UNINTENDED EFFECTS OF AMERICAN SOCIAL POLICY

ON DISADVANTAGED MEN:

THE AID TO FAMILIES WITH DEPENDENT CHILDREN PROGRAM

by

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B.A., Philosophy and Economics
Columbia College, Columbia University, 1987

Submitted to the Department of Economics in Partial
Fulfillment of the Requirements for the Degree of
DOCTOR OF PHILOSOPHY
in Economics
at the

Massachusetts Institute of Technology
February 1995

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ABSTRACT

Although the Aid to Families with Dependent Children (AFDC) program is targeted primarily towards women, it should also affect disadvantaged men. This thesis estimates a negative relationship between AFDC generosity and disadvantaged male labor supply. It also estimates a positive relationship between AFDC generosity and disadvantaged male schooling. The thesis addresses AFDC effects on crime and incarceration, but here no reliable statistical relationships are uncovered.

The thesis considers several behavioral routes for the AFDC effects. Much discussion is devoted to one hypothesis: that AFDC affects disadvantaged men because disadvantaged communities involve significant resource sharing. AFDC's categorical deprivation requirement notwithstanding, increases in AFDC can thus be thought of as loosening the budget constraints of many members of disadvantaged communities. To support this view, some parts of the ethnographic literature are quoted at length in the initial chapter.

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Acknowledgements

Introduction

The Aid to Families with Dependent Children (AFDC) program is the nation's largest means-tested cash transfer program.[1] It is large enough to have significant economic effects on low-income communities. During the 1980s, for example, about 3.8 million families annually received AFDC.[2]

There's a fairly large literature on the labor supply effects of AFDC, but almost all of the previous work has looked at effects among women.[3] Most of the previous work has looked at women because the great majority of AFDC benefits go to single-mother households; the great majority of adult AFDC recipients are women. In 1991 for example 89% of adult AFDC recipients were women.[4]

There are some official adult male recipients, and one currently active research program is the investigation of the economic behavior of these official-recipient men. For example, Hoynes (1993) has recently studied AFDC labor supply effects on official male recipients under the AFDC-Unemployed Parent (AFDC-UP) program.[5]

In this thesis, I take another tack--one inspired both by the ethnographic literature and also by the popular press. This tack is to look at effects of the program on men who are

not listed as official recipients--men who are not targeted by the program at all.

Chapter One sets a foundation for the subsequent empirical work by quoting and discussing at length the ethnographic literature on low-income, high-welfare receipt communities. This literature strongly suggests that individuals in low-income communities are economically embedded in extensive kin-based social networks. The arguments in this first chapter imply that it may be myopic to look for labor supply effects only among official recipients. An increase in AFDC benefits relaxes the budget constraint for all of the members of a social network which relies extensively on AFDC payments.

The primary purpose of Chapter One is to lay this foundation, but it also accomplishes some important secondary tasks. For example, writers in the public press have made arguments which on surface are similar to the ethnographic arguments, but which in fact significantly skew those arguments. Because of the public prominence of these arguments, it is important to disentangle the falsehoods from the insights, and Chapter One attempts to do this.

Another secondary purpose of Chapter One is to confront directly the AFDC rules. These rules in the past (and to an ambiguous extent currently) have explicitly required that AFDC recipients not be significantly involved in extended economic social networks. The chapter discusses the extent to which

the AFDC rules are in fact able to make and enforce this requirement.

With Chapter One as background, Chapter Two addresses a question which has been acknowledged in the literature to be both important and unanswered: What is the effect of AFDC on the labor supply of disadvantaged men? The discussion allows not only straightforward income effects on men, but also adds the possibility of an effect through marriage. The recurring statistical strategies--employed in Chapters Two through Four--are described in detail in the second chapter. The chapter's statistical results offer strong evidence that expansions in AFDC imply reductions in the labor supply of disadvantaged men.

Chapter Two thus establishes that if AFDC benefits were raised, then disadvantaged male labor supply would fall. This raises the question which is addressed in the final two chapters of the thesis: if following an AFDC expansion, men are spending less time working, then how are they using the additional time? This question is particularly important for judging whether the shift in time allocation is socially beneficial or harmful.

Avoiding the rhetorical trap of looking only for negative side-effects of social programs (Hirschman, 1991), Chapter Three tests whether AFDC positively impacts the schooling of disadvantaged men. Following an AFDC expansion, do disadvantaged men spend some of their extra time investing in

their own human capital? Although the link between AFDC and schooling may at first seem contrived, the chapter shows that in fact the link is a natural expectation in several contexts.

Chapter Four then turns to the issues of crime and incarceration. One cannot fully understand disadvantaged male labor markets without understanding crime and incarceration. The fourth chapter therefore entertains the possibility of AFDC effects in this area. The chapter presents several empirical explorations, but no robust statistical relationships are uncovered. The results may however serve as a useful foundation for future investigations into this area. Moreover, the chapter's discussion of data limitations and AFDC hypotheses should be useful for future researchers.

Before turning to the first chapter, two remarks may be helpful. First, the thesis focuses on the AFDC program; the Foodstamp program is not discussed at any length. This focus on AFDC does not, however, derive from a judgement that AFDC has more significant effects on men than the Foodstamp program. Rather, the focus on AFDC derives mainly from methodological considerations. Specifically, the estimation procedures below take advantage of the geographical variation in AFDC benefits. The Foodstamp program is not discussed at any length only because it does not create any significant geographical variation in welfare benefits. Foodstamp effects on men should however be an active area for future research.

Secondly, it is hoped that this thesis will change the way that some researchers think about welfare programs. Most obviously, the thesis attempts to increase interest in unintended effects on <u>men</u>. In addition, however, the results below should also spur research on unintended effects on <u>women</u> as well. That is, AFDC may affect more women than just those who are official recipients. For example, if one thinks of AFDC as lifting entire networks of people, then it lifts <u>all</u> women who are significantly embedded in these networks--not just the official recipients. Indeed, most of the arguments below should have parallels for the case of unintended AFDC effects on women. These parallel effects then should also be high priorities for future research.

Chapter One: Isolation

"this field of study needs to address the charge levied
by some that much of the work in it represents data-
mining and measurement that . . . contribute little to
the development of new theoretical perspectives. . . .
[This point] indicate[s] an important and perhaps growing
role for ethnographic research--field research involving
first-hand and in-depth knowledge of the behaviors,
choices, motivations, and constraints of individuals in
various family, community, and cultural settings. Such
interactive research has potential for the development of
theory . . . " (Haveman, 1987, p. 129-130)

Quantitative data analysis, by itself, often lacks depth
and richness. Ethnographic research can provide that
richness, but ethnographic evidence on its own lacks
replicability and generalizability. As Haveman notes, the
combination of the two may produce hypotheses which are fresh
and which also pass empirical tests.

Following Haveman's suggestion, this chapter cites and
quotes at some length the ethnographic literature on high-
welfare-receipt communities. It supports a theoretical
perspective in which programs such as AFDC are seen as
affecting entire social networks. AFDC need not affect just
the targeted members of AFDC units; it may also affect many
other members of disadvantaged communities--the many people
economically attached to AFDC economic networks.

The chapter is organized around the views of two popular critics of the AFDC program: George Gilder and Charles Murray. The obvious drawback of this organization is that many scholars do not consider one or the other (or both) worthy of scholarly debate. Haveman (1987) states that the work of Gilder and Murray has been "widely and not always inappropriately viewed as irresponsible and as a direct misrepresentation of some research findings." (p. 235)

There are on the other hand several advantages to this organization. First of all, the views of Gilder and Murray--views which are severe towards welfare recipients--have dominated public discussion of AFDC. Hence, their views form the backdrop against which non-specialists view the AFDC program. Secondly, whereas the scholarly literature has hardly investigated the link between AFDC and men, Gilder and Murray have emphasized it. Thus, when it comes to the link between AFDC and men, the only voices heard have been those of these critics.

My organization is centered around Gilder and Murray in the sense that I first present their views, and then I use the ethnographic literature to qualify and enrich them--to show where Gilder and Murray are correct and where they appear to be wrong.

Essentially, Gilder and Murray have argued that AFDC payments to single mothers severely lower the labor supply of some particular men--specifically the boyfriends of the single

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female recipients. I use the ethnographic literature to suggest three points.

First, the ethnographic literature supports the view that single AFDC mothers are not socially isolated: there are men who are to significantly attached to AFDC households as economic units. Second, although the ethnographic literature supports the hypothesis that boyfriends are involved, it stresses a much more broad notion of attachment--involving relatives, adult children, "absent" fathers, etc. Finally, whereas Gilder and Murray focus only on the AFDC income effect on members of these networks, the ethnographic literature shows the other side of the story. Specifically, AFDC is only one of many sources of income to low-income communities, and involvement in a network implies earning income as well as receiving it; in short, involvement in low-income communities involves <u>contributing</u> as well as <u>receiving</u>.

After making each of these three qualifications of the Gilder and Murray view, the chapter notes one limitation of the ethnographic evidence. Then the final section of the chapter discusses the possibility that low-income communities may involve <u>more</u> resource sharing than high-income communities.

§ Gilder and Murray.

The starting point for both Gilder and Murray is their assumption that the categorical deprivation requirement[6] is vacuous in practice; although it may have been a binding

constraint at one time, in current AFDC practice, it is at most vestigial. That is, they assume that two-parent families can receive AFDC: all that is required is for the mother to lie in her application--saying that she has been abandoned. Gilder and Murray argue that under current practices, there is little probability of detection of such a lie, and, even if such a lie were detected, the punishment would be trivial.

Thus, Gilder and Murray assume that it is only in nominal, official terms that AFDC children are deprived of their fathers' support. In practice, the father of the children can easily be present in an AFDC household. And if the father is not present, then another man (the current boyfriend) of an AFDC mother can a fortiori be present in the family.

In fact, if the household does not receive Food Stamps, then the presence of a boyfriend is even officially accepted under current rules. Most AFDC households do however receive Food Stamps, so the presence of a boyfriend in the family should be reported, but Gilder and Murray again assume that such a reporting requirement does not bind AFDC recipients. They assume that the rational economic action for a disadvantaged mother is to deny that her boyfriend is present; the optimal hence ubiquitous strategy is to claim that she is isolated from men, even though she isn't. Given this assumption, Gilder and Murray can proceed without hesitation to link AFDC payments to male labor supply.

Gilder's <u>Visible Man</u> (1978) takes the form of a novel. It is a story of a young disadvantaged black man whose limited employment options are inferior to his opportunities for dependence on women who receive AFDC. One legitimate job that he did receive was:

> "to push a wheelbarrow full of asphalt. . . . It was not the sort of work he would ever do as long as he had a woman on AFDC." (p. 107)

This man is in fact able to find girlfriends receiving AFDC, and he uses them to support himself.

Gilder claims such arrangements are commonplace. Many men are effective, but unofficial ("unlisted") AFDC recipients. In his story, the young man thus eventually consciously decides that:

> "he was better off as an unlisted welfare recipient: one of the hundreds of thousands of men who feed off the welfare system without joining it--by feeding off welfare mothers without marrying them."

Gilder has also in fact claimed that it is the men--not the women--who ultimately <u>control</u> most AFDC benefits.[7]

Murray's influential contribution was his 1984 book. Like Gilder, Murray assumed that AFDC affected male labor supply as easily as it affected female labor supply. Murray's well-known discussion centers on a couple he names "Harold" and "Phyllis"; he offers their case as typical for a low-income couple who have had a child out of wedlock. Harold's best employment option is to work at a menial position in a laundry. AFDC, however, is assumed to be an obvious

alternative to the work in the laundry. All that is required

is that Phyllis claim to AFDC that Harold has abandoned her.

Murray assumes that the categorical deprivation

requirement became vacuous during the 1960s. Thus, Harold is

able to rely on AFDC in 1970, but he would not have been able

to rely on it in 1960. (I will discuss historical changes in

categorical deprivation below.) The following passage clearly

shows Murray's direct linking of AFDC and disadvantaged male

labor supply; the passage is organized by comparing the

situation in 1960 and the situation in 1970. In 1970:

> Harold and Phyllis take the economically logical step--
> she has the baby, they live together without getting
> married, and Harold looks for a job to make some extra
> money. He finds the job at the laundry. It is just as
> unpleasant a job as it was in 1960, but the implications
> of persevering are different. In 1970, unlike 1960,
> Harold's job is not his basic source of income. Thus,
> when the back room of the laundry has been too hot for
> too long, it becomes economically feasible and indeed
> reasonable to move in and out of the labor market. In
> 1980 dollars, Unemployment Insurance pays him $68 dollars
> per week. As the sole means of support it is not an
> attractive sum. But added to Phyllis's package, the
> total is $202, which beats the heat of the presses. And,
> if it comes to it, Harold can survive even without the
> Unemployment payment. In 1970, Phyllis's welfare package
> is bringing in more real income than did a minimum-wage
> job in 1960. (Murray, 1984, pp. 161-162)

According to both Gilder and Murray, AFDC allows disadvantaged

men to reduce their labor supply; indeed men ultimately can

choose not to work at all--they can themselves rely on the

AFDC income.

§ AFDC rules concerning isolation: history.

Before turning to the ethnographic qualifications of

Gilder and Murray's views, it is useful to review some of the

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history of the categorical deprivation requirement, and also its current status.

Throughout the history of AFDC, legislators have been concerned that recipients should be isolated from men. Indeed, historically AFDC (and the categorical deprivation requirement) originated out of the Mothers' Pension movement, and the Mothers' Pension movement was explicitly concerned with isolation from men. The idea behind the Mothers' Pension movement was that it would be worthwhile to offer single mothers a pension in return for those mothers' upholding high moral standards in their home.

The difficult point of course was which mothers were so fit--which homes were so suitable--to deserve such a pension. In practice, high moral standards often meant isolation from men. Bell (1965) reports that:

> "critics ... insisted that further objective criteria were needed to identify 'suitable homes' and 'fit' mothers. In their opinion, it was improbable that such homes or mothers would be found except where the father was 'permanently eliminated.' Some states responded by amending their laws to exclude all families who might conceivably have what later became known as a 'phantom father,' now visible, now invisible." (p. 8)

When legislators formed AFDC in 1935, they kept the spirit of "isolation from men" in the law by legislating the categorical deprivation requirement. In early years, some states interpreted this requirement in extreme forms-- specifically they attempted to enforce "substitute parent" or "man in the house" regulations. These regulations interpreted

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categorical deprivation as requiring that single mothers on welfare could not have boyfriends.[8]

Such regulations were justified by the argument that if a recipient had a boyfriend, then that boyfriend qualified as a nonabsent parent in the home. In effect, the states argued that if there is a man in the home, then that man has responsibility for supporting the children in the home-- whether he is their father or not.

In fact, the 1968 Supreme Court outlawed these regulations in King vs Smith.[9] The Court ruled that, in the context of the Social Security Act, only the _father_ of children can be considered to have a legal duty to support the children in the family. Thus, only _fathers_ can be prohibited from an AFDC home as an eligibility requirement. Other men-- in particular, boyfriends ("paramours" as the Court referred to them)--cannot be prohibited from the household.

Nevertheless, these man-in-the-house rules operated in some states throughout the 1950s (and in some cases into the late 1960s), and welfare department tactics during that period are informative. They show the importance to government workers of the requirement that AFDC mothers be isolated from men. For example, Bell (1965) reports the following affidavit required of a particular mother for AFDC receipt:

> "I ... do hereby promise and agree that until such time
> as the following agreement is rescinded, I will not have
> any male callers coming to my home nor meeting me
> elsewhere under improper conditions." (Bell, 1965, p. 48)

The most infamous tactics during this period were welfare department searches of recipients' homes--looking for men.

"Across the country three levels of surveillance emerged: 1) a home was watched during the day or night or both; 2) two investigators made a surprise visit with one at the front door, the other at the back door, in the hope of apprehending an errant man; or 3) the investigators demanded entry and searched the premises for a man or evidence that a man might be included in the family unit." (Bell, 1965, p. 88)

Piven and Cloward (1971) report that:

"with 'man-in-the-house' rules, men run the risk of being tracked down and jailed for non-support by investigators who conduct 'midnight' raids or parked-car surveillance of homes." (Piven and Cloward, 1971, p. 127)

They also report a 1962 incident in which Senator Byrd:

"forced District of Columbia welfare officials to acquire nearly as many 'fraud investigators' as 'social investigators'. The fraud investigators quickly put hundreds of AFDC mothers under 'parked car' surveillance and 'proved' a year later that close to 60 per cent were 'ineligible' for benefits, chiefly because they appeared to have contact with men." (Piven and Cloward, 1971, p. 169)

Why have legislators been so concerned with the isolation of recipients from men? Piven and Cloward (1971) argue that the legislators were concerned precisely with the labor supply of these men: the legislators did not want to provide disadvantaged men with a viable alternative to low-income employment.[10] Other explanations have been advanced by other analysts.[11]

§ AFDC rules concerning isolation: the current situation.

Although King vs Smith did decrease the strength of isolation requirements, it by no means decimated them. For several reasons, isolation from men is still effectively

required of AFDC recipients. For at least three reasons,[12] the original isolation rules have important if vestigial current effects.

Consider an AFDC mother whose economic household contains an attached man (her boyfriend, or her brother etc.). This man stays or eats in the household or contributes income to it. Why might AFDC receipt be easier if the woman is isolated from men?

First and most obviously, categorical deprivation is intact if the attached man is also the acknowledged father of one of her children. Clearly in such a case the recipient benefits from not reporting his presence.

Secondly, as noted above, most AFDC recipients receive Food Stamps and in that case, a boyfriend or a brother is supposed to be counted as a member of the household. Thus, if a man's money contributions are high enough, it is optimal for a recipient to officially deny his attachment for the sake of increased total welfare benefits.

Third, all unearned income to an AFDC family is taxed at a 100% rate. Thus, if the attached man contributes money occasionally, then those contributions could be taxed at 100%. Specifically, they would be taxed at 100% if the caseworker interpreted the contribution as a "gift." There is some ambiguity here however, because if the boyfriend occasionally stays in her household and eats in her household, then it's not clear whether the contributions are a "gift" or "his

contribution to household expenses." In Massachusetts, if the contributions are considered as his part of the household expenses, then they would not be taxed.

For all three reasons, it remains true that receipt of welfare payments is more easily available for women who are socially isolated. At the very least, frequent economic cooperation with men could increase the paperwork and hassle involved in receipt.

Recipients, of course, have two options in the face of this incentive for isolation. They can decrease their economic contact with men. Or they can maintain economic contact with men--but not report this contact to AFDC.

Some of the evidence presented below indicates that many recipients choose the latter, but it would be unrealistic to assert that AFDC rules did not at least to some extent increase the isolation of some recipients.[13] It is probably incorrect to completely dismiss the categorical deprivation requirement, but the arguments below will suggest that the requirement of social isolation goes against the grain of much of low-income community life. The categorical deprivation requirement probably increases social isolation somewhat, but it is hardly dominant given the strength of resource-sharing in low-income life.

§ Ethnographic Evidence: Isolation.

We can now turn to the ethnographic qualification of the Gilder and Murray view. Although Gilder and Murray mislead in

some ways, the ethnographic evidence strongly supports them on the general point that AFDC recipients are not in practice socially isolated from men.   At least for low-income communities, there are often strong economic ties outside of the strictly defined nuclear family.   For example, Piore (1979) observed:

> "it is clear from contacts with welfare households and low-paying establishments that the welfare system constitutes a cushion for a variety of people (older children, men, even distant relatives) who are attached, often surreptitiously, to welfare households." (Piore, 1979, p. 90)

Perhaps the classic ethnographic source for this argument is the book by Stack (1974).   She wrote:

> "Welfare benefits which barely provide the necessities of life--a bed, rent, and food--are allocated to households of women and children and are channeled into domestic networks of men, women, and children.   All essential resources flow from families into kin networks." (Stack, 1974, p. 33)

Indeed, Stack explicitly questioned the relevance of the concept of 'household' for disadvantaged economic communities; the community she studied was 'The Flats':

> "It became clear that the 'household' and its group composition was not a meaningful unit to isolate for analysis of family life in The Flats.   A resident in The Flats who eats in one household may sleep in another, and contribute resources to yet another.   He may consider himself a member of all three households." (Stack, 1974, p. 31)

Sullivan, in a more recent ethnographic study (1993), also noted that AFDC families are not typically isolated from men:

> "Census statistics show that [one community he studied] is one of the poorest neighborhoods in New York City.   In

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1980 over 60 percent of the households were classified as female-headed and over half received some form of public assistance. These statistics, however, conceal as much as they reveal about the dynamics of family composition and the relationship between welfare and work. Many households do contain a working adult male. Many other families break up and re-form over time." (Sullivan, 1993, p. 55)

In the same essay, Sullivan noted that in both of the low-income communities he studied:

"There is considerable movement on and off AFDC, as males find and lose work. Some households also combine short-term, off-the-books wages with AFDC support for a period of time." (Sullivan, 1993, p. 66)

Another example is the research of Sharff (1987). In the mid-1970s, she studied in-depth twenty-four AFDC households in New York City. "All of the twenty-four households . . . represented themselves as "female-headed" to the [welfare department]. However, fifteen of them (62%) had a male resident performing the husband-father role, while nine (38%) did not." (p. 34)

Note that the ethnographic evidence is vital for establishing this lack of isolation. Especially for women, broad-based government-sponsored surveys should be questionable sources for data on isolation. As noted above, welfare recipients probably find it easiest to claim economic isolation in official circumstances (e.g., in the welfare department office). And if that is so, then it is unclear why we could expect recipients to suddenly become honest when interviewed by Current Population Survey (CPS) or NLSY officials who have come asking economic questions. Women have

more welfare-department contact than men, so the male data may be less questionable. But the virtue of ethnographic studies is that analysts build a rapport with the subjects thus guaranteeing more candid responses.

