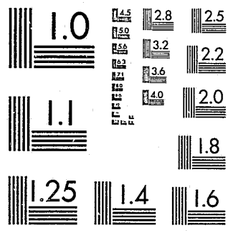

AID TO FAMILIES WITH DEPENDENT CHILDREN: AN ANALYSIS OF GRANT OVERPAYMENTS

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EXECUTIVE SUMMARY

This report presents a detailed analysis of the character, extent, and prospects for controlling overpayment of grants to AFDC recipients.* The research is based on information on AFDC recipients' income and family structure, which is both reliable and collected independently of the AFDC system. These data are available as a result of HEW-sponsored income maintenance experiments in Seattle (SIME) between 1970 and 1976 and in Denver (DIME) between 1971 and 1977. Detailed data on the monthly income and composition of families were collected by the income maintenance experimenter (SRI International) study teams. Information collected by the SIME/DIME research team should match similar information reported to AFDC program staff by recipient families. Estimates of individual family grant overpayments were generated by comparing the grant amounts calculated using data as reported to the income maintenance experimenters to the grant calculated using parallel data that the same households reported to the AFDC program.

Overpayments result from misreporting of household size and composition and from income underreporting. These inaccurate reports of household status may reflect errors or deliberate fraud and abuse. While our analysis can not distinguish between these two sources, errors would seem as likely to result in grant underpayments as overpayments. We have determined that the combined effects result in average monthly overpayments ranging between $31 in Seattle for a family with a single female head of household partially reporting earnings to $324 in Denver for a two-parent family reporting zero income to AFDC while actually working. Obviously, two-parent families usually have more opportunity to underreport income than do families with a single head of household. Income underreporting is often combined with family structure misreporting. Of AFDC families actually having male heads of household, 47% and 42% in Seattle and Denver respectively, failed to

* This research has profited from comments by Theodore Lyman, the project director, John Gardiner, and Stephen Hitchner and Philip Cook of the Department of Justice. However, any remaining errors or inaccuracies in this report are attributable to the authors.

report the existence of the male head to AFDC. However, fewer than 10% of all AFDC families reported an additional preteenage child or failed to report the existence of a teenager with earnings.

Average monthly earnings unreported were $165 for single headed Seattle families, $353 for dual-headed families, and $96 and $289 respectively in Denver. The ratios of unreported earnings to the AFDC grant depends on whether the family is totally or partially not reporting earnings. If no earnings are reported, then the ratios are 2.59 and 4.64 in Seattle and Denver respectively. If earnings are partially reported, the corresponding ratios are .97 and .85. Essentially, the only earnings which are reported are those of the female heads of households. In Seattle 78% and in Denver 51% of their earnings are reported. Less than 5% of the male heads of households' earnings are reported. In Seattle, about 17% of other household members' earnings are reported, in Denver, none seem to be. Of nonwage income, 22% is reported in Seattle and 48% in Denver. Alimony is a particularly interesting category of nonwage income since Seattle has a program where alimony payments are assigned directly to the State, while Denver has no such program. In Seattle, 37% of alimony payments were reported to AFDC, while in Denver, few, if any, were.

We have estimated the effect of fraud and abuse control efforts (such as investigations of suspected cases of fraud and referral of those cases for prosecution) on overpayments. We compared the costs of additional enforcement efforts with the savings in AFDC overpayments to see if an increase in control levels would be cost effective. In general, we found that the level of overpayments is responsive to control efforts and that the control efforts are cost effective in terms of more than recouping the additional costs of the controls in reductions in overpayments.

For various reasons detailed in the text, we feel that it is appropriate to present many of our summary results in terms of ranges rather than as single best estimates. In Seattle, the total amount of overpayments range between $1,420,236 and $7,101,178 annually. In Denver, the range is $1,975,032 to $9,875,175. The cost of doubling existing controls efforts

would be $193,104 in Seattle and $184,548 in Denver. Not all of the overpayments would be eliminated, of course, but the lower-bound savings would be $316,099 in Seattle and $ 38,435 in Denver. Thus, the benefit/cost ratios would be 1.64 and 2.38 in Seattle and Denver respectively.

From the results of our research, we conclude that:

(1) AFDC recipients tend to understate the number of family members capable of earning substantial income. To a limited extent, they also overstate the number of non-income earning dependents.

(2) AFDC recipients tend to report only a fraction of their wage and non-wage income to AFDC staff. Furthermore, there is a tendency to make a choice between two extremes regarding any particular income stream: either report a high percentage of the income, or not report the stream to the AFDC system at all.

(3) Increased fraud and abuse control efforts are a cost effective way to reduce grant overpayments.

These results have significant policy implications not only for the AFDC agencies in Denver and Seattle, but for the system as a whole. They argue strongly for an increased enforcement effort and for a broadening in the factors considered as benefits of such control efforts. Administrators of the AFDC programs in Seattle and Denver, and no doubt elsewhere, have asked enforcement units to demonstrate their cost effectiveness by showing that grant overpayment recoveries exceed enforcement costs. This is clearly too narrow a view since it ignores any deterrent effects of increased enforcement efforts and has probably contributed to the insufficient levels of enforcement we have discovered in Denver and Seattle. In addition, verification of AFDC recipient income streams and household status through use of other data systems such as social security would make a major source of grant overpayments--income underreporting--more difficult.

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I INTRODUCTION

Fraud and Abuse in the AFDC Program

The AFDC program was established in 1935 with the passage of Title IV of the Social Security Act. AFDC, Aid to Families with Dependent Children, provides cash assistance to families of needy children who are deprived of parental support through a parent's death, disability, absence from the home, and in some states, inability to find gainful employment. The amount of cash assistance varies with the size of the household and with the amount of income other than the AFDC grant. Such variation provides both the opportunity and incentive for fraud or abuse.

This report presents a detailed analysis of the character, extent, and prospects for control of the overpayment of grants to AFDC recipients. We have two objectives. The first is to measure the level of grant overpayments. The second is to determine how the AFDC grant overpayments to households change with AFDC overpayment control efforts.

Fraud or abuse of the AFDC program can be said to consist of manipulation of the system to obtain a larger amount of cash assistance than the household is entitled to by AFDC regulations. These manipulations can take two forms: (1) the direct misrepresentation of household size or income, and (2) the failure to seek gainful employment when circumstances permit and, thus, reduce or eliminate dependence on AFDC. Deliberate misrepresentation of household size or income by the recipient is fraud. Misrepresentation on the receipient's behalf by the caseworker, whether intentionally or in error, is abuse. In this study, we estimate the amount of extra cash paid out through the AFDC system as a result of inaccuracies, fraud or abuse, and the effects of internal AFDC quality control program efforts on reducing these overpayments.

Figure 1 shows the way in which an AFDC household's spendable, or disposable, income is related to the AFDC grant and to its earned income. The support level, S, is the AFDC household's minimum disposable income and the maximum AFDC grant. The grant, G, is reduced by two-thirds of a dollar for each dollar of earnings after the first $30.00 per month,* and dollar-for-dollar of nonwage income. Mandatory deductions, such as federal and state taxes and union dues, are reimbursed. Work-related expenses are also fully reimbursed. In Figure 1, we have assumed that nonwage income--for example, an ex-spouse's child support payment--is zero. Since nonwage income is 100% taxed, it simply substitutes for a portion of the support level.

Households have an incentive to overreport the number of members because larger families receive higher support levels. On the other hand, if a household member has earnings, then there is an incentive to exclude him or her from the AFDC household if the reduction in the AFDC grant through the earnings tax would exceed the increment to the support level. This effect is demonstrated graphically in Figure 2. Total household disposable income is higher when earnings are above E_1 if the earner is excluded from the household than it would be if he or she were included.

There is also an incentive to underreport earnings because the AFDC grant is reduced when earnings above $30.00 per month are reported. The dotted line in Figure 3 shows the effect of less than complete income reporting. The effect is to increase disposable income and to extend the AFDC income eligibility level. Because eligibility for an AFDC grant confers categorical eligibility for other welfare programs, such as the Medicaid program and the food stamp program, the incentive to misreport to the AFDC program can be substantial when eligibility for other programs is considered along with the increase in disposable income.

*This ratio is referred to as the "thirty and one-third" rule.

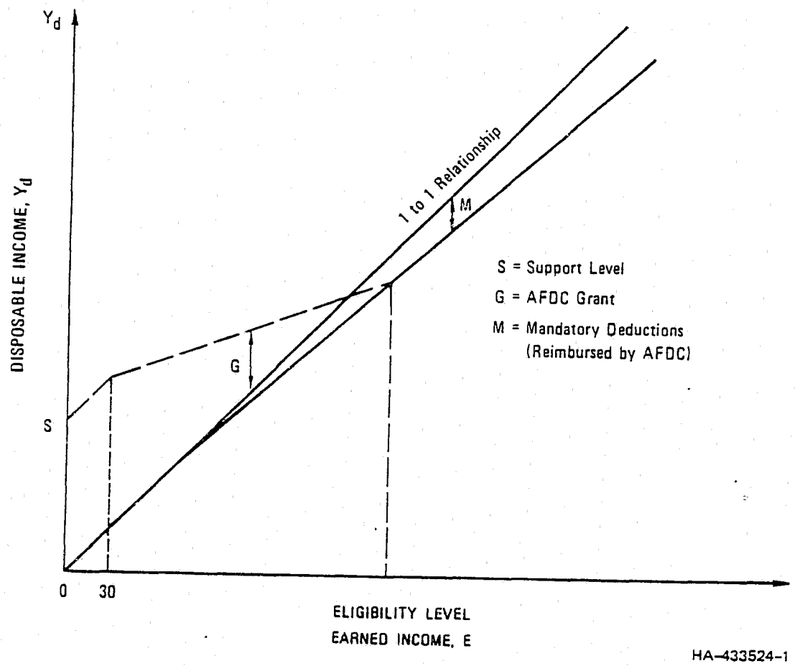

FIGURE 1 DISPOSABLE INCOME UNDER THE "THIRTY AND ONE-THIRD RULE"

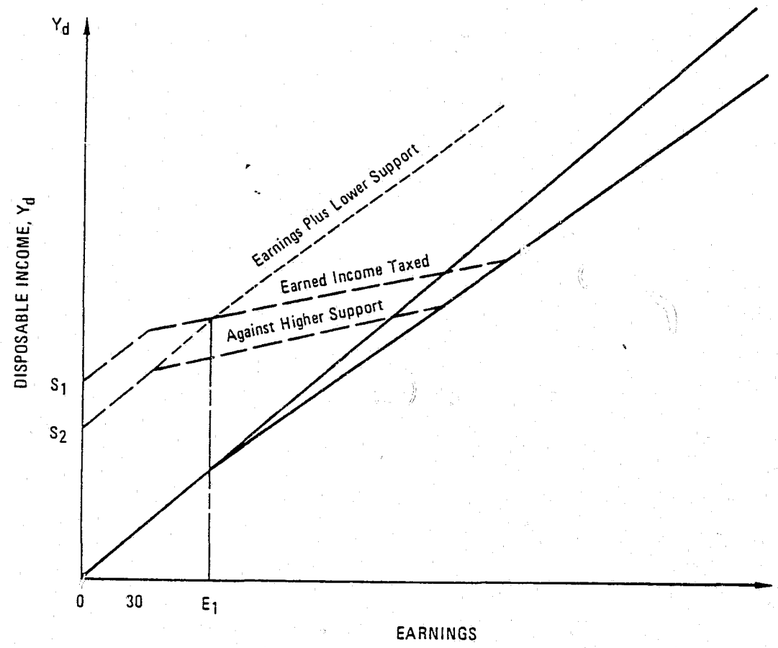

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FIGURE 2 DISPOSABLE INCOME WHEN AN EARNER IS EXCLUDED FROM THE AFDC TAXATION AND SUPPORT UNITS

