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IMPACT OF CASH TRANSFERS

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ABSTRACT

This paper seeks to measure the reduction in labor supply and earnings induced by the U.S. transfer system in 1972. Using a simulation methodology on the Current Population Survey (CPS) micro data, estimates are obtained of the aggregate declines in work effort and earnings, and of the distribution of the totals among families. Several of the assumptions in the analysis are varied in order to derive alternative estimates of those declines. While the findings are subject to important qualifications, they are a necessary step toward understanding the true net redistributive impact of public transfers and the real economic costs of such expenditures.

The Real Cost and Net Redistributive Impact of Cash Transfers

In recent years the impact of government transfers upon alleviating poverty and reducing income inequality has received considerable attention from economists and policymakers.[1] The redistributive effect of transfers has uniformly been measured by simply comparing pretransfer and posttransfer incomes of the group being studied. The assumption implicit in this approach is that without X dollars from public transfers, the recipient's income would be X dollars lower.

A large body of evidence has shown that such an approach is not empirically sustained. It has been well established that, ceteris paribus, the higher a person's nonemployment income, the lower his labor supply, and the higher his wage, the greater his work effort.[2] Because government transfers raise the level of income attainable without working and, in some cases, lower the effective wage rate, it is likely that the observed work effort and earnings of transfer recipients are below what they would have been in the absence of transfers. Ignoring this effect in previous research has produced overestimates of the redistribution of income and reduction of poverty produced by transfers.

This paper, therefore, seeks to measure the reduction in labor supply and earnings induced by the U.S. transfer system in 1972. Using a simulation methodology on the Current Population Survey (CPS) micro data, estimates were obtained of the aggregate declines in work effort and earnings, and of the distribution of the totals among families. In doing so, several of the assumptions in the analysis are varied in order to derive alternative estimates of these declines. While the findings are subject to important qualifications, they are a necessary step toward understanding the true net redistributive impact of public transfers and the real economic costs of such expenditures.[3]

The labor supply impacts during 1972 of the six largest public cash transfer programs are analyzed in this paper. These programs are Social Security (ignoring Medicare), public assistance (omitting Medicaid), Unemployment Insurance, Veterans' Pensions, Veterans' Disability Compensation and Workmen's Compensation.[4] In 1972, expenditures on these programs were $68 billion.[5] This figure equaled 7.2 percent of personal income and 10.7 percent of total earnings.[6] These percentages are large enough that one cannot immediately assume that the potential labor supply effects of these outlays are trivial.

The paper is organized as follows. Section 1 discusses the general equilibrium effects of transfers and reviews the basic labor supply model upon which the empirical work is based. In section 2 the methods used in the simulation are described. Section 3 explains how the procedures of the simulation can be varied to obtain alternative estimates. Problems and shortcomings of the analysis are noted in section 4. The fifth section presents and interprets the findings. A summary and concluding comments appear in section 6.

1. GENERAL EQUILIBRIUM EFFECTS OF TRANSFERS - THEORETICAL CONSIDERATIONS

Besides the direct, "first round" redistributive effects of a transfer system, there are three major ways by which the general equilibrium distribution of income may differ from what it would have been in the absence of such a system. First, labor supply decisions may change. To the extent that changes in labor supply lead to changes in earnings, the income distribution is modified beyond the first round impact. Moreover, aggregate work effort and output will be altered. Second, because transfers shift purchasing power among the nation's consumers, demand patterns may be affected. A shift in demand patterns would alter relative prices and generate changes in the demand for and prices of factors of production. Such adjustments would further modify the distribution of income. Empirically these effects may be small, but their presence must be acknowledged.[7] Third, persons' choices of living arrangements may be profoundly affected by the existence of transfers. For example, without old age insurance to maintain an independent household, elderly persons may choose or be forced to live with their children. These decisions affect the number of separate households and the distribution of income among them.

Analysis will be restricted to the first of these three effects. Analysis of the impact of transfers on prices, resource allocation and living arrangements is beyond the scope of this study.

The standard model of labor supply provides a useful framework for analyzing the change in labor supply attributable to transfers. Consider a person with wage rate W and, for simplicity, no source of nontransfer income except his labor. The utility maximizing equilibrium between labor and nonmarket activity is reached at E in figure 1, with H_E hours of labor. Consider the same individual when eligible for a transfer benefit. In figure 1, the recipient is guaranteed CD dollars if he earns no income. Benefits are reduced by X dollars for every W of

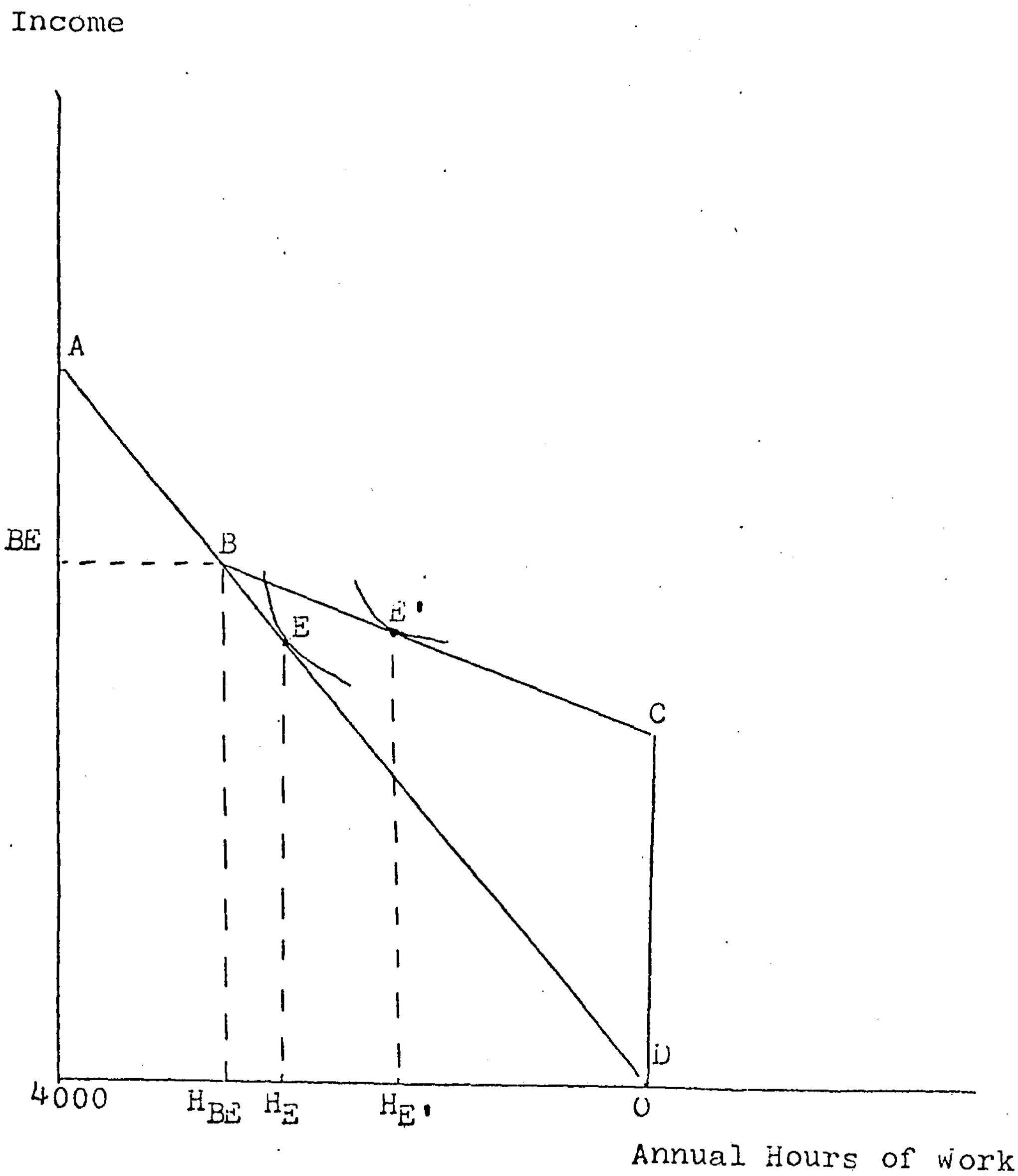

Figure 1. Labor supply with and without transfers.

earnings until, when earnings reach BE (the breakeven level), benefits are exhausted. The transfer imposes a tax rate of (W-X)/W on earnings. Beyond BE of earnings, corresponding to H_{BE} hours of work, the wage returns to W. As a result, the recipient faces budget constraint ABCD. Equilibrium along this line is at E', indicating that if the transfer reduced work effort below the level it would have been with no transfer. The next section will elaborate in detail how to implement this guarantee-tax rate approach to derive numerical estimates of the difference between H_E and $H_{E'}$, and the corresponding difference in earnings.