One important exception is some data available through the Seattle and Denver Income Maintenance Experiments (SIME/DIME) experiments . This data provided some reliable, non-ethnographic evidence on male attachment to AFDC households. The purpose of the SIME/DIME experiments was to determine the demographic outcomes of a negative income tax regime. The data is informative for AFDC, however, because the control group was subject to the prevailing AFDC regime (in Seattle and Denver between 1970 and 1977).[14]

There is good quality, somewhat confidential information about these control-group recipients because SIME/DIME researchers interviewed them three times per year during the experiment. Interviewers asked whether men were present. The results of the interviews were not reported to AFDC. Thus, the answers to these questions provide at least a weak test of AFDC reporting: SIME/DIME researchers may have received more truthful answers than AFDC administrators in AFDC offices. Halsey et al (1982)[15] investigated this possibility. They found that the SIME/DIME data indicated that in fact there were men present in 34% of the AFDC households.

For a final example of the low social-isolation of low-income people, we can consider an example which predates AFDC. Patterson (1986) speaks of poor people in America around 1900:

> "They developed a network of self-help activities, including unions, churches, mutual aid societies, building and loan associations, and urban political connections. They depended especially on kin and ethnic ties." (Patterson, 1986, p. 14)

§ Ethnographic Evidence: Boyfriends.

The ethnographic evidence does not, however, support Gilder and Murray in their emphasis on boyfriends. For Gilder and Murray, the men who are attached to AFDC are the boyfriends of AFDC recipients. Boyfriends are indeed an important example of attachment to AFDC families, and the ethnographic literature includes many references to this. For example, in Anderson's (1993) ethnographic study of a disadvantaged community, he notes that:

> "A number of men, married and single, incorporate their sexual lives into their more generalized efforts at economic survival. Many will seek to 'pull' a woman with children on welfare mainly because she usually has a special need for male company, time on her hands, and a steady income." (Anderson, 1993, p. 94)

Or consider the views of social-service staff of an early-1980s social service program. According to the Manpower Demonstration Research Corporation (MDRC), it became apparent to the staff over time that:

> "some [teenage mothers'] requests for separate grants and independent households were too often a sign of manipulation by boyfriends, in whose interest it was to have a girlfriend on welfare with an apartment of her own."[16]

The ethnographic literature, however, also indicates that boyfriend attachments are <u>not</u> the only common form. Other significant attachments are the attachments of the recipients' siblings, adult children, and the (officially absent) father of her children. Indeed many types of relations can form significant attachments to AFDC networks. Most generally the list includes a recipients' neighbors, friends, relatives, and also all the relatives of the absent father.

For example, Stack (1974) criticizes the analytic neglect of the sibling relationship in the disadvantaged community she studied.

> "These co-residential socializing units do indeed show the important role of the black female. But the cooperation between male and female siblings who share the same household or live near one another has been underestimated by those who have considered the female-headed household and the grandmother-headed household (especially the mother's mother) as the most significant domestic units among the urban black poor." (Stack, 1974, p. 104)

Halsey et al (1982)--in the SIME/DIME study mentioned above--found significant attachment of adult children to AFDC households. Thus, if a woman has a child on AFDC, and she also has other children too old for AFDC, then the older children may very well remain attached to her household. They may contribute time and income to the household, and in that case their economic choices will be affected by AFDC benefits.

The "absent" father can also be attached to an AFDC household. As Weiss and Willis (1985) point out, there is an important situational difference between <u>low-income</u> absent

fathers and high-income absent fathers. Because of AFDC, low-income AFDC fathers have a relatively obvious disincentive for making child-support payments. Namely, except for a $50 monthly "pass-through", any payments made through official channels will not benefit the father's children. As long as the child-support award is less than the AFDC benefit, his payments will only reduce the taxpayer burden.

This leaves open the possibility of unofficial contributions by the father. Undoubtedly these contributions would not be as high as we expect from custodial fathers, but they can exist. Indeed, Edin's ethnographic research (discussed further below) indicated substantial unreported involvement by disadvantaged absent fathers in their children's AFDC home.

More generally, not just the father but also the father's kin may be called upon to support the child and his mother. Stack (1974) repeatedly mentions the practical importance of the father's kin.

> "Both a child's mother's and father's socially recognized kinsmen are expected to assume parental rights and duties, and these expectations are borne out by actual events." (Stack, 1974, p. 73)

> "To maintain a stable number of people who share reciprocal obligations, at appropriate stages in the life cycle people establish socially recognized kin ties. Mothers may actively seek out their children's father's kin, consciously expanding the number of people who are intimately obligated to care for one another." (Stack, 1974, p. 29)

In summary, the ethnographic literature indicates that many men will be economically attached to AFDC households.

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Siblings, uncles, "absent" fathers, neighbors, and friends--in addition to boyfriends--could be called upon to offer more support when AFDC benefits fall (and less support when they rise).

§ Ethnographic Evidence: Supplementation.

Gilder and Murray focus solely on the flow of welfare funds into disadvantaged communities. But the ethnographic literature paints a more broad picture in which funds flow into these communities from numerous sources--especially employment.[17] Thus, the ethnographic work supports a picture of low-income extended families in which both men and women supplement public welfare payments with earned income.

The best evidence on this supplementation comes from the ethnographic research of Kathryn Edin and her colleagues.[18] Edin interviewed fifty welfare mothers in Chicago.[19] She built rapport with them and gathered detailed information on their family budgets--in particular, how much of their expenses were covered by welfare payments (AFDC plus Food Stamps) and how much were covered by other means. She also determined in each case, what income was reported to caseworkers and what income was not reported.

Her results indicated that AFDC and Food Stamps paid for only about 58% of the households' consumption spending. Of the supplementary income, most was income which the female recipients earned. In addition, however, boyfriends provided

about 20% of the supplementary income; absent fathers provided 8%; and relatives and friends provided another 15%.[20]

Edin found, incidentally, that not <u>one</u> of the recipients reported all of her supplementary income to AFDC, and in fact only a few reported <u>any</u> of the income. She "cross-validated" this result by interviewing caseworkers who, she showed, were aware that most clients were not isolated and did not subsist solely on welfare.

§ One Limitation of the Ethnographic Evidence on Isolation.

There is a potentially severe selection bias problem in using the ethnographic evidence to suggest that there is significant co-operation among members of a community. The ethnographer needs subjects who are willing to be interviewed and studied at length. That is, the ethnographer needs subjects who are willing to let a social scientist enter into their lives and study them. The selection bias problem is that those who assent to be studied are also likely to be more gregarious than normal. Thus ethnographic studies select people most likely to be involved in extensive social and economic cooperative networks.

Data sets such as the NLSY and CPS are of course also subject to this bias. We don't observe recluses in any survey data set. At first sight however, one expects that relatively short survey questionnaires will truncate less of the distribution than in-depth ethnographic research. For this reason, the empirical work below complements the ethnographic

work just as the ethnographic work enriches the empirical
work.

§ Contrasting Disadvantaged and Advantaged Communities.

Most policy analysts and economists are not from low-
income backgrounds. For readers who may be skeptical of the
notion of extensive sharing networks, it should be pointed out
that family structure may very well differ according to
income-level.

Gans (1982)[21] sketches several hypotheses on the effects
of income-level on family structure. In his view, the
"family" is broadest for low-income people. For low-income
people the family includes not only the members of the nuclear
family, but it also extends to include many relatives and also
some friends. In the vocabulary above, it is only the low-
income person who is integrated into an extensive kin-based
economic network.

For middle-income people in contrast, Gans suggests that
the family unit is more accurately thought of as the nuclear
family. Gans speculated that middle-income people's contacts
with relatives and friends is not as intense as it is for low-
income people. A person's identity derives mainly from his
role within the nuclear family.

For high income (professional) people, there is still
more isolation: in some ways the individual becomes the basic
unit of analysis as opposed to the nuclear family. Although
high-income families may be organized as nuclear families,

according to Gans each person's identity depends more on his roles _outside_ of the family (for example his professional roles) as opposed to his roles within the family.

Gans was dealing more with the question of people's conception of themselves than with the question of resource allocation. But it may also be that if we think of the family as the unit which shares resources, then here too the family unit is most extensive for lower-income people (and most restricted for higher income people).

Indeed, we can consider a simple, economic explanation for such a structure. Suppose that extensive sharing is primarily a means for coping with economic insecurity. People with advantaged backgrounds are more likely able to borrow (or deplete their savings) when their income unexpectedly drops. Disadvantaged people on the other hand are likely to be liquidity constrained: as a result, they may be more likely to develop behavior which involves significant resource sharing. It is through this sharing that they cope with unexpected income drops.

To say all this in another way, social isolation may be a normal good. As income increases, people can and do choose more isolation, more autonomy.[22]

It may seem at first sight that this notion of extensive sharing conflicts with other prominent theories of low-income life--in particular with the social "disorganization" in low-income life emphasized by culture of poverty theorists. In

fact, however, the descriptions of low-income life in research such as Lewis (1965) is quite compatible with this notion of extensive sharing. The lives described there are unstable and the social ties are hardly fixed through time; but the relationships (however brief) still involve a large amount of borrowing, lending, gift-giving, trading, etc.[23] In short, the assertion that the community is "disorganized" is a lot different from the assertion that individuals are "isolated."

§ Conclusion.

This chapter has suggested that AFDC recipients may not be socially isolated from men. Disadvantaged men may be significantly attached to AFDC-influenced economic networks. And this low social isolation may be representative of low-income communities generally.

In addition, the chapter has been organized around the views of the popular writers George Gilder and Charles Murray. It has shown first that they are correct that AFDC recipients are not socially isolated. Second, they are apparently wrong to only emphasize the economic involvement of boyfriends. And third, they mislead in paying attention to only one source of income for these networks: AFDC income. A complete picture acknowledges that the networks receive income from a variety of sources including earned income.

It is hoped that this chapter has presented a view of AFDC and low-income life which approaches--in Haveman's words--a new theoretical perspective. In the remaining three

chapters, the thesis will attempt to use this perspective to develop some specific testable economic hypotheses--and then to subject these hypotheses to econometric tests. We can turn now to a first substantive issue--that of the labor supply of disadvantaged men.

Chapter Two: Labor Supply

What is the magnitude of the AFDC effect on disadvantaged male labor supply? Robert Moffitt (1992) argued that this is one of the most important unanswered questions in the literature on the U.S. welfare system. It is an important question first of all because optimal AFDC policy depends on labor-supply effects.

It is also an important question because of the policy interest in the low employment rates of disadvantaged men. Most research on disadvantaged male employment has focused on the demand-side: i.e., the determination of employment opportunities. But for a complete understanding, we also need to understand the supply-side: i.e., the determination of non-employment opportunities--such as dependency (including AFDC dependency).[24]

There are two routes for such a general AFDC effect on male labor supply. First of all, following Chapter One, if disadvantaged communities involve significant resource sharing, then changes in AFDC will affect many members of the disadvantaged community--not just the official recipients. Secondly, there is another effect which we can consider which has been given attention in the literature: if AFDC affects

marriage rates then it might thereby indirectly affect male labor supply. Moffitt (1990a, 1992).

Although Moffitt and others have noted the potential importance of these effects, few researchers have addressed them empirically--presumably because of the problems inherent in using "official" data to estimate indirect and unofficial effects. One important purpose of this chapter is to propose and implement a strategy for identifying the effects.

The first section will present the theoretical framework which will be employed. It will be argued that both of the routes for an AFDC effect can be considered within the context of a simple Becker model of the family. The two routes will then be explained and elaborated in the context of that model. The second section will propose the estimation strategy. The essay will not attempt to disentangle the two routes of the effect; instead this chapter estimates the magnitude of the joint effect in a reduced-form setting. So little is known about these issues that identifying the joint effect is an important first step; the project of disentangling the two routes is left for future research.

The third section discusses the data. This is followed by the results, and then a discussion of their limitations. This chapter argues that it is indeed possible to empirically isolate a general AFDC effect on disadvantaged male labor supply. The empirical methods expose the underlying relation between AFDC and male labor supply, but they do not allow

precision in estimating the <u>magnitude</u> of the effect. Rather, the results can only give a sense of the possible magnitude of the relation. Specifically, the estimators give this "ballpark" magnitude as an educated guess: if 8% of men are affected by AFDC, then a 25% increase in AFDC would lower weekly male labor supply by about one-and-a-half hours per week.

§ The Becker Model and the Two Routes for an Effect.

Although some of the empirical effects discussed here are new to the literature (e.g. effects on entire disadvantaged social networks), the basic theoretical framework in the literature can be retained with little modification. Specifically, the Becker (1981) household production model can be used directly. Becker's basic approach is to think of the labor supply problem as the problem of allocation of time across different sectors. Becker's model will be reviewed, and then the two routes will be presented within the Becker framework.

In a simple household production model, utility is defined over m 'commodities':Z_i, i = 1 to m.

$$U = U(Z_1, \ldots, Z_m)$$

These commodities are few in number; they can be consumed only if they are produced in the household:

$$Z_i = f_i(x_i, t_{hi})$$

The scalar t_{hi} represents the amount of time devoted to home production of commodity i. The vector x_i represents market-purchased goods used in the production of commodity i.

Market goods are purchased from income:

$$p_i x_i = v + w t_w$$

where p_i is the price vector corresponding to x_i; v is unearned income; w is the hourly wage rate; and t_w is the time spent in market production. Total time, t, is divided between time spent at home and time spent in the labor market:

$$\sum_{j=1}^{m} t_{hj} + t_m = t$$

For current purposes, note that one important assumption in this model (which will be maintained throughout this thesis) is that market production is valued only for the money income it generates. Time spent at work does not directly generate any of the basic utility-producing commodities. Note also that an increase in unearned income implies a decline in market production (labor supply) and an increase in home production.

The first AFDC effect on male labor supply to consider is the one through "attachment"--through involvement in an AFDC-influenced kin network. Chapter One discussed such networks at length. For the more specific purposes of this chapter, we can define "attachment" by first considering a concrete example. Consider a single mother with children who is receiving AFDC. Suppose that there are periods in which her boyfriend (or her brother, her adult son, etc.) stays at her

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house. He sleeps and eats there. He works intermittently,
and he contributes income to the household.

Such a man is affected by AFDC for he is an effective
member of the household as an economic unit. In terms of the
Becker model, if benefits fell, he would have to increase his
market production at the expense of home production.
Likewise, if benefits rose, he could decrease his market
production in favor of more home production.

According to this definition, then, a man is "attached"
to an AFDC household if one or more of the following are true:

(1) he stays in the household;

(2) he eats in the household; or

(3) he contributes income or other resources to the
household to help the single mother.

Obviously, the contentious question here concerns the
extent of male attachment. It is obvious that there do exist
men who are attached to AFDC households. The ethnographic
evidence even suggests that such attachment is widespread and
characteristic of low-income communities. The contentious
question in this chapter, however, is whether attachment is
extensive enough to contribute to a noticeable effect in the
survey data.

We can note some ballpark estimates immediately. As
noted in the introduction, during the 1980s about 3.8 million
families annually received AFDC. There were about thirty
million men aged 21 through 34 in 1989.[25] If half of the AFDC

families had an attached man 21 to 34, then 6.3% of the men in this age group would be affected by AFDC. These ballpark estimates are just meant to establish that the AFDC program is indeed large enough to potentially affect a nontrivial portion of the male population.

The second route to consider is the route through marital status. Since AFDC aids single-parent families more than two-parent families, analysts have long suspected that increases in AFDC will lower marriage rates. There is some evidence that this occurs.[26]

There are two reasons to expect that if AFDC lowers marriage rates then it thereby lowers male labor supply as well. The first comes out of the Becker model, and the second is from the popular press.

First, in the context of Becker's model, we can assume that men typically have a comparative advantage in market production.[27] Given this assumption, when men marry, they should optimally allocate less time to home production and more time to market production. Marriage allows the husband and wife to increase their joint output by specialization. Thus, if an increase in AFDC increases the number of single men, it decreases the number of men who have increased their market specialization on account of marriage.

Although it does not fit conveniently into the basic (Becker) framework used here, a second argument linking male marriage and male labor supply--this one from the popular

press--should be mentioned. This argument stresses the well-known fact that on average single men supply less labor and receive lower wages than married men. In the popular literature, it is also sometimes asserted that single men are less responsible (also more violent, etc.) than married men. Obviously, these correlations say nothing about causality. It's not clear to what degree marital status implies labor supply (or responsibility)--or to what degree labor supply (or responsibility) implies marital status.

I think that in the popular press it is assumed that--to some degree anyway--there is causation <u>from marital status to labor supply behavior, responsibility, etc</u>. Specifically, marriage as a social act is thought to change a man's values. Marriage gives men a custodial social role in which they are more likely to value "providing"--"being a good provider" to their mate and offspring. Thus, under such popular reasoning if AFDC increases the number of single men, then AFDC thereby decreases male labor supply because it decreases the social importance of "being a good provider."

§ <u>Methodology.</u>

My estimation strategy takes advantage of the significant cross-state and cross-year variation in AFDC benefits. States choose their own benefit levels, and they choose how and when to change their benefit levels. As a result, AFDC opportunities vary both across states and within states across years.[28] The state-level proxy is the AFDC payment to a four-

person family if that family reports no supplementary income to the welfare department. This is the AFDC "guarantee" to a four-person family.

In January 1991, the average guarantee across states was $466 per month. It ranged from a low of $144 (Mississippi) to a high of $990 (Alaska). The standard deviation was $183.[29]

Between 1979 and 1991 most states' AFDC guarantees fell in real terms. In 1983 dollars, the average decline was $92. A populous exception is California which tied AFDC to inflation in 1979.

I estimate models of labor supply where labor supply is measured by annual hours of work. Letting $H \equiv$ labor supply, $M \equiv$ a marital status indicator, and $w \equiv$ the market wage rate, consider the simultaneous equations model:

(1) $$H = Z^H\beta + \delta M + \phi w + \epsilon$$

(2) $$M = Z^M\theta + \upsilon$$

(3) $$w = Z^w\alpha + u$$

where Z^i is a vector of exogenous variables affecting variable i, $i = H$, M, or w. For the reasons discussed above, the AFDC guarantee belongs (in some functional form) both in Z^H and in Z^M.

As noted above, I do not attempt to disentangle the two possible routes for the effect. Instead, I estimate the magnitude of the joint effect by estimating the reduced form:

(4) $$H = Z\gamma + \eta$$

where the AFDC guarantee is included in Z.

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A naive strategy would be simply to include AFDC (not interacted) as a regressor and then to interpret the AFDC coefficient as the average AFDC effect. We can devise a more reliable estimator, however, by taking advantage of the targeting feature of AFDC. AFDC should not affect all men in a state equally. Rather, AFDC should primarily affect men in low-income communities; AFDC should primarily affect disadvantaged men.

This targeting feature permits the more reliable strategy of estimating:

(5) $\qquad H = Z_0\gamma_0 + \gamma_A A + \gamma_D D + \gamma_{AD}(A*D) + \eta$

where Z_0 are controls (discussed later); A is the AFDC guarantee; and D is a disadvantaged indicator. (D = 1 if a person is disadvantaged and zero otherwise.)

This specification allows the effect of AFDC to differ for disadvantaged men and advantaged men. For <u>advantaged</u> men, the estimated marginal effect of AFDC on labor supply is:

$$\partial H/\partial A = \gamma_A$$

For <u>disadvantaged</u> men, the marginal effect is:

$$\partial H/\partial A = \gamma_A + \gamma_{AD}$$

In this setting, γ_{AD} is a reliable estimator of the magnitude of the AFDC effect on male labor supply.[30]

This is a reliable estimator because the primary estimation problem we face is possible omitted variable bias: unobserved person-specific or state-specific characteristics which are correlated with A. Specification (5) forces γ_A to

absorb the effect of many of these omitted variables. We know a priori that γ_A should be close to zero; i.e., we know a priori that the effect of AFDC on <u>advantaged</u> male labor supply should be close to zero. To whatever extent our estimate of γ_A differs from zero, we can interpret that as spurious correlation caused by unobserved variables correlated with A.