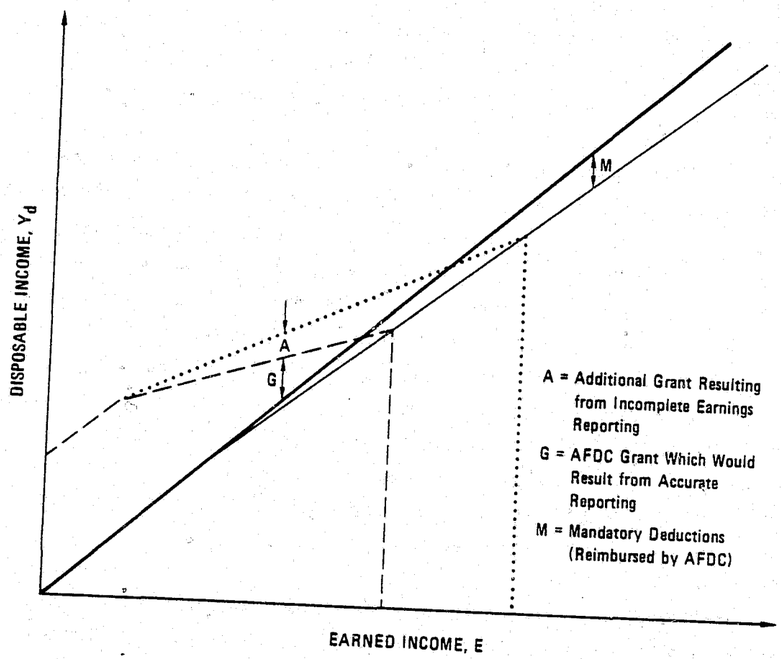

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FIGURE 3 THE EFFECT OF PARTIAL EARNINGS REPORTING ON DISPOSABLE INCOME

SIME/DIME Data

Data form the foundation on which our analysis and methodology rest. The Seattle and Denver Income Maintenance Experiments, known as SIME and DIME, provide a unique data source. SIME and DIME were the largest and latest of four income maintenance experiments conducted under the auspices of the federal government. SIME/DIME ran from 1970 through 1977 with data collection beginning in January 1970 and ending in 1977. SIME/DIME were time phased to allow for efficient data processing. As a result, families were interviewed for the same length of time, but not for exactly the same period. (The experiments were designed in the late 1960s when inflation was a relatively minor issue.) In each site a pre-enrollment interview was done on the entire future experimental group. This interview provides a year's baseline data retrospectively. The Seattle pre-enrollment interview covers 1969. The Denver pre-enrollment interview covers 1970. The Seattle data, however, was never processed for analysis. Seattle families were enrolled throughout 1970, Denver families throughout 1971. Accordingly, Seattle families were disenrolled throughout 1975 and Denver families throughout 1976. While the financial treatments were of 3 or 5 years duration (in Denver there was also a small 20-year sample), all families were interviewed for five years triannually. A post experimental interview was also conducted a year after disenrollment.

The SIME/DIME experiments were very similar to the AFDC program, except that eligibility was far less restricted and the support was more generous.* The purpose of these experiments was to simulate a

*Higher support levels were necessary to induce households to choose the experiment over AFDC. Treatment families were not allowed to receive both grants. Control families, of course, continued as they were, many of them on AFDC. Except for efforts to exclude financial treatment households from dual participation in SIME/DIME and AFDC, the experiments ignored AFDC. Non-treatment (control) families were free to participate in AFDC or not as they chose. Consequently, there is little reason to expect differential attrition by AFDC households. Overall experimental attrition averaged between 5 and 10% per year in both sites over the life of the experiment. Attrition in individual years is close to the average.

universal negative income tax. In a negative income tax program, households with incomes below a certain threshhold, rather than paying federal income taxes, receive a grant from the government. The size of the grant depends on the income level, hence the term "negative income tax." This is essentially what the AFDC program does.

In SIME/DIME, two-parent households, as well as households headed by a single parent, were enrolled. There was no requirement that anyone in the household be able to find work; however, husbands or single females who were household heads had to be physically capable of gainful employment. The disabled were defined as those having any physical condition preventing the individual from working at the time of the screening interview and for the next 3 years. Husbands and single females who were household heads had to be between the ages of 18 and 51 at enrollment.

The experimental sample was divided into an experiment treatment group and a control group. The treatment group received a grant similar to but more generous than the AFDC grant. The control group received no grant from the experiment and was allowed to participate freely in other welfare programs, including the AFDC program (for discussion, see Conlisk and Kurz, 1972). These AFDC participants within the control group provide data on income and household structure which we use in this report.

The SIME/DIME periodic interviews, identical in overall format, provide a continuous household record of many socioeconomic variables, including whether or not the household was on AFDC. Each interview was conducted personally by an interviewer and took approximately 1 and 1/2 hours to complete. The interviews were administered approximately triannually and each interview was retrospective over the period between interviews. Each household was paid $15 for each interview to offset the time and effort of the interview. The interviews were designed to encourage accurate data reporting. In many areas, this was not difficult, because there was little incentive to misreport and the data were easily checked, but there could be substantial incentives to underreport income. For this reason and because

income data are inherently complex, a major portion of the interview was devoted to the collection of income data.

A typical SIME/DIME interview book is 5/8 inch thick, containing 310 pages. The first 7 pages deal with household structure changes. The next 75 pages deal with earnings and employment for the first head of household. This section is followed by identical sections for the second head of household and other adult family members, age 21 and over. Earnings information for children age 20 or younger was also collected. Following the earnings and unemployment sections, there is a 24-page section dealing with nonwage income and household expenses. Clearly, income and household structure data received a major emphasis during the SIME/DIME interviews. In addition, SIME/DIME income measures were validated against other income sources (Halsey, 1980).

Fortunately for our present purpose, income data are recorded in a highly disaggregated form in the SIME/DIME interviews. Earnings, wages, and hours were collected separately for each job held by each individual during each month. Up to six jobs were tracked at any one time. Nonwage income was recorded separately from earnings and by individual source. These data are aggregated into earnings for each household member and household nonwage income variables by source. This disaggregation allows us to reconfigure SIME/DIME households to accurately represent their AFDC counterparts where necessary.

AFDC Data

In addition to data collected in the periodic interviews, data were collected directly from the AFDC program for each household that reported participation in AFDC. The purpose of this data collection effort was to better determine the characteristics of the control group, so that accurate experimental treatment effects could be estimated. The welfare rolls were also independently scanned to detect AFDC participation. Thus, the SIME/DIME experiments provide a remarkable data source from which a monthly

record containing income and household structure as reported to SIME/DIME and as reported to the AFDC program could be assembled.

The SIME/DIME interview process was independent of the reporting of data to welfare agencies. Therefore, the combined data set allows the analyst to detect fraud, abuse, and errors in reporting from a perspective not possible from strictly within the AFDC program.

Data on Efforts to Control Fraud, Abuse, and Error

Although measurement of the level of grant overpayments was our first objective, we also estimated the effects of control measures on AFDC grant overpayments in Seattle and Denver. The AFDC system fraud, abuse, and error control efforts can be divided into three categories: the care with which the AFDC programs are administered, internal efforts to control fraud and abuse, and criminal sanctions.

The first category includes factors such as the quality of caseworkers, their level of training, number, and level of supervision. There are structural differences between the two sites and marked differences in the availability of data. In Colorado, AFDC is administered at the county level. Overall, the data describing the Denver AFDC program are quite complete, including the number of caseworkers and supervisors. In Washington, the AFDC program was county-based until mid-1973, at which time, the state formed a central agency to administer the AFDC program. Seattle data from the period before reorganization became, for all practical purposes, inaccessible; nor were administrative data available for Seattle in the post-1973 period.

The second category captures internal efforts of the AFDC system to control fraud and abuse. Data on investigations initiated and numbers of cases referred to prosecutors are available on a monthly basis for the entire sample period in Denver, and from August, 1973, to October, 1977, in Seattle.

The third category describes the criminal sanctions invoked on proved defrauders of the AFDC program. Data on sanctions were unavailable in Seattle, because the local prosecutor's office did not begin differentiating AFDC cases from other fraud cases until after the end of our sample period. Although some data for Denver were available on the disposition of cases referred for prosecution, no information was available on the penalties imposed. On the whole, we were unable to develop a continuous series for either site that measured sanctions invoked as a result of investigations or prosecutions.*

*Some data is available on criminal justice activities involving individuals referred for prosecution for defrauding the AFDC program in the state of Washington. In the last 5 months of 1973, 335 individuals were referred for prosecution, 39% were convicted with 5% being sentenced to jail. Average court ordered restitution was $1,777 for those convicted. Apparently, contrary to widespread belief, the criminal justice system does impose sanctions on those individuals who are found guilty of defrauding the AFDC system.

II ANALYSIS OF DISPARITIES IN REPORTING

The AFDC Reference Household

To compare information reported to SIME/DIME with information reported to the AFDC program, we constructed a reference household from SIME/DIME data. This was necessary because the SIME/DIME household definitions were more inclusive than those of AFDC. For example, males performing the role of male head of household are included in the SIME/DIME household definition regardless of whether or not they were legally married to the female head of household. The AFDC program recognizes only legally married couples as two-parent households. Consequently, it is necessary to apply the AFDC household definition to the SIME/DIME data. Because the SIME/DIME data are disaggregated to the individual level, earnings can be identified with any household member. Thus, the first step was to construct SIME/DIME-AFDC reference households (always a subset of the complete SIME/DIME household) conforming to AFDC rules.

The AFDC support unit consists of parents and children under 18 years of age (21 years of age if the child is a full time student). For the household to be considered a two-parent household by AFDC, the parents must be married and the children must be living in the AFDC household (except in Denver, where full-time students away at school retain eligibility for AFDC support). The SIME/DIME-AFDC reference household was constructed by excluding adult family members age 21 or older who were not the household head and male heads unless they were married to the female head of household.

To analyze disparities between the reference families status as reported to SIME/DIME and as reported to the AFDC program, we first explored the magnitude of the grant overpayments resulting from differences in reported earned income, reported nonwage income, and reported household

structure. Each of these particularly important disparities can represent an independent source of grant overpayments, such as fraudulent reporting of nonexistent dependents; however, more complex error and misreporting can include all three elements. For example, fraudulently reporting the absence of a husband excludes any earnings he might have from the AFDC tax, but it also diminishes the AFDC support level for which the family is eligible because the household appears to have one less member. The two effects are offsetting. By simultaneously considering all disparities in household characteristics, we assessed the overall level of AFDC grant overpayments and the responsiveness of that level to control efforts. .