2. ESTIMATING LABOR SUPPLY EFFECTS OF TRANSFERS - PROCEDURES

Each of the six programs to be analyzed can be characterized in terms of a guarantee, G, and a marginal tax rate, T, which it imposes on each recipient. Before presenting these characterizations, however, the equations used to calculate the labor supply effects will be discussed. It is into these equations that the appropriate G and T for each person are substituted.

Operational Equations

An individual's annual hourly labor supply (LS) is related to the level of nonemployment income (NEY), his wage rate or expected wage (W), and a vector of demographic traits (Z), such as education, age, and sex. Assume the following functional form for this relationship:

$$LS = a + bNEY + c(\ln W) + dZ.^{8} \tag{1}$$

If the individual receives a transfer, NEY is replaced by NEY+G while the net wage falls to (1-T)W. Hence,

$$LS' = a + b(NEY+G) + c\ln[(1-T)W] + dZ. \tag{2}$$

The change in LS is the difference, DLS, between (1) and (2):

$$DLS = -bG + c\ln[1/(1-T)]. \tag{3}$$

The resulting change in earnings, DE, is:

$$DE = DLS \cdot W. \tag{4}$$

Empirically it has been generally found that $b<0$ and $c>0$. Also, $G>0$ and $1/(1-T) \geq 1$, which means $\ln[1/(1-T)] \geq 0$. Thus, DLS will be greater than zero. It follows that a transfer payment of value P raises final money income by only P - DE.

In a household with more than one member, labor supply decisions are interdependent according to economic theory. Receipt of transfer income by one family member can influence the work effort of all other members. The formula in (3) must be modified. Consider a two-person living unit. Person i receives payment P_i from a program with parameters G_i and T_i, i=1,2. In the absence of transfers, person 1's LS would simply be:

$$LS_1 = a_1 + b_1 NEY + c_1 \ln W_1 + d_1 Z_1, \tag{5}$$

where in this case NEY is family nonemployment income received from private sources.

With transfers the payment of P_2 to person 2 will have an income effect on person 1 because, as far as person 1 is concerned, P_2 increases the nonemployment income available to him. Person 1's own transfer, of course, generates its own wage and income effects by reducing the wage to $(1-T_1)W_1$ and raising NEY by G_1. Moreover, because transfers cause person 2 to alter his work behavior, the earnings of person 2 to alter his work behavior, the earnings of person 2 change by $DLS_2 \cdot W_2$. This change in family income also generates an income effect upon person 1. Thus:

$$LS_1' = a_1 + b_1(NEY+P_2+G_1 - DLS_2 \cdot W_2) + c_1 \ln(1-T_1)W_1 + d_1 Z_1. \quad (6)$$

[9]

The difference between (5) and (6) is the net labor supply change created by transfers:

$$DLS_1 = -b_1(P_2 + G_1 - DLS_2 \cdot W_2) + c_1 \ln[1/(1-T_1)]. \quad (7)$$

A similar expression with reversed subscripts expresses DLS_2. Solving the equations simultaneously yields the correct adjustments in labor supply for both people. For households with three or more members (or potential members) of the labor force, this procedure is readily extended.

If an individual receives more than one type of transfer, the guarantee in the above equations equals the sum of the separate program guarantees. The tax rate becomes cumulative:

$$T = 1 - (1-T_1)(1-T_2). \quad (8)$$

<u>Determining Parameter Values</u>

To solve (3) or (7) for each living unit that receives transfers, one must insert proper values for b, c, W, G and T. Methods for obtaining these values will be outlined below. Other technical aspects of the study and the data to which the simulation is applied are also described. In section 4 it will be shown how the procedures can be varied to obtain alternative results.

Values of b and c are taken from a study by Garfinkel and Masters[10] and assigned according to the demographic characteristics of the person involved. Labor supply functions were estimated for twenty groups distinguished by age, sex, and marital status. The groups and values of b and c associated with each are listed in Appendix Table A1.

In solving (4) or (7), hourly wage rates of all family members who work or might potentially work must be known. If a person reports full-time work for 50-52 (48-49) weeks, the wage is obtained by dividing reported earnings by 2000 (1940). For all others, a wage rate is imputed based on wage regressions for persons with similar socio-economic traits who worked full-time for 48-52 weeks.[11]

Assigning Guarantees and Tax Rates to Transfer Recipients

If a family reports zero transfer income, assume that eliminating transfers would not affect its labor supply choices. For such families, G = T = 0 for all members; hence DLS = 0 for all of them.

There are two possible reasons why this procedure may be faulty. One or more members may be eligible to receive a transfer, but earn enough income to exceed the breakeven point. For example, a 66-year-old man may continue working instead of collecting Social Security. Because he knows he can fall back on Social Security should the need or desire arise, he may work less than he would have in the absence of the program. Similarly, for some people, potential eligibility for public assistance or Unemployment Insurance may reduce work effort. In general, potential transfer income, though not being enjoyed currently, raises the persons's long-run consumption prospects and, therefore, can exert a negative impact on current work effort. Measuring this effect would be extremely difficult and I have not attempted to do so.

The assertion that households with zero transfers would not be affected by their removal can also be disputed when interhousehold transfers are considered. This line of argument may be best illustrated with an example. Consider an elderly couple living independently of their son and his family. The couple receives Social Security. The son has no transfer income and does not contribute to the support of his parents. In the absence of Social Security, he may desire to give them some income support, and, to do so, the might increase his earnings. Social Security, therefore, allows the son to work less, even though his immediate family is not the direct recipient of the benefit. Estimating this indirect labor supply impact of transfers is impossible given the data available and the general lack of understanding of interhousehold voluntary transfers.

Let us now consider how to assign G and T to a person who reports income from one of the six programs to be analyzed. Procedures for Social Security are discussed in some detail, for several of the problems which arise in setting the G and T for this transfer also occur for other programs.

Social Security:[12] If a recipient of Social Security (SS) received P dollars of benefits and earned less than \$1680 in 1972, (e.g., V in figure 2) G = P and T = 0. The same is true, regardless of earnings, if the person is 72 or older. The recipient behaves as if his budget constraint were BSO.

If earnings (E) were between \$1680 and \$2880 (e.g., X in figure 2), the law required that payments be reduced one dollar for every two earned dollars. Hence, T = .5. The amount of SS that would have been received if the person had earned nothing is P + .5(E - 1680). However, this value is not the correct G to enter in (3). Given that labor supply functions have been estimated under the assumption of simple linear budget lines, one must determine the linear constraint that passes through X with slope of -.5W. Clearly CS'O is this constraint. The recipient acts as if his labor supply choice had been constrained by CS'O. Thus, the analytically correct G (the "shadow guarantee") equals the distance OS', or OS + 840. And OS = P + .5(E - 1680).

If E exceeded \$2880, benefits were cut one dollar for every dollar earned over \$2880. The possibility that T = 1 raises a troublesome issue. A person observed at U in figure 2 can enjoy the same income at Z and work fewer hours. His behavior at U is economically irrational. Moreover, ln[1/(1-T)] is undefined if T = 1, making computation of DLS impossible.