Accordingly, γ_{AD} estimates the effect of AFDC on disadvantaged male labor supply--net of the γ_A omitted-variable effect. γ_{AD} measures the disadvantaged labor supply effect over and above whatever correlation there is between AFDC and advantaged labor supply.

I use two basic types of proxies for disadvantaged status. First, I use a variety of background variables; for example, according to one proxy a man is considered disadvantaged if there was no employed male in his age 14 household. The second type of proxy is based on skill. Specifically, I define a man as disadvantaged if he did not graduate from high school.[31]

Because I have data across 13 years[32], I can include state dummies in Z_0. In that way, I can use two sources of AFDC variation--to determine whether they give the same result. When I do <u>not</u> use state dummies, the specification takes advantage of variation across states as well as variation over time. When I <u>do</u> include state dummies, then the specification only uses variation over time within states. If the γ_{AD} results differed significantly depending on the

source of AFDC variation, then that would cast doubt on the causal interpretation of γ_{AD}.

For example, if γ_{AD} were statistically significant only when state dummies were omitted, then that would suggest that γ_{AD} was just picking up some correlation between omitted fixed state characteristics and disadvantaged male labor supply. Likewise, if γ_{AD} were significant only in the specifications with fixed effects, then that would suggest the results were simply due to some state-level correlation between changes in AFDC and changes in disadvantaged labor supply.

In short, if AFDC affects disadvantaged male labor supply, then we would expect to see significant results both without state fixed effects and with them. With multiple years of data, we can check for this correspondence.

A final methodological point is that I have used both OLS and Tobit methods to estimate the specifications. About 5% of the observations have censored labor supply (0 annual hours of work). The results have been very similar for the two methods. For simplicity, I will therefore only report the OLS results below.

§ Data.

The National Longitudinal Survey of Youth (NLSY) is described in Appendix 3 for readers who are not familiar with it. It is the best available data set for this study for several reasons. First, it contains data on a policy-relevant cohort in recent years--young adult men during the 1980s.

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Second, because it is a micro (person-level) data set, it allows comparison between disadvantaged men and other men.[33] Third, it contains cross-year as well as cross-state variation--so it allows state fixed effects. Fourth, it over-samples the disadvantaged population; it is thus particularly attractive for studying that group. Finally, it contains rich background information on the respondents. These background variables can be used as proxies for disadvantaged status.

My NLSY extract covers men aged 21 to 34 in the years 1979 to 1991.[34] The sample includes (but is not limited to) the poverty ("supplemental") sample. All estimates employ the NLSY sampling weights. Table 2.1 gives sample means for selected variables. All monetary values have been deflated by the regional Consumer Price Index (CPI) into 1983 dollars. In the process of preparing the extract for analysis, the data was "cleaned" substantially. Some of the important parts of this cleaning process are explained in Appendix 3.

§ Results.

Table 2.2 contains two initial specifications in the form of equation (5) above; i.e.:

(5) $\quad\quad\quad H = Z_0\gamma_0 + \gamma_A A + \gamma_D D + \gamma_{AD}(A*D) + \eta$

State dummies are not included in these first regressions; they will be added in Table 2.4. The difference between the two columns in Table 2.2 is that they use different proxies for "disadvantaged". In column 1 a person is disadvantaged if there was no employed man in his age 14 household. In column

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2 a person is disadvantaged if he did not graduate from high school.

The focus here will of course be on the (A*D) parameter estimates. I will however first briefly discuss the Z_0 controls. All specifications include as regressors: year dummies, schooling, schooling squared, age, age squared, and race indicators. Each specification also includes these personal characteristics:

(1) schooling interacted with a time trend (since it is well-known that the return to schooling increased through time in the 1980s);

(2) a dummy variable equal to one if the person is enrolled in school at the observation date;

(3) an urban indicator fully interacted with the race indicators; and

(4) "family income" i.e., household income net of the person's own earned income.

Finally, in addition to the AFDC Guarantee, each specification includes these county and state characteristics:

(1) the average hourly wage for production workers on manufacturing payrolls in the state of current residence (as a proxy for wage levels);

(2) the county unemployment rate (as a further proxy for labor market conditions);

(3) the maximum weekly Unemployment Compensation (UI) benefit in the state (as a proxy for the generosity of the state's unemployment insurance program); and

(4) an AFDC-UP indicator equal to one if the state offered AFDC-UP in the observation year

We can now turn to the estimates of interest, the (A*D) parameter estimates   In Table 2.2, the bottom set of parameter estimates--labelled "AFDC * Disadvantaged"--gives γ_{AD}.   Since A is measured in $100s of 1983 dollars, γ_{AD} estimates the male labor supply effect of a $100 increase in a state's guarantee. The mean AFDC guarantee is approximately $400; so the $100 increase is quite large.

Looking first at column 1, γ_{AD} is estimated to be -37. A 25% increase in AFDC would cause an average decline of thirty-seven hours per year in disadvantaged male labor supply.   To interpret this estimate, we need to know the percentage of the population which is "disadvantaged" according to this first "employed man" proxy.   Table 2.1 (which gives means) shows that in fact about 16% of the population is "disadvantaged" under this first definition. Therefore the γ_{AD} estimate in column 1 implies that--if the labor supply effect were spread evenly among 16% of the population--then the labor supply of each affected man would fall by 37 hours per year.

Of course it is unlikely that the effect is spread evenly among all of the disadvantaged. Suppose instead that half of

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the disadvantaged men were affected--so 8% of the population was affected. Then a 25% increase in AFDC implies a decline of about seventy-four annual hours of labor supply. In terms of <u>weekly</u> labor supply: a 25% increase in AFDC implies a decline of one-and-a-half hours per week for affected men-- assuming AFDC affects 8% of the male population.

As a baseline for interpreting the results, I will continue to assume that 8% of the male population is affected. Note that the estimation strategy itself does not allow us to determine whether there is a large effect spread over a small number of people--or a small effect spread over many. If 4% of the population were affected, then the average effect would just be twice the effect calculated using the 8% baseline. Thus we can summarize the results using any baseline, and I will choose 8%. Given the ballpark figures above[35], the 8% assumption is equivalent to a baseline assumption that about 65% of AFDC households affect a man.

Column 2 uses the skill-based proxy for disadvantaged. A man is disadvantaged if he did not graduate from high school. The resulting γ_{AD} parameter estimate is larger than the column 1 estimate, and it is spread over a greater percentage of the population. Using the 8% baseline, this second γ_{AD} estimate indicates that a 25% increase in AFDC implies a decline of three-and-a-half hours of labor supply per week for affected men.

Turning to Table 2.3, we can see the results from using four different background variables. These results show that the "no employed man" proxy is not recherche. The four additional background proxies are:

(1) $D = 1$ if the person's age 14 household did not receive a newspaper;

(2) $D = 1$ if the person's mother did not graduate from high school.

(3) $D = 1$ if the person's age 14 household did not receive a magazine;

(4) $D = 1$ if there was no library card in the person's age 14 household;

For simplicity, Table 2.3 reports only the γ_A, γ_D, and γ_{AD} parameter estimates (not any of the Z_0 parameter estimates). Using the 8% baseline, the four columns indicate weekly labor supply reductions of 0.9, 2.3, 3.5, and 0.8 hours following a 25% increase in AFDC benefits.

Before turning to the state fixed-effect models, I will discuss the statistical significance of the estimates in these initial tables. Looking first at the t-statistics for the γ_{AD} coefficient, we see that all but one of the specifications gives significant results. The column 4 specification in Table 2.3 (using the library card proxy) is the only one which is not statistically significant.

In addition to the t-tests, I also computed F tests for each specification in Tables 2.2 and 2.3. Specifically, I tested 3 joint null hypotheses for each specification:

(1) H_0: $\gamma_A = 0$ and $\gamma_{AD} = 0$

(2) H_0: $\gamma_D = 0$ and $\gamma_{AD} = 0$

(3) H_0: $\gamma_A = 0$ and $\gamma_D = 0$ and $\gamma_{AD} = 0$

For most of these F tests, it was possible to reject the null at the 1% level. For the specification using the library card proxy, however, none of the three tests were significant at the 10% level. In other words, in this case the F tests and the t tests gave the same result: the parameter estimates are statistically significant in all but the one specification.

Table 2.4 repeats the six specifications from Tables 2.2 and 2.3--except that fixed state effects are now included. Thus the specifications in Table 2.4 estimate separate intercept terms for each state. Only variation within states over time is exploited. If the γ_{AD} results depended on the AFDC variation across states, that would cast doubt on any causal interpretation.

The (perhaps surprising) result in Table 2.4, however, is that adding state dummies only slightly attenuates the γ_{AD} coefficients. The implied 8% baseline effects are thus not substantially different from those described above. Instead, Table 2.4 shows that the A*D interaction strategy (by itself) almost completely controls for omitted state variables; γ_{AD} is not susceptible to omitted state-variable bias because γ_A

effectively absorbs the effect of most omitted state variables. (Note that introduction of the fixed state effects does imply large changes in the γ_A coefficients: the nature of the bias absorbed by γ_A changes depending on the AFDC variation source).

Turning to statistical significance in Table 2.4, t tests indicate that both the "no library card" specification and the "no newspaper" specification are not significant. I also performed the three F tests above for each specification. All three tests were insignificant for the "no library card" proxy. All the other tests were significant at the 5% level, except for one: in the "no newspaper" specification, the joint hypothesis that $\gamma_A = 0$ and $\gamma_{AD} = 0$ was significant only at the 10% level.

We can thus summarize the results from Tables 2.2 through 2.4. The parameter estimates range fairly widely. They indicate that if 8% of men are affected by AFDC changes, then <u>a 25% increase in AFDC lowers the weekly labor supply of</u> <u>affected men by between 0 and 3.4 hours per week. The median</u> <u>estimate from the background variables is 1.5 hours per week.</u>

Tables 2.5 through 2.9 contain additional supportive evidence.[36] In Table 2.5, instead of interacting D with A, I interact D with a proxy for the generosity of the state's Unemployment Insurance (UI) program. The regressions above included (as a Z_0 regressor) the maximum weekly UI benefit by state and year. In Table 2.5, I switch the places of the AFDC

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generosity variable and the UI generosity variable. Instead
of letting the effect of <u>AFDC</u> vary for the disadvantaged and
advantaged, I let the effect of <u>UI generosity</u> vary for the two
groups.

The sign of this "Disadvantaged-UI" coefficient is not
clear a priori. One might initially think that the UI program
is progressive--and thus that it does affect disadvantaged men
more than non-disadvantaged men. On the other hand, there is
a well-known study by Feldstein (1974, 1977) in which he
argued that the UI program in fact affects the advantaged
approximately equally with the disadvantaged. Feldstein
suggests several factors which mitigate the presumed
progressiveness of UI: the disadvantaged are more likely to
work in occupations which are not covered by the program; they
are more likely to quit a job (as opposed to being laid off);
and they are more likely to exhaust their UI benefits on
account of extended unemployment.

Thus, there is no strong, prima facie reason to expect
the "Disadvantaged-UI" coefficient to be negative and
significant. Estimating the parameter is useful, therefore,
because it helps check the estimation method itself.
Specifically, if the estimated "Disadvantaged-UI" parameter
were estimated to be negative and significant, that would
leave open the possibility that the estimation method is
biased toward finding a negative AFDC effect on disadvantaged
men. A negative and significant "UI-disadvantaged" sign would

either indicate that UI also affects disadvantaged men more than advantaged men, or it would indicate that the disadvantaged interaction strategy is biased toward finding policy effects.

In fact, the results in Table 2.5 indicate that UI does not affect disadvantaged male labor supply more than advantaged male labor supply. The UI variable does not give the same results as the AFDC variable: the absolute t-statistic on the interaction term is only 0.2. Thus Table 2.5 provides a specification check on the strategy of interacting social program proxies with disadvantaged proxies. That estimation strategy is not inherently biased toward finding negative disadvantaged male labor supply effects.

Table 2.6 runs an hourly wage regression using the A, D, and A*D regressors. One possible (but contrived) alternative interpretation of my results is that disadvantaged labor supply is lower in high AFDC states because, by coincidence, disadvantaged wages fall as AFDC rises. Table 2.6 uses the AFDC-interaction strategy in a wage regression to test this. The result is that there is no evidence that disadvantaged wages fall as AFDC rises: the absolute t-statistic on the interaction term is just 0.3.

I now turn to discuss the micro-macro statistical problem noted above. Although I have established that my results are robust to fixed state effects, I have to this point ignored the disturbance correlation among observations on people in

the same state in the same year. I have to this point ignored state-year shocks: e.g., shocks common to everyone living in Massachusetts in 1984. The statistical problem is that accounting for such off-diagonal disturbance correlation can dramatically increase standard errors. (see, e.g., Moulton, 1990).

I addressed this issue in two ways. First, I estimated a random-effects model where the random effect was a state-year shock. In the process of forming the Feasible Generalized Least Squares estimator, however, my estimator produced a negative estimate of the variance of the random effect. One way to interpret this result is that it indicates that the random effect is not empirically important.

Table 2.7, however, addresses the problem in a second, more satisfactory manner. In Table 2.7 I include fixed effects for each state-year combination: 637 dummies in all. Thus:

$$H = \pi_{s,t} + Z_0 \gamma_0 + \gamma_D D + \gamma_{AD}(A*D) + \eta$$

where $\pi_{s,t}$ is a state-year fixed effect, and where Z_0 cannot contain any state characteristics for these would be collinear with the state-year dummies. In this specification, then, advantaged male labor supply is determined by:

$$H = \pi_{s,t} + Z_0 \gamma_0 + \eta$$

Disadvantaged male labor supply is determined by:

$$H = \pi_{s,t} + Z_0 \gamma_0 + \gamma_D + \gamma_{AD}A + \eta$$

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As before, γ_{AD} gives the disadvantaged male labor supply effect of increases in A. But now there are no state-year disturbance components. The important result from the table is that the t-statistic on the γ_{AD} coefficient does not fall. This indicates that ignoring state-year shocks does not compromise significance tests in this problem.

Table 2.8 includes the predicted log wage as a regressor in the labor supply equation. As an instrument for the wage, I use the interaction of schooling and a time-trend. It is well known that the effect of schooling on wages varied through the 1980s: the return to schooling increased. There is no reason, however, to believe that the effect of schooling on labor supply varied through the decade. Schooling belongs in the labor supply equation simply as a taste proxy.[37] There is no reason to expect that the effect of tastes on labor supply changed through the decade. Thus, the school*time-trend interaction can instrument the wage. The result from Table 2.8 is that, again, there is no dramatic change in the γ_{AD} coefficient in this alternative specification.

In Table 2.9, I experiment with dropping the State Manufacturing Wage Level proxy. Blank (1988) and Plotnick and Winters (1985) both note that high AFDC states tend also to be high wage states.[38] Thus, if we drop the State Manufacturing Wage regressor, then increases in AFDC will partly absorb the effect of increasing wages. If the AFDC-Disadvantaged

strategy is effective, then when we drop the Manufacturing Wage regressor:

(1) there should be no change in γ_{AD} since it is supposed to be net of omitted state variables; and

(2) the γ_A coefficient should increase--since now an increase in A also partly absorbs the effect of increasing wages, and increasing wages should imply increasing labor supply.

Table 2.9 shows the results. Column 1 reproduces the specification in column 1 of Table 2.4, Panel A. In column 2 the State Manufacturing Wage is dropped. The results again support our causal interpretation of these regressions: the γ_{AD} coefficient does not change; and the γ_A coefficient increases (becomes less negative).

There are a couple of final results which should be noted without reporting the tables. First, I stratified by race, and there was no substantial difference in the parameter estimates between blacks and whites. Second, since the "Family Income" variable is always problematic in labor-supply studies[39], I estimated some equations without it as a regressor, and the results did not change. Finally, in the regressions reported in the tables, I have included observations on people who are currently enrolled in school (and I included enrollment as a regressor). The conclusions do not change when the enrolled are dropped from the sample.

§   Three Limitations of the Results.

A first limitation of these labor supply results must be stressed.   These results can not explain any of the 1980s time-series trend in disadvantaged male employment. Disadvantaged male employment rates fell during the 1980s, and there has been much public attention to this fact.   Juhn (1992) following Welch (1990) argues that demand cannot explain all of this decline; she argues there must be a supply-side component to the decline.   At least on the face of it, however, it is not plausible to argue that public-dependency opportunities have been improving for men.

As is often-noted, AFDC benefits themselves fell throughout the decade.   The conventional wisdom is that this reflects increasingly conservative social policy.   Moffitt (1990b) however has argued that it instead reflects a shift away from cash transfers and toward in-kind transfers such as Food Stamps and Medicaid.   Food Stamps and Medicaid may also affect male labor supply, but even accounting for all transfer sources there was no dramatic increase in dependency benefits during the 1980s.   It is thus unlikely that public-dependency could explain any significant part of the time-series decline in disadvantaged employment.

A second limitation is that the results by themselves indicate nothing about the welfare consequences of the AFDC program.   The results here show that increases in AFDC

noticeably lower disadvantaged male labor supply. That does not imply any _valuative_ statement about AFDC.

For example, to the extent that the effect occurs through marriage rates, we can not _evaluate_ the results until we first adopt a position on the desirability of marriage. Likewise, to the extent that the effect occurs through AFDC-attached men, we can not _evaluate_ the results until we first adopt a position on the value of market production versus home production for disadvantaged men. Thus, the empirical results here should be considered as one input into a complicated social decision on AFDC generosity.

Chapters Three and Four begin to address these more complicated issues. Specifically, in order to know whether the fall in labor supply is socially beneficial, we should investigate how the men spend the additional time which previously was devoted to work. We do not have good data on types of home production, of course, but we can generalize our model to include two prominent forms of non-market activity: human capital investment, and crime. Chapters Three and Four address the AFDC effect on these two activities respectively.

A third and final limitation is that the particular parameter estimates here should not be interpreted as precise estimates of the magnitudes. It is better to think of them as ballpark estimates. To claim precision in these estimates would be disingenuous for a variety of reasons: the proxy variables used are quite weak for all regressors (implying

errors-in-variables); the estimates vary significantly depending on which disadvantaged proxy is used; the empirical model is reduced form in a broad sense; and the empirical study of male labor supply itself may not have a solid record.[40]

This final limitation, however, by no means decimates the empirical work here. The empirical results still accomplish something important: they provide convincing evidence for the first time that the general AFDC program does in fact affect male labor supply. The magnitude is hazy. But exposing the relationship is interesting in itself, and it is an important first step on the way to a long-term goal of precise, policy magnitudes.[41]

§ Conclusion.

In conclusion, there is good evidence that increases in AFDC generosity imply decreases in the labor supply of disadvantaged men. This supports the Moffitt hypothesis of marriage rate effects, and it also supports the hypothesis developed in Chapter One that disadvantaged men as economic actors should be considered to be integrated into community social networks.

The evidence does not support (but rather conflicts with) an alternative hypothesis, which can be mentioned here in conclusion. Even if disadvantaged women and disadvantaged men were isolated from each other, it may still be true that AFDC could affect disadvantaged men through a different type of

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community effect. Suppose that increases in AFDC, by increasing purchasing power in disadvantaged communities, affected men by increasing their employment opportunities. In other words, suppose that AFDC affected men not through family networks but rather through the amount of money to be spent in disadvantaged communities. The evidence in this chapter conflicts with this hypothesis because if it were true, one would expect a _positive_ relationship between AFDC benefits and male labor supply--not the exposed negative relationship.