In the following subsections, we discuss disparities in reporting of income and family structure, and then present our integrated analysis of the level of grant overpayments when all known sources of error, fraud, and abuse are considered simultaneously.

Income Reporting

The majority of households having earnings to report do report some of it to the welfare department. By law total earnings, the sum of earnings of the male head of household, the female head of household, and the other earning household members, are to be reported to the welfare department. Because earnings accrue to individuals, it is important to know how much of the total household earnings is reported by each individual. We have chosen to display the average amounts reported statistically using regression analysis.

Income reporting is a balancing decision. On one side is the gain in disposable income resulting from underreporting of income, which allows the implicit AFDC income tax to be avoided. On the other side are all of the costs and penalties, both financial and ethical. This decision, given that the household is already enrolled in the AFDC program and has income to report, can be regarded as a two-stage process. First, the household decides whether or not to report the existence of each component of income.

Second, the total amount to be reported is decided.* The two-stage decision is constrained by the need to report a minimum total income that is deemed credible on the basis of the household's easily observed standard of living. The minimum credible amount will be roughly proportional to income because the household standard of living will usually reflect true income.**

The probability of discovery is low for income from a source unknown to AFDC. (Income from each specific source is called an "income stream.") Small reported amounts of income from a steady source are, in most cases, not credible, and may spark an investigation that would result in almost certain discovery. Larger reported amounts are more credible and have less likelihood of arousing suspicion and, therefore, lower probability of discovery of the actual value.

The expected gain decreases (approximately linearly) as the fraction of income reported increases.+ The household will seek to maximize its expected gain. Since the expected gain is higher either for zero reported income or a large fraction of income reported, the rational household will not report a small fraction of an income stream. Thus, we expect to find our sample population composed of two subsamples: accurate or fractionally reporting households where the fraction is relatively large and nonreporting households.

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*We have not analyzed another factor in the household's decision: occupational choice. In selecting an occupation, a household member may take account of the likelihood that AFDC might independently discover the income stream. In addition the legality of the occupation chosen will have a large effect on the reporting decision. For example, drug dealing and prostitution are unlikely to be reported.

**There are exceptions such as drug use or gambling where household consumption may not be well reflected in its apparent standard of living.

+The expected gain is the probability of not being discovered multiplied by the grant overpayment obtained minus the probability of being discovered multiplied by the penalty.

These comments about income reporting suggest that the sample be divided first between those households that have earnings and those that do not. Obviously, households having no earnings are uninteresting as far as income misreporting is concerned. Households having earnings are then subdivided into those reporting zero earned income to AFDC and those reporting positive amounts.

We can make some generalizations as to what to expect in our analysis:

(1) Because of the implicit AFDC tax on income, income is likely to be underreported.

(2) The greater the probability of discovery, the more likely an income stream is to be reported. We expect regular sources with similar, frequent payments to be better reported than those yielding one-time or irregular payments.

Because wage and nonwage income are separate in the AFDC files, we can include this distinction in our analysis. Since nonwage income is often irregular and is more highly taxed (100%) while wage income is usually regular (and is taxed 66 2/3%), we have divided income into earned and nonwage categories for analysis.

The SIME/DIME control group samples contained 848 households in Seattle and 1,294 households in Denver, which were enrolled in AFDC at least once. About half (436 and 559 in Seattle and Denver, respectively) had earnings to report, according to SIME/DIME data.* Of these, 102 households (about one fourth, in Seattle) and 201 households in Denver (about one-third) reported no earnings at all to AFDC. 334 households in Seattle (39% of all Seattle households) and 358 households in Denver (28% of all Denver households) reported all or partial earnings to AFDC. To avoid serial correlation in the subsequent regression analysis, a single record was constructed from the monthly series for each household. This record is the average monthly income over the longest continuous period that the

*Consistent misrepresentation to both AFDC and SIME/DIME cannot be detected with our data.

household received a grant payment. Without adjusting for different price levels, the average amount of monthly earnings not reported to AFDC by households reporting some earnings to SIME/DIME is $317 in Seattle and $290 in Denver. These averages include both non-reporting and partial reporting households. Table 1 shows the numbers and proportions of households reporting income in each site.

Table 2 shows the amounts of income actually received, and, therefore unreported by households reporting no income to AFDC. While AFDC only records household income it is possible to relate the unreported income to individual household members using SIME/DIME data. As expected, male heads have the most earnings: $658 per month in Seattle and $605 per month in Denver. Single female heads have more earnings than wives.

As with the household structure issue discussed later, the question of report timing arises. Of households reporting no earnings to AFDC but reporting positive earnings to SIME/DIME, 57% in Seattle and 46% in Denver did so for periods exceeding 3 months. Overall average amounts of unreported earnings were $642 and $549 in Seattle and Denver, respectively. For periods of 3 months or less, the average amounts of unreported earnings were $685 and $568 in Seattle and Denver, respectively--about the same as for the longer periods. Consequently, these relatively large values do not reflect mainly transitory changes in income.

Households that reported earnings to AFDC underreported an average of $225 in Seattle and $145 in Denver (Table 3), again without adjusting for price levels. We cannot determine the amount of earnings reported by each household member because only a single earnings figure, the total, is recorded in the welfare department records. Therefore, we report household totals only in Table 3. The amounts not reported are less for earnings reporting households than they are for those reporting nothing; yet, these amounts are not insignificant. In fact, they are comparable to or exceed the AFDC grant amount itself, which averages $247 in Seattle and $155 in

Table 1

NUMBERS OF HOUSEHOLDS REPORTING EARNINGS TO AFDC

	Number
Seattle	
Total AFDC households	848
Households with earnings	436
Households reporting some or all earnings	334
Households having earnings but not reporting any	102
Denver	
Total AFDC households	1,294
Households with earnings	559
Households reporting some or all earnings	358
Households having earnings but not reporting any	201

Table 2

AVERAGE MONTHLY EARNINGS BY INDIVIDUALS WHO ARE IN HOUSEHOLDS HAVING EARNINGS BUT REPORTING NO EARNINGS AT ALL TO AFDC

SEATTLE	Average Monthly Earnings ($)	Number of Households
Total single headed families	468.64	30
Female head of family	459.53	25
Male head of family	0.00	0
Teen children	321.39	8
Total two-parent households	714.01	72
Female head of family	230.10	30
Male head of family	657.56	66
Teen children	369.11	3
Total of all households having unreported earnings	641.84	102
Female head of family	334.39	55
Male head of family	657.56	66
Teen children	334.40	11
DENVER		
Total single headed families	406.30	77
Female head of family	405.34	72
Male head of family	0.00	0
Teen children	175.01	12
Total two-parent households	638.34	124
Female head of family	253.29	37
Male head of family	605.10	113
Teen children	175.78	8
Total of all households having unreported earnings	549.45	201
Female head of family	353.75	109
Male head of family	605.10	113
Teen children	175.32	20

Table 3

AVERAGE MONTHLY EARNINGS NOT REPORTED TO AFDC BY HOUSEHOLDS REPORTING EARNINGS TO AFDC

	Average Unreported Earnings	Number of Families
SEATTLE		
Single heads of families	165.41	228
Two-parent families	352.56	106
All households that reported earnings to AFDC	224.80	334
DENVER		
Single heads of families	95.65	267
Two-parent families	289.14	91
All households that reported earnings to AFDC	144.83	358

Denver.* Table 4 shows a ratio of the average AFDC grant to the average amount of income underreported.

If we regress average total income reported to AFDC on the components as reported to SIME/DIME, we have a type of identity. If reporting were identical, then the income coefficients in Tables 5 and 6 would all be 1.0. Since the reporting is not identical the coefficients reflect the proportions of each earner's income that is reported on average. If the differences between reports were purely random then the coefficients would not be significantly different from 1.0. To this regression we have added control variables which may affect the amount of income reported: whether or not the household head is married, the number of household members, etc. If all households reported identically, then the coefficients on each of the income variables would be 1.0, indicating that each additional dollar of that source of income would increase the income reported to AFDC by one dollar. The coefficients of the other variables would be zero, indicating that they had no effect on the reporting of earnings. Because this is what the law specifies, we call these statutory values.

The estimated reporting coefficients are presented in Tables 5 and 6 for Seattle and Denver, respectively. In both sites, the coefficient of the earnings of the male head is significantly different from 1.0 (plus or minus 5%). (Here and in the following discussion by "significantly" we mean with at least 95% level of confidence.) This indicates that, on average, very little earnings of male head's of households are reported. In comparison, the earnings of the female head of household are much better reported. Approximately 78% are reported in Seattle and 52% are reported in Denver.

*Direct comparisons between Seattle and Denver should not be made because the Denver sample period was 1971 and 1974 while the Seattle sample period was 1970-1977. Adjustment for price level differences is attempted for the overall grant overpayment analysis presented later.

Table 4

AMOUNT OF EARNINGS UNDERREPORTED AS A PROPORTION OF THE AFDC GRANT

	Number of Households	Percent of Households Reporting No Earnings	Ratio of Unreported Earnings to the AFDC Grant	Percent of Households Reporting Partial Earnings	Ratio of Unreported Earnings to the AFDC Grant
Seattle					
Single heads of families	578	5	2.15	39	.75
Two-parent families	270	27	2.74	39	1.38
Single Headed and Dual Parent Families	848	12	2.59	39	.97
Denver					
Single heads of families	846	9	3.79	32	.61
Two-parent families	448	28	5.09	20	1.39
Single Headed and Dual Parent Families	1,294	16	4.64	28	.85

Table 5

ESTIMATED PARAMETER OF THE TOTAL INCOME REPORTING FUNCTION, SEATTLE, ALL YEARS (DEPENDENT VARIABLE: AVERAGE AFDC TOTAL INCOME (Standard Errors in Parentheses)

Independent Variables	Statutory Values *	Parameter Estimates (1)	(2)
Earnings, male head of households	1	.049 (.018)	.044 (.018)
Earnings, female head of households	1	.780 (.021)	
Earnings, female head of households, less than $30	1		.788 (.276)
Earnings, female head of households, $30-$300	1		.697 (.059)
Earnings, female head of households, greater than $300	1		.971 (.082)
Earnings, other family members	1	.168 (.117)	.146 (.117)
Nonwage income from private source	1	-.376 (.481)	-.389 (.479)
Nonwage income from public source	1	.225 (.040)	.224 (.040)
Alimony received	1	.381 (.116)	.368 (.116)
One of two-parent family	0	-10.336 (7.320)	-9.846 (7.336)
Number of family members	0	.775 (1.972)	1.035 (1.972)
Constant	0	5.789	6.312
R^2		.767	.770
Number of observations		774	746
Mean of dependent variable		69.353	69.353

*Coefficient if reporting were in accordance with statutory requirements.

+The sample consists of families who have no earnings (and therefore report no earnings), and families who have earnings and report all or part of these earnings to AFDC. Households having earnings but reporting zero to AFDC are excluded.

