed solely upon the sex of the borrower. In this construction, Model 1, any differences in loan rates related to differences in loan amounts, maturities, collateral, income, or other attributes potentially related to a borrower's sex would be reflected in the sex coefficient. Thus, this model would be most likely to show a sex-related difference in loan rates even if such a result were spurious. For instance, women have lower average incomes than men. As a result, they may obtain smaller loans. Since rates often are higher on smaller loans, a significant rate difference might reflect differences in job market opportunities and loan sizes by sex, rather than credit discrimination per se.

Table 3
Performance on Bank Loans By Sex

Type of Loan	# Male	# Female	% Male	# Male	# Female	% Male	t-test for differences in percentages
New Auto	1,717	184	90.3%	141	25	84.9%	+1.88
Used Auto	1,579	171	90.2	408	49	89.3	.59
Other Auto	388	45	89.6	75+	11+	87.2	.61+
Total Auto	3,684	400	90.2	625	85	88.0	+1.68
Furn. & Appliances	622	74	89.4	118	13	90.1	-.25
Radio, TV, HI FI	125	14	89.9	12+	1+	92.3	-.29+
Total Household Goods	747	88	89.5	130	14	90.3	-.30
Home Improvement	1,236	116	91.4	179	10	94.7	-1.83
Consolidate Bills	1,425	211	87.1	561	77	87.9	-.54
All Other (except plane, boat, mobile home, and business loans	3,378	550	86.0	1,057	151	87.5	-1.36
Grand Total	10,470	1,365	88.5	2,551	337	88.3	.20

+In these cases, insufficient observations exist for the normal approximation to be reliable. In particular, Ostle (1963, p.78) notes that "we should realize that, for a given n, the normal curve gives a better approximation when p is lose to ½ than when p is close to O or 1. On the other hand, if n is large enough (say 100 or more), the [normal] approximation will be satisfactory for most values for p." Ostle goes on to note that, when the sample size exceeds 100, only when p is very close to 0 or1, as in "Reliability work," the normal approximation may not be satisfactory.

The second model for loan rate determination used all objective factors relating to the terms of a loan (its size, maturity, collateral, year and metropolitan region in which it was originated, trade-in, cosigner, and down payment requirements) in addition to a sex-related dummy variable in an attempt to determine bank loan rates. This model tested whether members of one sex paid more or less to borrow once all objective factors related to the characteristics of the loan had been taken into account. It excluded all socioeconomic data related to the borrower except for the single variable, sex.

As a variation of the second model, the third model recognized that certain cross elasticity of demand considerations exist that might affect loan rates in addition to the objective factors related to a loan. Such factors, which frequently are taken into account by banks in establishing their loan policies, include: (1) whether a loan customer is a depositor of the bank, (2) whether the loan customer is an employee of the bank, and (3) whether a loan customer has engaged in previous financial transactions with the bank. In general, one would expect rates to be lower if any of these circumstances existed. Reportedly, many banks give preferred loan terms to depositors and customers, while less uncertainty should exist about the potential credit performance of previous customers.

TABLE 4

SIGNIFICANCE OF SEX VARIABLE IN EXPLAINING BANK DIRECT LOAN RATES (WHEN DIFFERING AMOUNTS OF DATA ON LOAN AND BORROWER CHARACTERISTICS ARE TAKEN INTO ACCOUNT)*