Two possibilities reduce the likelihood that persons observed earnings over \$2880 in calendar year 1972 are, in fact, at a point like U. First, some of those who earned more than \$2880 may have retired or otherwise become eligible for benefits in mid 1972 and then begun collecting SS. In such cases the tax rate faced when receiving transfers was quite likely to have been zero or .5. Second, SS is administered on a monthly basis. In 1972 one could receive full benefits in any month in which earnings were less than \$140. Situations in which a person concentrated his leisure within a few months and

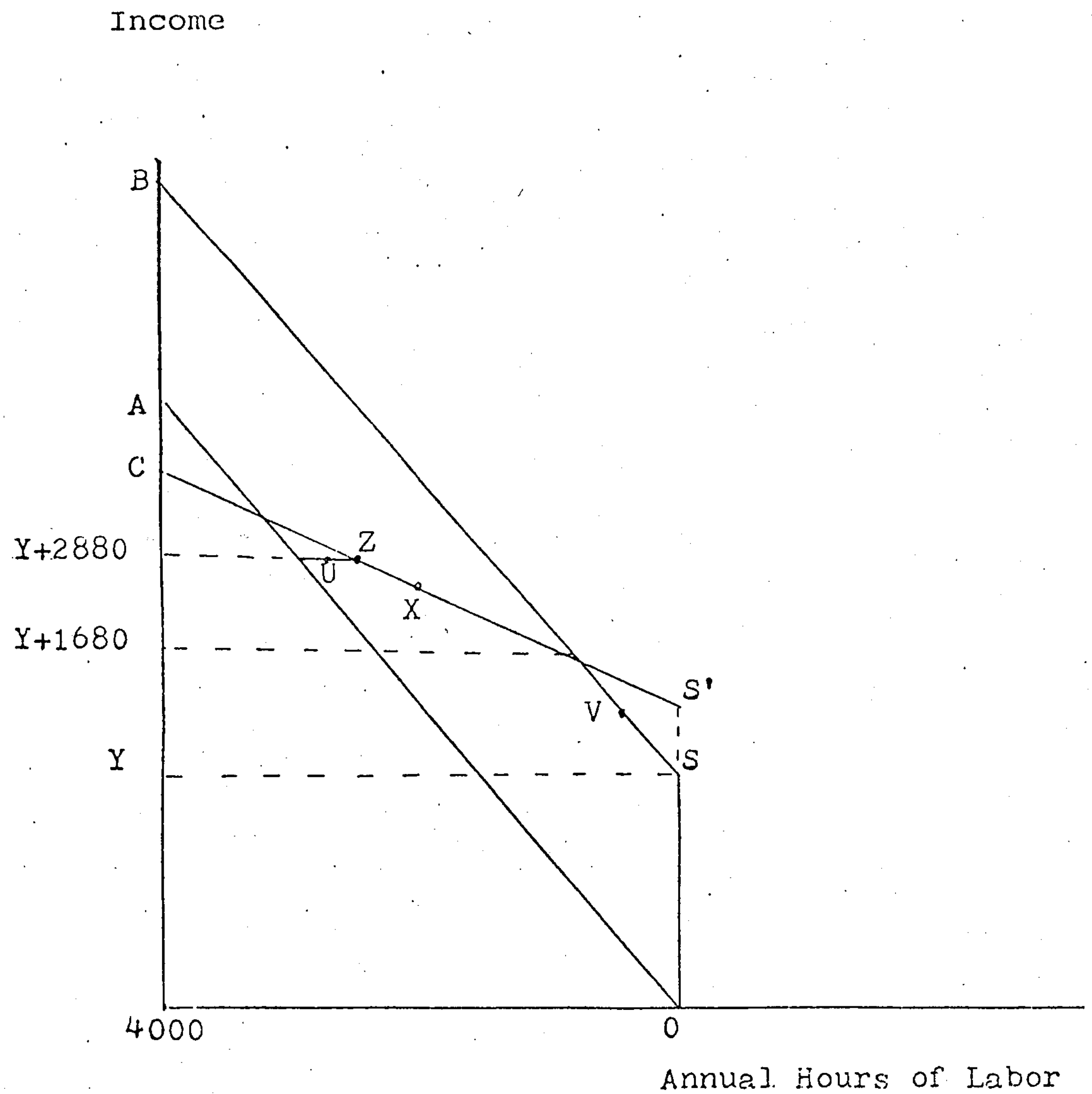

Figure 2. Budget constraints for Social Security recipients.

received SS during that period, but earned more than $2880 in the other months have been common. These beneficiaries faced marginal tax rates of zero or .5 in any given month even though annual earnings exceeded $2880.

Of course, some persons with SS benefits and earnings over $2880 undoubtedly really did face T = 1. Such individuals probably encountered institutional constraints which forced them to a point such as U.[13] In this situation the observed labor supply does not reflect an equilibrium position. Because the theory analyzes adjustments of equilibria to varying levels of G and T, under the assumption of no constraints on labor supply, it is not clear at the conceptual level how to treat these cases.[14]

To perform the simulation it was decided to set T = .5 in all cases where earnings exceeded $1680. The shadow guarantee was calculated as shown above.

The need for this procedure stems from a weakness of the CPS data used in this study. When transfer programs are administered with monthly accounting periods, it is best to use monthly data on labor force behavior. However, CPS data only provide information on earnings, work effort, and transfer income for the calendar year. Hence, the operation of the program must be annualized. In doing so, problems such as the above arise. Nonetheless, there is no other option if any results are to be obtained. The errors necessarily introduced with this approach temper the findings but not so much (in my view) as to invalidate the major conclusions.

Veterans Benefits: The veterans' cash transfer programs, like Social Security, operate under uniform federal standards. Veterans Compensation (VC) provides monthly benefits to persons disabled by injury or disease in the course of their military service. Benefits vary according to the percentage loss of earnings capacity caused by each disability. In 1972, monthly compensation payment ranged from \$28 to \$862.[15] Benefits are independent of the level of recipient earnings and other income. Thus, $T = 0$ and G simply equals the observed payment. Survivors of servicemen who died in service or from a service-related cause may also receive VC. For these individuals too, $T = 0$ and G equals the observed payment.

In contrast to VC, veterans' and survivors' pensions (VP) are income-tested programs available to needy veterans and their survivors. In 1972 a poor single veteran was guaranteed \$1560 per year and faced an implicit tax rate of .6, exhausting his benefits when earnings (and other countable income such as property income) exceeded \$2600. If the veteran has dependents, the G is larger and the tax rates to about .45. Survivors' pensions operate in a similar fashion, but with different combinations of G and T specified in the law.[16]

The CPS data report income received from a veterans' transfer program, but do not identify whether the aid was VC or VP. A decision rule is needed to distinguish between the programs in order to substitute the proper G and T into equation (3) or (7). If a recipient's countable income is below the breakeven point established by the pension program and his reported benefits are less than the pension G to which he would be entitled, I assume he is receiving a pension. Otherwise, I treat the income as compensation and set $T = 0$.

Unemployment Insurance: Estimating the labor supply affects of Unemployment Insurance (UI) presents problems at both the conceptual and empirical level.[17] Being unemployed is a state of disequilibrium, but the static theory of labor supply analyzes changes in equilibria. Hence, as a first step, assume all unemployment is voluntary--unemployed workers can find new jobs at equivalent wages if they so desire--and, further, that the work test has no effect on behavior. The budget constraint faced by someone who is newly laid off and eligible for weekly UI benefits for, say, the next 39 weeks, is shown in figure 3 by OSHW. The UI system increases NEY by 39 times the weekly benefit and, during the 39 week period, decreases his net wage by the ratio of weekly benefits to weekly earnings. The slope of HW shows the effect of the UI tax rate. Segment HS is parallel to OW, the budget line in the absence of UI. After 39 weeks, when entitlement to UI expires, the net wage returns to the preunemployment level.

If the observed labor supply of a UI beneficiary falls along HW, the person behaves as if his constraint were OS'W, where S' equals S multiplied by the ratio 52/39. This is another case of the need to use a shadow guarantee in (3). If the recipient is observed along HW, he was out of work for more weeks than he was entitled to UI. To ascertain whether a recipient is on HW or HS, assume he was eligible for 39 weeks of UI (the maximum allowed in 1972). If he reports more than 39 weeks of unemployment, he is on HS; if not, he is on HW.