Table 6

ESTIMATED PARAMETER OF THE TOTAL INCOME REPORTING FUNCTION, DENVER, ALL YEARS (DEPENDENT VARIABLE: AVERAGE AFDC TOTAL INCOME (Standard Errors in Parentheses)

Independent Variables	Statutory Values *	Parameter Estimates (1)	(2)
Earnings, male head of households	1	-.066 (.029)	-.072 (.028)
Earnings, female head of households	1	.517 (.017)	
Earnings, female head of households, less than $30	1		.638 (.126)
Earnings, female head of households, $30-$300	1		.615 (.062)
Earnings, female head of households, greater than $300	1		.207 (.062)
Earnings, other family members	1	.062 (.122)	-.032 (.120)
Nonwage income from private source	1	-2.958 (13.977)	-1.781 (13.625)
Nonwage income from public source	1	.462 (.073)	.479 (.072)
Alimony received	1	.009 (.130)	.013 (.127)
One of two-parent family	0	-6.388 (6.855)	-2.972 (6.718)
Number of family members	0	3.418 (6.855)	2.742 (6.718)
Constant	0	-1.839	-3.845
R2		.672	.690
Number of observations		1,093	1,093
Mean of dependent variable		60.670	60.670

*Coefficient if reporting were in accordance with statutory requirements.

+The sample consists of families who have no earnings (and therefore report no earnings), and families who have earnings and report all or part of these earnings to AFDC. Households having earnings but reporting zero to AFDC are excluded.

Because female household heads' earnings are the primary source of income reported to AFDC, we divided them into three ranges for analysis. Because earnings of less than $30 per month* are untaxed, they may not be recorded by the caseworkers, or the penalty for not reporting them may be different; therefore, we segregated these earnings. We also separated earnings above $300 on the ground that these earnings are likely to be from regular jobs carrying more documentation, so reporting of these earnings could differ. The bulk of earnings fall between $30 and $300 per month; segregating the lower end and upper end earnings allows better estimates in the mid-range, as well as of the end effects.

In Seattle, we do note an effect. The mid-range earnings coefficient drops from 78% to 70%, with the low- and high-range coefficients rising to 79% and 97% respectively (with neither significantly different from 100%). Thus, it appears that earnings below $30 and above $300 per month are quite well reported in Seattle. In Denver, a different pattern emerges. The low-range earnings coefficient rises to 64% (from 52%), the mid-range coefficient also rises to 62%, and the high-range coefficient falls to 21%. Apparently, in Denver, high-range earnings tend to be less well reported than mid-range earnings--not better reported as in Seattle.

The coefficients of other household members' earnings are significantly less than 100% and not significantly different from 0.0 in both sites. Apparently, these earnings are not well reported. None of the other coefficients, except that of the number of household members in Denver, is significantly different from its statutory value of zero. In Denver, the coefficient is very small, indicating that each additional household member adds about $3.00 to the amount reported. The constant terms are all satisfyingly small, suggesting that the coefficients indicate average, as well as marginal, effects.

*In Denver, AFDC recorded only earnings net of taxes and paid a flat $30 for work-related expenses. These two factors suggest a $75 per month untaxed range in the Denver regression, rather than the statutory $30.

Nonwage income reporting is reflected by the coefficients of nonwage income from private sources, nonwage income from public sources, and alimony received. The coefficient of nonwage income from private sources is not significantly different from zero; apparently very little or no private source nonwage income is reported. The coefficient of nonwage income from public sources is about .22 in Seattle and .48 in Denver; apparently, about one-quarter of this type of nonwage income is reported in Seattle and about one-half is reported in Denver. Practically no alimony is reported in Denver, however, while about 37% of it is reported in Seattle. The better reporting of alimony in Seattle may reflect the fact that the State of Washington requires alimony payments to be paid directly to the state under a HHS program designed to trace absent parents across state boundaries, if necessary, to ensure that they make their legal child support or alimony payments. Colorado did not participate in this program.

Therefore, we conclude that the earnings of female heads of households are almost the only earnings that are reported to AFDC. Earnings above $300 tend to be well reported (nearly 100%) in Seattle and rather poorly reported (21%) in Denver. Earnings below $30 tend to be very well reported in Seattle but only 64% reported in Denver. In general, earnings reporting seems to be significantly better in Seattle than in Denver.

Nonwage income is poorly reported in both Seattle and Denver--on average less than half is reported. It is interesting to note however, that alimony is better reported in Seattle where an extraordinary effort has been made.

Family Structure Reporting

The AFDC support level depends on the size of the household. In AFDC terminology a household is called the support unit.* The structure of the support unit is reported to the welfare department on the initial application for the AFDC program and is updated periodically by the head of the household as the structure changes. In most states, the support level consists of two components. One component covers rent and utilities and is independent of changes in household structure. The other component varies with household size and covers food, clothing, and personal items. An example of the support level structure for Denver in 1973 is given in Appendix A. The incremental support level varies from $34 to $65 per month for an additional child and is $57 for a second (usually male) head. The support for the first (usually female) household head is $141. The total support level for a household of four consisting of one head and three children is $261.

It is important to note that, although the incentives are to exaggerate household size when the additional reported person does not actually exist, there can be conflicting incentives for reporting the existence of real teenagers and household heads. If a person has income, usually from employment, then his presence in the support unit simultaneously increases the grant by the support level increment, and reduces it by the amount of the AFDC tax. Whether there is a net increase or decrease in the grant depends on the amount and type of the earnings. For example, if the support level increment were to be $50.00, then reported earnings of more than

*In fact, this is somewhat of a simplification. There are two AFDC households possible for every case. One is the support unit consisting of those individuals for whom the support level is computed. The other is a taxation unit, consisting of those individuals whose income is taxed against the AFDC support. Usually, the two units are identical but sometimes they are not. For example, suppose the female head of an AFDC household moves in with another individual or household that has income. AFDC may remove her from the support unit on the grounds that she is being supported by the support unit, or may retain her on the grounds that the new individual or household is not responsible for her support. The mother's income, however, would continue to be taxed against the childrens' AFDC support as before.

$105 per month would result in a net decrease in the AFDC grant (see Figure 3). Nonwage income in excess of $50.00 would have the same result. Consequently, there is an incentive to declare teenagers "emancipated" when their earnings are substantial, and to omit male (or female) household heads from the support unit if their income would reduce the AFDC grant more than their support would increase it. Further, under AFDC rules, if the household heads are married and one has substantial income, but is not the childrens' natural parent, then reporting the existence of the head with income would remove the other head from the support unit. Consequently, there are incentives to report the existence of individuals without income, as required, and to exaggerate the household size. On the other hand, there are incentives to exclude household members with income from the support unit when their income is large enough. This is the source of the supposed built-in AFDC incentive for household breakup. Obviously, this incentive is endemic to any program in which the grant accrues to the household as a whole and where household income is taxed, but where income accrues to individual household members.

The first step in determining the degree of error, fraud and abuse in household structure reporting is to determine the number of mismatches between the AFDC support unit and the SIME/DIME reference household. This information is shown in Table 7, which lists by site the total number of families, the number of households with matching size, and the number with the AFDC household exceeding the size of the SIME/DIME reference household (and vice versa). A household is considered mismatched in columns 2 and 3 if there is a mismatch in any month of the year. Columns 3 and 4 show a mismatch which persists for 3 or more consecutive months over the entire data period.

Because of the monthly accounting period of the AFDC data and of the SIME/DIME data, it is possible that household structure mismatches occur not because of misreporting, but rather because the same event is classified into adjacent months in the two data sources. Were this to be a frequent occurrence, there could be a large number of mismatches lasting 1 or possibly 2 months. We investigated this possibility by scanning the data at an early stage of the analysis looking for the type of mismatch patterns shown in

Table 7

SAMPLE SIZE AND HOUSEHOLD SIZE MISMATCHES BETWEEN THE AFDC SUPPORT UNIT AND THE SIME/DIME REFERENCE HOUSEHOLD ON ONE- AND THREE-MONTH BASES

	1-Month Mismatch		3-Month Mismatch	
	Number of Families	Percent	Number of Families	Percent
SEATTLE				
Total number of families	848	100	848	100
AFDC support unit equals SIME reference household	436	51	616	73
AFDC support unit is less than SIME reference household	148	18	60	7
AFDC support unit greater than SIME reference household	264	31	172	20
DENVER				
Total number of families	1,294	100	1,294	100
AFDC support unit equals SIME reference household	545	42	764	59
AFDC support unit is less than SIME reference household	342	26	1,196	15
AFDC support unit greater than SIME reference household	407	31	334	26

Figure 4. The pattern shown in (a) occurs fairly often for a period of much more than 1 month. This indicates a significant lag in reporting an unfavorable change in the AFDC household structure to the AFDC program. The other three patterns occur infrequently and usually only for 1 month. This could indicate a data timing problem in many cases. Pattern (b) did not occur in our sample.

To allow for timing errors and for the difference in incentives for reporting household members with and without income, we analyzed the reporting of individual household members by household position, earnings, and length of mismatch (months). The results are shown in Tables 8, 9, and 10. Two mismatch categories are shown in each case: less than or equal to 3 months, and greater than or equal to 4 months. In shorter periods of mismatch, timing errors are confounded with misreporting in many cases, but in the longer periods, this is unlikely.

As Table 8 indicates, our sample contains 848 and 1,294 families, respectively, in Seattle and Denver. By SIME/DIME definition, 270 and 448 are dual-headed families. Of these, 47% and 42% in Seattle and Denver, respectively, failed to report the existence of the male head for periods of more than 3 months. These fractions indicate substantial underreporting of male heads of households to the welfare department. This type of behavior has often been noted because, before the AFDC-U program was established, two-parent families were ineligible for AFDC if the male head was present and physically able to work. This requirement provided an even stronger disincentive to report male heads than does the present AFDC-U program.

The disincentive for households to report teenagers with earnings is similar.* As can be seen from Table 9, however, only a small fraction of families underreport the existence of teenagers with earnings, at most 7% in Seattle, and 3% in Denver. This is reasonable, because children do not generally have the freedom to enter and leave the household that male

*The earnings of teenagers not full time students and over age 16 are taxed as household earnings by the AFDC program.