Table 2.1

Sample Means and Number of Observations for Selected Variables

☐ AFDC guarantee by state for a family of four, in 100s of 1983 dollars	42544	3.96
☐ Average hourly manufacturing wage by state, in 1983 dollars.	42544	8.89
☐ County unemployment rate	42544	7.45
☐ Maximum weekly unemployment insurance benefit by state, in 1983 dollars	42544	173
☐ Annual number of hours of labor supply	42544	1869
☐ Age	42544	25.7
☐ Family Income net of own earned income in 100s of 1983 dollars	41298	94.1
☐ Schooling	42544	12.8
☐ Hispanic indicator	42544	.061
☐ Black indicator	42544	.131
☐ Urban indicator	42544	.795
☐ Enrollment indicator	42544	.115
☐ No employed man in age 14 household	42206	.163
☐ No magazine in age 14 household	42195	.332
☐ No newspaper in age 14 household	42398	.153
☐ No library card in age 14 household	42366	.265
☐ Mother's schooling less than 12	39459	.296
☐ Own schooling less than 12	42544	.209
☐ Annual Hours Worked equals 0	42544	.051

Table 2.2

OLS; Dependent Variable is Annual Hours of Labor Supply
Column 1 Uses A Background Proxy for Disadvantaged
Column 2 Uses A Schooling Proxy for Disadvantaged
No State Dummies

	(1)	(2)
Schooling * Time Trend	10.57	10.813
	(11.1)	(11.4)
Schooling	217.31	115.67
	(8.0)	(3.5)
Currently Enrolled	-651.52	-646.93
	(-28.1)	(-28.1)
County Unemployment	-21.688	-22.853
	(-6.3)	(-6.7)
State Manuf. Wage	-25.719	-23.043
	(-2.3)	(-2.1)
State UI Max Benefit	.414	.39637
	(1.5)	(1.5)
Age	324.54	331.87
	(10.6)	(11.0)
Family Income	-.201	-.18823
	(-4.2)	(-4.0)
AFDC-UP	9.4445	8.1368
	(0.4)	(0.4)
AFDC Guarantee (A)	-7.9874	.57012
	(-1.0)	(0.0)
Disadvantaged (D)	20.520	67.726
	(0.3)	(1.0)
AFDC * Disadvantaged (A*D)	-37.025	-72.725
	(-2.3)	(-5.2)
Disadvantaged Proxy	No Employed Man in Age 14 Household	Own Schooling < 12
Number of Obs	40966	41298
R-square	.1907	.1947

t-statistics robust to cluster sampling in parentheses. Each specification also includes: a constant, year dummies, Black, Hispanic, schooling squared, age squared, urban, urban interacted with Hispanic, and urban interacted with Black.

Table 2.3

OLS; Dependent Variable is Annual Hours of Labor Supply
4 Specifications (4 Background Disadvantaged Proxies)
No State Dummies

	(1)	(2)	(3)	(4)
A	-11.054	-1.377	-1.8946	-11.045
	(-1.4)	(-0.1)	(-0.2)	(-1.4)
D	18.20	33.193	104.85	53.803
	(0.3)	(0.6)	(2.1)	(1.0)
A*D	-24.871	-33.28	-43.749	-13.410
	(-1.7)	(-2.4)	(-3.5)	(-0.9)
D Proxy	No Newspaper in Age 14 Household	Mother's Schooling < 12	No Magazine in Age 14 Household	No Library Card in Age 14 Household
N	41158	38335	40957	41126
R-square	.1887	.1934	.1898	.1878

t-statistics robust to cluster sampling in parentheses. Specifications are isomorphic to the specifications in Table 2.2; i.e., each specification includes: schooling interacted with a time trend, schooling, a current school-enrollment indicator, the county unemployment rate, the average state manufacturing wage, the state's maximum weekly UI benefit, age, family income, an AFDC-UP indicator, a constant, year dummies, Black, Hispanic, schooling squared, age squared, urban, urban interacted with Hispanic, and urban interacted with Black.

Table 2.4

State Fixed Effect Models
OLS; Dependent Variable is Annual Hours of Labor Supply
3 Specifications (Corresponding to 3 Disadvantaged Proxies)

Panel A

	(1)	(2)	(3)
A	-40.060	-40.455	-29.301
	(-1.4)	(-1.4)	(-1.0)
D	20.750	53.190	86.725
	(0.3)	(0.9)	(1.7)
A*D	-35.902	-11.954	-40.349
	(-2.2)	(-0.8)	(-3.2)
D Proxy	No Employed Man in Age 14 Household	No Library Card in Age 14 Household	No Magazine in Age 14 Household
N	40966	41126	40957
R-square	0.1977	.1950	.1969

Panel B

	(1)	(2)	(3)
A	-40.204	-36.223	-38.156
	(-1.4)	(-1.3)	(-1.4)
D	-2.4897	22.643	64.379
	(-0.0)	(0.4)	(1.0)
A*D	-21.078	-33.701	-72.762
	(-1.4)	(-2.4)	(-5.2)
D Proxy	No Newspaper in Age 14 Household	Mother's Schooling < 12	Own Schooling < 12
N	41158	38335	41298
R-square	.1960	.2016	.2019

In both panels, t-statistics robust to cluster sampling in parentheses. Each specification includes state dummies, and then all other regressors also included in Tables 2.2 and 2.3: i.e., schooling interacted with a time trend, schooling, a current school-enrollment indicator, the county unemployment rate, the average state manufacturing wage, the state's maximum weekly UI benefit, age, family income, an AFDC-UP indicator, a constant, year dummies, Black, Hispanic, schooling squared, age squared, urban, urban interacted with Hispanic, and urban interacted with Black.

Table 2.5

Interacting D with <u>Max State UI</u> (Instead of A)
i.e., Switching the Places of AFDC and UI in the Regression

OLS; Dependent Variable is Annual Hours of Labor Supply
Disadvantaged Proxy is "No Employed Man in Age 14 Household"
State Dummies Included

Schooling * Time Trend	10.50
	(11.1)
Schooling	220.7
	(8.3)
Currently Enrolled	-650.8
	(-28.2)
County Unemployment	-18.97
	(-5.5)
State Manuf. Wage	53.20
	(1.7)
AFDC Guarantee	-42.98
	(-1.5)
Age	321.9
	(10.6)
Family Income	-.2229
	(-4.7)
AFDC-UP	-17.99
	(-0.8)
State UI Max Benefit	.0579
	(0.1)
Disadvantaged	-95.47
	(-0.9)
UI Max * Disadvantaged	-.14
	(-0.2)
Number of Obs	40966
R-square	0.1971

t-statistics robust to cluster sampling in parentheses. The regression also includes: a constant, state dummies, year dummies, Black, Hispanic, schooling squared, age squared, urban, urban interacted with Hispanic, and urban interacted with Black.

Table 2.6

Hourly Wage Regression

OLS; Dependent Variable is Hourly Wage
Disadvantaged Proxy is "No Employed Man in Age 14 Household"
State Dummies Included

Schooling * Time Trend	.0057
	(10.2)
Schooling	.0489
	(3.0)
Currently Enrolled	-.1961
	(-12.3)
County Unemployment	-.0197
	(-6.7)
State Manuf. Wage	.0745
	(3.9)
State UI Max Benefit	.0013
	(4.6)
Age	.1860
	(10.3)
Family Income	-.000
	(-0.8)
AFDC-UP	-.0180
	(-1.4)
AFDC Guarantee	.0058
	(0.3)
Disadvantaged	-.0621
	(-1.6)
AFDC * Disadvantaged	-.0035
	(-0.3)
Number of Obs	35813
R-square	0.2128

t-statistics robust to cluster sampling in parentheses. The regression also includes: a constant, state dummies, year dummies, Black, Hispanic, schooling squared, age squared, urban, urban interacted with Hispanic, and urban interacted with Black.

Table 2.7

Fully Saturated Model: 637 Dummy Regressors
[A Dummy for Each (State, Year) Combination]

OLS; Dependent Variable is Annual Hours of Labor Supply
Disadvantaged Proxy is "No Employed Man in Age 14 Household"

Schooling * Time Trend	10.492
	(11.2)
Schooling	222.70
	(8.4)
Currently Enrolled	-644.61
	(-28.1)
County Unemployment	-17.433
	(-3.9)
State Manuf. Wage	Collinear with (State, Year) Dummies
State UI Max Benefit	Collinear with (State, Year) Dummies
Age	317.6
	(10.4)
Family Income	-.23042
	(-4.8)
AFDC-UP	Collinear with (State, Year) Dummies
AFDC Guarantee	Collinear with (State, Year) Dummies
Disadvantaged	18.768
	(0.3)
AFDC * Disadvantaged	-35.882
	(-2.2)

Number of Obs	40966
R-square	0.2107

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t-statistics robust to cluster sampling in parentheses. The regression also includes: a constant, a dummy for each [state, year] combination, Black, Hispanic, schooling squared, age squared, urban, urban interacted with Hispanic, and urban interacted with Black. There were 637 [state, year] dummies: 13 years by 51 states except for 26 with no observations.

Table 2.8

Labor Supply with Instrumented Wage as Explanatory Var.
Instrument is Interaction of Schooling and Time Trend

OLS; Dependent Variable is Annual Hours of Work
D Proxy is "No Employed Man in Age 14 Household"
State Dummies Included

Predicted Log Wage	1823.
	(11.2)
Schooling	132.1
	(4.6)
Currently Enrolled	-292.9
	(-6.5)
County Unemployment	17.01
	(3.8)
State Manuf. Wage	-82.93
	(-2.5)
State UI Max Benefit	-2.433
	(-4.5)
Age	-17.79
	(-0.4)
Family Income	-.1764
	(-3.7)
AFDC-UP	15.96
	(0.7)
AFDC Guarantee	-50.74
	(-1.8)
Disadvantaged	134.0
	(2.1)
AFDC * Disadvantaged	-29.46
	(-1.8)
Number of Obs	40966
R-square	0.1977

t-statistics robust to cluster sampling in parentheses. The regression also includes: a constant, state dummies, year dummies, Black, Hispanic, schooling squared, age squared, urban, urban interacted with Hispanic, and urban interacted with Black.

Table 2.9

Experimenting with the State Wage Variable

OLS; Dependent Variable is Annual Hours of Labor Supply
Disadvantaged Proxy is "No Employed Man in Age 14 Household"
State Dummies Included

	(1)	(2)
Schooling * Time Trend	10.57	10.53
	(11.2)	(11.1)
Schooling	221.4	221.9
	(8.3)	(8.3)
Currently Enrolled	-650.5	-650.8
	(-28.2)	(-28.2)
County Unemployment	-18.95	-18.80
	(-5.5)	(-5.4)
State Manuf. Wage	52.92	--
	(1.6)	--
State UI Max Benefit	.0278	.1854
	(0.0)	(0.3)
Age	321.3	321.0
	(10.6)	(10.6)
Family Income	-.2238	-.2238
	(-4.7)	(-4.7)
AFDC-UP	-16.87	-17.31
	(-0.8)	(-0.8)
AFDC Guarantee	-40.06	-26.97
	(-1.4)	(-1.0)
Disadvantaged	20.75	20.63
	(0.3)	(0.3)
AFDC * Disadvantaged	-35.90	-35.94
	(-2.2)	(-2.2)
Number of Obs	40966	40966
R-square	0.1977	.1976

t-statistics robust to cluster sampling in parentheses. Each
specification also includes: a constant, state dummies, year
dummies, Black, Hispanic, schooling squared, age squared,
urban, urban interacted with Hispanic, and urban interacted
with Black.

Chapter Three: Schooling

Hirschman (1991) exposed the common rhetorical structure of some prominent arguments against progressive social policy. For any government policy, we can say that there is a first-order intended effect, and then there are accompanying second-order effects; using medical terminology, the second-order effects are "side effects."

Economic comments on social policy frequently take the rhetorical form of arguing that the second-order side effects are socially harmful. The second-order effects are thus said to mitigate the overall helpfulness of the policy. If you just consider the first-order effect, the policy looks good; but once you take into account the second-order effects, the policy looks less good--and even possibly harmful overall.[42] AFDC is a classic example. Its first-order effect is to transfer scarce resources towards some of the most disadvantaged members of society. But then there are the often-discussed harmful second-order effects such as labor supply effects, illegitimacy effects, and so on.

As Hirschman notes, the familiarity of this reactionary rhetoric blinds us to the possibility that second-order effects may very well be <u>positive</u>. The second-order effects

of a social policy need not <u>mitigate</u> the first-order effects;

they may instead <u>reinforce</u> the positive first-order effects.

The world is not structured such that unintended, secondary

effects of social policy are always socially harmful.

> "Almost two and a half centuries ago, Voltaire wrote his
> celebrated novel <u>Candide</u> to mock the proposition that
> ours is the 'best of all possible worlds.' Since then,
> we have been thoroughly indoctrinated in the power and
> ubiquity of the perverse effect in the social universe.
> Perhaps it is time for an <u>Anti-Candide</u> to insinuate that
> ours is not the most perverse of all possible worlds,
> either." (Hirschman, 1991, p. 42)

This chapter is offered as a counter to the trend which

Hirschman has identified.[43] It investigates the possibility

that increases in AFDC may have the positive side effect of

increasing the schooling of disadvantaged men. Although an

AFDC-to-schooling connection may at first sight seem strained,

the chapter will attempt to show that it is in fact a natural

connection both in a theoretical context and in the AFDC

social context.

The first section reviews the few studies I could locate

which treat the issue of AFDC schooling effects. The next

section presents a simple theoretical justification for the

study. The third section argues that AFDC's schooling effects

arise naturally out of the social context. The fourth section

discusses the empirical strategy, and the next section

discusses results. The empirical work does strongly suggest

that there is a statistically significant and positive

relationship between AFDC and disadvantaged male schooling.

This statistical relationship, however, is not as strong as

the labor supply results above. In particular, it is only found among non-blacks, and the statistical technique does not allow precision in understanding the economic magnitude of the effect.

§ Literature Review.

There has apparently been very little attention given to this issue in the literature. I could locate only three relevant studies. First, Garfinkel (1973) did not specifically address AFDC, but he did note the importance of schooling effects as side-effects of income maintenance programs. Specifically, Garfinkel was interested in making an efficiency comparison between two income maintenance programs: a negative income tax program and a wage subsidy program. He argued that compared to a negative income tax, wage subsidies may have relatively large negative effects on schooling (because the wage subsidies increase the value of the earnings foregone during schooling). Garfinkel's note just cautioned against ignoring this effect. Taking the schooling effect into account could reverse one's opinion about an income maintenance program.

Second, Lerman (1986) in the study mentioned in Appendix 2, investigated the effect of welfare on the schooling of young men who still live in their parental household. He found that if a young man's parental household receives welfare transfers, then that decreases the relative probability that the young man will currently be in school.[44]

Third, Haveman and Wolfe (1994) found the opposite result from Lerman: they found a _positive_ link between schooling and AFDC receipt. They estimated schooling regressions using the Panel Study of Income Dynamics (PSID). Two of their independent variables are of current interest. One, they included as a regressor the number of years that the individual's family was poor. Two, they also included as a regressor the number of years that the individual's family was both poor _and_ received AFDC. They found that, controlling for the number of years poor, schooling attainment increases with the number of years of AFDC-receipt. In effect, they were comparing two types of _poor_ families: those which received AFDC and those which didn't. They found schooling higher in the AFDC-receiving group. Haveman and Wolfe noted that their estimate, although statistically significant, was not quantitatively large.

Thus, there were conflicting results in the two empirical studies I was able to find. Lerman found negative schooling effects, and Haveman and Wolfe found positive effects. One explanation for the difference is that Haveman and Wolfe used more extensive controls for the families' earning power. Both studies, however, used receipt of AFDC to identify the effect. The empirical work below differs by using state program variation in identification.

§ A Theoretical Motivation.

AFDC payments are not explicitly targeted for schooling expenses, but since AFDC significantly affects the economics of low-income kinship networks, it should have second-order effects on low-income schooling.

As in Chapter Two, the Becker model can be helpful for explication. Again, the basic framework is one of the allocation of time across sectors. For the current chapter, we can add an investment sector to the Chapter Two model. In this version of the Becker model, time is divided among home production, market production, and schooling (investment):

$$\Sigma^m_{j=1} t_{hj} + t_m + t_q = t$$

where t_q is time spent in schooling.

Just as household production requires both time and market goods, human-capital production also requires both time and market goods. If Q is gross investment in human capital then:

$$Q(x_q, t_q)$$

where x_q are market goods used in the production of human capital. Thus schooling is expensive for two reasons--because of the direct costs of the x_q goods, and also because of the opportunity costs of the time spent in school. As with time spent in market production, time spent at school does not produce any of the basic Z commodities.

The added assumption of imperfect capital markets provides a theoretical context in which AFDC effects on

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schooling are important. The reason is that with imperfect capital markets, disadvantaged people will not be able to afford worthwhile human capital investments. Because disadvantaged people (and especially disadvantaged youth) are limited in their ability to borrow to finance human capital investments, they underinvest in schooling. They face relatively high marginal returns to schooling investments.

Becker and also Loury[45] have emphasized that as a result, the typically-assumed tradeoff between equity and efficiency does not hold in the presence of imperfect capital markets. There would be an efficiency <u>gain</u> if advantaged people financed the schooling of disadvantaged people. Disadvantaged schooling is after all a relatively profitable investment.

For present purposes, the important result is that arguments such as Becker's and Loury's imply that if we were to judge AFDC on efficiency criteria, then we would want to look at its effect on the schooling of the disadvantaged population. With imperfect capital markets, government programs such as AFDC can be seen as policy interventions which are designed to enforce the efficient outcome. AFDC may be justified on equity grounds alone, but if it also has efficiency advantages, then the natural place to look is its schooling effects.

§ The Social Context.

Following chapters One and Two, AFDC schooling effects should not be limited just to official AFDC recipients.

Generally AFDC should affect all members of high AFDC-receipt kin networks. In general, the higher are AFDC grants, the less pressure on the members of the economic networks to earn income. And since schooling in the Becker model requires investments of time and market goods, less income pressure implies a greater ability of family members to invest in the schooling sector. For concreteness, and to show that this schooling link arises naturally out of the social context, we can consider two prominent examples of men economically linked to an AFDC mother: her son, and the father of her child.

The most direct case is a young man whose mother is receiving AFDC officially to support him.[46] AFDC may allow the mother to purchase for this son the market goods which schooling requires. This effect of AFDC has been a frequent explicit concern of activists throughout the history of AFDC. To take a severe example, in 1960 the state of Louisiana created a crisis by suddenly dropping AFDC aid to about 22,000 children.[47] Community workers informed the Federal government of the effects of this mass dismissal. Schooling was among their primary concerns. Bell reports that after the mass dismissal:

> "In general the emergency needs were met by voluntary effort, organized for the most part by the Negro community. The federal representatives were told 'again and again' by organizations in the state that a 'large group of families were hungry, had 'no place to turn to,' were on the point of eviction from their homes, and would soon have to remove their children from school' because of lack of clothing, particularly shoes." (Bell, 1965, p. 141)

In another example, the 1967 Department of Health Education and Welfare surveyed recipients to determine the adequacy of AFDC benefit levels. One of their stated interests was to determine whether the benefits were generous enough for the children to attend school.[48]

The officially absent father is a second concrete example of AFDC-influenced schooling. As discussed in Chapter One, a disadvantaged father may have little incentive to make child support payments to the state. The ethnographic literature however supports the hypothesis that disadvantaged men do have an incentive to help their children by investing in their own schooling. A father's schooling investment helps his children because it increases his lifetime income and thus his lifetime ability to support his children. For example, Sullivan argues that low-income communities reward young unwed fathers for remaining in school, and the young fathers are well aware that schooling is one way to help their offspring.

> "None of the young fathers in our study had ever been involved in an enforcement action. Yet several of these fathers were making occasional cash contributions, and some were continuing to invest in their own education and training, to the possible long-term benefit of the child." (Sullivan, 1993, p. 69)

Although schooling benefits his children as well as himself, as in the previous two examples a disadvantaged young father will likely be liquidity constrained. Even if additional schooling is the best long-term investment for all concerned, his current money needs and the needs of his children may be too great to justify additional schooling.

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Higher welfare payments help a young father stay in school by providing more income to his children--reducing the need for him to earn current income for them.

Thus, in Charles Murray's example of Harold and Phyllis (discussed in Chapter One), Murray notes that when Phyllis' welfare benefits increase, there is less pressure on Harold to remain in his unpleasant laundry job. But Murray implicitly assumes that Harold spends any additional time in home production: specifically leisure. Is it Panglossian to note that Harold might instead use some of the time to invest in his own schooling--for his own and his family's future?

§ Methodology.

I identify AFDC's schooling effect by means of an interaction strategy as in Chapter Two. One major difference, however, between schooling and labor supply is that schooling is a cumulative stock. It takes a year to add a year of schooling; therefore a person's current schooling stock depends directly and obviously on lagged as well as current variables. Focusing on AFDC as the policy variable of interest, we have:

$$S_{i,t} = F[X; A_{i,t}, A_{i,t-1}, A_{i,t-2}, \ldots]$$

where: S measures schooling stock; the vector X represents all explanatory variables except AFDC; and $A_{i,t}$ measures the generosity of AFDC in the individual's state of residence in year t.

As in Chapter Two, I control for omitted variable bias by interacting AFDC-generosity with a proxy for disadvantaged status.

$$S_{i,t} = g[X] + \delta_A A_i + \delta_D D_i + \delta_{AD} A_i * D_i + \upsilon_{i,t}$$

where: A_i measures current and lagged state AFDC-generosity for individual i; D_i indicates that individual i is disadvantaged.

In this case, the identification argument is that we expect that the effect of AFDC on <u>advantaged</u> individuals should be very small; thus the measured AFDC-advantaged effect (δ_A) is interpreted as capturing the general omitted variable effect. δ_{AD} accordingly identifies the effect of AFDC-generosity on <u>disadvantaged</u> schooling: δ_{AD} estimates the effect of AFDC on disadvantaged schooling over and above the measured correlation between AFDC and advantaged schooling.