For persons on HS, G = OS = observed UI payments, and T = 0. To compute the G and T for a recipient on HW, one first observes that the weekly benefit is a stated fraction (usually .5) of the

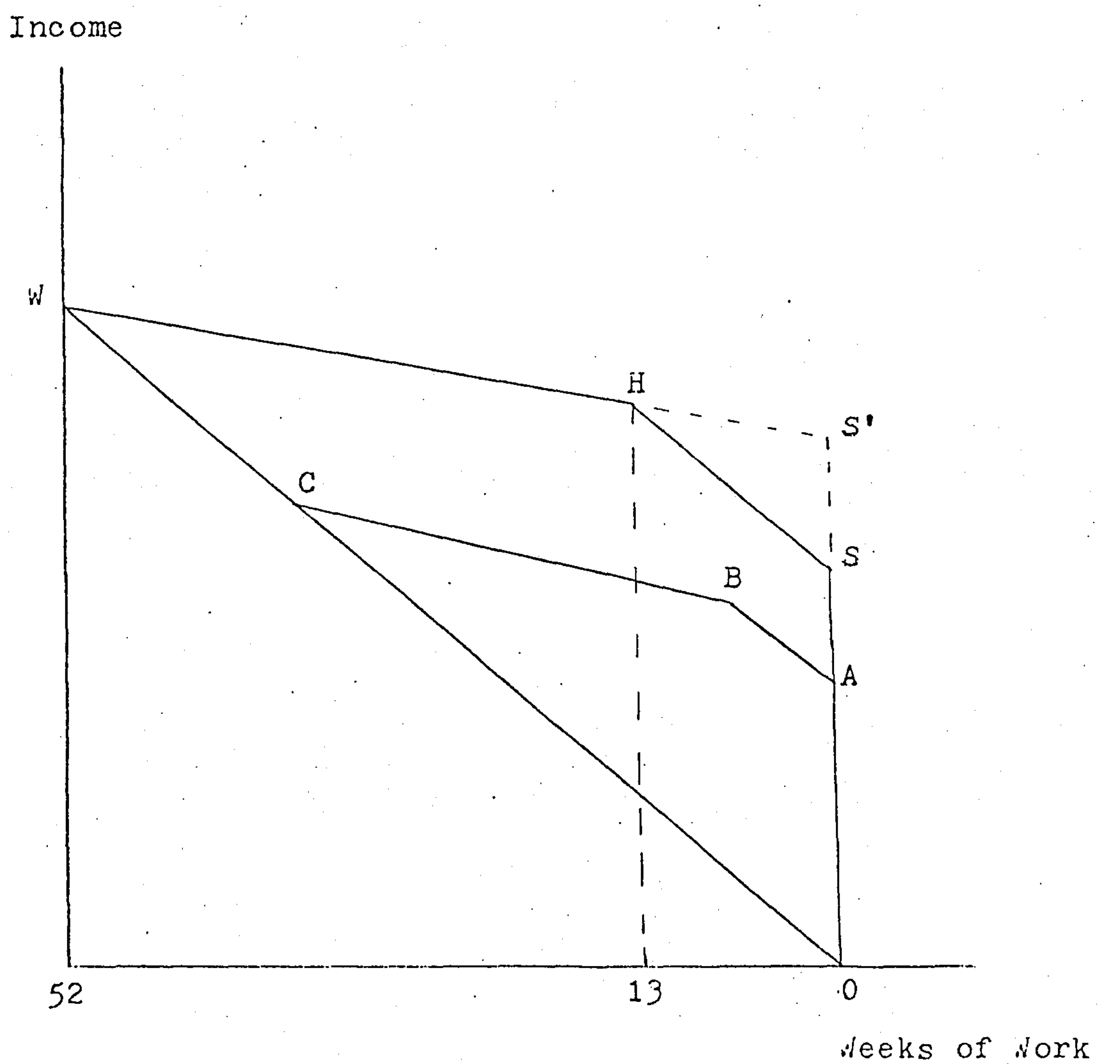

Figure 3. Budget constraints for recipients of Unemployment Insurance and Public Assistance.

recipient's average weekly wage in the period preceding unemployment. However, the payment cannot exceed an established maximum nor be less than a statutory minimum. The fraction and the constraining maximum and minimum vary from state to state. Thus, I first estimate his weekly earnings by multiplying the imputed hourly wage by 40 if he reports full-time work when employed, and by 20 if he reports part-time work. The result is multiplied by the fraction appropriate for his state of residence and constrained to fall within the state's upper and lower limits. Finally, this weekly benefit is multiplied by 52 to obtain the shadow guarantee. The tax rate equals the weekly payment divided by weekly earnings.[18]

Public Assistance: The budget line for recipients of public assistance (PA) is shown in figure 3 by OABCW. Persons are guaranteed a basic income OA if they earn nothing. If earnings rise slightly, no benefits are removed (AB is parallel to OW). These earnings are "set aside". As earnings continue to grow, benefits are reduced, which imposes a tax on the earned income, until breakeven point C is reached.

To determine the appropriate G and T for each recipient, several steps are needed. First, the data do not identify whether Old Age Assistance (OAA), Aid to Partially and Totally Disabled (APTD), Aid to the Blind (AB), AFDC (and AFDC-UP), or General Assistance (GA) is providing the household's benefit. (In 1972 SSI did not exist.)

If the recipient is age 65 or over, assume he/she receives OAA. If the recipient is under 65, has children under 18 and is female, she is assumed to get AFDC. If a recipient meeting the first of those two tests is male, assume he received AFDC-UP if his state provides this benefit and he reports being unemployed during part of 1972. For a person satisfying none of the above conditions, if he is disabled, assume he obtains aid from APTD.[19] Welfare recipients who pass none of these tests are assumed to collect GA.

After establishing a recipient's program, state rules are consulted to discover the maximum yearly benefit payable to him if his household had no other income.[20] Following PA regulations, from this maximum the value of the household's property income and other nonemployment income is subtracted. In doing so, the guarantee from any non-PA transfer is counted as part of nonemployment income in determining the maximum payment, i.e., the guarantee from PA.[21]

For all programs except AFDC and AFDC-UP, the net guarantee was calculated according to official regulations. For AFDC, Irene Lurie estimated for 23 states the effective benefit reduction rate applied to all types of nonemployment income. The effective rate generally differs from the statutory rate (which in most states is 100 percent after a small exclusion) due to administrative error or discretion, and to misreporting or fraud by recipients. Because the effective rate better describes the way in which AFDC guarantees are actually set, her estimates are used in the calculations. For states she did not analyze, the result for a neighboring state is used. Reasoning that AFDC-UP is probably administered along the lines of AFDC, the same estimates are used to determine the net G from this program.

With AFDC and AFDC-UP, a household can earn up to $30 per month without loss of benefits. The annual set-aside is $360. Thus, if annual family earnings are below $360, T is set at zero. Lurie has also prepared estimates of the effective AFDC tax rate on earnings that exceed the set-aside. These results, which typically show marginal rates far below the statutory 67 percent are used for T if earnings exceed \$360. These tax rates are also applied to the AFDC-UP program.

For OAA and APTD, marginal tax rates on earnings are determined by reference to the official regulations. Recipients may earn up to \$20 a month without a loss of aid ($T = 0$). If monthly earnings exceed this value but are less than \$80, benefits are reduced by 50 percent of earnings over \$20 net of work expenses. Since work expenses average about 35¢ an hour,[22] an hour of work at wage W would reduce benefits by $.5(W - .35)$. The tax rate is $.5(W - .35)/W$. The tax rate jumps to 100 percent on all earnings over \$80 per month.

The data force me to annualize these transfer programs. Thus, $T = 0$ if yearly earnings are less than \$240 and $T = .5(W - .35)/W$ if they are more than \$240. As noted in the SS discussion, setting $T = 1$ leads to indeterminate results. Thus, persons earning over \$960 ($12 \cdot 80$) are assumed to be on welfare only in months when earnings fall below \$80, so that the tax rate is never equal to one.

Regulations for GA vary across states. Information on how states tax earnings of GA recipients is scanty, but a 100 percent rate on all earnings appears to be common.[23] In such a situation, homo economicus earns nothing. Hence, if a GA recipient reports positive earnings, I assume the income accrued prior to or after being on GA and set $T = 0$.

In analyzing any program of PA, it should be noted that the welfare payment depends on the nonemployment income and the earnings of all family members, even though the actual benefit is paid to one person, usually the head. Hence, theoretically, the net G and the T directly effect the work behavior of each family member since earnings by any one of them can lead to lower PA payments. Thus, a common value for G and T must be entered into the labor supply equation of each member.[24] (For the other programs, in contrast, only the work behavior of the recipient affects the level of payment, so the appropriate G and T appear only in his own labor supply equation.)