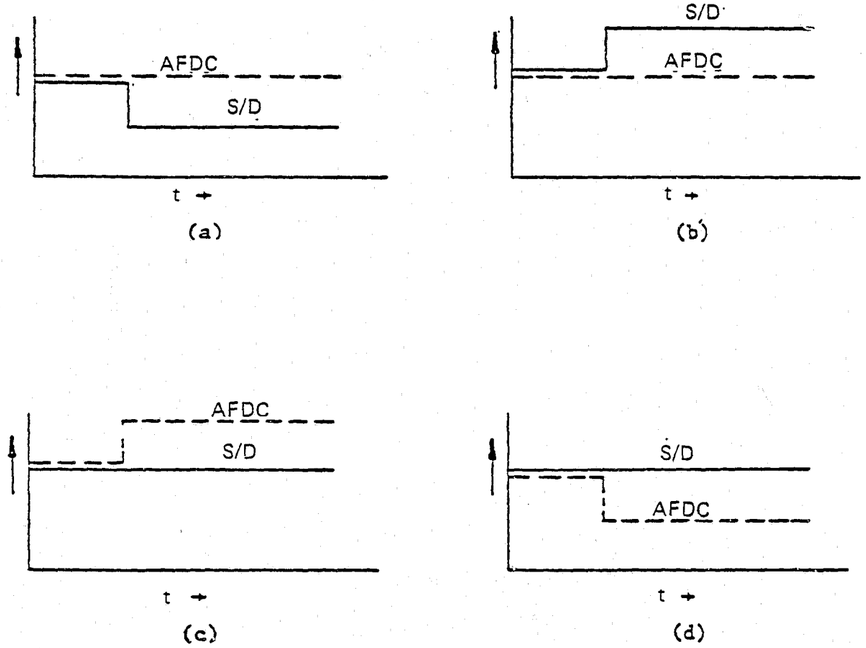

FIGURE 4 THE TIMING OF FAMILY STRUCTURE MISMATCHES

Table 8

HOUSEHOLDS REPORTING MALE HEAD-OF-HOUSEHOLDS

Site	Families With Male Heads		Families Not Reporting Male Heads: Less Than or Equal To 3 Months		Families Not Reporting Male Heads: Greater Than or Equal To 4 Months	
	Number	Percent	Number	Percent	Number	Percent
Seattle	270	32	59	22	128	47
Denver	448	35	59	13	188	42

Table 9

FAMILIES UNDERREPORTING TEENAGERS WITH EARNINGS

Site	Families With Teenage Children		Families With Teenage Children With Earnings		Families Underreporting Teenage Children With Earnings: Less Than Or Equal To 3 Months		Families Underreporting Teenage Children With Earnings: Greater Than Or Equal To 4 Months	
	Number	Percent*	Number	Percent*	Number	Percent+	Number	Percent+
Seattle	564	67	139	16	34	24	27	19
Denver	869	67	124	10	22	18	13	10

*Percent of all families.

+Percent of families with teenage children with earnings.

heads do, and a teenager's existence is likely to be known to the welfare department either from years before the earning period or from school records. Also, because of their lower earnings potential, fewer teenagers than household heads will have earnings high enough to lower the families net income. Here, however, a complicated issue arises. The teenagers earnings usually accrue to him, not to the household head, but the AFDC tax on his earnings is paid by the household head. Therefore it is possible for net household disposable income to rise, while that portion under the control of the household head falls.

Table 10 shows the number of families overreporting preteenage children. Here, the incentive is to increase the support level rather than to avoid the earnings tax. The rate of such overreporting is small: 8% in Seattle and 9% in Denver.

Table 10

HOUSEHOLDS REPORTING PRE-TEENAGE CHILDREN

Site	Families With Pre-Teenage Children		Families Overreporting Pre-Teenage Children: 3 Months		Families Overreporting Pre-Teenage Children: 4 Months	
	Number	Percent*	Number	Percent+	Number	Percent
Seattle	778	92				
1 child overreported			29	4	23	3
2 or more overreported			8	1	2	1
Denver	1,139	88				
1 child overreported			54	5	27	2
2 or more overreported			10	1	15	1

*Percent of all families.

+Percent of families with pre-teenage children.

Summary

With the exception of exclusion of male household heads, household structure is reasonably accurately reported by AFDC recipients to the AFDC program. Although exclusion of a male head diminishes the support, it also precludes consideration of his earnings for the household's eligibility and taxation. Discovery of those earnings by the AFDC system would also be difficult.

III ANALYSIS OF GRANT OVERPAYMENTS AND CONTROL STRATEGIES

The usual result of income and/or household misreporting is to increase the size of the AFDC grant above the amount to which the household is entitled.* In Section II, we discussed in detail the magnitude of various types of disparities in reported household structure. In this section we compute the size of the resulting AFDC grant payment error associated with these disparities, examine the effects of sanctions on overpayments, and assess the cost effectiveness of such sanctions.

AFDC Overpayments

As in Section II, we assume that SIME/DIME reference household portrays the household accurately. Using the reference household as the basis for comparison, we disaggregate the grant overpayment into the component resulting from erroneous or deliberately inaccurate reports of household size, which alters the support level alone--and the component resulting from error and misreporting in earnings--which affects the earnings tax. Table 11 shows the average monthly grant overpayments resulting from earnings disparities alone, from household structure disparities alone, and from these disparities in combination.

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* The AFDC grant is given by the relationship:

$$G = \begin{cases} S - Y_n - \frac{2}{3}(E-30) + W + T & \text{if } E \geq 30 \\ S - Y_n + W + T & \text{if } E \leq 30 \end{cases}$$

where G = the grant, S = the support level, Y_n = nonwage income, W = work related expense, and T = reimbursement for mandating deductions such as taxes and union dues. Using the true values for S, Y_n, E, W, and T we get the correct value of the grant. The support level, S, is determined by the true household structure.

Table 11

AVERAGE MONTHLY AFDC GRANT PAYMENT ERROR

Site	Earnings Difference Only		Support-Level Difference Only		Both Earnings and Support-Level Differences	
	Payment Error ($)	Number of Households	Payment Error ($)	Number of Households	Payment Error($)	Number of Households
SEATTLE						
All households with earnings but reporting 0 to AFDC	347.11	102	-77.68	74	290.60	102
Single head of household	251.05	30	-64.27	19	210.34	30
Two-parent household	387.14	72	-82.31	55	324.04	72
All households with earnings reporting partially to AFDC	80.55	334	-27.88	155	61.19	366
Single head of household	38.05	228	-10.24	79	30.93	255
Two-parent household	171.96	106	-46.22	76	130.70	111
DENVER						
All households with earnings but reporting 0 to AFDC	283.30	201	-68.53	129	243.86	196
Single head of household	203.47	77	-15.90	37	198.29	76
Two-parent household	333.27	124	-89.69	92	272.72	120
All households with earnings reporting partially to AFDC	78.29	358	.71	152	88.74	304
Single head of household	52.71	267	25.19	94	68.48	225
Two-parent household	153.35	91	-38.97	58	146.44	79

Of course, the support level effect is smaller for families with a single head of household than for two-parent families, because a single-household head lacks the opportunity to exclude the principal earner; they can only attempt to exclude teenagers with full-time jobs. In one case (Denver single heads of households reporting partial earnings to AFDC), the support level difference is actually positive, indicating that the overreporting of members without earnings more than offsets the exclusion of earners.

Overpayments as a result of disparities in earnings reporting are large and positive. They are larger for households reporting zero earnings than for those reporting partial earnings and larger for two-parent families than for families with a single head of household, as is to be expected. Because of the negative effect of the support level increase, the combined effect is smaller than the earnings effect alone, but it is still large. In fact, because of overpayments, the amount received is twice the legitimate grant. For example, two-parent households in Denver that did not report earnings received an average of $273 extra per month. In Seattle, the overpayment amount is $324.

Enrollment in AFDC also confers other welfare benefits such as food stamps and health care.* Accurate assessment of the amount of unwarranted benefits derived from such programs by households not qualified to be on AFDC requires data not collected for SIME/DIME . In our sample, averaged over the time span of the SIME/DIME study, 9% of recipient households observed in Seattle and 22% in Denver in any given month did not quality for AFDC. Consequently, our estimates of grant overpayments resulting from error, fraud, and abuse in the reporting of household structure to the AFDC program may substantially understate the amount of misallocated resources in the entire welfare benefit system if many of these households are receiving

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* AFDC participants are often "categorically eligible" for other public welfare programs. Categorically eligible means that the household does not need to meet any tests for program eligibility other than the fact of AFDC participation.

benefits from other programs for which they would not otherwise be eligible. The inescapable conclusion is that errors and deliberate inaccuracies in reporting result in significant grant overpayments.

The Effect of Sanctions on AFDC Overpayments

The second objective of this research project is to determine whether actions to control fraud and abuse taken by the AFDC program and justice system reduce grant overpayments. In this section, we assess a version of the deterrence hypothesis: fraudulant or abusive misrepresentation of household structure and income is negatively related to the threat of discovery and of the sanctions applied to those discovered to have made such claims. Although we cannot distinguish between errors and misrepresentation in our calculation of grant overpayments, we can determine whether total overpayments are responsive to the threat of possible investigation, prosecution, and/or restitution plus fine. This section presents our main empirical results, and a discussion of those results and their role in our appraisal of the effectiveness and cost effectiveness of control strategies.

The dependent variable in our analysis is the monthly average disparity in grants for SIME/DIME reference household.* Because the data cover a number of years and two sites, we decided that it was prudent to adjust this dependent variable for changes in the level of grants as reflected by a cost-of-living index for low-income families.** Thus, the resulting modified dependent

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*Households move on and off AFDC as their reported structure and income change. Our monthly average grant overpayment variable is based on all those households enrolled during a particular month and in the SIME/DIME control sample.

**The sources for this cost-of-living index, which had a base of 1971, were U.S. Department of Labor, Bureau of Labor Statistics, The Consumer Price Index, U.S. City Average and Selected Areas, various issues, and University of Denver, Denver Metropolitan Area Consumer Price Index, various issues.

variable is adjusted for inflation.* When we take into account the availability of matching data on the control efforts undertaken by the AFDC programs, we have a total of 73 usable months of observations: 22 months of data for Denver and 51 months of data for Seattle.

The independent variables were constructed from the limited data that we obtained about the administration of AFDC in Seattle and Denver.** The raw material we had included the numbers of investigations and procecutions along with a measure we developed of the number of families that received substantially larger grants than their circumstances, as reported in SIME/DIME, appeared to warrant. A measurement problem common to aggregate analyses of crime affects our analysis: offense rates are unknown or known only imprecisely.+ Although AFDC agencies calculate an error rate, our empirical result on the regularity of overpayment of grants indicates that these error rates are not useful indicators of prevalence. Consequently, we generated an indicator of the prevalence of overpayment using the SIME/DIME data.

From our sample, we determined the number of reference families that were paid a grant exceeding the warranted grant by $20 or more. The percentage of the sample of families found in a particular month to be in this category was multiplied by the total number of cases handled by the agency, producing a number representing total cases overpaid. The ratio of overpaid cases to the number of investigations in the same month gives us a

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*With the exception of the time trend variables, the qualitative results of our analysis were not particularly sensitive to adjustments for price changes.