The NLSY is an attractive data base for this problem for some of the same reasons as in Chapter Two: e.g., its rich background variables and its over-sampling of the disadvantaged. In addition, it has the added benefit in this context of following people through time. Therefore, it is ideal for measuring lagged as well as current explanatory variable values.

Implementation of the model is straightforward. Because my NLSY extract ends with the year 1991, the dependent variable will measure the individual's schooling stock as of 1991. I use a simple average of all current and lagged AFDC values to represent an individual's lifetime AFDC generosity

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variable (A_i). (The specification thus takes advantage of AFDC variation caused by migration). Because my extract includes at most 13 years of observations per individual, A_i averages at most 13 AFDC guarantee variables. To maximize the number of observations, if an individual is observed for x of the 13 years, then A_i for him is the average of the x AFDC values.

The additional state characteristics and local labor market characteristics are calculated in the same way--as simple averages for as many years as the individual is interviewed. The state variable proxies are the same as in Chapter Two--for example, the AFDC proxy is the guarantee for a four person household in the interview year.[49]

§ Results.

Table 3.1 presents means for selected variables for the sample used in estimation. All the means (as well as all the regressions presented below) are weighted by the NLSY sampling weights. In 1991, the NLSY cohort was aged twenty-six to thirty-three. Only 4% of the population is currently enrolled in school. Mean schooling is 13 years attained.

Table 3.2 presents a first set of regression results. The dependent variable for this table is simply "years of schooling attained in 1991." The table presents five specifications corresponding to five background variable proxies for disadvantaged status. Specifically, the specifications in columns one through five employ respectively the disadvantaged proxies: (1) no employed man in the

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respondent's household when he was aged fourteen; (2) mother did not graduate from high school; (3) no magazine in age fourteen household; (4) no newspaper in age fourteen household; (5) no library card in age fourteen household.)

As in Chapter Two, I present results from five different disadvantaged proxies just as a robustness check.[50]

The results of interest are the estimated coefficients on the A*D interaction term--the bottom row in the table. Of the five specifications, three are statistically significant in the anticipated direction: an increase in AFDC generosity implies an increase in disadvantaged male schooling relative to advantaged male schooling. I also computed F tests for each specification for three joint null hypotheses:

(1) H_0: $\delta_A = 0$ and $\delta_{AD} = 0$

(2) H_0: $\delta_D = 0$ and $\delta_{AD} = 0$

(3) H_0: $\delta_A = 0$ and $\delta_D = 0$ and $\delta_{AD} = 0$

The F tests indicated that none of the null hypotheses could be rejected for the first specification. For the second through fourth specifications, all the null hypotheses could be rejected at the 5% level (and most at the 1% level). For the fifth specification, it was possible to reject the second and third joint null but not the first.

Therefore, the F tests and the t tests present the same statistical significance results--summarizing, AFDC appears to have positive effects on disadvantaged men in three of the five specifications.

We can turn to Table 3.3 which presents results using a different dependent variable (a different measure of school attainment). In this table, the dependent variable is a dummy variable indicating high school graduation (at least 12 years of schooling). Each specification is estimated as a Probit with standard errors robust to heteroscedasticity. The table asks whether AFDC liberalizations imply increases in disadvantaged graduation rates--using the correlation between AFDC and advantaged graduation rates as a control. As the table shows, the results are not as significant as the schooling regressions in Table 3.2. In fact, none of the t-statistics[51] indicates statistical significance.

I also computed Wald tests for the Probit model--I computed test statistics for each of the three joint hypotheses above. With the Wald tests, the important result is that for each specification, it was impossible to reject the joint hypothesis that $\delta_A = \delta_{AD} = 0$. Both by the t tests and by the chi-square tests, then, there was no evidence that increases in AFDC imply increases in disadvantaged graduation rates.

In the discussion of labor supply in Chapter Two, it was noted that the results did not vary significantly when I stratified by race. In the example of schooling, however, the results did differ when broken down by race. Tables 3.4 and 3.5 address these differences.

Table 3.4 presents the δ_A, δ_D, and δ_{AD} parameter estimates for all of the specifications in Tables 3.2 and 3.3--except in this table the samples are limited to blacks. Table 3.5 repeats the exercise for non-blacks (whites and hispanics). In both tables, the regressors are the same as those in Tables 3.2 and 3.3 (except for those race variables which would be collinear); it is for simplicity that only the three δ coefficients are presented.

Table 3.4 indicates that for blacks there is no apparent relation between AFDC levels and schooling levels. This indicates that the significant results in Table 3.2 apparently came from the non-black population. Indeed, when years of schooling is the dependent variable, only one disadvantaged proxy produces a statistically significant result, and in that case the estimated coefficient indicates a negative relation between AFDC benefits and black schooling. That result is in the bottom set of rows in the table--using the "no library card" proxy.

Thus, when the dependent variable is "years of schooling attained," it is impossible to reject the hypothesis that increases in AFDC have no positive secondary effect on black schooling. This pessimistic conclusion is softened somewhat by the results using the dependent variable of "high school graduation." Table 3.4 indicates that with the graduation measure, there are two specifications producing positive and statistically significant effects of AFDC on disadvantaged

black schooling. These two significant coefficients, however, merely indicate the need for future research. They do not represent enough evidence to conclude positive effects in the case of blacks. The conclusion with respect to blacks from this study has to be simply that: it has been impossible to reject the null hypothesis of no positive schooling effect.[52]

Table 3.5 presents the δ coefficient estimates when the sample was restricted to non-blacks. When blacks are no longer in the sample, the positive AFDC effect appears sharper. There remains no strong evidence of an effect on high-school graduation rates. In the schooling-years regressions however, the significant coefficients have higher t-statistics and larger magnitudes. In the three middle specifications, Wald tests rejected each of the above three joint hypotheses at the 1% level.

We can now turn the discussion from statistical significance and toward economic significance. It should be noted up front, as in Chapter Two, that the empirical strategy employed here is not designed for accurate calibration of policy effects. The use of noisy background proxies in the interaction strategy, in particular, is designed to look broadly for effects which have not been identified in the data before; it is designed to notice broad brush strokes not precise parameters.

Nevertheless, it is useful to calculate the economic meaning of the parameters. Taking the case with the most

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significant effects--non-blacks and years of schooling as the dependent variable--the economic magnitudes vary widely across the different disadvantaged proxies. We can take the median estimate as an "educated guess" of AFDC's schooling effects.

The median estimate, according to Table 3.5 is the estimate using the "no newspaper" proxy. δ_{AD} is estimated to be .0025 where this effect is an average of the effects for the 14% of non-blacks who were disadvantaged according to this proxy.[53] To interpret this parameter, note that the experiment asks the effect of an increase in the over-time _average_ of AFDC benefits. Note also that mean benefits (according to Table 3.1) are $400. Thus, consider the effect of a $100 increase in benefits (thought of as approximately a 25% increase) which was _sustained_ over time (thus implying an increase in the _average_). According to the median estimate, such a sustained, 25% expansion in AFDC benefits would imply an average increase of 0.2 years of schooling--where the average is taken across 14% of the population.

Of course, if half of the "disadvantaged" were affected (i.e. if 7% of the population were affected by AFDC) then the schooling increase would be an average of 0.4 years of schooling for the affected group of 7%. Or, to put it in a final way, the (admittedly imprecise) best guess is that: _a sustained 25% increase in AFDC could ultimately add a year of schooling to 3.5% of the non-black population._

Before concluding, the two final tables should be discussed. Table 3.6 conducts the specification test of estimating the Table 3.2 regressors except for switching the places of the AFDC-generosity proxy and the UI-generosity proxy. That is, the UI-generosity proxy is interacted with disadvantaged. The question of interest is whether the regressions imply that UI positively impacts disadvantaged schooling. If so, then either UI is as progressive as AFDC, or the interaction technique is biased towards finding significant effects of social programs. In fact, the results in Table 3.6 support the causal AFDC hypotheses which have been discussed. The UI-disadvantaged interaction does not imply significant effects.

Finally, Table 3.7 drops the Family Income regressor. This also does not change the substantive results.

§ Conclusion.

This chapter has found positive disadvantaged schooling effects of AFDC. It thus supports the empirical work of Haveman and Wolfe (1994), but it does so by taking advantage of variation in state AFDC programs--not variation in reported AFDC receipt by families. This work raises of number of important questions for future research. In particular, it raises the question of why the effect was noticeable in the non-black population but not the black population. Also, the variation in economic magnitudes calls for studies which can

more accurately pinpoint the economic magnitude of these

secondary effects.

Table 3.1

Sample Sizes and Means for Selected Variables
Used in Chapter Three Empirical Work

Variable	N	Means
Age in 1991	4471	29.76
Hispanic	4471	.0673
Black	4471	.1425
Years of Schooling Attained by 1991	4461	13.03
NEM (No Employed Man in Age 14 Household)	4429	.1721
MSCHL (Mother did not Finish 12 Years of School)	4136	.2998
MAG (No Magazine in Age 14 Household)	4442	.3327
NEWS_P (No Newspaper in Age 14 Household)	4458	.1653
LIBR_CD (No Library Card in Age 14 Household)	4455	.266
Average State Max AFDC, 1979 - 1991 (measured in 1983 dollars [not hundreds of dollars])	4441	402
Average Urban Indicator, 1979 - 1991	4439	.7884
Average State Manuf. Wage, 1979 - 1991 (measured in 1983 cents [not dollars])	4441	896.6
Average State UI Max, 1979 - 1991 (measured in 1983 dollars)	4441	171.9
Average County Unempl. Rate, 1979 - 1991	4439	7.622
Average Family Income, 1979 - 1991 (measured in 1983 dollars [not hundreds of dollars])	4437	12115

All means employ the NLSY sampling weights. For more complete descriptions, see Table 2.1.

Table 3.2

Dependent Variable: Schooling Attained in 1991

OLS with White Robust Errors

	(1)	(2)	(3)	(4)	(5)
Constant	7.600	9.111	8.740	8.339	8.423
	(10.7)	(12.8)	(12.5)	(11.6)	(11.6)
1991 Age	.1334	.130	.1202	.1247	.1256
	(6.6)	(6.5)	(6.2)	(6.2)	(6.3)
Hispanic	.3658	.7692	.686	.3956	.1557
	(0.9)	(1.8)	(1.6)	(0.9)	(0.3)
Black	.0791	.443	.3757	.2607	.1918
	(0.3)	(1.9)	(1.6)	(1.1)	(0.8)
Urban Ave.	.9545	.7209	.9621	.8719	.67
	(6.6)	(5.3)	(7.0)	(6.1)	(4.8)
Black*Urban Ave.	-.8032	-.8601	-.8395	-.8849	-.8602
	(-3.2)	(-3.5)	(-3.4)	(-3.6)	(-3.4)
Hispanic*Urb Ave.	-1.583	-1.411	-1.578	-1.432	-1.235
	(-3.6)	(-3.2)	(-3.6)	(-3.3)	(-2.8)
State Wage Ave.	.0004	.0000	.0003	.0002	.0003
	(0.7)	(0.0)	(0.6)	(0.4)	(0.5)
State UI Ave.	.0001	-.0004	.0000	-.0002	-.0002
	(0.0)	(-0.2)	(0.0)	(-0.1)	(-0.1)
County Unempl Ave.	-.0720	-.0627	-.0622	-.0652	-.0565
	(-3.1)	(-2.7)	(-2.7)	(-2.8)	(-2.5)
Family Income Ave.	.0000	.0000	.0000	.0000	.0000
	(17.7)	(14.8)	(16.1)	(17.6)	(17.4)
A	-.0001	-.0007	-.0009	-.000	-.0003
	(-0.3)	(-1.7)	(-2.2)	(-1.2)	(-0.8)
D	-.2545	-2.088	-1.763	-1.491	-.9963
	(-0.7)	(-8.3)	(-7.1)	(-5.5)	(-3.7)
A*D	.0002	.0016	.0013	.0018	-.0000
	(0.2)	(2.7)	(2.3)	(2.8)	(-0.0)
Disadvantaged Proxy (D)	NEM	MSCHL	MAG	NEWS_P	LIBR_CD
N	4383	4094	4396	4412	4409
R-Squared	0.1695	0.2214	0.2140	0.1820	0.1955

t-statistics in parentheses. For explanatory variables, averages are taken for each individual across as many years between 1979 and 1991 that the individual has valid responses. For D proxy abbreviations, see Table 3.1.

Table 3.3

Dependent Variable: High School Graduate Indicator

(1 if schooling > 11; 0 otherwise)

Probit with Huber Errors

	(1)	(2)	(3)	(4)	(5)
Constant	-2.5027	-1.9166	-2.1761	-2.2342	-2.3479
	(-5.61)	(-4.03)	(-4.75)	(-5.02)	(-5.15)
1991 Age	.0817	.08981	.08140	.07962	.08363
	(6.63)	(6.94)	(6.54)	(6.51)	(6.79)
Hispanic	-.25995	-.05979	-.0789	-.21993	-.34213
	(-0.98)	(-0.21)	(-0.30)	(-0.82)	(-1.26)
Black	.0740	.18826	.15835	.11431	.06971
	(0.50)	(1.18)	(1.06)	(0.78)	(0.46)
Urban	.30810	.1509	.30516	.25019	.1816
	(3.21)	(1.48)	(3.19)	(2.60)	(1.90)
Black*Urban	-.25879	-.2319	-.242	-.26479	-.24549
	(-1.58)	(-1.29)	(-1.46)	(-1.62)	(-1.48)
Hispanic*Urb	-.27953	-.22060	-.33312	-.22545	-.15525
	(-1.01)	(-0.74)	(-1.21)	(-0.79)	(-0.54)
State Wage	-.00019	-.00047	-.0000	-.00019	-.00013
	(-0.50)	(-1.15)	(-0.21)	(-0.49)	(-0.35)
State UI	.00129	.00082	.00120	.0009	.00092
	(1.28)	(0.76)	(1.19)	(0.98)	(0.92)
County Un.	.02323	.03147	.02277	.02076	.02443
	(1.65)	(2.10)	(1.59)	(1.46)	(1.73)
Family Inc	.00005	.00004	.00004	.00004	.00005
	(11.22)	(9.49)	(10.33)	(11.20)	(11.35)
A	.00012	-.00014	-.00032	-000002	.00003
	(0.52)	(-0.49)	(-1.17)	(-0.00)	(0.15)
D	-.23053	-.94174	-.7436	-.55350	-.35786
	(-1.38)	(-5.95)	(-4.99)	(-3.62)	(-2.30)
A*D	.00014	.0005	.00049	.00044	-.00003
	(0.37)	(1.41)	(1.42)	(1.21)	(-0.08)
Disadvantaged Proxy (D)	NEM	MSCHL	MAG	NEWS_P	LIBR_CD
N	4383	4094	4396	4412	4409
Pseudo R2	0.1006	0.1472	0.1272	0.1086	0.1100

t-statistics in parentheses. For D proxy abbreviations, see Table 3.1. Regressors are averages as in Table 3.2.

Table 3.4

Schooling Results with Sample Restricted to Blacks

A, D, & A*D Parameter Results
for Both Years Attained and High School Graduation

D Proxy & Sample Size	Parameter	Dependent Variable	
		Years of Schooling Attained	High School Graduate
No Employed Man in Age 14 Household	A	-.0013 (-2.1)	-.00149 (-3.4)
	D	-.15458 (-0.4)	-.52985 (-2.5)
N = 1302	A*D	.00046 (0.5)	.0011 (2.1)
Mother's Schooling < 12	A	-.00123 (-1.8)	-.00105 (-2.1)
	D	-.88955 (-2.8)	-.36950 (-1.6)
N = 1181	A*D	.00036 (0.4)	.00033 (0.5)
No Magazine in Age 14 Household	A	-.00107 (-1.5)	-.00146 (-2.9)
	D	-.65621 (-2.0)	-.57170 (-2.5)
N = 1315	A*D	-.00051 (-0.6)	.00035 (0.6)
No Newspaper in Age 14 Household	A	-.00154 (-2.8)	-.00153 (-4.0)
	D	-.91978 (-2.9)	-.71762 (-3.4)
N = 1318	A*D	.00104 (1.2)	.00123 (2.1)
No Library Card in Age 14 Household	A	-.00089 (-1.5)	-.00094 (-2.5)
	D	.10762 (0.3)	.00440 (0.0)
N = 1319	A*D	-.00154 (-1.7)	-.00053 (-0.8)

All regressors in Tables 3.2 and 3.3 were also included in each specification.

Table 3.5
Schooling Results with Sample Restricted to Non-Blacks
(Whites and Hispanics Included)

A, D, & A*D Parameter Results
for Both Years Attained and High School Graduation

D Proxy & Sample Size	Parameter	Dependent Variable	
		Years of Schooling Attained	High School Graduate
No Employed Man in Age 14 Household	A	-.00001 (-0.0)	.00036 (1.3)
	D	-.45689 (-0.9)	-.28687 (-1.2)
N = 3081	A*D	.00054 (0.5)	.00025 (0.4)
Mother's Schooling < 12	A	-.00083 (-1.6)	-.00008 (-0.2)
	D	-2.5407 (-8.3)	-1.1924 (-6.1)
N = 2913	A*D	.00235 (3.4)	.0008 (1.9)
No Magazine in Age 14 Household	A	-.00101 (-2.1)	-.00019 (-0.6)
	D	-2.2040 (-7.2)	-.91661 (-5.0)
N = 3081	A*D	.00220 (3.2)	.0008 (2.0)
No Newspaper in Age 14 Household	A	-.00036 (-0.8)	.00027 (1.0)
	D	-1.8464 (-5.2)	-.61692 (-3.1)
N = 3094	A*D	.00253 (3.1)	.00052 (1.1)
No Library Card in Age 14 Household	A	-.00034 (-0.7)	.00021 (0.7)
	D	-1.4235 (-4.3)	-.51679 (-2.7)
N = 3090	A*D	.00071 (0.9)	.00023 (0.5)

All regressors in Tables 3.2 and 3.3 were also included in each specification.

Table 3.6

Dependent Variable: Schooling Attained in 1991

OLS With White Robust Errors

Interacting D with UI Proxy (Instead of AFDC Proxy)

	(1)	(2)	(3)	(4)	(5)
Constant	7.496	9.020	8.726	8.307	8.287
	(10.4)	(12.6)	(12.1)	(11.5)	(11.2)
1991 Age	.1329	.1305	.120	.1232	.1260
	(6.6)	(6.5)	(6.2)	(6.1)	(6.3)
Hispanic	.3754	.9190	.7516	.4263	.1622
	(0.9)	(2.2)	(1.8)	(1.0)	(0.3)
Black	.0445	.4003	.3393	.2006	.1725
	(0.1)	(1.7)	(1.5)	(0.9)	(0.7)
Urban Ave.	.9514	.7364	.9596	.8682	.6802
	(6.6)	(5.4)	(7.0)	(6.0)	(4.8)
Black*Urban Ave.	-.765	-.8189	-.8011	-.8267	-.8450
	(-3.1)	(-3.3)	(-3.3)	(-3.4)	(-3.3)
Hispanic*Urb Ave.	-1.587	-1.514	-1.61	-1.417	-1.244
	(-3.6)	(-3.4)	(-3.6)	(-3.2)	(-2.8)
State Wage Ave.	.0004	-.0000	.0003	.0002	.0003
	(0.7)	(-0.0)	(0.6)	(0.4)	(0.6)
U (State UI Ave.)	.0007	-.0011	-.0009	-.0006	.0003
	(0.4)	(-0.6)	(-0.5)	(-0.4)	(0.2)
County Unempl Ave.	-.0725	-.0627	-.0617	-.0679	-.0565
	(-3.1)	(-2.7)	(-2.7)	(-2.9)	(-2.5)
Family Income Ave.	.000	.0000	.0000	.0000	.0000
	(17.7)	(14.8)	(16.2)	(17.6)	(17.4)
A	-.0001	-.0002	-.0005	-.0001	-.0003
	(-0.3)	(-0.6)	(-1.4)	(-0.4)	(-1.0)
D	.449	-1.935	-1.7	-1.139	-.6138
	(0.8)	(-4.4)	(-4.0)	(-2.0)	(-1.3)
U*D	-.003	.002	.0031	.0021	-.0023
	(-1.2)	(1.1)	(1.2)	(0.6)	(-0.9)
Disadvantaged Proxy (D)	NEM	MSCHL	MAG	NEWS_P	LIBR_CD
N	4383	4094	4396	4412	4409
R-Squared	0.1698	0.2199	0.2131	0.1804	0.1957

t-statistics in parentheses. For explanatory variables, averages are taken for each individual across as many years between 1979 and 1991 that the individual has valid responses. For D proxy abbreviations, see Table 3.1.