Workmens' Compensation: Workmen's Compensation (WC) benefits are paid to persons who suffered work-related injuries that resulted in loss of earnings. Most WC recipients have short-term injuries that totally prevent any work activity.[25] For such persons, WC has no wage effect (T = 0). Partially disabled persons who resume work but collect WC do not lose benefits as earnings rise, so again T = 0. However, WC does exert an income effect. In (3) or (7), therefore G = observed WC benefit.

Further Technical Considerations

Sample Used: The March 1973 Current Population Survey provided the sample for this study. Data on each family member's earnings and transfer income for 1972 were collected, along with considerable other socio-economic information. Observations of this sample and predictions based upon it are inflated to national levels using sample weights in the data file.

This analysis used a subsample of the CPS that excluded all households with heads under 20 years of age, all persons under age

20 or in school, and members of the armed forces. Estimates of the labor supply functions for these groups were not available.[26] The subsample contains 81,649 persons in 45,400 living units representing 125.0 million Americans and 69.2 million living units.

Correcting for Underreporting of Transfers: Transfer income is greatly underreported in the CPS. To remedy this problem, which would lead to downwardly biased results, all reported transfer payments for a given program were uniformly inflated by an appropriate correction factor.[27]

Constraining the Solution: Solutions of (7) sometimes indicate that the elimination of transfers would lead to a reduction in labor supply greater than the observed initial number of work hours. In such cases the net predicted labor supply is constrained to equal zero.[28]

3. VARIATIONS OF THE SIMULATION

Obtaining Lower Bound Estimates

There are several reasons why the procedures just described will give upper bound estimates of the labor supply effects of transfers. Consider the treatment of UI. Not all unemployment is voluntary. Many, perhaps most UI beneficiaries would not be able to find jobs--particularly at their normal rate of pay--for as long a period as they received UI even if this transfer did not exist. Not all UI recipients are entitled to benefits for 39 weeks, nor do all receive the highest fraction of average weekly wages allowed by state law. The work test is not completely ineffective. Further, economic theory suggests that major economic choices, such as one's consumption stream, investment in human capital, or labor market activity, depend upon long-run considerations. Because the increase in permanent nonemployment income from receipt of UI is very small, the UI guarantee probably has a much smaller income effect than what the simulation will calculate. Also, to the extent that UI benefits simply displace private savings for the rainy days of unemployment, there is no income effect.

The SS guarantee may also overestimate the increase in a recipient's long-run nonemployment income. If SS had not existed, elderly persons might have saved enough money to generate the same stream of income as they receive from the program.[29] Persons receiving SS because of disability or the death of the breadwinner might have purchased an equivalent value of private insurance in the absence of SS. It is highly unlikely that private choices in the absence of SS would yield an income flow which precisely matches the pattern produced by this transfer. Nonetheless, it can be argued that the true income effect of SS is much smaller than would be computed with the procedure outlined earlier. A similar argument could be made about Workmens' Compensation.

The treatment of veterans benefits and public assistance is less likely to lead to upper bound estimates. The G from these programs probably does represent a real increase in NEY and, consequently, induces an income effect.

The change in labor supply was calculated using procedures which reflect the above arguments. It was assumed that the first ten weeks of unemployment were involuntary and that all recipients were entitled to only 14 weeks of UI. Further, the G from SS, UI, or WC was not entered into equation (3) or (7) for recipients of these transfers, nor was P_2 entered in (7), where P_2 would be the amount of SS, UI, or WC received by the other family member. Within the framework of this study, the results provide a lower bound on the estimate of the labor supply effects of transfers.

Changes in the Wage Rate

If the removal of transfers leads to noticeable increases in labor supply, theory suggests that the wage level must fall if the new supply is to be absorbed. The simulation procedures described in section three assumed all wages would remain constant. To extend the simulation to account for the possibility of lower wages, estimates of the change in labor supply were derived using the upper bound assumptions plus the additional condition that wages uniformly decline by 5 percent (except that no wage was permitted to fall below the minimum federal wage). This figure was chosen arbitrarily, but seemed reasonable after results based on the constant wage assumption were examined.[30]

The Positive Tax System

Labor supply, theoretically, is partly determined by the wage rate, net of <u>all</u> taxes. The preceding methods have incorporated the effect of the tax rate imposed by transfers, but have ignored the direct taxes of the normal tax system. If the combined income and payroll tax at the margin is t, the net wage when the transfer tax rate is zero is (1-t)W. The transfer tax rate, T, is typically levied on gross wages. Hence, with transfers the net wage is (1-t-T)W. The solution to equation (3) becomes:

$$DLS = -b^G + c\ln(1-t)/(1-t-T). \tag{9}$$

To conduct the simulation with (9) instead of (3), t was approximated for each family by calculating its marginal federal income tax rate and adding to this the employee share of the Social Security tax.[31]

4. SHORTCOMINGS OF THE SIMULATION METHODOLOGY

Though the parameters of the various cash transfer programs have been modeled as accurately as possible, there are problems with the methodology which qualify the empirical findings. These shortcomings will be discussed in this section (beyond what has been discussed in footnotes or earlier text) before turning to the results themselves.

The methods assumed that all transfer programs were administered strictly according to well defined laws and regulations. In fact, administrative error and discretion, fraud and honest mistakes lead to frequent occasions where persons face effective guarantees and tax rates that are too high or low compared to those prescribed by the program regulations. This has been well documented for AFDC, but the phenomenon surely is present in all programs. The results of the simulation are distorted, therefore, because persons made labor supply decisions conditioned by parameters other than the ones imposed by my methods. Except for the use of Lurie's AFDC findings, no attempt is made to treat this problem. Indeed, given the data, can anything be done?

The analysis assumed direct taxes on earnings would remain constant even though transfers are removed. If taxes were reduced by an amount equal to transfer expenditures, labor supply might increase because the net wage will have risen, unless the income effect offsets this movement. This issue is not considered.

Yet another issue for future work concerns the effect on property incomes of an increase in labor supply. A rise in labor supply, whether it succeeds in bidding wages down or not, makes capital relatively scarcer and its marginal product should, theoretically, rise. Recipients of property income will experience an increase in income as a result. No attempt was made to determine the magnitude and distribution of this income.

Finally, the results of this analysis are based on the assumption that persons will find employment at their expected wage for the full length of the predicted change in work effort. This raises two problems. First, given an economy with persistent unemployment, could this labor supply be absorbed, even if wages fell slightly? I assume it could be, but this is probably not true. Because of this, the results of each simulation actually represent the maximum loss of output caused by transfers, under the given set of assumptions. Second, it is assumed that someone who was not in the labor force would join the labor force, work the predicted number of hours, and then withdraw from the labor market. If the predicted change in labor supply is large, this may be a plausible occurrence. But if the change equals, say 40 hours, is this sensisble? A useful refinement of this simulation would seek to predict whether the person would enter the labor market if transfers were removed and then, given the choice to participate, the number of work hours.

5. RESULTS

The aggregate changes in labor supply generated by cash transfers are presented first. The impact of these changes on the incidence of poverty, the poverty gap and the income distribution are then discussed. All the findings discussed are only for the year 1972, of course. As changes occur in the eligibility rules, the implicit structure of guarantees and tax rates of each separate program and the business cycle, the results of this type of simulation will also change.

Table 1 provides data on the total number of hours worked, total earnings and total transfer income in 1972. The CPS sample used in the analysis represented 125 million persons age 20 and over. They worked 140 billion hours and earned \$632 billion. Benefits from the six programs analyzed in this study equaled \$60.7 billion.[32]

Table 2 contains findings on the transfer induced loss of work effort and earnings. Column 1 contains what is called the "basic" upper bound results, that is, those based on the simulation procedure outlined above in detail. Columns 2, 3 and 4 present results obtained from the lower bound assumptions, the effects of a 5 percent wage decline and the adjustment for normal taxes, respectively.

The basic simulation shows a 7.4 billion decline in labor supply and a corresponding loss of earnings of \$19.8 billion. Though large

Table 1

Hours, Earnings and Transfer Income of Samply Population, 1972 (in billions)*

Hours Worked	140.09
Total Earnings	$632.49
Total Cash Transfers	$ 60.73
Social Security	38.08
Public Assistance	9.93
Unemployment Insurance	5.32
Veterans Benefits	5.80
Workmens Compensation	1.59

*Details do not sum to total due to rounding error.