**As discussed earlier, we were not able to obtain very extensive data in either site.

+See Nagin (1978) for a discussion of possible biases that can emerge in later statistical analysis when only estimates of crime rates are available; also see Block, Nold, and Sidak (1981) for an example of a situation in which an offense rate was created, and....

measure of the probability of being investigated. The ratio of the number of investigations initiated to the number of cases referred to the prosecutor gives us a measure of the conditional probability that an investigated case will be referred to the prosecutor's office. These two measures of control (the rate of investigation and the rate of referral to the prosecutor) are central to the empirical work presented.*

Because the number of explanatory variables is small, we can easily graph the most important data for both Denver and Seattle. While this is often a useful exercise, it is a particularly valuable step in this case. Figures 5 and 6 reveal basic relationships between average grant overpayments and the control variables that also emerge in the quantitative results presented later. Aside from the jagged nature of the graph, the most prominent aspect of the average grant overpayment series presented in these figures are the difference in the average levels in Seattle and Denver. Also apparent is the tendency for the average real overpayment to grow over time: the second section of the Denver series is at a much higher level than the first, and the Seattle series is flat in the early period but shows relatively steady growth during most of our sample period. The Seattle data also appear to have some seasonal variation, with monthly average real grant overpayments higher during the summer months; we report some results based on this possibility. From these observations, we anticipate that quantitative results will reveal that Denver had a higher level of grant overpayment and that both grant overpayment series increased over time.**

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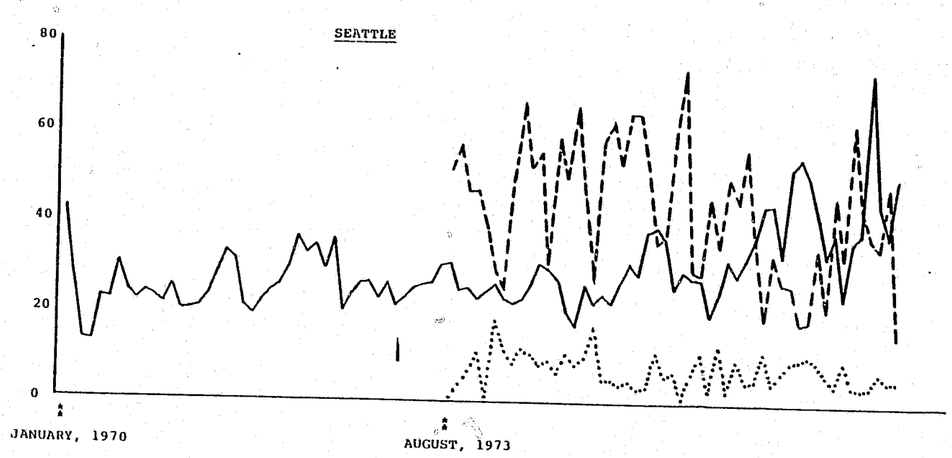
* Data on the major series that we assembled are presented in Appendix B, Table 1.

** Another aspect of the average monthly real overpayment time series is worth noting. Month 91 in Seattle has an extremely large value. This value is accurate. However, a perusal of the raw data presented in Appendix A shows that the number of households upon which this estimate rests is low: 29, compared to an average of 204 for the entire Seattle sample. In fact, the last 4 months of data for Seattle are based on rather small samples and, despite the fact that the regression technique we use takes account of the varying sample size, the quantitative results we present later were checked and found insensitive to the exclusion of these four observations.



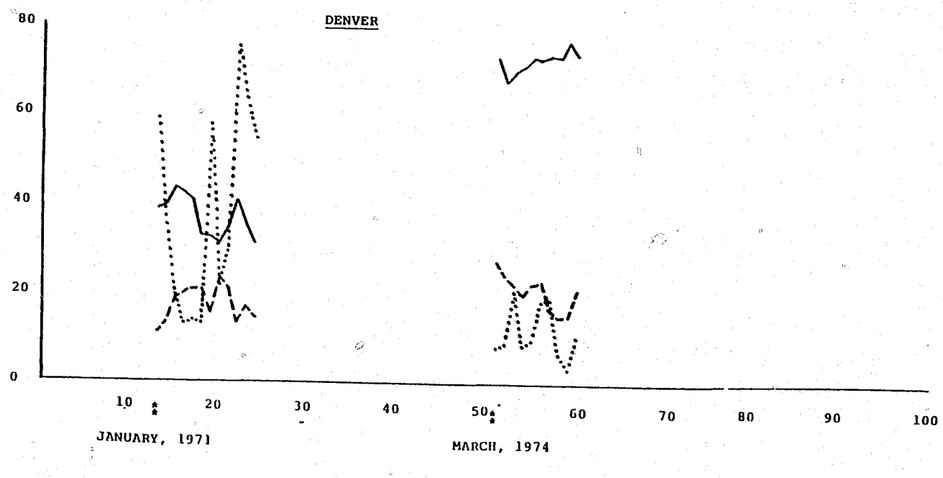
KEY

——— AVERAGE GRANT OVERPAYMENT

- - - INVESTIGATION RATE

····· RATE OF REFERRAL OF CASES INVESTIGATED

Figure 5 BASIC RELATIONSHIPS - SEATTLE



KEY

——— AVERAGE GRANT OVERPAYMENT

- - - INVESTIGATION RATE

..... RATE OF REFERRAL OF CASES INVESTIGATED

Figure 6 BASIC RELATIONSHIPS - DENVER

The investigation rate and rate of referral to prosecutor are also presented in Figures 5 and 6. In Denver, the rate of referral for prosecution declines dramatically between the early period (months 13-24) and the later period (months 51-60). The investigation rate is relatively constant, but substantially lower in Denver than in Seattle. In Seattle, both control variables decline slightly over the entire period.

Significantly for our study, these general movements in control variables inversely correspond to general movements in average grants overpayments (i.e., as control efforts decrease, overpayments apparently increase). We used multiple regression methods* to assess these effects. Loglinear specification** of the relationship between average real overpayments and variables describing the AFDC system are presented in Table 12. The independent variables are: a dummy variable for site, which is one in Seattle and zero in Denver; the log of the rate of investigations, and the log of the rate of referral to the prosecutor of those investigated for receiving unwarranted payments. A constant was included and we also allowed for separate time trends in average overpayment for each of the two sites.

These regression results are quite striking. The site variable has a negative and highly statistically significant coefficient. This indicates that there is a statistically significant higher overpayment of grants in Denver. Evaluated at the mean values and controlling for the influences of the variables included in the regressions, these results suggest that a

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*The weighted least squares method was used throughout to reflect the fact that different numbers of reference families are involved from month to month and across sites. The number of reference families available in the site for a particular month varies between 318 and 1 in Seattle and 821 and 446 in Denver. This series is given in Appendix B, Table 1.

**Both linear and loglinear models were estimated. The linear model is perhaps the most natural selection. However, in the loglinear model, outliers are given relativaely less weight in determining coefficients, and the coefficients, themselves, are more easily interpreted. The results for the linear models were quite similar and so are not presented.

Table 12

WEIGHTED REGRESSION LOG LINEAR RESULTS USING CONTEMPORANEOUS SANCTION PROBABILITIES (DEPENDENT VARIABLE LOG OF MONTHLY AVERAGE REAL GRANT OVERPAYMENT)

Variable	Parameter Estimates
Constant	2.51
Site dummy	-.585 (3.28)
Time trend, Seattle	.010 (4.48)
Time trend, Denver	.016 (10.6)
Log of investigation rate	-.178 (2.54)
Log of rate of referral to prosecution	-.082 (2.75)
R^2	.90
DFE	67

Denver reference household received, on average, a monthly grant overpayment approximately $20 larger than a Seattle reference household. (For comparison, the weighted average grant overpayment from Table 11 is $23 greater in Denver.)

The coefficients of site-specific time trends indicate that, although grant overpayments grew over time in both sites, the rate of growth was much larger in Denver than in Seattle. Annual pecentage growth rates were 13% in Seattle and about 21% per annum in Denver. These results, in combination with those discussed for the site variable, suggest that Denver had a worse and more rapidly deteriorating AFDC grant overpayment situation than Seattle. The remaining variables included in these regressions represent the control strategies for which we assembled viable empirical counterparts. These numerical results confirm the observations drawn previously from Figures 5 and 6.

The consistency and strength of the negative association between grant overpayments and efforts to investigate and prosecute those receiving these overpayments is our major finding.* We have checked the results for robustness using a substantial number of other specifications of the relationship of these variables to real grant overpayments. For example, we have split the sample and estimated similar, separate models for each of the two sites. The control strategy variables remained negative and significant with only insignificant differences between coefficients estimated for

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* Of course, it is a nuisance to be investigated and the time necessary to defend oneself is a penalty of sorts. However, it is interesting to combine our analysis of the risk of being investigated and, perhaps referred for prosecution, with the limited data on actual penalties which we obtained in Seattle. In Seattle for some five months in 1973, we estimate that the probability of a household receiving a grant overpayment of more than $20 for the entire year faced a probability of .285 of being investigated while the probability of referral for prosecution given investigation was .048. Compared to the gain of at least $240 for such a household ($20 a month), the expected loss through restitution for such an individual was $9.37 and the probability of going to jail was .0007.

different sites.* Consequently, the specifications estimated on the data from both sites accurately reflects the magnitudes of coefficients applicable to either Seattle or Denver.**

The most important variation in specification is the development of distributed lag estimates. We noted in the discussion of the Figures 5 and 6 that, as with most frequently recorded data describing a phenomena, our monthly series was rather jagged.+ We might conjecture that individual recipients implicitly smooth out monthly variations in the control variable when forming their expectation about the chances of being investigated or ultimately prosecuted for keeping grant overpayments. This is in contrast to the results presented in Table 12 where we used only the contemporaneous control variables adopting the assumption that recipients only adjust their level of grant overpayment to contemporaneous influences. Implementing a

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* Standard F-tests were used and no statistically significant differences in the site-specific coeffiencts on control variables were uncovered.

** Two statistical issues that are related to this work have received attention in the literature on the empirical analysis of crime. First, there is the issue of simultaneity. This issue, as it relates to the criminal justice system, is discussed in detail in Fisher and Nagin (1978). Second, there is the issue of incapacitation. This is discussed in Nagin and is relevant to the results we have obtained in that a household that is investigated or referred for prosecution is likely to, at least, have any grant overpayments suspended. Neither of these issues is trivially dealt with, especially considering the paucity of information we have been able to assemble about the AFDC system in Seattle and Denver. In fact, to check the robustness of the negative effects of investigation and referral for prosecution on the prevalence of grant overpayments, we should analyze not only the level of grant overpayments received but also the decision by households to remain on or join the AFDC rolls under misreported conditions. Preliminary analysis of the data on individual households supported the findings of our aggregate work, but the detailed analysis of these underlying individual observations is beyond the scope of this research project.

\+ As a mundane example, monthly consumption expenditures by a household might be jagged due to the irregular purchase or repair of durables like automobiles. Quarterly or annual data for the same household would be a much smoother time series.

model in which individuals can respond to past as well as contemporaneous changes in control variables generally requires more sophisticated statistical techniques.*

The estimation technique used for this model allowed us to see how individuals respond to a short, moving average of the explanatory variables where the weights used in producing the average have been selected in a special way. The results of these additional regressions are shown in Table 13. Aside from the usual regression summary statistics,** Table 13 lists estimated coefficients for site, site-specific time trends, site-specific dummy variables for the summer and winter months,+ and the contemporaneous and lagged control variables.

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* Methods have been developed for estimating coefficients for contemporaneous values and several lagged values of explanatory variables. The method we used, the polynomial distributed lag technique, was developed by S. Almon and is discussed in Johnson (1972), Chapter 10.

** Because we need lagged values of the control variables, we lose observations at the beginning of each time series. For example, if we want to include control variables lagged two periods, then for estimation we lose the first two observations in Seattle and four observations in Denver--two at the beginning of each segment of data.