TYPE OF LOAN		NUMBER OF LOANS	COEFFICIENT FOR "FEMALE" VARIABLE			FULL REGRESSION CHARACTERISTICS			
			VALUE	STANDARD ERROR	(1-SIGNIFICANCE) BASED ON PARTIAL F	R^2	$\bar{R}^2$	F	(1-SIGNIFICANCE) OF F-VALUE
NEW AUTO	MODEL 1	1171	-.51	.28	.069	.003	.002	3.31	.069
	MODEL 2		-.45	.28	.100	.074	.056	3.99	.000*
	MODEL 3		-.26	.28	.346	.095	.074	4.60	.000*
	MODEL 4		-.48	.32	.137	.156	.113	3.60	.000*
USED AUTO	MODEL 1	1084	.18	.40	.656	.000	.000	0.20	.656
	MODEL 2		-.01	.38	.977	.110	.093	5.68	.000*
	MODEL 3		.14	.39	.712	.119	.097	5.47	.000*
	MODEL 4		.08	.47	.857	.177	.131	3.86	.000*
OTHER AUTO	MODEL 1	253	-.35	.91	.703	.001	.000	0.15	.703
	MODEL 2		-1.32	.88	.133	.288	.201	3.51	.000*
	MODEL 3		-1.17	.87	.182	.312	.223	3.49	.000*
	MODEL 4		-1.16	1.11	.297	.445	.276	2.62	.000*
HOUSEHOLD GOODS	MODEL 1	407	-2.04	.65	.002*	.023	.021	9.68	.002*
	MODEL 2		-2.51	.62	.000*	.232	.179	4.41	.000*
	MODEL 3		-2.21	.64	.001*	.248	.190	4.29	.000*
	MODEL 4		-2.75	.80	.001*	.356	.244	3.18	.000*
HOME IMPROVEMENT	MODEL 1	708	.72	.50	.145	.003	.002	2.13	.145
	MODEL 2		.28	.47	.551	.239	.211	8.56	.000*
	MODEL 3		.32	.47	.497	.251	.220	8.11	.000*
	MODEL 4		.48	.61	.430	.284	.219	4.38	.000*
DEBT CONSOLIDATION	MODEL 1	830	-.22	.38	.569	.000	.000	0.34	.569
	MODEL 2		-.29	.36	.432	.150	.123	5.45	.000*
	MODEL 3		-.15	.36	.678	.164	.134	5.42	.000*
	MODEL 4		.21	.47	.648	.222	.161	3.65	.000*
ALL OTHER LOANS**	MODEL 1	2235	-.26	.31	.389	.000	.000	0.74	.389
	MODEL 2		-.65	.30	.031*	.091	.080	8.20	.000*
	MODEL 3		-.49	.30	.103	.098	.086	7.96	.000*
	MODEL 4		-.63	.37	.093	.129	.105	5.28	.000*

*MODEL 1 USED ONLY SEX AS AN INDEPENDENT VARIABLE.

MODEL 2 USED SEX, THE AMOUNT AND TERM TO MATURITY OF THE LOAN, INFORMATION ON TRADE-IN AND DOWNPAYMENT VALUES AND THE NATURE OF COLLATERAL USED ON A LOAN, INFORMATION ON WHETHER THERE WAS A COSIGNER ON THE NOTE, AND DATA ON THE YEAR AND REGION OF THE COUNTRY IN WHICH THE LOAN WAS MADE AS INDEPENDENT VARIABLES.

MODEL 3 WAS IDENTICAL TO MODEL 2 EXCEPT IT ADDED THREE INDEPENDENT VARIABLES THAT COULD BE ASSOCIATED WITH JOINT-PRODUCT CONSIDERATIONS THAT COULD AFFECT LOAN PRICING. IN PARTICULAR, IT INCLUDED

(I) A VARIABLE THAT INDICATED WHETHER CONSUMERS EITHER DID NOT HAVE, OR THE BANK DID NOT KNOW WHETHER THEY HAD, CHECKING AND SAVINGS ACCOUNTS--AS OFTEN BANKS GIVE PREFERRED RATES TO THEIR OWN DEPOSITORS,

(II) A VARIABLE THAT INDICATED WHETHER THE BORROWER WAS EMPLOYED IN "BANKING, FINANCE, OR REAL ESTATE," AS OFTEN BANKS GIVE PREFERRED RATES TO THEIR OWN EMPLOYEES, AND

(III) A VARIABLE THAT INDICATED WHETHER THE BORROWER HAD BORROWED AT THE BANK BEFORE, AS THAT FACT COULD REFLECT AN ONGOING RELATIONSHIP THAT COULD AFFECT THE LOAN RATE.

MODEL 3 THEREFORE, INCLUDED ALL OBJECTIVE FACTORS RELATED TO THE NATURE OF THE LOAN THAT CONCEIVABLY COULD AFFECT LOAN RATES.

MODEL 4 WAS IDENTICAL TO MODEL 3, BUT ALSO INCLUDED FAIRLY COMPLETE SOCIOECONOMIC INFORMATION ON THE BORROWERS--INCLUDING DATA ON OCCUPATION, SOURCE OF EMPLOYMENT, MARITAL STATUS, DEPENDENTS, INCOME, DEBT-TO-INCOME RATIO, YEARS-ON-THE-JOB, YEARS-AT-CURRENT-ADDRESS, AND CREDIT REFERENCES. IN ADDITION, IT INCLUDED HOME OWNERSHIP AND PHONE OWNERSHIP DATA--AS SUCH INFORMATION IS OFTEN USED IN CREDIT SCORING MODELS.

**"ALL OTHER" CONSUMER LOANS, INCLUDE OTHER LOANS MADE BY BANK CONSUMER LOAN DEPARTMENTS WITH THE EXCEPTION OF LOANS MADE TO PURCHASE "PLANES, BOATS, OR MOBILE HOMES" OR "BUSINESS" LOANS.