Table 2

Aggregate Impact of Transfers on Labor Supply and Earnings, 1972

	Simulation			
	Basic	Lower Bound	5% Wage Fall	Tax Adjusted
Labor Supply				
(billions of hours)	7.43 hrs	2.20 hrs	7.43 hrs	8.23 hrs
(percent of total)	5.08	1.50	5.08	5.62
Earnings				
(billions of dollars)	\$19.82	\$5.82	\$18.84	\$21.91
(percent of total)	3.13	0.92	2.98	3.46
Efficiency Ratio (change in earnings/benefits paid)				
All Transfers	.33	.10	.31	.36
Social Security[a]	.30	.01	---	.30
Public Assistance[a]	.32	.32	---	.37
Unemployment Insurance[a]	.62	.10	---	.83
Veterans Benefits[a]	.27	.27	---	.30
Workmens Compensation[a]	.35	.00	---	.35

[a]Based on families who report transfer income only from the one program in the row.

in absolute value, these figures represent only 5.1 percent of total work hours and 3.1 percent of total earnings. Results based on lower bound assumptions are about 30 percent the size of these upper bound estimates. The losses are 2.2 billion hours (1.5 percent) and $5.8 billion (.9 percent).

Adjusting the basic simulation for a fall in wages yields essentially no difference in the aggregate results. The reduction of labor supply is a trivial 1.1 million hours less than the basic results (too small to appear in the table). Earnings fell $18.8 billion. This is 5 percent less than the figure in column one, which simply reflects the assumption that wages fell 5 percent.

Finally, integrating federal income and payroll rates into the simulation raises the estimated loss of labor supply and earnings by 11 percent. One should not be surprised by this modest difference. Using equation (9) instead of (3) will not give different results if T (marginal transfer tax rate) = 0. For many recipients of SS and for some getting UI and PA, this is the case. The same is true for all those receiving WC and Veterans Compensation. Further, (9) will yield substantially larger impacts on labor supply than (3) only if t (normal tax rate) is not close to zero. However, the majority of transfer recipients have such low levels of income that, after subtracting deductions and exemptions, the marginal income tax rate is zero. For these persons, t equals the low payroll tax rate.

The lower panel of Table 2 provides another measure of the economic cost of transfers. Suppose the transfer induced decline in earnings approximately indicates the value of output sacrificed by the provision of transfers (the leak in Okun's bucket).[33] The basic simulation shows that on the average one dollar of transfers cost the economy 33 cents of foregone output. This cost varies across programs because they impose different structures of guarantees and tax rates and are distributed among demographic groups in very different ways. For households in which SS is the only transfer received, the cost is 30 cents per dollar of aid.[34] The analogous statistics for PA, UI, WC and veterans benefits are 32, 61, 35 and 27 cents. Lower bound estimates, in contrast, suggest the overall cost is 10 cents per dollar. The cost of SS is a trivial penny per dollar; the cost of a dollar of UI falls to 10 cents and WC imposed no efficiency losses. Results for the other programs do not change because they were treated identically in both simulations. Column four shows that the tax adjustment raises the basic simulation ratios by a modest amount.[35]

The reduction in earnings caused by transfers is actually an overestimate of the social cost of these programs. The additional leisure of transfer recipients is of value, but this benefit is ignored in Table 2. Measuring the welfare loss created by transfers provides perhaps the most appropriate indicator of their true cost. Arnold Harberger has shown that, under suitable conditions, the welfare cost of a tax on earnings is approximately $1/2\ (eT^2wL)$, where e is the supply elasticity, T the marginal tax rate, w the pretax wage and L the quantity of labor. When the Harberger formula is computed for each of the 20 demographic groups and the results summed, it is found that the welfare cost of transfers was $1.00 billion in 1972. This equals 0.16 percent of total earnings. The cost is trivial.[36]

Tables 3, 4 and 5 indicate the net redistribution of money income created by transfers after one corrects for the transfer induced loss of earnings. The tables thus show the extent by which simply comparing pretransfer and posttransfer incomes overstates the real reduction of poverty and income inequality caused by public benefits.

Table 3 reports for eight demographic groups the observed incidence of pretransfer ("no adjustment") and posttransfer poverty (as defined by the federal government) and the level of poverty that would have been observed in the absence of transfers according to the four simulations. One would expect substantial differences among demographic groups because they receive different mixes of program benefits and their members have different labor supply functions. Consider column one. The top entry shows that if transfers had zero impact on earnings, 7.2 percent of all families with male heads age 20 to 64 would have been poor if transfers had not existed. The second line indicates that 6.5 percent of this group would have been poor if transfers had been removed and the assumptions of the basic simulation held. Most of the pretransfer poor in the no adjustment world would remain poor after adjusting their labor supplies. The other rows and columns can be similarly interpreted. For all groups the tax adjusted (lower bound) model yields the greatest (smallest) difference from the unadjusted level of pretransfer poverty. This finding is consistent with the aggregate results in Table 2.

Table 3

Incidence of Poverty Without Transfers Using Different Adjustments for Labor Supply Responses Compared to the Incidence of Posttransfer Poverty, 1972, by Demographic Groups

Poverty without transfers	Demographic Group								
	Families (by sex and age of head)				Unrelated Individuals				Total
	M,20-64	F,20-64	M,65+	F,65+	M,20-64	F,20-64	M,65+	F,65+	
1. No adjustment	7.2	39.2	48.6	43.9	20.7	30.4	70.6	76.5	23.3
2. Basic model	6.5	34.4	39.4	38.2	19.4	27.7	68.2	73.5	20.9
3. Lower bound model	6.9	36.4	48.4	42.7	19.9	29.8	70.3	76.3	22.7
4. 5 percent wage decline model	6.9	35.9	40.1	38.9	19.9	28.7	68.4	73.9	21.5
5. Tax adjusted model	6.5	33.5	39.4	38.0	19.0	27.4	68.1	73.6	20.7
6. Posttransfer poverty	4.9	23.5	9.6	15.7	15.3	23.6	24.0	37.4	11.9
Percentage overstatement of pretransfer poverty using no adjustment compared to:									
7. Lower bound model	4	8	0	3	4	2	0	0	3
8. Tax adjusted model	11	17	23	16	9	11	4	4	13
Percentage overstatement of antipoverty impact of transfers using no adjustment compared to:									
9. Lower bound model	10	14	0	2	13	8	0	0	3
10. Tax adjusted model	30	35	6	9	34	61	2	4	15

Rows seven and eight display the minimum and maximum extent by which the unadjusted statistics overstate the level of poverty that would exist in a no transfer world. Rows nine and ten indicate the range of the upward bias when one measures the antipoverty effectiveness of transfers by the conventional, unadjusted method.[37] (Intermediate figures derived from the basic and wage decline models are omitted.)

The simulations suggest that transfers induce a decline in work effort that raises the overall incidence of pretransfer poverty by between 3 and 13 percent. The size of this increase varies across the eight demographic groups.[38] Because of this effect, the overall net reduction in the incidence of poverty is overstated by between 3 and 15 percent. Here, too, the degree of overstatement varies widely among the groups. It is particularly striking for nonelderly unrelated women and is fairly large for the other three non-aged categories.