\+ Summer was defined as June, July, and August; winter as December, January, and February. Exactly how one should interpret these results on seasonal coefficients is a matter of conjecture. Because the Denver sample is short--22 months--it is difficult to estimate accurately the seasonal components. On the other hand, while there is probably a seasonal pattern in the opportunity to generate income in casual work, it may also be the case that there is some seasonal variation in the levels of control variables due to the effect of summer vacations on staffing levels. In any event, while a seasonal pattern is probably present in Seattle, taking account of this effect leaves the results on the control variables unchanged.

Table 13

WEIGHTED, LOG LINEAR POLYNOMIAL DISTRIBUTED LAG REGRESSION RESULTS (DEPENDENT VARIABLE: LOG OF MONTHLY AVERAGE REAL GRANT OVERPAYMENT)

Variable	Parameter Estimates
Constant	2.19
Site dummy	-.476 (2.33)
Time trend, Seattle	.008 (3.53)
Time trend, Denver	.016 (10.7)
Summer dummy, Seattle	.233 (4.19)
Summer dummy, Denver	-.063 (1.38)
Winter dummy, Seattle	-.104 (1.83)
Winter dummy, Denver	-.177 (2.60)
Log of investigation rate	-.247* (2.78)
Log of rate of referral to prosecutor	-.115** (2.98)
R^2	
DFE	55

*The coefficients contributing to this sum for the contemporaneous and the two lagged periods are -.123, -.824, -.0412, respectively.

**The coefficients contributing to this sum for the contemporaneous and the two lagged periods are -.0574, -.0383, -.0191, respectively.

Comparing Tables 12 and 13, we see that the numerical results are reasonably consistent across different specifications of the model.* We can conclude from the other version that the control strategies we have measured are effective in reducing grant overpayments.** However, in order to perform the benefit calculations that are of central importance to the policy discussion in the next section, we must settle on reasonable estimates of the magnitudes of coefficients on variables representing control strategies.+ The estimated coefficient of the log of the investigation rate is about -.17 in the nonlagged version and has a value of -.247 for persistent changes in the specification presented in Table 13. The coefficient of the log of the rate of referral to the prosecutor is

*We do not know much, a priori, about relative magnitudes of the distributed lag coefficients. The polynomial distributed lag technique forces the lag coefficients to lie on a polynomial and we have discretion in selecting the characteristics of that polynomial. The polynomials used to generate the weights presented in Table 13 were of first order with the intercept constrained. The results for other reasonable selections of polynomials are quite similar to those presented in Table 13.

**Since we will be discussing the effect of changes in the levels of the control variables which would be maintained for an extended period, we have tabled the sum and standard error of the sum of the lagged coefficients for each control variable. We present only the loglinear results in Table 13, since estimates of the linear model were similar. As before, we estimated separate models for Denver and Seattle and found that the coefficients on the control variables were insignificantly different. Consequently, we present only the estimates of the coefficients we feel are common to both sites.

+The comments made earlier about the different levels of grant overpayments in the two sites still apply for the estimates in Table 13. Denver is still roughly $20 higher in average grant overpayments when account is taken of other variables. One difference does emerge: the rate of growth of the overpayment in Seattle is lower and Denver is higher than for the estimates given in Table 12. This would amplify differences in grant overpayments in the two sites much more dramatically since the compound rate of growth is 23% in Denver and 8% in Seattle. To emphasize the importance of this seemingly trivial difference in coefficients, if Denver and Seattle started with the same level of average grant overpayment, in 4-1/2 years, the average Denver grant overpayments would be twice those in Seattle.

about -.08 in the non-lagged version and has a distributed lag sum of about -.115.*

For the cost-effectiveness discussion, we shall adopt the values given in Table 13 for the sums of the lag coefficients for the logs of the control variables. These are estimates of the responsiveness of average grant overpayments to changes in the levels of apprehension or referral rates that persist for at least 3 months.**

The Cost Effectiveness of Control Strategies

We have estimated the effect of control strategies on average monthly AFDC overpayments. However, there remain two critical issues regarding the calculation of the decline in overpayments associated with changes in the control variables. First, had overpayments and control variable data been stable, we could have used mean values. However, we know from the graphs and statistical estimates that overpayments have been growing and control levels have been declining. Consequently, we evaluated the savings

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* We also explored whether the results were sensitive to adjusting for serial correlation. One might expect serial correlation to be present because of our use of monthly observations and our inability to measure several likely important determinants of grant overpayments. Point estimates of the serial correlation coefficient ranged up to .4 but re-estimation adjusting for the serial correlation yielded results quite similar in magnitude and significance levels to those present for the control variable in Tables 12 and 13.

** No single number is entirely adequate for developing policy implications, but these numbers represent our best estimate and are quite representative of results from the wide variety of specifications we have tried. We have used the loglinear specification of our model because the estimated coefficients are also the elasticities. Elasticities refer to the percentage change in the dependent variable, real grant overpayments, corresponding to a 1% change in an independent variable. For example, a 1% increase in the investigation rate, maintained for 3 months, is associated with a -.25% change in real grant overpayments using the coefficients presented in Table 13.

in grant overpayments in the last year of our time series in each site.* These savings are the most relevant data for current policy decisions. Second, the SIME/DIME experiments were conducted on a nonrandom sample of the AFDC population. Furthermore, the AFDC agencies do not have disaggregated information on their recipient populations, so we cannot determine how our sample could be weighted to reflect the entire AFDC population. Because there appears to be no clear solution to this problem, we present the effects of the control variable on grant overpayments under two different assumptions about the representativeness of our SIME/DIME sample of reference families.

The first assumption is that our sample is representative only of the approximately 20% of the AFDC households that include members who could work. Because SIME/DIME eligibility rules excluded the permanently disabled, this would at first seem reasonable. However, analysis of the SIME/DIME sample presented above suggests that large numbers of households with a single female head and with small children (households that AFDC classifies as outside the 20% who are able to work) generate incomes and often have unreported male heads of household as well. Thus, our sample is probably representative of more than 20% of the AFDC population. Consequently, this assumption provides at least a lower bound to the savings effects of control strategies. These effects are shown in columns 1 and 2 of Table 14.

Our second assumption allows us to develop an upper bound for the effects of our control strategies on grant overpayments. These results are produced by extrapolating the estimates from our SIME/DIME reference

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* We chose the 10-month segment in Denver beginning in March 1974, and the 12-month segment in Seattle beginning in July 1976. Note that this excludes the last four months of data on Seattle. These may not be very reliable observations since their average monthly grant overpayments are based on small numbers of reference families.

Table 14

ANNUAL SAVINGS AND COSTS ASSOCIATED WITH INCREASES IN CONTROL VARIABLES (1971 Dollars)

Assumption	Lower Bound (20% of AFDC Population Capable of Working)		Upper Bound (Extrapolation to Entire AFDC Population)		Annual Costs of Increasing Control Variables	
	Seattle	Denver	Seattle	Denver	Seattle	Denver
One additional case/month:						
Investigated	$ 1,162	$ 1,996	$ 5,811	$ 9,978	$ 760	$ 931
Investigated (rate of referral to prosecutor constant)	2,195	3,755	10,977	18,774	1,190	1,403
Referred to prosecutor	14,202	14,942	71,056	74,712	6,128*	4,041**
100% increase:						
Investigations	125,298	172,724	626,491	863,621	122,724	123,084
Investigated (rate of referral to prosecutor constant)	316,099	438,435	1,580,497	2,192,173	193,104	184,548
Cases referred to prosecutor from those already investigated	110,031	151,335	550,157	756,675	70,472	61,423
Total estimated over-payments	$1,420,236	$1,975,032	$7,101,178	$9,875,175		

*Sum of prosecutorial costs, $5,079; and court costs, $1,049.

**Sum of prosecutorial costs, $2,037; and court costs $2,004.

household sample to the entire population of AFDC recipients.* This is equivalent to contending that our sample is a random sample from the whole population.

The annual savings and costs associated with changes in the control variables are shown in Table 14.** The savings are calculated by multiplying the savings in grant overpayment per household by the number of households. Two changes in the annual levels of effort are computed: one additional case referred to prosecutor per month, and a 100% increase in the existing enforcement level as measured by the number of investigations. The lower bound on the annual savings that would occur if one additional case per month had been referred to prosecutor from among those investigated is $14,202 in Seattle and $14,942 in Denver. The respective upper bounds are $71,056 and $74,712.

Associated with the annual savings are the costs which must be incurred in additional personnel time and capital. We are not able to give very precise estimates of costs but, starting with budget and activity measures, we estimated the entries in Table 14 for costs.

When we compare these cost estimates to the lower bound of savings, we see that, for Denver, the annual benefits of one additional investigation per month holding prosecution rates constant are two and a half times the costs. The corresponding benefits in Seattle would be roughly twice the

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* The average AFDC populations used for these calculations are 14,327 in Seattle and 11,382 in Denver.

** The denominator in the calculation of the investigation rate is not assumed to decline in response to the hypothetical increase in investigations. Therefore, the calculation underestimates the responsiveness of average grant overpayments to an increase in investigations. We did not pursue this point because it would require additional estimation, and because later qualitative results would not be affected.

costs. Apparently, one dollar spent on the control strategies we have studied would return over $2 in a decline of average grant overpayments. An additional investigation, even without a commensurate increase in prosecutorial resources, would also be quite cost-effective. In Denver, the return would be over $2 per dollar spent on investigation; in Seattle, the return would be approximately $1.50 per dollar spent on investigation. Similiarly, an additional referral to prosecution from those cases already investigated appears to be a very cost-effective control strategy. Our results suggest returns in excess of $2 per dollar spent in either site.

In Rows 3 through 6 of Table 14, we again find that increases in the control variables are associated with declines in grant overpayments that exceed the associated costs of control. Although the ratio of benefits to costs for doubling the levels of the control variables are smaller than the marginal change discussed above, all of the ratios exceed one, indicating that each of the strategies is cost-effective.* These estimated benefits do not include recoveries of past overpayments or savings derived from the removal of individuals from the AFDC rolls and disqualification for other aid programs, such as health care or food stamps. Consequently, on the basis of this analysis, substantial increases in the control strategies we have studied are justified on grounds that savings exceed costs.

* Increasing the values of the control variables by up to 100% does not produce values outside of the range of the control variable within our data. The mean of the investigation rate in our sample is .036, while the minimum and maximum values are .012 and .074. The mean of the rate of referral to prosecutor rate is .131 and the range is .010 to .754. The values used for the calculations given in Table 14 are given in Table 3, Appendix B. We have assumed that the costs of enforcement will be linear. Because enforcement in Seattle and Denver is a relatively small-scale effort, there are likely to be increasing returns to scale, thus improving the benefit/cost ratios.