*SIGNIFICANT AT THE 95% (TWO-TAILED) CONFIDENCE LEVEL.

The final model, Model 4, used all available socioeconomic information, including data on the borrower's sex, age, occupation, employment source, job longevity, address stability, marital status, number of dependents, home ownership, phone ownership, debt ratio, income, and credit references in addition to all independent variables used in Models 2 and 3 to explain bank loan rates. In this model possible collinearity among variables could reduce the significance of the sex-related dummy variable. Thus, if that variable were to remain significant in Model 4, a strong case could be made that the sex-variable was significantly related to rates charged by banks on new loans. Table 4 summarizes the results of these tests for all major types of consumer loans.[7]

The data summarized in Table 4 show that in only one case, household goods loans, was the sex variable significant in both the weakest (Model 1) and the strongest (Model 4) tests of its impact on loan rates. The negative sign of the sex variable (which equaled one for loans made to females) indicates that women paid significantly less for household goods credit at banks than men. Hence, if anything, it shows rate discrimination against men. In all other cases -with the sole exception of Model 2 for the "all other" loan category, where women again paid less-there was no significant difference between rates charged males and females on any type of consumer loan in any of the models tested.

Overall, the results of our tests show that the null hypothesis (H_O: $G_{m/j} = G_{f/j}$ is rejected for only one of seven types of consumer loans. That type was household goods loans, for which men apparently paid significantly higher rates than women. This finding could have resulted from a loan composition problem. For instance, men may typically purchase different types of household goods on credit than women, and rates may vary with the types of goods financed. However, insufficient information exists to test this hypothesis.

4. Limitations of the analysis

M The statistical analysis in the preceding section depends on several key assumptions that should be made explicit. First, equation (1) indicates that the ex ante probability of default for the marginal borrower of either sex should be equal if no discrimination occurs. However, the statistical tests of Section 3 were based on ex post average probabilities of default. These tests were justified on the assumption that risk distributions of accepted borrowers from each group were "similar." This assumption was necessary because only ex post average probabilities of default were observed.[8]

Whenever the risk distributions of two pools of credit applicants are similar, the finding that ex post average default probabilities are equal implies that ex ante marginal default probabilities are also equal -provided that ex ante default probabilities are realized and that all the best credit applicants are granted credit. This is illustrated by equations (2) and (3) and Figure 1. Equation (2) is

[7] Greater detail on these regressions can be obtained from the author upon request.

[8] Credit evaluation methodologies and applicant characteristics differ among firms. Thus, the only way a priori risk could be measured uniformly would be to have access to the credit decision criteria used by each sample bank. That information was not available in this study. However, if a study were made of a single firm that used a credit-scoring model, objective measures of that firm's assessment of borrowers' relative risk would be available. In that case, possible discriminatory bias could be investigated by using data on the treatment and loss performance of credit applicants that fell into the marginal zone of the credit-scoring model.

$$\frac{D_j^i}{N_j^i} = \frac{\int_0^{A_j^i} F_j^i(p) \cdot p\,dp}{\int_0^{A_j^i} F_j^i(p)\,dp}, \tag{2}$$

where

D_j^i = the number of defaults on loans to members of group i, for loans of type j;
N_j^i = the number of loans of type j made to members of group i;
F_j^i = the frequency distribution that shows the *ex ante* frequency with which credit applicants of class i have a given probability of default p, on loans of type j; and
A_j^i = the maximum probability of default that a creditor will accept for members of class i on loans of type j.

Equation (2) says that *ex post* the average rate of default, D/N, will equal the weighted average of *a priori* default probabilities for all accepted credit applicants. Thus, our finding that D_j^f/N_j^f is not significantly different from D_j^m/N_j^m is equivalent to the following equation:

$$\frac{\int_0^{A_j^f} F_j^f(p) \cdot p\,dp}{\int_0^{A_j^f} F_j^f(p)\,dp} = \frac{\int_0^{A_j^m} F_j^m(p) \cdot p\,dp}{\int_0^{A_j^m} F_j^m(p)\,dp}, \tag{3}$$

where all variables are as defined above.