Table 3.7

Dependent Variable: Schooling Attained in 1991

OLS with White Robust Errors

Family Income Variable Omitted

	(1)	(2)	(3)	(4)	(5)
Constant	11.2	12.14	11.96	11.98	11.93
	(15.9)	(17.2)	(17.2)	(16.8)	(16.6)
1991 Age	.0365	.0531	.0374	.0301	.0352
	(1.8)	(2.6)	(1.9)	(1.4)	(1.7)
Hispanic	.5397	1.033	.9261	.5947	.2765
	(1.2)	(2.4)	(2.1)	(1.4)	(0.6)
Black	-.1359	.2523	.1730	.0068	-.0831
	(-0.5)	(1.1)	(0.7)	(0.0)	(-0.3)
Urban Ave	1.272	.9287	1.224	1.135	.9097
	(8.5)	(6.6)	(8.7)	(7.6)	(6.2)
Black*Urban Ave.	-.9399	-.9720	-.9667	-1.028	-1.01
	(-3.7)	(-3.9)	(-3.9)	(-4.2)	(-3.9)
Hispanic*Urb Ave.	-2.105	-1.859	-2.090	-1.930	-1.723
	(-4.6)	(-4.1)	(-4.5)	(-4.3)	(-3.7)
State Wage Ave.	.0006	.0003	.0007	.000	.0007
	(1.0)	(0.5)	(1.1)	(0.9)	(1.1)
State UI Ave.	.0013	.0004	.0009	.000	.0006
	(0.8)	(0.3)	(0.6)	(0.3)	(0.4)
County Unempl Ave.	-.1131	-.0976	-.0993	-.1084	-.0966
	(-4.7)	(-4.2)	(-4.3)	(-4.5)	(-4.1)
A	.0000	-.0007	-.0010	-.0004	-.0003
	(0.0)	(-1.6)	(-2.3)	(-1.0)	(-0.8)
D	-.5671	-2.333	-2.082	-1.721	-1.201
	(-1.6)	(-9.0)	(-8.2)	(-5.9)	(-4.2)
A*D	-.0001	.0014	.0013	.0016	-.0000
	(-0.1)	(2.4)	(2.3)	(2.3)	(-0.1)
Disadvantaged Proxy (D)	NEM	MSCHL	MAG	NEWS_P	LIBR_CD
N	4387	4097	4400	4416	4413
R-Squared	0.0797	0.1566	0.1435	0.0948	0.1104

t-statistics in parentheses. For explanatory variables, averages are taken for each individual across as many years between 1979 and 1991 that the individual has valid responses. For D proxy abbreviations, see Table 3.1.

Chapter Four: Incarceration

Freeman (1992) has recently argued that we cannot claim to understand disadvantaged male labor markets without understanding the roles of crime and incarceration. Freeman's own estimates and also the highly publicized estimates of the Sentencing Project (1990) indicate that significant portions of disadvantaged men--especially disadvantaged young black men--have had some contact with criminal activity and the criminal justice system. For example, the Sentencing Project estimated that at one point in time nationwide 25% of young black men aged 20 to 29 were either in prison, on parole, or on probation.

One conceptual approach is to think of crime as a non-market activity: an activity outside of the labor market. There are some data caveats to such an approach, and these will be discussed in the first sections of this chapter. At least as an analytic starting point, however, one can think of crime as an alternative time-use category outside of the three sectors already considered (employment, home production, and schooling). The general framework of this thesis has been to analyze how AFDC benefits alter disadvantaged men's allocation of time among these different categories. This chapter

extends the study to the possible effect of AFDC benefit levels on crime levels.

This chapter should be thought of as a set of empirical explorations; it should be considered more speculative than the other chapters. The reason for this is just that the empirical study of crime (largely because of the data limitations discussed below) seems not nearly as well-established as either the study of labor supply or the study of schooling. The empirical investigations in this chapter are offered nevertheless because of the common perception that crime represents an important social problem. The importance of the problem motivates the explorations, but it should not impel us to overstate theses gleaned from inadequate data.

The chapter will proceed as follows. The first two sections will discuss two data caveats which need to be noted up front. The third section will present several hypotheses concerning the direction of the effect of AFDC generosity on criminal activity. The fourth section then introduces the methodology. The fifth section introduces the results. The results give no strong indication of the direction of the AFDC effect. The few statistically significant results, however, suggest that expansion in AFDC may increase incarceration rates.

§ First Data Caveat.

I will below treat crime as something different from employment. In terms of the allocation of time, criminal

activity will be treated as a separate category--different from employment. This conceptual approach is common in recent studies relating crime and employment.[54] A first contribution of this chapter, however, is to note (and provide some evidence) that at least some criminal activity is more profitably thought of types of employment--examples of employment rather than alternatives to employment.

In particular, criminal activity which produces income is clearly similar to employment. Crime which does not produce income may be appropriately thought of as different from employment. But for crime which does produce income, the best conceptual approach may be to think of it within the framework of occupational choice--crime as one type of employment. In such a framework, one chooses crime after comparing expected costs and benefits of criminal versus legal employment. One salient cost of criminal activity is the stochastic cost of detection and punishment. But the presence of this cost does not change the structure of the economic problem. The economic problem remains one of choosing the optimal way to earn income for the market goods used in home production.

Unfortunately, this question--whether crime is a type of employment or an alternative to employment--is not just theoretical. It is also empirical in the sense that extant data sets may include criminal time within their labor supply measures. That is, consider an individual who spends twenty hours per week engaging in income-producing crime. In extant

data sets such as the NLSY, these twenty hours may be included within the labor supply variable. In general it is unclear to what extent crime will be classified as employment in our data.

To take the simplest employment-status question, consider the classification of a person into either "employed," "unemployed," or "out of the labor force." The initial survey question for establishing employment status is: "What were you doing most of last week--working, going to school, or something else?"[55] The interviewer classifies the response, and several other questions are asked to determine employment status.

What is important for this discussion is that empirical classification depends on how the respondent views the time he spends in criminal activity. He may think that "working" refers only to legal work. On the other hand, he might very well think of his illegal income-producing activity as "work" or as a "job." In the prominent case of drug-dealers, there is ethnographic evidence that dealers do in fact think of the enterprise as "work"--their "job."[56] Moreover, even if he doesn't think of his illegal activity as work, a criminal may lie to the interviewer claiming that he is working. In this way, he could justify his standard of living without informing an official interviewer of his illegal activities.

We can gauge how crime is reported using the 1980 NLSY. In that year, the survey included a form on illegal activity

which was completed by the respondent. On the form, the individuals first answered twenty questions on whether they had ever engaged in twenty respective illegal or semi-illegal acts. The questions included questions about theft, fencing, drug-dealing, etc. Then the survey asked, "Still thinking about the last year, how much of your total income or support during the last year came from illegal activities?" There were 25 men[57] who answered in response "all or almost all." If we can think of these 25 men as being full time criminals during that year, we can profitably inquire how they were classified in the employment questions.

The answer is that sixteen of them were classified as "employed;" five were classified as "unemployed;" and four were classified as "out of the labor force". If we look at their labor supply responses, twenty-three of them had valid responses. Of these, the average annual hours worked was 983 hours--about twenty hours per week. Since these were individuals who reported that all or almost all of their income that year came from crime, this suggests that at least some criminal activity is counted in the data as "employment."

Moreover the picture should be even more clouded for people whose criminal income amounts to moonlighting--those who earn criminal income to supplement income from a legal job they hold. In fact, there is evidence that criminals often hold legitimate jobs in addition to engaging in crime.[58] The

question of how such people report their time from the two different jobs may even more ambiguous.

To summarize this first caveat, it may be incorrect both theoretically and empirically to approach criminal activity as something different from labor supply. For time devoted to crime that does <u>not</u> produce income, it seems reasonable to think of it as something other than employment. But time spent in income-producing crime may in fact be counted as labor supply in the data.

Note that to the extent that it is counted as labor supply, then Chapter Two suggests a first guess as to the effect of AFDC on crime. Our first guess should be that expansions in AFDC lower time spent in income-producing crime; an AFDC expansion lowers income pressure on disadvantaged men, and this implies less need to earn income through illegal as well as legal means.

§ Second Data Caveat.

The second data issue may be even more significant than the first. The fundamental problem of empirical economic approaches to crime (both crime which produces income and crime which doesn't) is that economic theories are typically based on models of individual choice. Yet in the area of crime we do not observe whether someone has <u>chosen</u> to engage in crime. As a result, it appears impossible to achieve structural form estimation.

Occasionally data sets include questions such as the one in the 1980 NLSY used above. But these questions are rare, and even when they are posed, we are naturally suspicious of respondents' replies. What incentive do the respondents have to tell an official interviewer that he is engaging in illegal activity? In the end, economists are left without good empirical counterparts to our theoretical models.

In the spirit of making preliminary explorations, this chapter will analyze incarceration as a proxy for criminal activity. The second data caveat is an up-front acknowledgement that in one fundamental sense, this is an inadequate proxy. It is inadequate because people do not "choose" incarceration; they do choose actions which lead to incarceration, but there are many complex factors intervening between the choice of actions and the incarceration. For example: the criminal act has to be detected (and many are not); the crime has to be severe enough to warrant incarceration conditional on detection (and many crimes are not). And of course there is the well-known problem that the probabilities of detection, arrest, and incarceration all depend on ascriptive characteristics such as race.

The empirical results below will use incarceration as a proxy for criminal activity, but these results should not be interpreted in terms of "individual choice" as much as the labor supply and schooling results above.

§ Hypotheses.

Having noted the two caveats, we can now tentatively investigate effects of AFDC on crime--thinking of crime as an alternative to employment and measuring criminal activity by incarceration.

The effect of an AFDC expansion on criminal activity and incarceration is unclear a priori. On the one hand, if the AFDC expansion eases the pressure on disadvantaged men to earn income, that could decrease crime and subsequent incarceration. For example, as noted above, the AFDC expansion should decrease the amount of income-producing crime.[59] Moreover, to the extent that lack of money creates the pressures which lead to crime--including crime which does not produce income such as some violent crime--easing the income constraint on disadvantaged communities will also lower crime rates.

On the other hand, there are at least two reasons to expect that AFDC liberalizations could increase crime rates. First, there is an argument which mirrors an argument in Chapter Two. To the extent that the AFDC liberalization lowers marriage rates it will also lower crime rates if marriage as a social act causes men to put more value on the role of "providing" for their family.

Second, an AFDC expansion will increase the amount of cash available in disadvantaged neighborhoods. This increase in cash may increase the rewards to crime in those

neighborhoods--either by increasing the ability of neighbors to buy illegal commodities or simply by increasing the amount which can be stolen locally.

Indeed, if you think of a large city with a few slum neighborhoods and also some very wealthy neighborhoods, you might at first think that theft opportunities are highest in the high-income neighborhoods. In fact, of course, opportunities will be higher in the low-income community as long as detection rates are enough lower there. Locks, lighting, and even police presence are typically much less adequate in low-income communities compared to high-income communities.[60]

Note though one immediate problem with this hypothesis. The Chapter Two results conflict somewhat with it-- specifically they conflict with the idea that AFDC improves the market in illegal goods. If an AFDC expansion increases illegal income opportunities, then you would also expect it to increase legal income opportunities. If it helps local fences and drug-dealers, then you would also expect it to help local mechanics and painters. But as the conclusion to Chapter Two notes, because the relation between AFDC and male labor supply is empirically negative, it is awkward to think that AFDC expansions somehow improve employment opportunities for disadvantaged men. The results in Chapter Two suggest supply-side resource-sharing effects--not demand-side employment opportunity effects.

§ Methodology.

Given that the direction of AFDC effects is not evident prima facie, we can build an empirical framework to test whether the data supports one direction much more than the other. As in previous chapters, we can use the general strategy of interacting state-level AFDC benefits with disadvantaged background proxies. The AFDC benefit will be included as a regressor alone; this regressor should absorb the effect of state characteristics which are correlated both with AFDC benefits and with incarceration rates. The coefficient on the interaction term will be interpreted as measuring the effect of AFDC on disadvantaged incarceration-- over and above whatever correlation exists between AFDC and the incarceration rates of men with advantaged backgrounds. Thus:

$$\text{Incarceration}_{i,t} = h[X] + \delta_A A_i + \delta_D D_i + \delta_{AD} A_i * D_i + \upsilon_{i,t}$$

where: A_i measures current and lagged state AFDC-generosity for individual i; D_i indicates that individual i is disadvantaged.

In addition to the advantages mentioned in previous chapters, the NLSY is an attractive data set in this context because it codes incarceration. Household-based surveys such as the CPS do not interview people who are incarcerated.

Besides the control variables which have been used throughout this thesis, for the purposes of this chapter an additional state-level variable was merged into the data set. Since attitudes toward crime--the amount of tolerance of

crime--can vary both across states and within states over time, a state crime-level proxy was created. Specifically, the regressions below control for the index crime rate by state and year.[61]

The index crime rate is calculated by the Federal Bureau of Investigation using data reported by state and local police agencies. The "index crimes" are: murder and nonnegligent manslaughter, forcible rape, robbery, aggravated assault, burglary, larceny-theft, and motor vehicle theft. The "index crime rate" is then the number of index crimes reported in a state scaled by the state's population. The results with this variable included are presented below. It should be noted, however, that in fact the results of interest were not changed when I experimented with excluding the variable from the regressions.

As in Chapter Three, most demographic regressors were calculated as average values for each individual--averaged across as many years of the previous thirteen in which the individual was interviewed. The dependent variable is an indicator capturing incarceration in 1991. All parameters are estimated by means of Probit analysis.[62]

§ Results.

The sample used for estimation in this chapter is identical to the sample used in Chapter Three. Most questions concerning sample means can therefore be answered by consulting Table 3.1. A few changes, however, should be

mentioned here. First, one change in this chapter is that for simplicity in presentation, the family income variable has been rescaled so that it is now expressed in thousands of dollars (not dollars). In addition, as noted above, the state-level index crime ratio has been introduced. The mean value for it is 55.0--measured in index crimes per 1000 people; in other words, on average there were .055 index crimes per person in the 1980s.

Finally, we can consider mean values for the incarceration indicator which will be used as the dependent variable. Table 4.1 presents means of the incarceration indicator both for the total population and also for some notable sub-groups in the population. As the table indicates, approximately 1.5% of the cohort population was incarcerated in 1991. Breaking this down by race, the incarceration rates were: 0.7% for whites; 5.6% for blacks; and 2.6% for hispanics. The bottom section of the table disaggregates by the disadvantaged background proxies. This disaggregation indicates that the disadvantaged proxy which best predicts incarceration is the proxy which captures whether there was an employed man in the individual's age fourteen household.

We can now turn to Table 4.2 which presents the initial results from the incarceration probits. In this first table, the entire sample is included. The five columns correspond to the five different background proxies for "Disadvantaged". In this table, all of the δ_{AD} coefficients are positive, but only

two of the five approach statistical significance. Joint tests of the hypothesis that $\delta_A = \delta_{AD} = 0$ give the same results as the t-ratios: viz., we can reject the null at the 5% level only in the first and third of the five specifications. Thus the table weakly supports the hypothesis that expansions in AFDC imply increases in the probability of incarceration.

This weak support breaks down however when we stratify by race. Table 4.3 presents the incarceration probits estimated on a sample of blacks only. In the case of blacks (the group in which incarceration is most prominent) none of the δ estimates are statistically significant. In fact, the results for blacks were not significant even in the Wald tests. Specifically, in each of the five specifications, it was impossible to reject the null that $\delta_A = \delta_{AD} = 0$ at even the 10% level.

Table 4.4 then presents the results when the sample is restricted to non-blacks. Here the pattern of statistical significance reappears (i.e., the first and third specifications seem strongest), but the smaller sample sizes reduce statistical significance relative to Table 4.2. Only the third of the five specifications produces marginally significant results. In that specification, it is possible to reject $\delta_A = \delta_{AD} = 0$ at the 10% level. None of the other specifications however produced significant joint tests.

Table 4.5 introduces fixed state effects into the specifications. Fifty dummy variable regressors are added to

capture the state of current residence in 1991.[63] These are
not collinear with the AFDC-generosity variable because of
migration across states.[64] The primary benefit of this
technique is that it allows each state to have its own
incarceration intercept. Incarceration probabilities should
vary widely across states. Since these incarceration
probabilities should be correlated with AFDC generosity, we
would not want to rely on estimates which were not robust to
fixed state effects. In fact, Table 4.5 indicates that the
results with fixed state effects are very similar to the
results without them. Table 4.5 gives very similar estimates
to those in Table 4.2. Specifically, two of the five
specifications produce marginally significant positive AFDC
effects on later incarceration. This should bolster the
suggestion that AFDC generosity increases later
incarceration.[65]

§ Conclusion.

Even if the results in these specifications were highly
significant, they should only be suggestive because of the
data limitations discussed at the beginning of the chapter.
In fact of course the specifications do not produce highly
significant results. In each table, the median estimate is
not statistically significant. And the statistical relations
disappear when we stratify by race.

It should be noted that these results do not rule out the
possibility of strong but counteracting effects of AFDC on

crime. For example, AFDC expansions may increase crime to the extent that they lower marriage rates. And at the same time, they may decrease crime to the extent that imply less pressure on disadvantaged men to earn illegal income. The results here do not rule out the possibility that both effects are present but they offset each other.

Table 4.1

Percentage Incarcerated in 1991

For Various Sub-Groups (By Race and Background Variables)

Population	% Incarcerated
Total	1.5
White	0.7
Black	5.6
Hispanic	2.6
No Employed Man in Age 14 Household	4.1
Mother's Schooling less than 11	2.4
No Magazine in Age 14 Household	2.8
No Newspaper in Age 14 Household	3.9
No Library Card in Age 14 Household	2.6

All means are calculated using the NLSY sampling weights.

Table 4.2

Incarceration Probits

(Dependent Var is 1 if Incarcerated in 1991; 0 Otherwise)

	(1)	(2)	(3)	(4)	(5)
Constant	-1.70	-.8241	-1.173	-1.654	-1.62
	(-1.4)	(-0.6)	(-1.0)	(-1.3)	(-1.4)
Age in 1991	-.0257	-.057	-.0371	-.031	-.036
	(-0.9)	(-1.9)	(-1.3)	(-1.1)	(-1.3)
Hispanic	1.003	.6515	.8259	.9207	.9950
	(2.6)	(1.3)	(2.1)	(2.3)	(2.6)
Black	.7308	.8111	.7479	.7159	.744
	(2.6)	(2.5)	(2.7)	(2.5)	(2.5)
Urban Average	-.2573	-.1765	-.2219	-.1846	-.16
	(-0.9)	(-0.6)	(-0.8)	(-0.6)	(-0.6)
Black*Urban Ave.	.0685	.0296	.0656	.1046	.1165
	(0.2)	(0.0)	(0.2)	(0.3)	(0.3)
Hispanic*Urban Ave.	-.7519	-.4131	-.6052	-.7378	-.7148
	(-1.9)	(-0.8)	(-1.5)	(-1.8)	(-1.8)
Manuf. Wage Ave.	.0006	.0007	.0004	.0004	.0003
	(0.9)	(1.0)	(0.6)	(0.7)	(0.6)
UI Max. Ave.	-.0028	-.0040	-.0030	-.0026	-.0026
	(-1.6)	(-2.1)	(-1.7)	(-1.5)	(-1.4)
Unemployment Ave.	-.0261	-.0210	-.0216	-.0192	-.022
	(-0.7)	(-0.6)	(-0.6)	(-0.5)	(-0.6)
Family Income Ave.	-.0144	-.0153	-.0169	-.0173	-.0178
	(-1.5)	(-1.4)	(-1.7)	(-1.8)	(-1.7)
Index Crime Rate Ave.	.0071	.0056	.0070	.007	.008
	(1.1)	(0.8)	(1.1)	(1.1)	(1.2)
A	.0000	.0002	-.0000	.0004	.0007
	(0.0)	(0.4)	(-0.0)	(0.8)	(1.5)
D	-.1576	-.0666	-.3184	.0846	.2421
	(-0.5)	(-0.2)	(-1.0)	(0.3)	(0.8)
A*D	.0012	.0006	.0013	.0006	.0001
	(1.8)	(0.8)	(1.9)	(1.0)	(0.2)
Disadvantaged Proxy (D)	NEM	MSCHL	MAG	NEWS_P	LIBR_CD
N	4393	4103	4406	4422	4419
Pseudo R2	0.1405	0.1329	0.1371	0.1378	0.1343

t-statistics in parentheses. For explanatory variables, averages are taken for each individual across as many years between 1979 and 1991 that the individual has valid responses. For D proxy abbreviations, see Table 3.1.