Table 4 is constructed like Table 3, but contains statistics on poverty gaps. One sees from the simulations that the gap would have been between \$29.1 and \$24.8 billion. The observed pretransfer gap was \$31.7 billion; the posttransfer gap equalled \$10.1 billion. Thus, on net, cash aid lowered the gulf between minimum needs and actual income by between \$14.7 and \$19.1 billion, not \$21.6 billion. The unadjusted calculation overstates the absolute reduction by 13 to 47 percent. Rows seven and eight reveal that the induced loss of earnings raised the total gap by 9 to 28 percent. The amount of increase differed greatly among the eight groups, and the pattern of increases was similar to the pattern in rows seven and eight of

Table 4

Size of Poverty Gap Without Transfers Using Different Adjustments for Labor Supply Response Compared to the Posttransfer Poverty Gap, 1972, by Demographic Groups (in billions of $'s)

	Demographic Group								
Poverty gap without transfers	Families (by sex and age of head)				Unrelated Individuals				Total
	M,20-64	F,20-64	M,65+	F,65+	M,20-64	F,20-64	M,65+	F,65+	
1. No adjustment	$6.4	$7.4	$5.6	$1.1	$1.6	$2.3	$1.6	$5.8	$31.7
2. Basic model	5.2	5.6	3.8	0.8	1.3	1.8	1.4	5.1	25.0
3. Lower bound model	5.8	6.0	5.4	1.0	1.4	2.1	1.6	5.7	29.1
4. 5 percent wage decline model	5.6	5.9	3.9	0.8	1.4	1.9	1.4	5.2	26.0
5. Tax adjusted model	5.2	5.4	3.8	0.8	1.3	1.7	1.4	5.1	24.8
6. Posttransfer poverty	3.4	2.6	0.5	0.2	0.9	1.2	0.2	1.1	10.0
Percentage overstatement of pretransfer poverty gap using no adjustment compared to:									
7. Lower bound model	10	23	4	10	14	10	0	2	9
8. Tax adjusted model	23	37	47	38	23	35	14	14	28
Percentage overstatement of decline in poverty gap due to transfers using no adjustment compared to:									
9. Lower bound model	13	14	0	2	22	11	0	0	4
10. Tax adjusted model	35	25	5	9	42	63	2	3	15

Table 3. After correcting for this increase, one finds that transfers cut the total poverty gap by between 59.3 percent (comparing rows five and six) and 65.3 percent (comparing rows three and six). The unadjusted figures show a drop of 68.1 percent. Hence, the conventional approach overestimates the impact of public cash benefits on the poverty gap by 4 to 15 percent (rows nine and ten).

Table 5 focuses on the entire income distribution instead of just the poverty population. It presents Gini coefficients that reflect the degree of inequality that would exist among families, unrelated individuals and all living units if transfers did not exist and only labor supply responses are accounted for. The actual posttransfer Gini coefficient is also shown.

If transfers had zero effect on earnings, the Gini index for all units in a no transfer world would have been .458. Simulating a labor supply response under varying assumptions shows that distributing the earnings that were foregone because of the transfer system would change the index to between .434 and .446. These simulated Gini coefficients fall below the unadjusted value simply because transfers are largely concentrated upon living units in the lower tail. Comparing the posttransfer and no adjustment Gini indices reveals that transfers reduce income inequality among all units by 14.4 percent. This conventional method clearly gives upwardly biased results. The simulations indicate the net fall in inequality ranges between 9.7 and 12.1 percent. Hence, the standard technique overstates the equalizing effect of transfers by 19 to 48 percent. The overstatement is especially large for the distribution of family incomes.

Table 5

Gini Coefficients With and Without Transfer Income Using Different Adjustments for Labor Supply Response, for Families, Unrelated Individuals and All Units

Gini coefficients for nontransfer income:	Demographic Group		
	Families	Unrelated Individuals	All Units
No adjustment	.398	.597	.458
Basic model	.377	.554	.435
Lower bound model	.387	.573	.446
5 percent wage decline model	.378	.551	.436
Tax adjusted model	.376	.553	.434
Gini coefficient for posttransfer income	.342	.443	.392
Percentage decline in gini:			
No adjustment	14.1	25.7	14.4
Lower bound model	11.6	22.7	12.1
Tax adjusted model	9.0	19.9	9.7
Percentage overstatement of decline in gini index using no adjustment compared to:			
Lower bound model	22	13	19
Tax adjusted model	57	29	48

6. SUMMARY AND CONCLUDING COMMENTS

In this paper I have developed and applied a methodology for measuring the reduction in work effort and earnings in 1972 caused by the major cash transfer programs. Estimating these effects permitted me to compute the net redistributive effect of transfers. The methods used and the principal findings may be summarized as follows:

* The neo-classical theory of labor supply provided the conceptual foundations for this investigation.
* To solve the operational equations for measuring the change in work effort produced by transfers, knowledge of five parameters was required. These were the recipient's wage rate, the guarantee provided by his transfer and the tax rate it imposed, and two coefficients of his labor supply function. Procedures for obtaining the first three parameters were discussed at length. The other two values were drawn from another study.
* Despite efforts to model the characteristics of the transfer programs and the behavioral responses of recipients and their family members as accurately as possible, the empirical results must be qualified by a number of shortcomings in the methods.
* Simulations were conducted under four sets of assumptions. Three of these sets yielded noticeably different outcomes. Introducing a small wage change did not markedly alter the findings. Cash transfers reduced labor supply by between 2.2 and 8.2 billion hours (1.5 to 5.6 percent of the observed total) and lowered earnings by $5.8 to $21.9 billion (.9 to 3.5 percent).

* On average, a dollar of transfer income reduced earnings 10 to 36 cents. This real cost varied considerably across the separate transfer programs. The welfare cost of the transfer system was trivial.
* By simply comparing pretransfer and posttransfer incomes, I found that transfers allowed 49 percent of the pretransfer poor to escape poverty, erased 68 percent of the pretransfer income gap and lowered the Gini coefficient by 14 percent. The simulations suggest that the fraction of all households lifted from poverty was overstated by between 3 to 15 percent, the fraction of the gap eliminated was biased upward by between 4 and 15 percent and the measured fall in inequality was 19 to 48 percent too high. These differences varied widely among major demographic groups.

A major conclusion from this analysis is that transfers do have a substantial impact on the level of poverty and degree of money income inequality even after corrections for the labor supply effects. Naturally, the impact is less than what is measured by the standard unadjusted approach. Simulations similar to the one presented here may help policy makers estimate the total expenditure on additional transfers needed to achieve any given amount of monetary redistribution.

The outcome of these simulations indicates that the reported increase in the antipoverty effectiveness of transfers during the past decade has been somewhat overstated.[39] Transfer expenditures grew rapidly in this period. The results imply that the level of pretransfer poverty in recent years would have been a bit lower if transfers had remained at their earlier level. The increased expenditures, in other words, helped raise the severity of pretransfer poverty. But these findings further show that the additional transfer income more than offset the loss of earnings, so that overall a net reduction of poverty was achieved.

APPENDIX

Table A1

Income and Wage Coefficients Used in Simulation

Group	Income Coefficient	Wage Coefficient
Males:		
25-54, married, healthy	-.0119	22
25-54, married, unhealthy	-.1206	167
55-62, married	-.0228	40
55-62, single	-.0469	107
63-64	-.0183	206
65-72	-.0896	-82
73+	-.0104	12
25-54, single	-.0309	100
20-24, married	-.028	8
20-24, single	-.012	413
Females:		
25-54, married, not head	-.0298	298
25-54, head	-.1209	773
25-54, single	-.0871	513
55-64, married, not head	-.0273	245
55-64, single or head	-.205	363
65-72	-.150	81
73+	-.0048	24
20-24, married, no children	-.0316	781
20-24, married, children	-.051	287
20-24, other	0	439

NOTES

[1]See Robert Lampman (1971), Kenneth Boulding and Martin Pfaff (1972), Robert Plotnick and Felicity Skidmore (1975).

[2]See Glen Cain and Harold Watts (1973).

[3]Dollars directly spent on transfers are a cost to taxpayers, but not social or economic costs. The economic cost of transfers is the value of the output not produced because recipients choose to work less.

[4]Government employees pensions are not treated. In many ways these transfers are really substitutes for private pensions and should not be considered part of any public effort to redistribute income. Thus their labor supply effects are not of interest.

[5]Plotnick and Skidmore (1975), appendix A.

[6]Personal income and earnings figures from U.S., President (1976), Table B-15.

[7]See Fredrick Golladay and Robert Haveman (1976). The authors assess the effect of a negative income tax upon quantities demanded of goods and factors, but hold relative prices constant.

[8]The functional form is dictated by practical considerations. This project did not estimate the values of b and c, the income and wage coefficients. Instead results obtained by Irwin Garfinkel and Stanley Masters (forthcoming) are used.

These estimates were chosen for several reasons. Garfinkel and Masters have carefully treated the conceptual and econometric issues surrounding the problem of estimating labor supply functions. They provide estimates for 20 demographic groups differentiated by age, sex and marital status. Taken together, the groups include nearly all persons over 20. The full set of estimates is derived from a common analytic framework and common data (the Survey of Economic Opportunity). No other studies offer a set of estimates as comprehensive as these. After testing many specifications, Garfinkel and Masters found that the form in (1) generally gave the best fit. Hence, this study also adopts this form.