Clearly, if equation (3) holds and $F_j^f(p) = F_j^m(p)$ for all p, then $A_j^f = A_j^m$. Thus, where *a priori* risk distributions of credit applicants are equivalent, the finding that *ex post* average default rates are equal implies that *a priori* marginal default (i.e., credit acceptance) criteria are also equal. Further, the equal credit acceptance conclusion holds for other cases as well. For instance, it also holds if $F_j^f(p) = \lambda F_j^m(p)$ for $p \leq \max [A_j^f, A_j^m]$ and $\lambda > 0$. Thus, when the risk distributions of accepted groups of credit applicants are either identical or

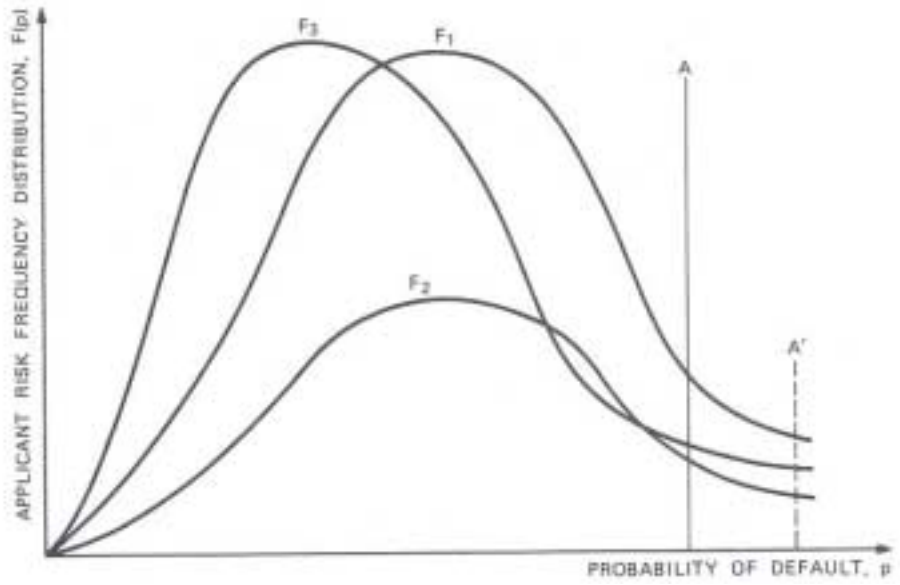
sufficiently similar, one expects that their average default rates will be equal whenever creditors impose equivalent credit acceptance criteria.

Nonetheless, the assumption that credit applicant risk characteristics are similar is a strong assumption. Figure 1 illustrates that that assumption may not in every case. In particular, in Figure 1, frequency distributions F_1 and F_2 are highly similar since $F_2=(1/2)F_1$ from 0 to the maximum risk cutoff point, A. Thus, their average default rates will be the same. However, frequency distribution F_3 represents a group with proportionately more very low risk credit applicants than the other two distribution. Thus, ex post, the average default rate for the group represented by that distribution will be below that of the other groups even if the acceptance criterion, A, were the same for each group. Consequently, equal average loss rates for groups 1,2, and 3 could exist only if lower marginal credit acceptance standards, A', were imposed for group 3 than for groups 1 and 2; but that would imply that the marginal risk members of groups 1 and 2 were discriminated against relative to the marginal risk members of group 3. Thus, if the (plausible) assumption that a priori risk distributions are similar for male and female credit applicants is not correct, then discrimination against one group could exist even if average default rates were equal for both groups.

A second important assumption was also made in Section 3. That was the assumption that all discrimination would occur at the margin – and above that margin all qualified applicants would be accepted while, below that margin all higher-risk applicants would be rejected. This assumption allows us to integrated

from O to $A_{m/j}$ or $A_{f/j}$, as the case may be, to calculated average default risk in equations (2) and (3). It also is necessary to eliminate the possibility of random discrimination (i.e. discrimination unrelated to credit worthiness in general). However, it is possible that some creditors could practice discrimination in general, yet engage in sufficient random behavior that their discriminatory practices were not obvious. For instance, a creditor could discriminate against females in general by not making loans to low, or even marginal risk, credit applicants unless they were beautiful. However, loans to beautiful women could cause sufficient losses that the pooled data would not indicate that any discrimination had occurred-when, in fact, both men and nonbeautiful women were discriminated against relative to beautiful women, and nonbeautiful women were discriminated against relative to men. While it is highly probable that creditors will not behave as posited in this example, it is not certain. Thus, the assumption that all discrimination is related to credit risk is also important to the results obtained in Section 3.

FIGURE 1



Finally, the statistical analysis may be affected by the fact that data were obtained from numerous banks in several regions of the country. Conceivably, in the pooled data, discriminatory practices by one bank could either be masked by the opposite behavior of other banks or diluted by the weight given other banks in the sample. This would particularly be true if banks determined loan rates differently.