Table 4.3

Incarceration Probits

Sample Restricted to Blacks

	(1)	(2)	(3)	(4)	(5)
Constant	-1.056	-1.056	-.9589	-1.1	-.8417
	(-1.3)	(-1.2)	(-1.2)	(-1.5)	(-1.0)
Age in 1991	-.0258	-.0344	-.0290	-.0298	-.0307
	(-1.0)	(-1.3)	(-1.2)	(-1.2)	(-1.2)
Urban Average	-.0709	-.0888	-.0665	-.0123	-.0807
	(-0.3)	(-0.3)	(-0.3)	(-0.0)	(-0.3)
Manuf. Wage Ave.	-.0007	-.0005	-.0008	-.0008	-.0009
	(-0.8)	(-0.6)	(-0.9)	(-1.0)	(-1.0)
UI Max. Ave.	.0010	.0003	.001	.0014	.0012
	(0.4)	(0.1)	(0.4)	(0.6)	(0.5)
Unemployment Rate Ave.	.0170	.0311	.0239	.0248	.0273
	(0.4)	(0.8)	(0.6)	(0.7)	(0.7)
Family Income Ave.	-.0123	-.0095	-.0130	-.0152	-.0152
	(-0.9)	(-0.7)	(-1.0)	(-1.2)	(-1.1)
Index Crime Rate Ave.	.0098	.011	.0100	.0100	.0102
	(1.8)	(2.0)	(1.8)	(1.8)	(1.9)
A	.0002	.0001	.0002	.0008	.0004
	(0.4)	(0.3)	(0.3)	(1.6)	(0.8)
D	-.0504	-.051	-.1315	.3039	-.2862
	(-0.1)	(-0.1)	(-0.4)	(1.0)	(-0.9)
A*D	.0006	.0007	.0007	-.0002	.0009
	(0.9)	(0.8)	(1.0)	(-0.3)	(1.1)
Disadvantaged Proxy (D)	NEM	MSCHL	MAG	NEWS_P	LIBR_CD
N	1307	1186	1320	1323	1324
Pseudo R2	0.0276	0.0280	0.0256	0.0256	0.0228

t-statistics in parentheses. For explanatory variables, averages are taken for each individual across as many years between 1979 and 1991 that the individual has valid responses. For D proxy abbreviations, see Table 3.1.

Table 4.4

Incarceration Probits

Sample Restricted to Non-Blacks (Whites and Hispanics)

	(1)	(2)	(3)	(4)	(5)
Constant	-1.805	-.1861	-.8928	-1.77	-2.021
	(-1.0)	(-0.0)	(-0.5)	(-0.9)	(-1.1)
Hispanic Indicator	.9967	.6913	.815	.9254	1.074
	(2.5)	(1.4)	(2.0)	(2.3)	(2.7)
Age in 1991	-.0233	-.0707	-.0414	-.0258	-.0367
	(-0.6)	(-1.5)	(-0.9)	(-0.6)	(-0.8)
Urban Average	-.320	-.2000	-.2687	-.2051	-.124
	(-1.0)	(-0.6)	(-0.9)	(-0.6)	(-0.4)
Hispanic * Urban Ave.	-.7409	-.41	-.6032	-.7720	-.818
	(-1.8)	(-0.8)	(-1.4)	(-1.9)	(-2.0)
Manuf. Wage Ave.	.0015	.0015	.0012	.0014	.0011
	(1.6)	(1.6)	(1.3)	(1.5)	(1.3)
UI Max Ave.	-.0057	-.0073	-.0060	-.0061	-.0052
	(-2.4)	(-3.0)	(-2.7)	(-2.7)	(-2.1)
Unemployment Rate Ave.	-.042	-.036	-.0396	-.0359	-.04
	(-0.8)	(-0.7)	(-0.8)	(-0.7)	(-0.9)
Family Income Ave.	-.015	-.0192	-.0196	-.0167	-.0199
	(-1.1)	(-1.3)	(-1.4)	(-1.2)	(-1.4)
Index Crime Rate Ave.	.0050	.0009	.0046	.0043	.0069
	(0.4)	(0.0)	(0.4)	(0.3)	(0.6)
A	-.0000	.0002	-.0002	.0001	.0011
	(-0.0)	(0.2)	(-0.2)	(0.1)	(1.4)
D	-.2174	-.106	-.4679	.0420	.9033
	(-0.4)	(-0.1)	(-0.9)	(0.0)	(1.6)
A*D	.0014	.0006	.0017	.0010	-.0009
	(1.3)	(0.5)	(1.6)	(1.0)	(-0.9)
N	3086	2917	3086	3099	3095
Pseudo R2	0.0966	0.0810	0.0955	0.1007	0.1047

t-statistics in parentheses. For explanatory variables, averages are taken for each individual across as many years between 1979 and 1991 that the individual has valid responses. For D proxy abbreviations, see Table 3.1.

Table 4.5

Incarceration Probits with Entire Sample

Including State Dummies
(Indicator for State of 1991 Residence)

A	.0011	.0011	.0009	.0012	.001
	(1.1)	(1.2)	(0.8)	(1.3)	(1.6)
D.,	-.1477	-.0110	-.3367	.0832	.265
	(-0.5)	(-0.0)	(-1.1)	(0.3)	(0.9)
A*D	.001	.000	.0014	.0006	.0001
	(1.7)	(0.7)	(1.9)	(0.9)	(0.2)
Disadvantaged Proxy (D)	NEM	MSCHL	MAG	NEWS_P	LIBR_CD
N	3848	3465	3862	3876	3873
Pseudo R2	0.1833	0.1694	0.1828	0.1802	0.1798

All regressors in the specifications in Table 4.2 were also included in these specifications. (For simplicity only the three parameters of interest are reported here.) t-ratios in parentheses. For D proxy abbreviations, see Table 3.1.

Table 4.6

Incarceration Probits with Entire Sample

Family Income Regressor Omitted

	(1)	(2)	(3)	(4)	(5)
Constant	-2.177	-1.296	-1.695	-2.200	-2.18
	(-1.8)	(-1.0)	(-1.4)	(-1.8)	(-2.0)
Age in 1991	-.0139	-.0469	-.0248	-.0183	-.0235
	(-0.5)	(-1.6)	(-0.9)	(-0.6)	(-0.8)
Hispanic Indicator	1.00	.6195	.8001	.9176	1.002
	(2.6)	(1.3)	(2.1)	(2.4)	(2.6)
Black Indicator	.7735	.8667	.8042	.7797	.8197
	(2.7)	(2.6)	(2.9)	(2.7)	(2.7)
Urban Average	-.2795	-.188	-.249	-.2032	-.1826
	(-1.1)	(-0.6)	(-1.0)	(-0.8)	(-0.7)
Black*Urban Ave.	.0617	.0149	.0578	.0936	.1026
	(0.2)	(0.0)	(0.1)	(0.3)	(0.3)
Hispanic*Urban Ave.	-.712	-.3582	-.5345	-.6863	-.6653
	(-1.8)	(-0.7)	(-1.4)	(-1.8)	(-1.7)
Manuf. Wage Ave.	.0005	.0006	.0003	.0004	.0003
	(0.8)	(0.9)	(0.5)	(0.5)	(0.4)
UI Max. Ave.	-.0028	-.0040	-.0030	-.0026	-.002
	(-1.6)	(-2.0)	(-1.7)	(-1.5)	(-1.5)
Unemployment Rate Ave.	-.0219	-.0164	-.0170	-.0140	-.0171
	(-0.6)	(-0.4)	(-0.5)	(-0.4)	(-0.5)
Index Crime Rate Ave.	.0070	.0058	.0069	.0070	.008
	(1.1)	(0.8)	(1.1)	(1.1)	(1.3)
A	.000005	.0003	-.00004	.0004	.0006
	(0.0)	(0.4)	(-0.0)	(0.8)	(1.5)
D	-.1033	-.0097	-.2614	.1272	.2595
	(-0.3)	(-0.0)	(-0.8)	(0.4)	(0.8)
A*D	.0012	.0006	.0013	.0006	.0001
	(1.9)	(0.8)	(1.9)	(1.0)	(0.2)
Disadvantaged Proxy (D)	NEM	MSCHL	MAG	NEWS_P	LIBR_CD
N	4397	4106	4410	4426	4423
Pseudo R2	0.1348	0.1260	0.1293	0.1293	0.1252

t-statistics in parentheses. For explanatory variables, averages are taken for each individual across as many years between 1979 and 1991 that the individual has valid responses. For D proxy abbreviations, see Table 3.1.

Conclusion

This thesis has attempted to uncover some empirical relationships which have not been identified previously. The empirical work has not attempted to estimate precise magnitudes. Rather, the point has been to use admittedly noisy proxies to try to find broad relations in extant data. This thesis has also attempted to be innovative in its use of insights from outside of economics. Specifically, the first chapter developed several ideas which arose out of the ethnographic literature. In the last three chapters, the ethnographic insights were combined with traditional economic theory to develop several testable hypotheses.

The empirical work included strong evidence that increases in AFDC imply decreases in disadvantaged male labor supply. There was also some weaker evidence that increases in AFDC may in the long-run imply increases in disadvantaged male schooling. It was not possible to identify AFDC effects on male crime rates; it is hoped however that the chapter on crime will be useful to future researchers. For example, that chapter included some evidence which conflicts with a much-used theoretical approach to crime: the chapter showed that

some criminal activity may be included as labor supply in extant data sets.

Patterson (1986) argued that throughout American history, people have attempted to distinguish between the "deserving" and the "undeserving" poor. As a result, American social policy has always been <u>categorical</u>: legislators have attempted to target it towards those deemed "deserving." One of the primary lessons of this thesis is that economists should not accept categorical eligibility requirements at face value. For the variety of reasons developed in the text (marriage rate effects, effects through social embeddedness, etc.) welfare programs may noticeably affect people outside of the targeted categories. In the 1980s, at least, this appears to have been the case.

Appendix 1: Background Information on AFDC.

"AFDC" refers to the U.S. transfer program "Aid to Families with Dependent Children". It was established by the Social Security Act of 1935.

For a family to be eligible for AFDC, it must satisfy four conditions. The first three conditions are straightforward: (1) the family has to have a low level of assets; (2) the family has to have low income; and (3) the family has to contain children.

The fourth condition is less straightforward: (4) the family must be deprived of the support of one parent. This fourth condition is known as the "categorical deprivation" requirement. For the vast majority of AFDC families, categorical deprivation is officially satisfied because one parent is absent from the family. That is, because of the categorical deprivation requirement, the vast majority of AFDC families are officially single-parent families.

There are only two ways in which two-parent families can satisfy the categorical deprivation requirement. First, if one parent is medically judged to be physically or mentally incapacitated, then the family qualifies. Second, there is a small segment known as "AFDC-UP" (Unemployed Parent) in which

two-parent families can qualify _if the primary earner has a history of employment but is currently unemployed_.

Between 1967 and 1979, the segment was known as AFDC-UF (Unemployed Fathers). During that period it was required that the primary earner had to be the husband. (It was not enough for the wife to have a history of work--the husband had to have a history of work). The courts ruled this unconstitutional in 1979.

Needless to say, policy-makers are very averse to legislating any aid to able-bodied men without work histories. The results in the text suggest that legislators may only be _nominally_ (not _really_) able to restrict aid in this way.

One reason AFDC-UP is relatively small because it counts a parent's potential Unemployment Insurance (UI) income as it would count any other income. If the primary earner in the family has a history of work and is currently unemployed, then he or she will often be eligible for UI. And if the unemployed parent is in fact eligible for UI, then AFDC-UP will only _supplement_ the UI income. This supplement will often not be large enough to justify the costs of applying for welfare. AFDC-UP will pay the entire AFDC guarantee, only if the parent has exhausted his or her unemployment benefits.

Before 1991, states were not required to participate in AFDC-UP. The Family Support Act of 1988, however, required that all states offer AFDC-UP by 1991. Figure A1 plots the AFDC-UP caseload as a percentage of the total AFDC caseload

from 1970 to 1992.[66] The AFDC-UP caseload never reached 8% of the total caseload, and there is no evident upward trend in its importance through the 1980s. In 1991 there were 268,000 AFDC-UP cases.[67] In that year, there were 24,397,000 married couples with children under 18.[68] Thus, about 1% of married couples with children received AFDC-UP.

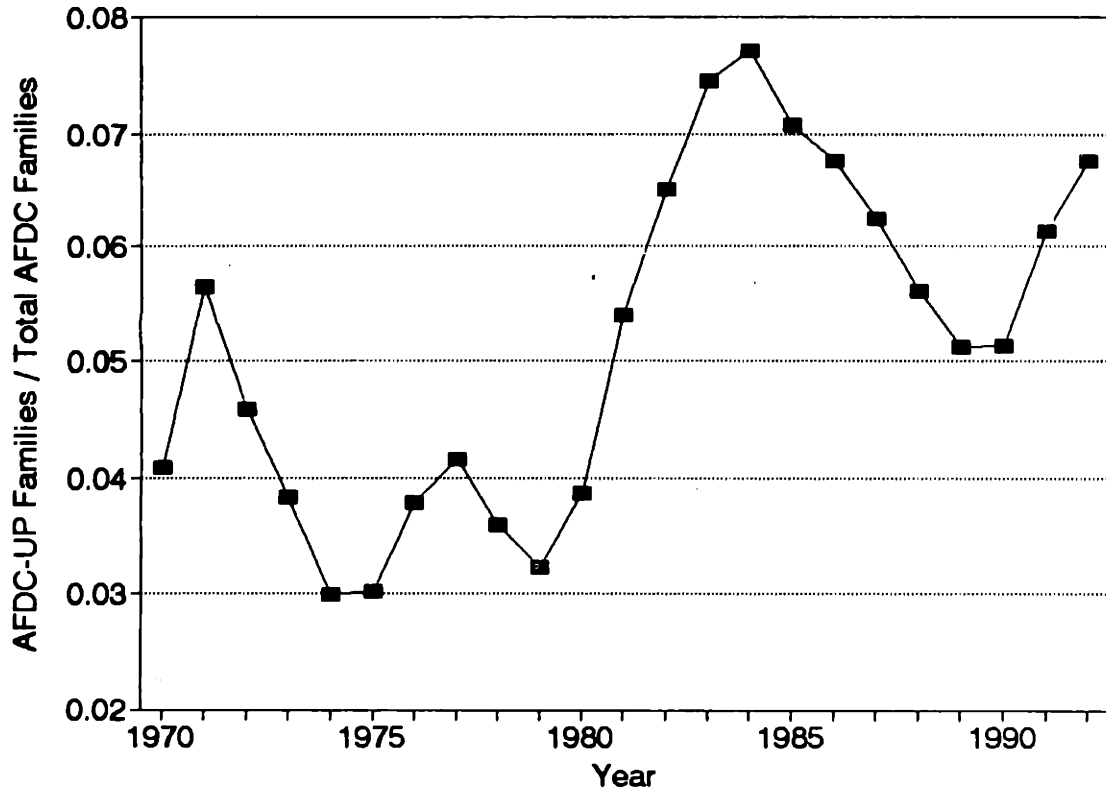**Figure A1**
UP Caseload as a % of Total; 1970-1992

Table A1.1 classifies 1991 AFDC families by reason of deprivation.[69]

Table A1.1 Reason For Deprivation of Youngest Child AFDC Families 1991	
Absence	87.5%
Incapacitation	3.3%
Unemployment (AFDC-UP)	5.7%

The table confirms that the vast majority (at least 87.5%) of AFDC families are officially single-parent families. Because of its small size, I will largely ignore the AFDC-UP program in the text.

Since the vast majority of adult recipients have been officially single-parents, it follows that the vast majority of adult recipients have been women.[70] In total in 1991, only 11% of official adult AFDC recipients were men.[71] Most of these were the fathers in two-parent AFDC families. Presumably the remainder were single-father recipients.[72]

The nominal tax rate for AFDC recipients is essentially 100%. It was decreased to 33% in 1967, and increased back to 100% in 1981.

States set their own benefit levels, and there is substantial variation across states and across time in AFDC generosity.

Appendix 2: A Literature Review for Chapter Two

A first general comment is that for some time, the conventional wisdom was that female labor supply was more wage and property elastic than male labor supply. Mroz (1987) however argues that reasonable estimates of female labor supply elasticity are not necessarily larger than those for men. Hoynes (1993) also found similar labor supply responses for men and women under AFDC-UP. It is by no means necessary that AFDC effects on men be trivial.

Concerning the effect of transfers on labor supply, there have been two major literature reviews: Danziger et al. (1981) and Moffitt (1992). Danziger et al. review several studies of the AFDC effect on women's labor supply. There was general agreement among those studies that increases in the AFDC income guarantees decrease the employment of women. There was, however, much disagreement on the specific magnitudes of the effect.

Moffitt (1992) reports that there was little work on AFDC labor supply effects since the Danziger et al. review. There were a few exceptions which were studies of female labor supply; these studies did not overturn the earlier work. I

know of four studies which have related AFDC to men's labor-market status.

First, Hosek (1980) studied participation in the AFDC-UP program. He administered the CPS questionnaire to a sample of AFDC-UP families in Los Angeles, Chicago, and Detroit in 1977. He then merged this data with the 1976 CPS observations from these same cities. He estimated weighted welfare participation probits. The explanatory variable of interest was AFDC plus Food Stamps benefits given state and family size.[73] Hosek's major result was that participation in the AFDC-UP program does increase with the size of the AFDC-UP benefit. He did not estimate labor supply effects.

Second, Lerman (1986) investigated the effect of AFDC on young men (aged 16 to 24) who continue to live in their parental, AFDC-supported household. That is, he investigated labor supply among the working-aged sons of AFDC single-mothers. He found that these young men were relatively unlikely to be either employed. As he notes, however, he is unable to distinguish whether AFDC itself causes the adverse outcomes, or whether omitted variables are causing the partial correlation.

A third relevant study is the study by Moffitt (1990a). Extending the labor supply argument in the text, if AFDC lowered marriage rates, and if marriage caused men to receive higher wages, then an increase in AFDC would lower male wages

through its effect on marriage rates. Moffitt (1990a) tested this hypothesis.

Specifically, Moffitt tested whether AFDC effects (through marriage) might possibly explain some of the well-known, recent increase in wage inequality. Moffitt's empirical results decisively reject this hypothesis. AFDC plays (at most) a small role in explaining decreasing marriage rates, and the decrease in marriage rates in turn can explain only a trivial portion of the increase in wage inequality.

Fourth and finally, Hoynes (1993) used a sample of two-parent families to estimate a structural model of the labor supply effects of AFDC-UP. She found that AFDC-UP significantly reduces labor supply among two-parent families. For example, one experiment she considered was to increase AFDC benefits by about $100 per month. Her estimates imply subsequent average labor supply reductions of .5 hours per month for men, and .7 hours per month for women. That is, if the increase affected 5% of the population, then for those affected, male labor supply would fall by an average of 10 hours per month, and female labor supply would fall by an average of 14 hours per month.

Appendix 3: Data Appendix

As noted in the text, the primary data set used in the empirical work in the text is the National Longitudinal Survey of Youth (NLSY). In this appendix, I first describe the NLSY generally. I then describe some data which have been merged into the NLSY for the purposes of this thesis. Finally, I discuss how the sample was pared down and also how it was "cleaned" for the purpose of estimation.

The NLSY is funded by the U.S. Department of Labor.[74] It is administered by the U.S. Bureau of Labor Statistics. The Center for Human Resource Research of the Ohio State University manages the survey, and NORC at the University of Chicago conducts the data collection.

The NLSY is a nationally representative sample of 12,686 men and women who were between the ages of 14 and 21 in 1979. It is a "micro" data set in that an observation corresponds to an individual person. The original sample was based on 8,770 households: 2,862 households included several respondents.

The NLSY is a "panel" data set in that individuals are followed through time. Specifically, the NLSY aims to interview each individual once per year. The extract in this thesis includes the years through 1991. In all years except

1987, the interviews were personal interviews. In 1987, they were conducted by telephone.

Although the NLSY attempts to interview everyone, of course there are people who cannot be located or who refuse to be interviewed. If we define the "retention rate" as the percentage of respondents eligible for interview who were in fact interviewed, then although retention rates have declined over time, they have generally remained above 90%. As examples: in 1983, the retention rate was 96.3%; in 1987 it was 90.3%; and in 1991, it was 90.5%.

The NLSY originally included three independent probability samples: (1) a cross-sectional sample of the non-institutionalized civilian segment of the population aged 14 to 21 in 1979; (2) a supplemental sample which over-represents Hispanics, blacks, and disadvantaged white youth; and (3) a military sample aged 17 to 21. In 1979, the cross-sectional sample represented 48% of the observations; the supplemental sample represented 42%; and the military sample represented 10%.

Since 1979, there have been two changes in the populations surveyed. First, in 1985 about 85% of the military sample was dropped. Second, in 1991 the non-black, non-Hispanic members of the supplemental sample (i.e., the disadvantaged whites) were dropped.