[9]The minus sign before $DLS_2 \cdot W_2$ is easily explained. Given the order of subtraction used to determine DLS, if $DLS_2 > 0$, person 2 reduces his work effort. Earnings <u>fall</u> by $DLS_2 \cdot W_2$ because of the transfer, which means a minus sign is needed.

[10]Garfinkel and Masters.

[11]The reduced form specification was either:

(a) $\ln W = a + bDEM = cREG = dOCC = eIND = u$ or

(b) $\ln W = a' + d'DEM + c'REG = u'$, where

DEM is a vector of personal characteristics, REG is a vector of geographic variables and OCC and IND are the person's occupation and industry of employment. If an individual reported no occupation or industry (which often occurs since many did not work in 1972), (b) is used. Otherwise, wages are predicted using (a), subject to the constraint that no wage could be less than the federal minimum.

Persons who work full year, full time usually earn more per hour than demographically similar persons holding part time or part year jobs, or not choosing to work at all (here the comparison is to the imputed wage of these nonparticipants). Thus, the wage imputed to all those who didn't work 48-52 full time weeks will generally be overestimated. This is unfortunate, but the data leave no reasonable alternative. Weeks of work are recorded with a series of dummy variables representing 0, 1-13, 14-26, 27-39, 40-47, 48-49, and 50-52 weeks. Except for the last two categories, the procedure described in the text was not applicable. For example, if a person reported 14-26 weeks of full time work, should one divide earnings by 14x40, 26x40, 20x40 (the average) or some other figure. Any general rule would be arbitrary and likely created distortions at least as serious as the bias noted above.

This procedure is somewhat unsatisfactory for self-employed individuals. Part of their reported earnings represents a return to physical capital. However, omitting them would not be desirable for this study, for the self-employed themselves received 4.25 percent of all cash transfers in 1972 and lived in households that obtained 8.22 percent of total benefits. While recognizing that I am introducing error, I assume that all reported earnings are wages.

[12]Income from railroad retirement, survivors and disability programs is treated as if it was Social Security income. The data report income from all these programs with the same variable and no satisfactory procedure for separating them was available. However, railroad and Social Security programs operate with similar rules, so the labor supply effects will not be greatly distorted by my procedure.

[13]For example, given the choice between no job and one requiring a fixed number of hours and paying $3000 per year, a SS recipient might work even though his marginal tax rate equals one. (Indeed, with income and payroll taxes, it could exceed one!) Such a person probably considers the average tax rate in reaching his decision.

[14]The data do not allow one to distinguish between equilibrium and disequilibrium situations.

[15]U.S., Department of Health, Education and Welfare (1973, p. 125).

[16]An appendix, available from the author upon request, explains the legal provisions that determine G, T and countable income. Other parameters used in this study are also contained in this appendix.

[17]For a lengthy treatment of the appropriate way to model the labor supply effects of unemployment insurance, and for estimates of this program's impact on the unemployment rate, work effort and earnings, see Garfinkel and Plotnick.

[18]The CPS data uniquely identify the most populous states but assign the same code for groups of smaller states. When two or more states are grouped together, the fraction and maximum and minimum are determined by taking a weighted average of the several separate values. The weights are proportional to the number of unemployed persons in each state in 1972. This introduces error, but the data leave no choice.

To simplify matters, it was assumed that benefits for all recipients in a given state (or group of states) are determined by a common rule. In practice, the fraction of one's earnings and the length of eligibility are partly dependent on one's past earnings and employment record. The common rule used in this work assigned all recipients the maximum fraction of their weekly earnings allowed by state law and assumed everyone was eligible for 39 weeks. These extreme assumptions insure that the labor supply effects will be upper bound effects.

[19]Blind persons getting AB would probably report themselves as disabled and, so, are treated as receiving APTD. The benefit structure of the two programs are very similar, so no serious distortion is introduced. A person is assumed to be disabled if the reason given for part year work or no work is "illness." The data do not provide a more satisfactory criterion of disablement. Unfortunately, this procedure misses disabled full year workers whose low earnings qualify them for APTD.

[20]PA benefits are paid to one member of the living unit, but are based on household financial circumstances. Thus one must examine household income to determine the recipient's G. When the state is not uniquely identified by the data, I use a weighted average of all states covered by the code. The weights are the number of recipients of the particular program in each state.

[21]This means, for example, that I count the guarantee from Social Security, not the amount actually received, which will be less if the person earned over $1680. The reasoning is as follows. The correct G for PA is the amount a recipient obtains if he doesn't work. But if

he doesn't work, he will receive a guarantee of say, H, from the non-PA program. Given the way PA operates, the maximum welfare benefit (i.e. the G from PA) will be reduced by H dollars. For these calculations it is correct to use the real guarantee, not the shadow guarantee. (A similar procedure was used to compute the G for veterans pensions.)

[22]Approximate work expenses per hour based on U.S., Congress (1974), appendix A.

[23]U.S., Congress (1972), p. 331.

[24]Of course, if some members also receive transfers from other programs, their guarantees will equal the sum of the common G for PA plus the G for the other programs.

[25]U.S., Department of Health, Education, and Welfare (1973, p. 75).

[26]Garfinkel and Masters (forthcoming) explain why they did not estimate these functions.

[27]Underreporting is present because some recipients do not admit to receiving income from a particular program, while others simply understate the actual amount received. I only corrected for the latter problem; devising rules to correct the former was not feasible in this study. The correction factors were taken from Timothy Smeeding's (1975) work. Uniformly inflating recipient's reported benefits introduces errors because everyone does not underreport to the same degree. However, no alternative procedure was available.

[28]This constraint was used in only 1.1 percent of all cases when the above methods of computing G were used.

[29]Martin Feldstein provides some evidence that this substitution has been occurring.

[30]Incorporating a wage change into the analysis requires slight modifications in equation (3) or (7). First, the wage effect becomes $c\ln(.95/(1-t))$. Second, in (7) the earnings of all household members, excluding person 1, are multiplied by .05 and then by $-b_1$. The result indicates person 1's income effect generated by the loss of other members' earnings. One cannot foretell whether the impact of transfers with no wage change will exceed or be less than the impact with wage decline, since the decline generates wage and income effects that act in opposite directions.

[31]In estimating marginal tax rates I assumed all married couples jointly, all separated and non-married heads of families filed as taxpayers who qualify as household heads, and all unrelated persons filed singly. Adjusted gross income was approximated by summing all taxable types of income reported in the CPS (this does not include capital gains). Taxable income was determined with the assumption that all units used the standard deduction. State and city income taxes were ignored. The employee share of the Social Security tax (5.2 percent in 1972) was added to the marginal income tax rate if earnings were below the legal ceiling ($9000 in 1972).

[32]This figure does not match the one cited in the introduction for two reasons. The sample excludes some categories in the population that receive transfers. Also, the correction for underreporting is not perfect (see footnote 27).

[33]See Arthur Okun (1975, chapter 4).

[34]I ignore households that enjoy income from two or more different transfers because I cannot readily allocate the loss in earnings among the several programs. Results are not reported for simulations in which the wage changes; I cannot disentangle the effect of the transfer from the reaction to the wage decline.

[35]The exception of UI is explained by the high tax rate imposed on most recipients of this benefit. Comparing equations (3) and (9) shows that, given t, the higher is T, the greater the difference between the two terms involving the logarithm.

[36]The text's calculation ignored normal tax rates. If normal taxes are considered, the formula becomes $1/2\ ewL(T+t)^2 - t^2)$, where T and t are defined as in the text. This tax adjustment doubles the welfare cost to $1.99 billion--still a trivial amount.

[37]Assume NA is the no adjustment incidence of pretransfer poverty, A is the level of poverty in a no transfer world when one adjusts for labor supply effects, and P is the observed level of posttransfer poverty. Then NA/A is a measure of the overstatement of poverty in a no transfer world. Also, (NA-P)/NA is the conventional antipoverty impact (the fraction of pretransfer taken out of poverty by transfers), (A-P)/A is the adjusted antipoverty impact and $\frac{(NA-P)/NA}{(A-P)/A}$ measures the upward bias in the unadjusted figures.

[38]Note the negligible differences for the elderly groups when lower bound assumptions are used. These groups mainly receive SS and once the income effect of this benefit is suppressed, a major work disincentive exists only for the relatively few persons earnings over $1680.

[39]Plotnick and Skidmore (1975, chapter 6).

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