However, diluted data would still be expected to provide evidence of discrimination, if it existed. In particular, if even one bank discriminated, the mean values of the pooled data would still reflect its discriminatory practices even if, because of data pooling, regression variances were large and coefficients were not statistically significant. Thus, by analyzing mean differences in coefficient values, it may be possible to discern discriminatory practices that otherwise might be masked by high variance.

A review of the available data does not indicate that any hidden sex discrimination against females existed that was masked by high variances. An analysis of all tests conducted on seven types of loan categories (new auto, used auto, other auto, total household goods, home improvement, debt consolidation, and "all other" loans) and presented in Tables 2, 3 and 4 indicates that (ignoring variances altogether) observed differences in

means suggest that discrimination against females is possibly indicated in only 20 of 49 tests, while discrimination against males is possibly indicated in the other 29 tests. Further, in no case where all seven separate tests were conducted (two on loss ratios, one on probabilities of loss, and four on interest rates) were more than five of the tests consistent with the hypothesis of discrimination against either males or females. Thus, even if variances are ignored, comparisons of means alone do not indicate that any systematic patterns of discrimination existed in bank lending.

Overall, then, the statistical analysis presented in this article has several limitations. Those limitations occur because several strong (but not implausible) assumptions must be made to relate the statistical tests to the theoretical model. In addition, the use of data obtained from many banks may increase estimated variances, thereby reducing the significance of various statistical tests. However, analysis of mean differences without regard to variances does not suggest that systematic patterns of discrimination existed against either sex.

5. Conclusions and implications

A number of tests were conducted to determine whether commercial banks systematically engaged in prejudicial credit discrimination before the enactment of the ECOA. Overall, the tests provided extensive evidence that commercial banks did not systematically discriminate against potential borrowers based upon their sex before ECOA was passed.

These findings are important because they suggest that legislation based on anecdotal evidence may be misdirected. That is particularly likely to be the case if the legislation is based on the premise (as both the ECOA and proposed extensions to the ECOA are) that firms are not profit-maximizing institutions. While such legislation may still provide clear benefits, the benefits may be less extensive than expected and the costs of compliance may be higher than would be the case if specific problem areas had been better defined before regulators moved to rectify them.

The ECOA was a costly piece of legislation. It imposed substantial compliance costs (Board of Governors of the Federal Reserve System, 1978; Smith, 1977) and limited creditors' abilities to use certain information in credit evaluation. This reduced the efficiency of credit evaluation systems-with possible adverse effects on the intended beneficiaries (Chandler and Ewert, 1976).

The ECOA also provided a number of benefits. It stopped, more quickly than market forces were doing, the offensive practice of some creditors of refusing to provide working married women with credit accounts in their own name. Relatedly, it prevented creditors from canceling women’s' accounts, but not men's, just because they married. It also prevented creditors from asking offensive questions regarding women credit applicants' fertility, birth control and child-bearing plans, and it made it possible for married women to establish their own credit record. Further, it provided for substantial fines to be levied upon creditors who were found to be engaging in "discriminatory" behavior.

However, some of the ECOA's presumed benefits may have been overstated. The results of this article find no systematic credit discrimination before the enactment of the ECOA.[9]

These findings also suggest that ECOA regulations could have been designed so they achieved their objectives in a less costly manner. Since creditors, as a group, apparently avoid prejudicial discrimination,

[9] In the case of commercial banks, it is often alleged that they are insulated from competition by regulators who do not wish them to fail. Thus, they can afford to discriminate prejudicially if they wish. However, regardless of whether they operate in competitive markets or not, it is not in their interest as profit maximizers to discriminate.

ECOA enforcement efforts likely would be less costly if they were focused only on creditors alleged to discriminate, rather than on all creditors. Such creditors could be subjected to intense statistical scrutiny (possibly of the sort proposed in this article)[10] to test for prejudicial discrimination. If the statistical tests confirmed discrimination, substantial penalties could be imposed. If the penalties were large enough, creditors would modify their behavior to avoid them. Meanwhile, creditors that did not discriminate would not bear substantial compliance costs.

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[10] There are some possible patterns of discrimination for which these tests are not adequate. For instance, in the tables presented in this article, it is clear that a higher proportion of loans were made to men rather than to women. Other data showed that male borrowers were more likely to be married than female borrowers. No data were available, however, to indicate whether married females did not choose, so frequently as men, to borrow from the banks in their own name (since these data were primarily drawn from the 1960s) or were not allowed to do so. Detailed records of all loan applications would be needed to determine whether banks systematically denied individual credit to married women.