My extract includes only men. There were a total of 6,403 male respondents in the 1979 interview: 47% from the

cross-sectional sample, 40% from the supplemental sample, and 13% from the military sample. Breaking the different 1979 male sub-samples down by race: in the cross-sectional sample, 81% were white; in the supplemental sample, 29% were white (these were dropped in 1991); and in the military sample, 74% were white.

Because the NLSY contains state codes, it is straightforward to merge state characteristics into the data set. The regressions in the text employ six such state characteristics which have been merged in.

First, as noted in the text, the empirical work uses the AFDC "guarantee" for a family of four. These figures were taken from various issues of the "Green Book." This is an annual volume written for the benefit of the Committee on Ways and Means of the U.S. House of Representatives. Its official title varies slightly from year to year; in earlier years, it was titled Background Material and Data on Programs Within the Jurisdiction of the Committee on Ways and Means. In later years (since 1990) it has been titled Overview of Entitlement Programs.

A second yearly state characteristic which is used in the text is the maximum weekly Unemployment Insurance (UI) benefit. For 1979 and for 1982 through 1991, this data is obtained from various issues of the Green Book. For 1980 and 1981, the data is obtained from U.S. Department of Labor pamphlets entitled "Significant Provisions of State

Unemployment Insurance Laws." The 1980 data come from the version of this pamphlet dated July 6, 1980. The 1981 data come from this pamphlet dated July 5, 1981.

A third yearly state characteristic which is employed in the text is the average hourly earnings for production workers on manufacturing payrolls. This data is taken from various issues of the publication Employment and Earnings which is issued by the Bureau of Labor Statistics.

Fourth, the empirical work uses information on which states offered AFDC-UP in each year. For the years 1980 to 1991, this data comes from the Green Book. For 1979, the data comes from a pamphlet entitled "Public Assistance Statistics" which was issued by the U.S. Department of Health and Human Services.

Fifth, the empirical work uses the Consumer Price Index (CPI) for "all items" by four regions: Northeast, North Central, South, and West. This data is taken from the 1989 Statistical Abstract of the United States table 763, and the 1992 Statistical Abstract of the United States, tables 743. The base years are 1982 through 1984; i.e., the 1983 CPI is close to 100. It is important to note that the regional CPI does not capture variation in cost of living across the regions. Instead it is meant to capture regional differences in inflation rates. In other words, the CPI for each region is set equal to 100 in the base years.

Sixth and finally, the empirical work in Chapter Four includes as a regressor state-level index crime rates. This data was taken from <u>Uniform Crime Reports</u>, various years. <u>Uniform Crime Reports</u> is published annually by the Federal Bureau of Investigation. The specific variable used is the number of index crimes per 1,000 people in the state.

In the process of completing this study, although generally attempts were made to interfere as little as possible with the data, there were some instances in which the data was "cleaned" to correct apparent imperfections. Some of the more important examples of this cleaning will now be discussed. If any reader has data questions beyond the issues dealt with here, then he or she may consult the computer programs which were used. These programs will be made available upon request from the author.

Undoubtedly the most contentious part of the cleaning process involved the "family income" variable. The goal for this variable was to get a proxy for the amount of income accruing to members of the man's family other than himself. The variable which was in fact created however is flawed in several respects--as this appendix will show. Before discussing the flaws, however, it should be noted that in the text all important results are shown not to depend on this variable. Specifically, the text attempts to establish that all important results remain the same when the created family income variable is omitted from the specification. Having

said that, it still may be useful for future researchers to know how the variable was constructed.

To start with, the variable which was created was constructed around an NLSY variable called "net family income", which is the sum of the individual's own income and the income of all the members of the family. The first and most important problem in creating the variable was that many respondents were simply unable to give a valid answer for their net family income. In later years, the NLSY coded such people with net family incomes equal to negative two: "don't know". In fact, in some years over 1,000 of the respondents are coded in this way. In the earlier years of the survey as many are coded as "invalid skips": over a thousand are coded in this way both in 1980 and 1981.

Because it would be unacceptable to just drop over 1,000 observations each year, each of these observations was given a value of zero for their net family income. This was adopted as the least of alternative evils; one justification which has been offered is the (admittedly weak) one that if individuals do not know the income accruing to their family, then they are not basing economic decisions on it. They might be instead thought of as assuming that for the purposes of their economic decisions, the amount of income going to their family members is approximately zero.

The next step in the variable creation was to create a variable representing the total amount of income which the

individual himself earned. This was then subtracted from the net family income variable. In any case that the resulting sum was negative, the family income variable was set equal to zero.

In doing this, there appeared to be some mistakes in the data in 1989, and NLSY material verified this. The data was supposed to be entered in terms of <u>dollars</u>. That is, five thousand dollars should have been entered as "5000." In 1989, however, it appears that some figures were incorrectly entered in terms of dollars and cents. Five thousand was entered as "500000." The problem is noticeable if you look at the income data for these individuals over the entire panel. There appears to be an income spike of about two orders of magnitude for these individuals in the year 1989. To handle this, 1989 income values were divided by 100 if they exceeded 300000.

The problem appears in some years for the hourly wage data, and this data was therefore edited as well. Specifically, mean hourly wages apparently double between 1986 and 1987. In nominal terms, they go from 7.4 to 19.5. To handle this, if hourly wages exceeded 100 in 1987 or subsequent years, they were scaled down by a factor of 100. In the case of 1987, this results in 15 changes, and the subsequent mean hourly wage is 8.6 dollars per hour.

In addition, the wage data was cleaned by setting to missing any hourly wages which either exceeded seventy-five dollars per hour or fell below one dollar per hour.

Another important part of the cleaning process was that the military subsample was not included in the Chapter Two regressions. There were a couple of reasons for this. First of all, as noted above, 85% of the sample was dropped by the NLSY in 1985 anyway. Second, the NLSY assigns members of the current active military non-valid responses for some important regressors. For example, the NLSY assigns the value of negative four (valid skip) to military members for the urban indicator.

Two final elements of the cleaning process which should be mentioned are that: (1) the labor supply variable was set equal to missing if more than 10% of the individual's work history was incomplete; and (2) if the schooling variable was not valid in any year, then it was set equal to its value in the last year with a valid value.

We can turn now to explaining the sample sizes for the different chapters. First, the sample size for Chapter Two will be justified. Then, since Chapters Three and Four include essentially the same sample, the sample size for Chapter Three will be justified. Variation in sample sizes within any chapter depend just on variation in the number of observations with missing values for the regressors in a given specification.

Thus we can first consider Table 2.1 in Chapter two. The largest sample size in that table is 42,544. This sample arose in the following way. There were 5,579 men in the

cross-sectional and supplemental samples; each was observed at most 13 times (once in each year between 1979 and 1991). Thus we start with at most 72,527 observations. The largest number of dropped observations occurs because Chapter Two is only concerned with men aged twenty-one to thirty-four. After dropping observations on people under twenty-one, there are at most 53,033 observations remaining. Just under 10% of these potential observations were not observed by the NLSY. Taking out people who were not interviewed, there are 47,744 observations remaining.

After this, observations are dropped only because they did not include valid responses for different regressors in the empirical models. Specifically, 2,356 were dropped for not having valid state codes; 1,372 were dropped for not having a valid annual labor supply variable; 1,304 were dropped for not having a valid urban indicator; 154 were dropped for not having a valid schooling variable; fourteen were dropped for not having a valid local unemployment rate. This leaves the 42,544 observations found in Table 2.1.

We can now turn to the sample size in the Chapter Three regressions. In fact, the Chapter Three empirical work is structured so that all sample size variation comes from missing values for the different regressors. There were 4,471 men interviewed in 1991, and some of the variables in Table 3.1 contain 4,471 valid responses.

Appendix 4: Social Isolation for the Advantaged versus Disadvantaged: Some Initial Tables Using the Background Proxies.

This appendix uses the NLSY to compare advantaged and disadvantaged means of two variables which proxy degree of isolation.

The first variable I use is an indicator variable in the 1991 survey. The variable is one if the respondent says that he lives with a relative other than his spouse and children. A man who lives with his sister receives a one for this variable. A man who only lives with his spouse and children receives a zero. The sample mean for the variable is .18. Table A4.1 breaks the means down for sub-populations of advantaged and disadvantaged. I use the same background variables as in Chapter One to proxy disadvantaged status. The table shows that for all background variables, disadvantaged men are more likely to live with "other relatives" than are advantaged men. This supports the hypothesis that social isolation is a normal good.

Table A4.2 reports analogous results for a second isolation proxy. This proxy is a 1979 variable. Not all respondents have coded responses. To have a coded response,

the individual had to still be living with his parents. (In 1979, NLSY respondents were aged fourteen to twenty-one.) If the respondent was living at home, then one of his parents was asked whether their family received income from friends or relatives. If the family did receive income, then they receive a 'one' for the variable; otherwise the variable value is zero.

Table A4.2 indicates that advantaged respondents were more likely to live in families which reported receiving income from friends or relatives. Thus, this variable does not support the hypothesis that disadvantaged communities involve more resource-sharing than advantaged communities.

One explanation is that disadvantaged communities may involve more in-kind sharing as opposed to income sharing. If the question asked about resources generally instead of income, then the results might change. A second explanation is that disadvantaged families are more likely to receive public assistance. Conditional on receiving income from friends and relatives, then, a disadvantaged family may be more likely to deny this aid when questioned by an official interviewer.

Table A4.1

Percentage Men (Aged 26-33) Reporting
that they live with Relatives other than Spouse/Children

By Disadvantaged Status

Disadvantaged Proxy	Disadvantaged Mean	Advantaged Mean
Mother's Schooling less than 12	.243	.150
No Employed Man in Age 14 Household	.245	.165
No Magazine in Age 14 Household	.243	.149
No Newspaper in Age 14 Household	.236	.169
No Library Card in Age 14 Household	.205	.171

Variable is reported in 1991; sample mean is .180; 1991 sampling weights are used.

Table A4.2

Percentage Families Reporting Income
from Friends or Relatives

By Disadvantaged Status

Disadvantaged Proxy	Disadvantaged Mean	Advantaged Mean
Mother's Schooling less than 12	.013	.029
No Employed Man in Age 14 Household	.017	.025
No Magazine in Age 14 Household	.014	.027
No Newspaper in Age 14 Household	.014	.025
No Library Card in Age 14 Household	.013	.027

Variable is reported in 1979; sample mean is .023; 1979
sampling weights are used.

Endnotes

1. For readers who are not familiar with AFDC, Appendix 1 describes the program generally.

2. Table 612, page 372, 1991 Statistical Abstract.

3. Appendix 2 reviews the empirical literature.

4. See Appendix 1.

5. AFDC-UP is described as part of the general discussion of AFDC in Appendix 1. It is a politically prominent segment of the program which averaged between 3% and 8% of the caseload during the 1980s.

6. For readers unfamiliar with the "categorical deprivation requirement," its basic features are reviewed in Appendix 1.

7. quoted in Moynihan (1986) p. 210.

8. The terminology of the regulations varied across states. Patterson (1986) p. 88 gives three examples: "Arkansas cut off aid to mothers engaged in a 'nonstable, nonlegal union'; Michigan, to families with 'male boarders'; Texas to 'pseudo-common law marriages.'"

9. [392 U.S. 309]. See Davis (1993) for a description of the legal tactics used in this case and also in other welfare-related litigation.

10. Piven and Cloward also argued that tactics such as midnight raids were effective in humiliating recipients, and that this humiliation was useful in supporting the work ethic in the country.

11. Bell (1965) for example argues that state officials twisted the meaning of the categorical deprivation requirement in order to maintain Federal approval while satisfying their taste for discrimination against blacks and illegitimate children. The legislators themselves sometimes argued that they did not want boyfriends in AFDC households because it was immoral and would therefore badly influence the children in the household.

12. Some caveats are required up-front for any discussion of current rules. First, AFDC laws vary across states. Second, implementation by caseworkers varies even within states. Not

only do caseworkers have discretion, but there is much caseworker turnover and little caseworker training.

13.Jencks (1992, especially pages 80-81) makes a similar point.

14.The experiments contrasted a negative income tax to the prevailing AFDC regime; they did not contrast a negative income tax to a regime in which there was no public aid.

15.See also Gardiner and Lyman (1984).

16.Branch, Riccio, and Quint (1984) p. 60. Also quoted in Loury (1986a and 1986b).

17.cf Harrison (1977) who distinguishes four basic sources of income to a disadvantaged stratum he calls the "periphery" of the economy. His four sectors: the secondary labor market, the welfare sector, the training sector, and the irregular economy.

18.See Edin (1989, 1991, 1993a, 1993b); also see Edin and Jencks (1992). Edin (1989) contains a helpful review of some of the ethnographic studies mentioned in this chapter.

19.In fact, she and her colleagues have recently increased the sample to cover 500 welfare mothers across 5 sights nationally. This expanded research is not yet available at the time of my writing. Of the works which I cite in this essay, only the 1993a article is based on the expanded data set.

20.Adapted from Edin and Jencks (1992), Figure 6.1, p. 208.

21.See also Rainwater (1970) (e.g., p. 233) for arguments which complement those of Gans.

22.Sawhill (1988) makes this point in a different context: "Privacy and autonomy, like leisure, are normal goods." (p. 1080) Her point was that to compare standard of living across time, you should give a positive weight to later periods in which people are more likely to have their own household.

23.See also Liebow for evidence on extensive mobility in low-income communities.

24.Holzer (1990) reviews both the supply-side and the demand-side literature.

25.Table 12, page 12, 1991 _Statistical Abstract_. These are the ages of the men in the Chapter Two empirical work. See Appendix 3.

26. Reviews of this empirical literature include Sawhill (1988), Moffitt (1992), and Murray (1993). The most influential study is Ellwood and Bane (1985) which Moffitt (1989) analyzes.

27. As Becker notes, this might arise because of discrimination against women in the market sector. Alternatively, it could result from basic biological differences--e.g., women are able to bear and nurse children whereas men are not. Any basic biological differences might be magnified by human capital investments (women might invest in human capital which raises productivity in the home and men might invest in human capital which raises productivity in the market).

28. Blank (1985) showed that the cross-state AFDC variation (along with cross-state labor market variation) causes non-trivial behavioral differences across states for similar people.

29. Source 1992 Greenbook, pages 599 to 600, Table 8.

30. This interaction strategy (using the parameter on the interaction term to identify the effect) is by no means novel. In the AFDC context, I am following the work of Hutchens (1979) and especially Ellwood and Bane (1985). Moffitt (1989) contains an illuminating discussion of those studies.

There are also of course studies which employ this interaction approach in contexts other than AFDC. The discussion in the text benefits in particular from the discussion of the "DDD" estimator in Gruber (1992). Gruber uses the interaction strategy to estimate the wage effect of laws which mandate maternity benefits. He uses time dummies; his treatment group is young married women; and his experimental variable captures the passage of a maternity benefit law.

31. These proxies are obviously noisy measures of the AFDC-affected population. With these "disadvantaged" definitions, there will be some disadvantaged men who are not affected by AFDC, and there will be some advantaged men who are affected by AFDC. This noise creates the problem of low statistical power in the tests. Even if AFDC affects men, these tests may not be able to reject the null of no effect. γ_A may pick up some real effects; and there may be little precision in γ_{AD} because of the presence of disadvantaged men who are not affected by AFDC.

Thus, there would be little informative value if the results of the test were that the null of $\gamma_{AD} = 0$ could not be rejected. In fact, however, the tests are informative because it turns out that in all but one specification, we are able to reject the null of $\gamma_{AD} = 0$ at standard test sizes. That is,

it is possible to isolate the male labor supply effect even using quite noisy proxies.

32. The data is explained in the next section.

33. The micro data creates a statistical problem, however, given that the explanatory variable, A, is an aggregate variable. (see, e.g., Moulton (1990)). It will be shown below that the significance tests in this chapter are not compromised either by state shocks or state-year shocks.

A related statistical issue is the error correlation among the various yearly observations of a given individual. The t-statistics below will be robust to such cluster sampling.

34. Appendix 3 elaborates on much of the information in this paragraph. It also shows how the data was pared down to the sample sizes found in the empirical work.

35. viz., that there have been about 3.8 million AFDC families per year and that there were about 30 million men aged 21 to 34 in 1989.

36. All remaining tables in this chapter will include both state dummies and the A*D interaction.

37. Thus, schooling un-interacted does not suffice as an instrument. (Pencavel, 1986, p. 67)

38. Plotnick and Winters interpret this as indicating that as wage levels rise, voters in a state become better off, and as they become better off they choose to offer more charity to the disadvantaged in their state.

39. See the data appendix.

40. See Pencavel (1986) for one pessimistic view.

41. Having introduced the disadvantaged background proxies in this chapter, it may be interesting for readers to see whether these proxies can support the hypothesis introduced at the end of Chapter One: that disadvantaged people are less socially isolated than advantaged people. Appendix 4 provides two tables of means which address this issue. It is offered as a possible start for future research.

42. When the economist takes the extreme position that the negative side-effects actually _dominate_ (so that the ultimate program effect is negative), Hirschman says the economist is employing a "perversity" thesis.

43.Of course this is not the only counter-example; there are other studies which have found positive policy side-effects. Two good examples are Gruber (1994) and Currie and Cole (1992). Gruber estimates the consumption smoothing benefits of unemployment insurance. Currie and Cole estimate some positive relationships between AFDC participation and infant birth weight.

44.As noted in Appendix 2, Lerman also found that family welfare-receipt lowers the expected probability of employment.

45.See, e.g., Becker and Tomes (1986), Becker and Murphy (1988), Loury (1981), and Loury (1986b).

46.Thus, this chapter is somewhat more general than the last. Chapter Two only dealt with men aged 21 through 34. This chapter will consider effects through men aged as young as 14 (the youngest age observed in the NLSY).

47.Louisiana justified the move by arguing that the mothers of these children were not providing morally suitable homes. Primarily, the mothers were carrying on relations with men. (Bell, 1965)

48.Patterson, 1986, p. 106.

49.Some of the variables are measured in units different from the units in Chapter Two. See Table 3.1.

50.It is also worth repeating (from Chapter One) that there is no presumption here that any of the proxies perfectly captures "disadvantaged"--as if there were such a Platonic form. All proxies are recognized to be noisy. That is, for each specification, the group characterized as "disadvantaged" includes some who are not affected by AFDC; likewise it excludes some who are affected. This noise reduces the statistical power of the tests. Statistically insignificant results are not particularly interesting.

51.In my discussions of the Probit model both in this chapter and the next, I will use the term "t statistic" to refer to the ratio of the parameter estimate to its estimated standard error.

52.One possible explanation worthy of further research is that school quality is too low for blacks in high welfare-receipt neighborhoods. The marginal return to schooling for truly disadvantaged blacks may not warrant further investment (even when the income pressure on their community declines as a result of an AFDC increase).

53. This is the median estimate because the "mother's schooling" and "no magazine" proxies are averaged over larger portions of the population. For non-blacks, 27% and 29% respectively are disadvantaged according to these proxies.

54. As in, e.g., Freeman (1992), Holzer (1990), Juhn (1992), and Welch (1990).

55. In the 1980 NLSY this is Question 1 in Section 7 of the survey.

56. See, for example, Williams (1989) or Taylor (1990).

57. (aged at least eighteen)

58. In a survey of probationers who admitted selling drugs in the six months prior to their probation, Reuter et al (1990) found that 82% of them also reported receiving some form of legitimate income during the same period.

59. (some of which may be thought of as non-employment, and some of which could lead to incarceration)

60. DiIulio (1989) makes a similar point.

61. My source for much of the following paragraph is the useful discussion in Bogess and Bound (1993). For additional information, see that article. See also the data appendix below.

62. As I will discuss below, some specifications will include fixed state effects.

63. Washington DC is the fifty-first category. Of course, not all states are represented in the 1991 sample. The indicators for the unobserved states are of course dropped from the regressions.

64. and also because the AFDC variable is averaged only across those years in which the individual is interviewed.

65. There is one remaining table in the chapter. Table 4.6 shows that the results do not depend on the specific family income variable which was created for this thesis.

66. The source for Figure 1 is Table 24, p. 685 in the 1993 Green Book.

67. 1993 Green Book, Table 24.

68. 1992 Statistical Abstract. Table 67, p. 53.

69.Source for Table 1: Administration for Children and Families (undated), p. 7. For 3.5% of families, the reason for deprivation of the youngest child is unknown.

70.Most single parents are women. Specifically, in the entire U.S. population in 1990, 85% of single parents were women. In 1980, the figure was 90%. Source: 1992 <u>Statistical Abstract</u>, p. 53, Table 67.

71.Administration for Children and Families (undated), p. 2.

72.Nord (1990) estimated that single father families constituted 1.9% of the 1988 AFDC caseload.

73.The other controls were standard except that Hosek did not include site dummies. Instead, he reported that when he had included site dummies, they were not jointly significant.

74.Much of the general NLSY information in this appendix comes from the 1992 <u>NLS Handbook</u> published by the Center for Human Resource Research at The Ohio State University.

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