Appendix A

EXAMPLE OF AFDC STANDARDS OF ASSISTANCE

4322.1 · CONCL.
ST. BD. APPROVAL: OCT. 5, 1973
EFFECTIVE: NOV. 1, 1973
T.L. #:
SUPERSEDES T.L. # 1296

Table A-1

AFDC STANDARDS OF ASSISTANCE TABLE-INCLUDING TOTALS FOR SUMMER-WINTER ALLOWANCES

ADULTS – AFDC			CHILDREN										
			1	2	3	4	5	6	7	8	9	10	EA. ADD
NONE	BASIC		36	76	114	152	191	229	259	290	320	352	30
	SHEL.		18	35	53	70	76	81	83	86	88	90	2
	UTIL.		3	7	11	14	18	21	22	23	24	25	1
	SUMMER – TOTAL		57	118	178	236	285	331	364	399	432	467	33
	TOTAL – WINTER	(1)	60	125	189	250	303	352	386	422	456	492	34
1-ALONE	BASIC	51	–	–	–	–	–	–	–	–	–	–	–
	SHEL.	64	–	–	–	–	–	–	–	–	–	–	–
	UTIL.	13	–	–	–	–	–	–	–	–	–	–	–
	SUMMER – TOTAL	128	–	–	–	–	–	–	–	–	–	–	–
	TOTAL – WINTER	141	–	–	–	–	–	–	–	–	–	–	–
1-W/OTHERS	BASIC	46	84	123	161	198	237	267	297	329	358	390	30
	SHEL.	64	64	68	72	76	81	83	86	88	90	92	2
	UTIL.	13	13	13	14	18	21	22	23	24	25	26	1
	SUMMER – TOTAL	123	161	204	247	292	339	372	406	441	473	508	33
	TOTAL – WINTER	136	174	217	261	310	360	394	429	465	498	534	34
TWO	BASIC	91	129	168	206	244	275	306	335	366	397	427	30
	SHEL.	68	68	76	76	81	83	86	88	90	92	95	2
	UTIL.	13	13	14	18	21	22	23	24	25	26	27	1
	SUMMER – TOTAL	172	210	258	300	346	380	415	447	481	515	549	33
	TOTAL – WINTER	185	223	272	318	367	402	438	471	506	541	576	34

(1) UTILITIES ALLOWANCE DOUBLED FOR THE FIVE (5) "WINTER" MONTHS; NOV. THROUGH MARCH.

Superseded By
T. L. # 1393
H.E.W. Sub. # 7-1-74

Appendix B

DEFINITION OF VARIABLES AND DATA

DEFINITION OF VARIABLES

MONTH:	Month of data where numbering starts with one for January 1970
GOVER:	Average grant overpayment for the month calculated from all the reference households available in the SIME and DIME data base
RGOVER:	Real average grant overpayment where the adjustment of GOVER is for a cost of living index--see text for details
POPOVER20:	Estimated number of recipients receiving grant overpayment of at least $20 calculated according to TOTCASES (OVER20/CASESPER)
INVGTNS:	Number of investigations carried out in a particular month by AFDC agency investigators
REFPROS:	Number of cases referred to prosecutors by AFDC agency investigators
INVRAT:	Estimated probability of investigation calculated as the ratio of INVGTNS to POPOVER20
REFRAT:	Estimated probability of referral to prosecutor given investigation and calculated as the ratio of REFPROS to INVGTNS
TOTCASES:	Total number of recipient household on AFDC in Seattle or Denver
OVER20:	The number of reference household in our SIME and DIME samples which received grant overpayments in excess of $20
CASESPER:	The number of SIME and DIME reference households in our sample in a particular month

Table B-1
SEATTLE

MONTH	GOVER	RGOVER	POPOVER20	INVGTNS	REFPROS	INVRAT	REFRAT	TOTCASES	OVER20	CASESPER
44	33.65	30.73	4854.87	251	3	.0517	.0119	14953	100	308
45	27.52	24.95	4293.91	247	10	.0575	.0404	15004	87	304
46	28.49	25.66	4187.11	198	14	.0472	.0707	14947	86	307
47	25.79	23.08	3058.67	184	21	.0476	.1141	14339	81	301
48	27.88	24.67	3925.93	152	2	.0387	.0131	15008	79	302
49	29.98	26.25	4094.81	116	21	.0283	.1810	14934	85	310
50	26.96	23.36	4036.43	102	12	.0252	.1176	15101	85	318
51	25.66	22.02	4269.48	214	18	.0501	.0841	15255	89	318
52	27.30	23.21	3916.91	263	32	.0671	.1216	15039	81	311
53	31.64	26.67	4280.35	225	25	.0525	.1111	14791	90	311
54	37.17	31.07	4411.63	247	21	.0559	.0850	14549	94	310
55	35.88	29.72	4665.37	146	14	.0312	.0958	14416	100	309
56	33.55	27.54	4662.14	274	18	.0587	.0656	14406	100	309
57	25.04	20.35	4087.92	204	23	.0499	.1127	14540	88	313
58	22.15	17.80	3876.01	256	22	.0660	.0859	14197	86	315
59	32.93	26.17	3997.27	183	19	.0457	.1038	14263	88	314
60	27.89	21.97	3880.25	107	18	.0275	.1682	14338	82	303
61	31.27	24.42	3936.71	231	13	.0586	.0562	14386	81	296
62	28.93	22.40	3921.77	248	14	.0632	.0564	14446	79	291
63	35.43	27.29	4415.78	237	10	.0536	.0421	14531	86	283
64	41.06	31.43	4345.00	280	14	.0644	.0500	14465	79	263
65	37.40	28.46	4252.02	274	10	.0644	.0364	14154	73	243
66	50.53	38.25	4842.69	252	10	.0520	.0396	13990	81	234
67	52.72	39.66	5166.08	185	21	.0358	.1135	14013	80	217
68	49.09	36.68	4843.69	183	11	.0377	.0601	14079	75	218
69	34.75	25.85	3664.61	218	15	.0594	.0688	14387	54	212
70	39.91	29.56	4208.13	313	3	.0743	.0095	14487	61	210
71	38.10	28.07	4020.02	116	8	.0288	.0689	14429	56	201
72	37.97	27.85	4217.39	113	13	.0267	.1150	14470	58	199
73	26.86	19.62	3797.93	174	6	.0458	.0344	14660	50	193
74	33.78	24.56	4203.90	148	19	.0352	.1283	14674	53	185
75	44.21	32.15	4669.9	235	8	.0503	.0340	14722	59	186
76	39.38	28.49	4268.6	192	19	.0449	.0989	14647	51	175
77	44.83	32.43	4376.8	247	12	.0564	.0485	14531	50	166
78	51.04	36.93	4956.7	178	10	.0359	.0561	14220	61	175
79	62.45	44.32	5400.9	101	12	.0187	.1188	14268	67	177

Table B-1 (concluded)

SEATTLE

MONTH	GOVER	RGOVER	POPOVER20	INVGTNS	REFPROS	INVRAT	REFRAT	TOTCASES	OVER20	CASESPER
80	62.97	44.69	5491.9	184	8	.0335	.0434	14298	58	151
81	47.02	33.37	4588.5	125	9	.0272	.0720	14421	42	132
82	75.27	52.71	5617.2	149	14	.0265	.0939	14426	44	113
83	79.76	55.85	6146.2	111	11	.0180	.0990	14290	40	93
84	72.14	50.51	5964.9	111	12	.0186	.1081	14348	37	89
85	61.40	42.14	5170.2	181	18	.0350	.0994	14510	31	87
86	49.45	33.93	5475.7	117	8	.0213	.0683	14270	33	86
87	55.66	38.20	6074.5	283	13	.0465	.0459	14542	33	79
88	36.03	24.05	4612.6	153	15	.0331	.0980	14299	20	62
89	55.34	36.94	4160.0	261	11	.0627	.0421	14263	14	48
90	58.28	38.90	3996.9	172	7	.0430	.0406	13989	12	42
91	114.19	74.53	7710.9	289	12	.0374	.0415	13976	16	29
92	68.28	44.56	5320.9	187	14	.0351	.0748	14189	6	16
93	57.42	37.48	5677.2	278	16	.0489	.0575	14193	2	5
94	78.72	50.72	14162.0	222	13	.0156	.0585	14162	1	1
Mean	44.13	32.59	4803.47	198.37	13.76	.0433	.0757	14467	61.64	204.31
Minimum Value	22.14	17.80	3664.61	101	2	.0156	.0095	13976	2	5
Maximum Value	114.19	74.53	7910.9	313	32	.0743	.1810	15255	100	318
Std. Deviation	18.29	10.88	1547.15	59.29	5.87	.0149	.0375	309.17	28.00	102.55
# of Obs.	51	51	51	51	51	51	51	51	51	51

NOTE: Data for August, 1973 to September, 1977

Table B-2
DENVER

MONTH	GOVER	RGOVER	POPOVER20	INVGTNS	REFROS	INVRAT	REFRAT	TOTCASES	OVER20	CASESPER
13	37.76	38.80	4587.34	53	31	.0115	.5849	9918	216	467
14	38.78	39.81	4867.21	67	24	.0137	.3582	10079	226	468
15	42.84	43.89	5097.74	95	19	.0186	.2000	10283	233	470
16	41.36	42.29	5055.03	102	14	.0201	.1372	10307	231	471
17	39.64	40.32	5056.03	107	15	.0211	.1401	10470	226	468
18	32.58	33.00	4805.15	102	14	.0212	.1372	10544	211	463
19	32.13	32.38	4724.60	74	42	.0156	.5675	10706	203	460
20	31.15	31.33	4552.24	108	24	.0237	.2222	10059	192	458
21	34.96	35.03	4800.35	102	31	.0212	.3039	11014	197	452
22	40.48	40.48	5144.40	69	52	.0134	.7536	11069	211	454
23	35.52	35.52	4813.42	82	51	.0170	.6219	11223	193	450
24	31.42	31.38	4600.60	68	37	.0147	.5441	11274	182	446
51	84.89	72.86	6324.71	162	13	.0256	.0802	11530	452	824
52	79.45	67.50	6306.78	141	13	.0223	.0921	11532	449	821
53	83.13	69.85	6480.22	137	28	.0211	.2043	11439	460	812
54	85.07	70.71	6418.24	127	11	.0197	.0866	11309	458	807
55	88.73	72.96	6448.28	144	13	.0223	.0902	11263	450	786
56	89.12	72.51	6417.77	147	27	.0229	.1836	11288	423	744
57	91.01	73.27	6622.21	110	21	.0166	.1909	11266	482	820
58	91.69	73.11	6707.79	102	7	.0152	.0686	11309	484	816
59	96.87	76.45	6757.60	103	4	.0152	.0388	11367	475	799
60	94.38	73.73	6662.68	142	15	.0213	.1056	11623	454	792
Mean	60.13	53.05	5602.29	106.54	23.00	.0188	.2596	10985	323.09	615.21
Minimum Value	31.14	31.33	4552.24	53	4	.0115	.0388	9918	182	446
Maximum Value	96.86	76.45	6757.60	162	52	.0256	.7536	11623	484	824
Std. Deviation	26.83	18.33	871.96	30.18	13.31	.0038	.2137	508.34	127.84	174.87
# of Obs.	22	22	22	22	22	22	22	22	22	22

NOTE: Data for January, 1971 to December, 1971 and March, 1974 to March, 1975.

Table B-3

MEANS OF VARIABLES USED IN CALCULATIONS FOR TABLE

	Seattle	Denver
RGOVER	41.30	72.30
INVRAT	.078	.114
APPRAT	.032	.020
INVESTIG	162,.33	131.50
PROSCTD	11.50	15.20
POP20	5224.97	6514.63
CASESPER	96.58	802.10
TOTCASES	14,327	11,382

END